Dissertations in Economics 2024:4

Stockholm University

Essays in Labor, Public, and Health Economics

Miika Päällysaho

Doctoral Thesis in Economics at Stockholm University, Sweden 2024

Essays in Labor, Public, and Health Economics

Miika Päällysaho

Academic dissertation for the Degree of Doctor of Philosophy in Economics at Stockholm University to be publicly defended on Friday 11 October 2024 at 09.00 in Hörsal 4, Albano Hus 2, Vån 2, Albanovägen 18.

Abstract

Labor Market Returns and the Evolution of Cognitive Skills: Theory and Evidence
A large literature in cognitive science studies the puzzling "Flynn effect" of rising fluid intelligence (reasoning skill) in rich countries. We develop an economic model in which a cohort's mix of skills is determined by different skills' relative returns in the labor market and by the technology for producing skills. Combining Swedish data from exams taken at military enlistment with earnings records, we document an increase in the labor market return to logical reasoning skill relative to vocabulary knowledge. The estimated model implies that changes in labor market returns explain 37 percent of the measured increase in reasoning skill, and can also explain the decline in knowledge. A survey of parents as well as analyses of school curricula and occupational characteristics show evidence of increasing emphasis on reasoning relative to knowledge.

Adoption of Medical Innovations Across Hospitals and Socioeconomic Groups: Evidence from Sweden
We study the adoption of innovations in the context of healthcare using Swedish data on 58 novel medicines for 47 conditions. We find significant variation in adoption rates across hospitals and socioeconomic groups, with a positive correlation between patient income rank and adoption rates. Using a novel antiplatelet drug as a case study in a back-of-the-envelope calculation, we find that equalizing adoption rates between top and bottom income deciles could have reduced the gap in 12-month survival rates by 1.2 percent among first-time heart attack patients.

Unemployment Insurance Generosity and Health: Evidence from Sweden
We study how the generosity of unemployment insurance (UI) affects benefit recipients' healthcare use using Swedish administrative data. Our measure of healthcare use accounts for inpatient and outpatient care visits and drug purchases and measures full system costs, not just out-of-pocket expenses. Exploiting caps in the amount of daily benefits in a regression kink design, we find little evidence that more generous unemployment benefits affect the total costs of recipients' healthcare use. This finding holds across gender and age groups as well as short-term and long-term benefit recipients.

Family-Level Stress and Children's Educational Choice: Evidence from Parent Layoffs
We analyze the effect of parental layoffs on the educational outcomes of their children. Using Swedish administrative data, we exploit shocks to firm labor demand to estimate the age-specific impact of parental layoffs on high school graduation rates. We find that parental layoffs have a significant impact on high school completion rates and that the effect is strongest in the year of application to high school (age 15). We then exploit variation in the fine timing of the layoff to link this effect to a short window before a student chooses where to apply to high school. A parental layoff in the month before the school choice deadline decreases the likelihood that the child will finish high school on time by 9 percentage points relative to a layoff in the same school semester but after the deadline. The effect is higher for families with less information about high school choice, consistent with the hypothesis that family stress, even if temporary and without financial effects, may disrupt educational choice.

Keywords: *skill investment, human capital, innovation, diffusion, health, social insurance, unemployment, layoffs.*

Stockholm 2024
http://urn.kb.se/resolve?urn=urn:nbn:se:su:diva-232838

ISBN 978-91-8014-911-2
ISBN 978-91-8014-912-9
ISSN 1404-3491

Stockholm University

Department of Economics

Stockholm University, 106 91 Stockholm

ESSAYS IN LABOR, PUBLIC, AND HEALTH ECONOMICS

Miika Päällysaho

Stockholm University

Essays in Labor, Public, and Health Economics

Miika Päällysaho

ISBN print 978-91-8014-911-2
ISBN PDF 978-91-8014-912-9
ISSN 1404-3491

Printed in Sweden by Universitetsservice US-AB, Stockholm 2024

Äidille ja isälle
Isovanhempieni muistolle

Doctoral Dissertation
Department of Economics
Stockholm University

Abstracts

Labor Market Returns and the Evolution of Cognitive Skills: Theory and Evidence

(with Santiago Hermo, David Seim, and Jesse M. Shapiro)

A large literature in cognitive science studies the puzzling "Flynn effect" of rising fluid intelligence (reasoning skill) in rich countries. We develop an economic model in which a cohort's mix of skills is determined by different skills' relative returns in the labor market and by the technology for producing skills. We estimate the model using administrative data from Sweden. Combining data from exams taken at military enlistment with earnings records from the tax register, we document an increase in the relative labor market return to logical reasoning skill as compared to vocabulary knowledge. The estimated model implies that changes in labor market returns explain 37 percent of the measured increase in reasoning skill, and can also explain the decline in knowledge. An original survey of parents, an analysis of trends in school curricula, and an analysis of occupational characteristics show evidence of increasing emphasis on reasoning as compared to knowledge.

Adoption of Medical Innovations Across Hospitals and Socioeconomic Groups: Evidence from Sweden

(with Fabian Sinn)

We study the adoption patterns of innovations in the context of healthcare using Swedish administrative data. For a set of 58 novel medicines related to 47 health conditions, we document sizable variation in adoption rates across hospitals and socioeconomic groups. For example, at the end of our analysis period, the adoption rate of novel medicines for hospitals at the 90th percentile was roughly three times as large as the adoption rate for hospitals at the 10th percentile. We also document a positive association between the patient's income rank and the adoption rate of novel medicines for a diverse set of health conditions ranging from cardiovascular diseases to lung diseases to ADHD.

Using a novel antiplatelet drug as a case study in a back-of-the-envelope calculation, we find that equalizing adoption rates between top and bottom income deciles could have reduced the gap in 12-month survival rates by 1.2 percent among first-time heart attack patients over the time period we study. We find no effect from including new medications in regional guidelines or from better hospital management practices.

Unemployment Insurance Generosity and Health: Evidence from Sweden
(with Arash Nekoei and David Seim)
We study how the generosity of unemployment insurance (UI) affects benefit recipients' healthcare use using Swedish administrative data. Our measure of healthcare use accounts for inpatient and outpatient care visits and drug purchases and measures full system costs, not just out-of-pocket expenses. Exploiting caps in the daily benefit amount in a regression kink design, we do not find evidence that more generous unemployment benefits would affect the total costs of recipients' healthcare use. This conclusion holds across gender and age groups as well as short-term and long-term benefit recipients.

Family-Level Stress and Children's Educational Choice: Evidence from Parent Layoffs
(with Julia Tanndal)
We analyze the effect of parental layoffs on the educational outcomes of their children. Using Swedish administrative data, we exploit shocks to firm labor demand to estimate the age-specific impact of parental layoffs on high school graduation rates. We find that parental layoffs have a significant impact on high school completion rates and that the effect is strongest in the year of application to high school (age 15). We then exploit variation in the fine timing of the layoff to link this effect to a short window before a student chooses where to apply to high school. A parental layoff in the month before the school choice deadline decreases the likelihood that the child will finish high school on time by 9 percentage points relative to a layoff in the same school semester, but after the deadline. The effect is higher for families with less information about high school choice, consistent with the hypothesis that family stress, even if temporary and without financial effects, may disrupt educational choice.

Acknowledgements

In this section of the dissertation, the author typically reflects on the struggles of completing a Ph.D., thanks those who supported them, and concludes that the journey was worth it in the end. The first two parts certainly apply, but I am unsure whether the third part does.

I entered the Ph.D. program passionate about research and eager to work in academia. Now, near the end of my studies, I am still passionate about doing research but also disappointed about my performance and painfully aware of how hard doing a Ph.D. can be for mental health. Looking back, I perhaps lacked the self-confidence, discipline, and drive to excel. I did not cope well with stress and anxiety, nor did I have a proper work-life balance.

It is sometimes said that a researcher needs to be their own harshest critic in order to do good work, but problems arise when one takes this advice too much to heart and starts applying it to other parts of life. I hope that, moving back to Helsinki, I will learn that there is more to life and one's self-worth than work.

I am indebted to my advisors, David Seim and Arash Nekoei. Both are exceptional, ambitious, curious, driven, and open-minded researchers —— true role models for any aspiring economist. I could not have wished for more encouraging people to guide me along the way. I realize I was a frustrating and disappointing student to advise, and I apologize for this.

I am grateful to my co-authors, Santiago Hermo, Arash Nekoei, David Seim, Jesse Shapiro, Fabian Sinn, and Julia Tanndal. I learned valuable skills and lessons on how to do research and collaborate from each of you. In particular, working with Santiago, David, and Jesse was the highlight of my studies and led to my first academic publication.

I thank Arizo Karimi at Uppsala University for providing constructive feedback on earlier versions of the dissertation chapters as the discussant at my final seminar. At Stockholm University, I thank several faculty members of the Department of Economics, the Institute for International Economic Studies, and the Swedish Institute for Social Research for useful feedback and discussions, including Markus Jäntti, Ann-Sofie Kolm, Matthew Lindquist, José Montalbán Castilla, Peter Nilsson, Sten Nyberg, Torsten Persson, Per Pettersson Lidbom, Jósef Sigurdsson, Peter Skogman Thoursie, David Strömberg, Joonas Tuhkuri, and Jonas Vlachos. I am also very grateful to many current and former administrative staff members at the Department of Economics, especially Anneli Eriksson, Anne Jensen, and Susanne Påhlsson.

I have had the privilege to meet several bright and talented current and former Ph.D. students along the way. Among others, I have enjoyed discussions related and unrelated to research with Ulrika Ahrsjö, Jakob Beuschlein, Chloé Castagnet, Cristina Clerici, Mohammadreza Farajpour Bibalan, Simon Handreke, Philipp Hochmuth, Roza Khoban, Viktoria Kozek, Chiara Latour, Tobias Lehmann, Anna Linderoth, Evelina Linnros, Jinci Liu, Charlotte Lucke, Isabella Maassen, Patrizia Massner, Filip Mellgren, Thomas Mikaelsen, Chloé Nibourel, Nicklas Nordfors, Jens Oehlen, Carolina Persson, Ossian Prane, Vaibhav Rathi, Jimena Romero Pinto, Oliver Schlenker, Mikko Silliman, Aapo Stenhammar, and Viking Waldén. I thank each of you.

A special *kiitos* to Maija Kaartinen, a talented macroeconomist, a dear friend, and fellow Finn in the Ph.D. program with whom I shared many pleasant conversations over lunch. A special *danke* to Dominik Wehr, a talented labor economist who had to endure me as an officemate during the stressful last stretch of putting this dissertation together. A special *tack* to Jens Wikström, a terrific health and public economist with whom I shared an office and many fun chats over the course of three years. All of them cheered me on when I was struggling, for which I am particularly grateful.

I am grateful to my friends outside academia, especially Leo Malinen, Visa Mikkola, and Eerik Tuovinen, for helping me unwind and talk about more mundane things. I also thank my sisters Essi and Satu, as well as my parents' adorable Jack Russell terriers Nemo and Nipa, for helping me forget about work during my visits to Finland.

Finally, I am beyond grateful to my mother Taru and father Juha. They were on the receiving end of far too many phone calls during which I vented my frustration over the poor progress in my research. They did not deserve the sleepless nights I caused them as a result. *Kiitos ja anteeksi. Toivon, että olette ylpeitä minusta.*

Miika Päällysaho
Stockholm, Sweden
August 27, 2024

Contents

Introduction

This thesis consists of four self-contained essays on topics in labor economics, public economics, and health economics. Chapter 1 studies the role of labor market returns in explaining cohort trends in measured cognitive skills among Swedish men. Chapter 2 studies the adoption of novel medicines in Sweden, with a focus on differences between hospitals and socioeconomic groups. Chapter 3 studies how the generosity of unemployment insurance affects the healthcare use of the unemployed. Chapter 4 studies how parental job loss affects children's educational outcomes. This introduction provides non-technical summaries of each chapter.

Chapter 1 – Can market incentives help explain trends in cognitive skills? A large literature, starting with Flynn (1984, 1987) and reviewed by Schaie et al. (2005) and Pietschnig and Voracek (2015), among others, has documented a large and secular increase in measures of cognitive skill across birth cohorts in developed countries. This "Flynn effect" has received widespread attention and has been given several explanations in cognitive science. Typically, these proposed explanations emphasize factors such as improvements in health and nutrition (e.g., Pietschnig & Voracek, 2015; Rindermann et al., 2017). To economists, such factors can be seen as increasing the *supply* of skill.

Yet, some scholars have proposed that changing societal *demands* could also shape cohort trends in cognitive skills (e.g., Dickens & Flynn, 2001). Even James R. Flynn (2018, p. 79) himself noted that *"[w]hen society asks us to increase our use of any skill over time, the brain responds"*, but it is unclear how important these demand factors are in practice.

The first chapter, **Labor Market Returns and the Evolution of Cognitive Skills: Theory and Evidence**, joint work with Santiago Hermo, David

Seim, and Jesse M. Shapiro, tackles this question by studying whether *market incentives*, as measured by returns to skill in the labor market, can help explain the cohort trends in cognitive skills.

Our analysis is based on administrative data with scores from standardized tests of cognitive skills, taken at military enlistment, matched to register data on earnings and socioeconomic background for the near population of Swedish men born in 1962–1975. The tests used to measure cognitive skills remained identical over our study period, which allows us to measure trends in cognitive skills across birth cohorts.

We begin by developing a novel economic model of investment in multidimensional skills. In the model, an individual's skills depend on an exogenous endowment (capturing supply factors, such as health) and investments in skills made before entering the labor market (by parents, children, and schools). Investments in skills, in turn, depend on the lifetime returns to these skills.

We take the model to the administrative data, focusing on two dimensions of cognitive skill: logical reasoning and vocabulary knowledge. The former is a typical measure of *fluid* intelligence (Carroll, 1993), for which cognitive scientists have documented particularly pronounced gains over time. The latter is a typical measure of *crystallized* intelligence (Carroll, 1993), for which observed gains have typically been less pronounced. We estimate the model under the assumption that determinants other than labor market returns have not disproportionately favored one skill dimension over the other.

Our analysis yields three main findings. First, consistent with existing research (Castex & Dechter, 2014; Edin et al., 2022; Markussen & Røed, 2020), we find that lifetime labor market returns to both types of cognitive skill fell across birth cohorts. However, the returns to vocabulary knowledge *relative* to logical reasoning fell by 46 percent. At the same time, performance in the logical reasoning test improved by 4.4 percentile points, while performance in the vocabulary knowledge test fell by 2.9 percentile points, both measured relative to the test score distribution of men born in 1967.

Through the lens of our skill investment model, the increase in the labor market returns to logical reasoning relative to vocabulary knowledge spurred an increase in investment in logical reasoning at the expense of vocabulary knowledge. But how much of the trends in test scores can be explained by changes in labor market returns? Taking the model to the data, our second main finding is that changing labor market returns can explain 37 percent of the increase in logical reasoning scores, with the rest being accounted for by other factors. Furthermore, changing labor market returns can fully explain the decrease in vocabulary knowledge, as we estimate that these skills would have improved had labor market returns remained constant at their 1962 level.

As our third main finding, we provide evidence that parents and schools, two key actors in children's skill investment, have placed increasing emphasis on developing reasoning skills relative to knowledge. Using an original survey, we show that parents of more recent cohorts see reasoning skills as more important for their children than factual knowledge. Furthermore, we conduct a text analysis showing that Swedish primary school curricula have shifted focus over time toward developing reasoning skills relative to knowledge, a finding consistent with an extensive pedagogical literature. We view these findings as consistent with our account of the trends in logical reasoning and vocabulary knowledge scores.

Our findings suggest that it is useful to incorporate market incentives and the tools of economics into the study of the determinants of cohort trends in cognitive skills. Our analysis gives impetus to studying many interesting open questions: Are schools the main drivers behind the increase in the supply of reasoning skills? Why does the labor market increasingly reward logical reasoning over factual knowledge? Can changes in labor market returns also affect trends in *non-cognitive* skills?

Chapter 2 – How are novel medicines adopted across socioeconomic groups?
When new technologies are introduced, there is typically wide variation in their adoption patterns. Large disparities in technology diffusion have been documented within and across countries and in contexts such as agriculture, manufacturing, transportation, and medicine (see e.g., Comin & Mestieri, 2014; Miraldo et al., 2019; Skinner & Staiger, 2007, 2015).

In the context of healthcare, understanding to what extent the adoption of innovations such as novel medicines varies across hospitals and patient groups is important because slow adoption is costly when new treatments substantially improve over existing ones. Differences in adoption between socioeconomic groups may also contribute to health disparities (e.g., Chetty et al., 2016; Finkelstein et al., 2021; Mackenbach, 2012; Zhang et al., 2010).

The second chapter, **Adoption of Medical Innovations Across Hospitals and Socioeconomic Groups: Evidence from Sweden**, joint work with Fabian Sinn, studies the adoption of novel medicines using administrative data from Sweden. Our analysis combines individual-level register data on inpatient and outpatient care visits and prescription drug purchases with register data on socioeconomic background and labor market histories.

To measure the adoption of a novel medicine, we first approximate its target patient group by mapping its indications to diagnosis and procedure codes included in the register data. We then measure adoption by matching dates of healthcare visits with the prescription dates of purchased drugs. To measure adoption at the hospital level, we track the share of patients visiting a given hospital who purchase the medicine after discharge.

Focusing on 58 novel medicines across 47 health conditions (such as cardiovascular conditions, lung diseases, and diabetes), we document substantial differences in adoption rates across hospitals and socioeconomic groups. For instance, at the end of our study period, the adoption rate for hospitals at the 90^{th} percentile was approximately three times that of hospitals at the 10^{th} percentile. Similar patterns hold when looking at specific groups, such as heart attack, atrial fibrillation, or chronic obstructive pulmonary disease (COPD) patients.

Moreover, we find a positive correlation between a patient's income rank (measured before hospitalization) and the adoption rate of novel medicines across diverse health conditions, ranging from cardiovascular diseases to lung diseases to ADHD. Pooling across all our novel medicines, we find that moving from the bottom to the top income percentile increases the probability of purchasing a novel medicine by around 0.1 percentage points, or 10 percent relative to the average adoption rate.

To assess the potential consequences of differences in adoption patterns, we take as a case study a novel antiplatelet drug that saw widespread adoption during our study period. In a back-of-the-envelope calculation, we find that harmonizing adoption rates between the top and bottom income deciles could have potentially narrowed the 12-month survival rate gap among first-time heart attack patients by 1.2 percent. Notably, for this drug, we do not find that better hospital management practices or the drug's inclusion in regional guidelines were associated with faster adoption, suggesting that other factors may be driving the differences in adoption rates.

Chapter 3 – Does unemployment insurance affect healthcare use?

An extensive literature in economics, sociology, public health, psychology, and other social sciences shows that job displacement and unemployment are stressful events harmful for mental and physical health (e.g., Brand, 2015; Dooley et al., 1996; Jahoda, 1982; Picchio & Ubaldi, 2023; Wanberg, 2012).

In addition to causing distress for those losing their jobs, these health consequences of unemployment could also be costly to society if the unemployed increase their healthcare use e.g. due to prolonged stress. These costs could potentially be large because individuals typically pay a small share of the total costs of the healthcare they receive. For example, only 6% of inpatient care expenses, 18% of outpatient care expenses, and 25% of prescription drug expenses were paid out-of-pocket by households in 2016 in OECD countries (Organisation for Economic Co-operation and Development, 2019).

The third chapter, **Unemployment Insurance Generosity and Health: Evidence from Sweden**, joint work with Arash Nekoei and David Seim, studies whether unemployment insurance (UI) affects the benefit recipients' healthcare use. If access to more generous unemployment benefits helps alleviate the negative health impacts of unemployment, this should be taken into account when deciding on the optimal level of unemployment benefits. Studying effects on healthcare use also helps shed light on whether the adverse health consequences of unemployment are primarily related to the decline in income after losing a job or whether other factors, such as social stigma or the loss of social contacts and identity (as emphasized by e.g., Jahoda, 1982) matter more.

Our analysis uses individual-level administrative data on around 340,000 unemployment spells, which we link to detailed register data on inpatient and outpatient care visits and prescription drug purchases. Our measure of costs aims to capture the full costs of healthcare use, including out-of-pocket costs but also costs covered by prescription drug insurance for drug purchases and costs of resources used on the patient along with underlying (e.g., personnel and administrative) costs for inpatient and outpatient care visits.

To estimate the causal effect of unemployment benefits on healthcare use, we use a regression kink design. Intuitively, this method exploits the fact that the amount of benefits individuals can receive is capped – a typical feature of unemployment insurance systems worldwide. This feature produces a kink in the relationship between unemployment benefits and pre-unemployment earnings at the point where the individual reaches the benefit cap. As long as individuals with pre-unemployment earnings slightly below and above this kink are similar in terms of other determinants of healthcare use, we can attribute any kinks in the relationship between healthcare use and pre-unemployment earnings to a causal effect of unemployment benefits on healthcare use.

We find little evidence that the generosity of unemployment insurance affects the healthcare use of people with pre-unemployment earnings close to the kink point. For example, over the first 20 weeks of the unemployment spell, our estimates can rule out changes in the total costs of healthcare use greater than 9 percent in response to a one percent increase in daily unemployment benefits. Our findings are similar for both men and women, younger and older individuals, and short-term and long-term benefit recipients.

The findings of this chapter indicate that, at least in the Swedish setting, slightly adjusting the level of unemployment benefits would not greatly affect the healthcare use of the unemployed. An interesting open question is whether the same applies to other social insurance programs, such as disability insurance whose recipients are generally in worse health than the unemployed. For example, recent evidence from the United States shows that access to more generous disability insurance can even reduce the mortality rate of benefit recipients (Gelber et al., 2023).

Chapter 4 – Does parental job loss affect children's educational choices? Educational attainment is highly correlated with parental income in many settings. For example, the correlation between family income and college enrollment increased between the 1980s and the 2000s in the United States (Belley & Lochner, 2007), and families of higher socioeconomic status take advantage of free school choice more often in Sweden and other European countries (Ambler, 1994; Skolverket, 2003). While this correlation may reflect credit constraints, it might also reflect other factors, such as economic insecurity restricting the time parents have for being involved in their children's education.

The fourth chapter, **Family-Level Stress and Children's Educational Choice: Evidence from Parent Layoffs**, joint work with Julia Tanndal, studies how the layoff of a parent affects children's educational outcomes, focusing on the context of upper secondary school track choice in Sweden. Because there are no tuition fees, but school and track choice may be a complex task for families, this setting helps with distinguishing between the effects of financial and non-financial constraints on the educational outcomes of children in low-income families.

We use Swedish register data on events where an employer plans to lay off five or more employees of the same workplace due to a long-term reduction in labor demand. We link information on parents employed at firms with at least one such event with information on the educational attainment of the parents' children. Key to our analysis is that the timing of the layoff events is unlikely to be related to the characteristics of the affected parents or how old their children are at the time of layoff. We therefore use variation in the child's age at the time of layoff to estimate how layoffs occurring at different points in the child's life affect educational attainment.

We find that children in families with a parental layoff are less likely to finish high school relative to their peers, especially when the layoff coincides with the transition from compulsory to upper secondary school (ages 15–16). The likelihood of completing high school on time falls by 15 percentage points (from 73 to 58 percent) for children whose parents are laid off 6–12 months before the school transition. In contrast, probability of graduation only falls by around 3 percentage points for children already enrolled in high school at the time of parental layoff.

Two findings suggest that our results are driven by layoffs reducing the time parents have for investing in their children's education. First, we see that effects on high school completion are larger when the layoff occurs before the high school application deadline, a period when parental support is more crucial. Second, the effects of parental layoffs are more pronounced when they affect the oldest child. In contrast, for younger siblings we cannot rule out that layoffs do not affect high school completion. This latter finding is consistent with parental layoffs being more harmful in families with less information about the school choice system, since younger siblings should have access to more knowledge about school choice before the high school track choice becomes relevant for them.

Overall, our findings highlight that the timing of parental job loss and how it interacts with critical junctures of the education system are important for determining how harmful the effects of layoffs are for children's educational outcomes.

References

Ambler, J. S. (1994). Who Benefits from Educational Choice? Some Evidence from Europe. *Journal of Policy Analysis and Management*, *13*(3), 454–476.

Belley, P., & Lochner, L. (2007). The Changing Role of Family Income and Ability in Determining Educational Achievement. *Journal of Human Capital*, *1*(1), 37–89.

Brand, J. E. (2015). The Far-Reaching Impact of Job Loss and Unemployment. *Annual Review of Sociology*, *41*, 359–375.

Carroll, J. B. (1993). *Human Cognitive Abilities*. Cambridge University Press.

Castex, G., & Dechter, E. K. (2014). The Changing Roles of Education and Ability in Wage Determination. *Journal of Labor Economics*, *32*, 685–710.

Chetty, R., Stepner, M., Abraham, S., Lin, S., Scuderi, B., Turner, N., Bergeron, A., & Cutler, D. (2016). The Association Between Income and Life Expectancy in the United States, 2001-2014. *Journal of the American Medical Association*, *315*(16), 1750–1766.

Comin, D., & Mestieri, M. (2014). Technology Diffusion: Measurement, Causes, and Consequences. In P. Aghion & S. N. Durlauf (Eds.), *Handbook of Economic Growth* (pp. 565–622, Vol. 2). Elsevier.

Dickens, W. T., & Flynn, J. R. (2001). Heritability Estimates Versus Large Environmental Effects: The IQ Paradox Resolved. *Psychological Review*, *108*, 346–369.

Dooley, D., Fielding, J., & Levi, L. (1996). Health and Unemployment. *Annual Review of Public Health*, *17*, 449–465.

Edin, P.-A., Fredriksson, P., Nybom, M., & Öckert, B. (2022). The Rising Return to Non-cognitive Skill. *American Economic Journal: Applied Economics*, *14*(2), 78–100.

Finkelstein, A., Gentzkow, M., & Williams, H. (2021). Place-Based Drivers of Mortality: Evidence from Migration. *American Economic Review*, *111*(8), 2697–2735.

Flynn, J. R. (1984). The Mean IQ of Americans: Massive Gains 1932 to 1978. *Psychological Bulletin*, *95*(1), 29.

Flynn, J. R. (1987). Massive IQ Gains in 14 Nations: What IQ Tests Really Measure. *Psychological Bulletin*, *101*(2), 171.

Flynn, J. R. (2018). Reflections About Intelligence Over 40 Years. *Intelligence*, *70*, 73–83.

Gelber, A., Moore, T., Pei, Z., & Strand, A. (2023). Disability Insurance Income Saves Lives. *Journal of Political Economy*, *131*(11), 3156–3185.

Jahoda, M. (1982). *Employment and Unemployment: A Social-Psychological Analysis*. Cambridge University Press.

Mackenbach, J. P. (2012). The Persistence of Health Inequalities in Modern Welfare States: The Explanation of a Paradox. *Social Science & Medicine*, *75*(4), 761–769.

Markussen, S., & Røed, K. (2020). Economic Mobility Under Pressure. *Journal of the European Economic Association*, *18*, 1844–1885.

Miraldo, M., Hauck, K., Vernet, A., & Wheelock, A. (2019). Variations in the Adoption of Healthcare Innovation: A Literature Review. *Oxford Research Encyclopedia of Economics and Finance*.

Organisation for Economic Co-operation and Development. (2019). *Out-of-Pocket Spending: Access to Care and Financial Protection*. Retrieved May 27, 2024, from https://www.oecd.org/health/health-systems/OECD-Focus-on-Out-of-Pocket-Spending-April-2019.pdf

Picchio, M., & Ubaldi, M. (2023). Unemployment and Health: A Meta-Analysis. *Journal of Economic Surveys*, 1–36.

Pietschnig, J., & Voracek, M. (2015). One Century of Global IQ Gains: A Formal Meta-analysis of the Flynn Effect (1909–2013). *Perspectives on Psychological Science*, *10*, 282–306.

Rindermann, H., Becker, D., & Coyle, T. R. (2017). Survey of Expert Opinion on Intelligence: The FLynn Effect and the Future of Intelligence. *Personality and Individual Differences*, *106*, 242–247.

Schaie, K. W., Willis, S. L., & Pennak, S. (2005). An Historical Framework for Cohort Differences in Intelligence. *Research in Human Development*, *2*, 43–67.

Skinner, J., & Staiger, D. (2007). Technology Adoption from Hybrid Corn to Beta-Blockers. In E. R. Berndt & C. R. Hulten (Eds.), *Hard-to-Measure Goods and Services: Essays in Honor of Zvi Griliches* (pp. 545–570). University of Chicago Press.

Skinner, J., & Staiger, D. (2015). Technology Diffusion and Productivity Growth in Health Care. *Review of Economics and Statistics*, *97*(5), 951–964.

Skolverket. (2003). *Valfrihet och dess effekter inom skolområdet* (Report No. 230).

Wanberg, C. R. (2012). The Individual Experience of Unemployment. *Annual Review of Psychology*, *63*(1), 369–396.

Zhang, Y., Baicker, K., & Newhouse, J. P. (2010). Geographic Variation in the Quality of Prescribing. *New England Journal of Medicine*, *363*(21), 1985–1988.

Chapter 1

Labor Market Returns and the Evolution of Cognitive Skills: Theory and Evidence*

*This chapter is co-authored with Santiago Hermo, David Seim, and Jesse M. Shapiro and has been published in the *Quarterly Journal of Economics* (Volume 137, Issue 4, November 2022, pp. 2309–2361). Reprinted with permission from the Oxford University Press. We thank Dan Benjamin, Ernesto Dal Bó, Peter Fredriksson, Pete Klenow, Erik Lindqvist, Olivier Marie, Neil Mehrotra, Kevin M. Murphy, Björn Öckert, Jakob Pietschnig, Eka Roivainen, David Strömberg, Jonas Vlachos, editors Andrei Shleifer and Larry Katz, five anonymous reviewers, and seminar participants at UC Berkeley Haas School of Business, BI Norwegian Business School, CIDE, Stockholm University, University of Mannheim, Vanderbilt University, VATT Institute for Economic Research, Bocconi University, Collegio Carlo Alberto, ESE Rotterdam, and Tel Aviv University for helpful comments and exchanges. We thank our dedicated research assistants for their contributions to this project. We acknowledge support from the Jan Wallander and Tom Hedelius Foundation (Grant P2015-0095:1), and from the James M. and Cathleen D. Stone Wealth and Income Inequality Project, the Eastman Professorship, the JP Morgan Chase Research Assistant Program, and the Population Studies and Training Center at Brown University. The study is in part based on data from the research infrastructure UGU at the University of Gothenburg. UGU is funded by Swedish Research Council (dnr. 2017-00628) and University of Gothenburg. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the authors and do not necessarily reflect the views of the funding or data sources. Translations from Swedish to English are our own, with assistance from Google Translate.

1.1 Introduction

A large and important literature in cognitive science documents substantial gains in intelligence (IQ) scores across successive cohorts in developed countries, sometimes called the "Flynn effect" (see, for example, Flynn, 2007, 2012; Flynn & Shayer, 2018; Pietschnig & Voracek, 2015; Schaie et al., 2005; Trahan et al., 2014).[1] These gains are especially pronounced for *fluid intelligence*, a notion of general reasoning ability often measured with abstract reasoning tasks (Pietschnig & Voracek, 2015). There are less pronounced gains, or even declines, in *crystallized intelligence*, a notion of domain knowledge often measured with knowledge assessments such as vocabulary tests (Pietschnig & Voracek, 2015; Schaie et al., 2005).[2] Understanding the causes of these trends is important in part because of evidence that a population's level of cognitive skills influences its economic productivity, economic growth, and distribution of income (e.g., Bishop, 1989; Hanushek & Woessmann, 2008, Section 5).[3]

There is no consensus on the precise causes of cohort trends in cognitive performance, which some consider to be an important puzzle.[4] Research in cognitive science emphasizes factors, such as improvements in health and nutrition, that expand the supply of skill (e.g., Pietschnig & Voracek, 2015; Rindermann et al., 2017). But the incentive to invest in particular dimensions of skill may also evolve over time in response to the demands of the economy.

In this paper, we study the role of labor market returns in determining cohort trends in skill levels and skill composition. We focus on Sweden, where an administrative data join between standardized test scores (collected for military conscription typically at age 18 or 19) and earnings (collected by the tax agency over the lifecycle) allows us to measure the level of and return to skill in a consistent way across cohorts for the near-population of men.

[1] Rindermann et al., 2017 write, "Among the most discussed topics in intelligence research is the rise of average IQ test results across generations in the 20th century" (p. 242).

[2] Cattell (1943) writes, "Fluid ability has the character of a purely general ability to discriminate and perceive relations between any fundaments, new or old... Crystallized ability consists of discriminatory habits long established in a particular field." (p. 178).

[3] There is also evidence that a population's level of cognitive skills is related to its levels of patience and risk aversion (Falk et al., 2018; Potrafke, 2019).

[4] Deary (2020) writes, "If there were a prize in the field of human intelligence research, it might be for the person who can explain the 'Flynn effect'..." (also quoted in Wai & Putallaz, 2011).

We develop a model of an economy whose aggregate output is determined by the aggregate skills of workers. Skills, which can be multidimensional, are determined both by an exogenous endowment (e.g., health) and an investment decision made early in life (by parents, children, and schools). The investment decision is in turn influenced by the lifetime labor market returns to different skills. We identify the relative returns to different skills by assuming that unobserved determinants of an individual's earnings are correlated with the individual's skill endowment only through its market value. Under this assumption, the relative returns to different skills can be recovered from a Mincerian regression of the log of earnings on skills in a cross-section of individuals.

We parameterize the model so that a single unknown parameter governs the degree to which individuals can substitute investment across skill dimensions. We identify this parameter by assuming that long-run average shocks to the technology for producing skills are proportional across fluid and crystallized intelligence.

We take the model to the data. Across the birth cohorts 1962–1975, we find that performance on a logical reasoning task—our proxy for fluid intelligence—improved by 4.4 percentile points, measured in terms of the distribution in the 1967 cohort. The estimated lifetime earnings premium to an additional percentile point of logical reasoning performance fell by 0.08 log points, from a base of 0.48 log points. Turning to performance on a vocabulary knowledge test—our proxy for crystallized intelligence—we find that performance declined by 2.9 percentile points. The estimated lifetime premium to an additional percentile point of vocabulary knowledge fell by 0.07 log points, but from a much lower base of 0.16 log points.

Because logical reasoning performance rose while its market return fell, a model in which logical reasoning is the only skill dimension would imply that there must have been an increase in the supply of skill, consistent with the hypothesis of a growth in the endowment of fluid intelligence of the sort emphasized in the cognitive science literature. A richer picture emerges when incorporating the second skill dimension. Vocabulary knowledge performance fell along with its market return, suggesting a decline in the demand for this skill dimension. Moreover, the premium to vocabulary knowledge relative to logical reasoning fell by 38 log points. Seen through the lens of our model, the declining relative premium to crystallized intelligence drove a reallocation of effort towards developing abstract reasoning and away from acquiring knowledge.

We use the model to decompose the observed trends in skills into a portion driven by changing labor market returns and a portion driven by other factors. According to the estimated model, if the market returns to different skills had remained constant at their 1962 level, logical reasoning and vocabulary knowledge performance would have increased by 2.8 and 3.0 percentile points, respectively. The estimated model thus implies that trends in labor market returns explain 37 percent of the growth in logical reasoning performance (roughly, $100 \times \frac{4.4-2.8}{4.4}$) and more than fully explain the decline in vocabulary knowledge.

We extend our baseline analysis in a few directions. First, we use a nationally representative survey linked to earnings records to expand our analysis to a broader set of birth cohorts, from 1948 to 1977, and to skills measured at a younger age, around age 13. We find that the relative level of and return to logical reasoning performance rose across these cohorts, though our estimates are less precise than those from the (much larger) enlistment sample. Second, we adjust the estimated trends in skill levels and skill returns to account for the role of covariates such as height and secondary school completion. Although adjusting for covariates is conceptually delicate, as some covariates may themselves respond to labor market returns, we find broadly similar conclusions across a variety of sensitivity analyses. Third, we extend our model to incorporate non-cognitive skills. We estimate a smaller, but still important, role for changes in labor market returns in explaining the evolution of cognitive skills, and we highlight limitations of the analysis that arise because the measure of non-cognitive skills in our data is not directly comparable across cohorts.

We also explore whether the main actors in skill investment—parents and schools—place increasing emphasis on reasoning relative to knowledge. In an original survey, we find that parents of more recent cohorts tend to regard reasoning ability as more important for their children than knowledge of facts. In a review of pedagogical scholarship, and an original quantitative text analysis, we find evidence of a trend towards increasing emphasis on reasoning relative to knowledge in primary school curricula in Sweden. Turning to the demand for skills, we show evidence of relative growth in occupations that place more emphasis on reasoning as opposed to knowledge. We view this evidence as consistent with the mechanism underlying our estimated model.

Our analysis has some important limitations. A first limitation is that we treat the skill demand portion of the model fairly abstractly and do not offer a precise account of why some skills have become relatively more valuable in the labor market over time, though we show some suggestive evidence based on occupational characteristics. A second limitation is that our conclusions require assumptions on unmeasured determinants of earnings and skills. We specify and discuss these assumptions, their plausibility, and their importance in more detail in the body of the paper, where we also discuss evidence on sensitivity to departures from key assumptions. A third limitation is that we focus on the labor market returns to skills and do not measure their nonmarket returns, though we show that our conclusions are preserved if market and non-market returns to skill move in proportion across cohorts. A final limitation is that, due to the nature of the military enlistment data that we use, our main analyses are limited to men only, though in an appendix we show results for women in the survey sample.

The main contribution of this paper is to develop and apply an economic model to quantify the role of labor market returns in determining cohort trends in multidimensional cognitive skills. We are not aware of prior work that does this. A large literature in economics studies the determinants and market value of (possibly multidimensional) cognitive and non-cognitive skills (see, for example, the review by Sanders and Taber, 2012 and recent papers by Roys and Taber, 2020 and Agostinelli and Wiswall, 2020). Our analysis of the market for skills is closely related to the work of Katz and Murphy (1992) and the large literature that follows (see, e.g., Deming, 2017 and the review by Acemoglu and Autor, 2011), but differs in focusing on explaining trends across cohorts (rather than time periods) and in offering an explicit quantitative model of the supply of (rather than demand for) skills. As we do, Heckman et al. (1998) develop a general-equilibrium model of the supply and demand for skill. Their model is richer than ours in its treatment of labor demand but does not incorporate multiple dimensions of skill.[5]

[5]Our model of the supply of skill, which focuses on cohort-level trends, is more stylized than in work that focuses on the skill formation process itself (see, e.g., Cunha et al., 2006, 2010; Doepke et al., 2019). In particular, unlike much of the work reviewed in, e.g., Heckman and

A large literature in cognitive science (reviewed, for example, in Pietschnig & Voracek, 2015) studies causes of trends in various measures of ability or intelligence. Although some work in this literature considers the possibility that social demands affect the development of skills, we are not aware of work in this literature that quantifies trends in the economic returns to different types of skills, or that uses an estimated model to link trends in skills to trends in their returns.[6] We are also not aware of prior work that quantifies long-term trends in parents' and schools' emphasis on reasoning vs. knowledge.[7]

An additional contribution of this paper is to document trends in the relative labor market returns to different dimensions of cognitive skill. Much prior work in economics and other fields studies trends in the level of and returns to skills,[8] including some work using linked administrative data from elsewhere in Europe,[9] as well as some work using the same data from Sweden that we use.[10] Rönnlund et al. (2013) report trends in test scores in Sweden from 1970–1993. Lindqvist and Vestman (2011) study the labor market return to cognitive and non-cognitive skills in Sweden. Especially related, Edin et al. (2022) estimate trends in the returns to cognitive and non-cognitive skills in

---

Mosso (2014), we treat the skill investment decision as static and do not model the dynamics of skill formation during childhood.

[6]Dickens and Flynn (2001) specify and simulate a quantitative model in which genetic endowments and environmental factors interact to produce measured intelligence. They discuss the role of occupational demands in driving cohort differences in skills, but do not incorporate labor market returns into their quantitative model, and do not estimate the model's parameters. Flynn (2018, p. 79) notes that "When society asks us to increase our use of any skill over time, the brain responds," and cites research by Maguire et al. (2006) on the effect of occupational demands on brain structure in the context of London taxi and bus drivers.

[7]Okagaki and Sternberg (1993) study group differences in parents' conceptions of intelligence. Bietenbeck (2014) studies the effects on reasoning and knowledge skills of traditional and modern teaching practices. Cunha et al. (forthcoming) , among others, study the relationship between parents' beliefs about the technology of skill formation and parents' investments in children's skills.

[8]For example, Castex and Dechter (2014) use survey data to document falling returns to cognitive skills as measured by Armed Forces Qualification Test scores in the US between the 1980s and 2000s.

[9]For example, Jokela et al. (2017) document cohort trends in personality traits using scores from military conscripts in Finland, and argue based on estimated labor market returns that the economic significance of cohort trends in personality traits is similar to that of cohort trends in cognitive abilities. Markussen and Røed (2020, Section 4.2) document declining labor market returns to men's cognitive skills using test scores from enrollment in military service in Norway.

[10]These data have also been used to study, among other topics, the effect of schooling on measured skills Carlsson et al. (2015) and the effect of officer training on occupational outcomes later in life (Grönqvist & Lindqvist, 2016).

Sweden. None of these papers documents trends in the relative lifetime labor market returns to different dimensions of cognitive skill, or quantifies the role of labor market returns in driving cohort trends in skill levels in a model with multidimensional skills.[11]

The remainder of the paper is organized as follows. Section 1.2 presents our model and approach to identification. Section 1.3 describes the data we use. Section 1.4 presents our main findings. Section 1.5 discusses additional evidence related to the mechanisms in the model. Section 1.6 extends our analysis to incorporate non-cognitive skills. Section 1.7 concludes.

1.2 Model

1.2.1 Production and Earnings

There is a finite population of workers $i \in \mathcal{N}$, each of which is associated with a cohort $c(i) \in \{\underline{c}, ..., \overline{c}\}$. Each worker is characterized by a skill level $\mathbf{x}_i \in \mathbb{R}^J_{\geq 0}$ for $J \geq 2$.

In each time period t, each worker i has an experience level $a(i,t) = t - c(i)$ and supplies efficiency units $z_{it} \in \mathbb{R}_{\geq 0}$, where $z_{it} > 0$ if $a(i,t) \in \{1, ..., A\}$ and $z_{it} = 0$ otherwise. Thus, members of cohort c enter the labor force in period $c+1$ and exit the labor force after period $c+A$, and we identify the cohort c with the period immediately before workers in the cohort enter the labor force.

Let $\mathbf{X}_t$ be the $J \times A$ matrix whose a^{th} column is given by the sum of $z_{it}\mathbf{x}_i$ over all workers i with experience level $a(i,t) = a$. This matrix collects the total supply of skill in period t for each dimension j and experience level a. Let $\mathbf{X}_t^{-i}$ be the analogue of $\mathbf{X}_t$ excluding worker i.[12]

[11] Jokela et al. (2017) document trends in the within-cohort rank correlation between three different dimensions of cognitive skill and earnings at age 30 (Figure 2, panel B) or ages 30-34 (Figure S1, panel B), but do not report trends in lifetime labor market returns from a model of earnings that accounts for multiple skill dimensions simultaneously. Lindquist (2005) models trends in the demand for skill in Sweden arising from capital-skill complementarity.

[12] That is, the a^{th} column of $\mathbf{X}_t$ is

$$\sum_{\{l \in \mathcal{N}:\ a(l,t)=a\}} z_{lt}\mathbf{x}_l$$

and that of $\mathbf{X}_t^{-i}$ is

$$\sum_{\{l \in \mathcal{N} \setminus \{i\}:\ a(l,t)=a\}} z_{lt}\mathbf{x}_l.$$

Total output Y_t at time t is given by

$$Y_t = F_t\left(\mathbf{X}_t\right)$$

where $F_t\left(\cdot\right)$ is a scalar-valued differentiable function that may vary over time, for example due to changes in production technology.

In each period t, a worker i earns his marginal product w_{it}, which is given by

$$\begin{aligned} w_{it} &= F_t\left(\mathbf{X}_t\right) - F_t\left(\mathbf{X}_t^{-i}\right) \\ &\approx z_{it} \nabla F_{t,a(i,t)}^{'} \mathbf{x}_i \end{aligned}$$

where $\nabla F_{t,a}$ is the gradient of $F_t\left(\mathbf{X}_t\right)$ at $\mathbf{X}_t$ with respect to the a^{th} column of $\mathbf{X}_t$. We will assume that $\nabla F_{t,a(i,t)}^{'} \mathbf{x}_i > 0$ for all workers i in all periods t of working life. Motivated by a large-population setting, we will treat $\mathbf{X}_t$ as fixed from the perspective of any individual worker i.

Pick a period t of worker i's working life, so that $z_{it} > 0$, and rewrite the earnings equation as

$$\ln\left(w_{it}\right) \approx \ln\left(z_{it}\right) + \ln\left(\nabla F_{t,a(i,t)}^{'} \mathbf{x}_i\right).$$

Now take a first-order approximation around the mean skill level $\mathbf{x}_{t,a(i,t)}$ of individuals who share worker i's experience level at time t to get

$$\ln\left(w_{it}\right) \approx \ln\left(z_{it}\right) + \ln\left(\nabla F_{t,a(i,t)}^{'} \mathbf{x}_{t,a(i,t)}\right) + \frac{\nabla F_{t,a(i,t)}^{'}}{\nabla F_{t,a(i,t)}^{'} \mathbf{x}_{t,a(i,t)}} \left(\mathbf{x}_i - \mathbf{x}_{t,a(i,t)}\right),$$

where we will again treat $\mathbf{x}_{t,a(i,t)}$ as fixed from the perspective of any individual worker i. We can write the preceding as

$$\ln\left(w_{it}\right) \approx B_{t,a(i,t)} + \mathbf{p}_{t,a(i,t)}^{'} \mathbf{x}_i + \ln\left(z_{it}\right) \tag{1.1}$$

where $B_{t,a}$ is a scalar, $\mathbf{p}_{t,a}$ is a vector of skill premia, and both of these are specific to a time period and experience level.[13]

We will proceed taking equation (1.1) to be exact. Although we have derived (1.1) from a particular model of the labor market, any model in which earnings take the form in (1.1) will be equivalent for the purposes of our subsequent analysis. Moreover, although for concreteness we refer to z_{it} as efficiency units, (1.1) makes clear that z_{it} captures any individual-and-period-specific determinants of earnings that are not included in $\mathbf{x}_i$.

1.2.2 Skill Investment

At the beginning of life, each worker i chooses his skills $\mathbf{x}_i$ subject to the constraints

$$\begin{gathered} \mathbf{x}_i \geq \mu_i \\ S_{c(i)}\left(\mathbf{x}_i - \mu_i\right) \leq \overline{S}_{c(i)} \end{gathered} \tag{1.2}$$

where $\mu_i \in \mathbb{R}^J$ is an individual skill endowment, $\overline{S}_c \in \mathbb{R}_{>0}$ is a cohort-specific skill budget, and $S_c(\cdot)$ is a cohort-specific transformation function.

We can think of $\mathbf{x}_i - \mu_i \in \mathbb{R}^J_{\geq 0}$ as the skill investment of individual i, i.e., the increment in skills over and above the individual's endowment μ_i. The endowment μ_i represents cross-sectional differences within a cohort, say in ability or access to schooling. The budget $\overline{S}_c$ can be seen as representing the total time and effort available for skill investment. The transformation function $S_c(\cdot)$ may be thought of as governing the ease of skill investment and of substituting investment across skill dimensions. The budget $\overline{S}_c$ and the function $S_c(\cdot)$ may differ across cohorts because of trends in the technology of skill formation, say because of improvements in health or nutrition. Although for simplicity we refer to the decision-maker as the worker, we may alternatively think of the skill investment decision as being made by the worker's parents, or by a collective decision-making process involving the worker, his parents, and the schooling system.[14] Because we take the timing of entry into the labor

---

[13]Specifically,

$$B_{t,a} = \ln\left(\nabla F_{t,a}^{'}\mathbf{x}_{t,a}\right) - 1, \qquad \mathbf{p}_{t,a} = \frac{\nabla F_{t,a}}{\nabla F_{t,a}^{'}\mathbf{x}_{t,a}}.$$

[14]For example, we may think of the skill budget $\overline{S}_c$ as reflecting the sum of the effective

market as given, we do not account for any foregone earnings due to time spent acquiring skills.

Each worker consumes his earnings in each period and has time-separable preferences with a felicity function given by the log of consumption. Each worker discounts future felicity by a discount factor $\delta \in (0,1]$. At the time of choosing the skill investment, worker i has full knowledge of the path of skill premia over his lifecycle, $\left\{\mathbf{p}_{c(i)+a,a}\right\}_{a=1}^{A}$. We further assume that worker i's skill investment does not influence the path of z_{it}.

It follows that the worker's problem is equivalent to maximizing $\mathbf{P}^{'}_{c(i)}\mathbf{x}_i$ subject to (1.2), where

$$\mathbf{P}_{c(i)} = \frac{\sum_{a=1}^{A} \delta^a \mathbf{p}_{c(i)+a,a}}{\sum_{a=1}^{A} \delta^a} \tag{1.3}$$

is the net present value of the skill premia $\mathbf{p}_{c(i)+a,a}$ at different experience levels a, normalized by the constant $\sum_{a=1}^{A} \delta^a$ to have a convenient interpretation as a weighted average. We refer to $\mathbf{P}_c$ as the *lifetime skill premia* faced by cohort c. Although we have assumed for concreteness that workers have full knowledge of the path of skill premia, the linearity of equation (1.1) in $\mathbf{x}_i$ means that we can alternatively allow for uncertainty in skill premia by replacing $\mathbf{p}_{c(i)+a,a}$ in (1.3) with its expectation.[15] Likewise, although we have assumed that skills $\mathbf{x}_i$ are fixed throughout working life, it is possible to accommodate a linear, deterministic evolution of skills over the lifetime under a suitable reinterpretation of $\mathbf{p}_{c(i)+a,a}$ in (1.3).[16]

The worker's problem is also equivalent to maximizing $\mathbf{P}^{'}_{c(i)}\tilde{\mathbf{x}}_i$ subject to $\tilde{\mathbf{x}}_i \geq 0$ and $S_{c(i)}\left(\tilde{\mathbf{x}}_i\right) \leq \overline{S}_{c(i)}$, where $\tilde{\mathbf{x}}_i = \mathbf{x}_i - \mu_i$. The solutions to this problem

time and effort available from the worker, his parents, and his teachers.

[15] That is, taking $\mathrm{E}_c[\cdot]$ to be an expectation with respect to the information set of workers in cohort c at the time that skill investments are made, we can take the worker's expected discounted utility to be

$$\frac{\sum_{a=1}^{A} \delta^a \mathrm{E}_{c(i)}\left[\mathbf{p}^{'}_{c(i)+a,a}\right]}{\sum_{a=1}^{A} \delta^a}\mathbf{x}_i.$$

[16] Specifically, suppose that each worker enters working life with chosen skills $\mathbf{x}_{i,0} = \mathbf{x}_i$, which then evolve with experience according to $\mathbf{x}_{i,a} - \mathbf{x}_{i,a-1} = \Lambda_{c(i),a}\mathbf{x}_{i,a-1}$ for $a \in \{1,...,A\}$, with $\Lambda_{c,a} > -I_J$ elementwise for all c,a. Then we can take $\mathbf{p}^{'}_{c(i)+a,a} = \tilde{\mathbf{p}}^{'}_{c(i)+a,a}\prod_{a'=1}^{a}\left(\Lambda_{c(i),a'} + I_J\right)$ where $\tilde{\mathbf{p}}_{c(i)+a,a}$ are the (contemporaneous) premia to the worker's skills $\mathbf{x}_{i,a}$ at experience level a.

depend only on the cohort $c(i)$ of the worker and not on the worker's identity. In this sense, within-cohort variation in skill levels arises only due to variation in the individual skill endowment μ_i. We assume that μ_i has mean zero within each cohort. This assumption is without loss of generality since we can always define $\mathbf{x}_i$ and μ_i relative to a cohort-specific mean endowment.[17]

1.2.3 Parameterization and Identification

We will assume that the transformation function $S_c(\cdot)$ takes the constant elasticity form

$$S_c(\tilde{\mathbf{x}}) = \left(\sum_{j=1}^{J} K_{cj}^{\rho-1} \tilde{x}_j^{\rho} \right)^{\frac{1}{\rho}} \tag{1.4}$$

where $\mathbf{K}_c \in \mathbb{R}^J_{>0}$ is a vector-valued parameter that we may think of as describing the cost of increasing skill along each of the J dimensions for cohort c, and $\rho > 1$ is a scalar parameter that determines the substitutability of effort across different skill dimensions.

Worker i's problem has a unique solution, with $\tilde{\mathbf{x}}_i = \tilde{\mathbf{x}}_{i'}$ if $c(i) = c(i')$. Therefore write $\tilde{\mathbf{x}}_c = \tilde{\mathbf{x}}_c(\mathbf{P}_c)$ as the optimal $\tilde{\mathbf{x}}_i$ for all workers i in cohort c. Here $\tilde{\mathbf{x}}_c(\cdot)$ is a *skill supply function* that returns the optimal skill investment for members of cohort c given the lifetime skill premia $\mathbf{P}_c$.[18] We assume that $\mathbf{P}_c > 0$ for all c.

Imagine an econometrician who has data $\{(\mathbf{P}_c, \tilde{\mathbf{x}}_c)\}_{c=\underline{c}}^{\overline{c}}$ and wishes to learn the skill supply function $\tilde{\mathbf{x}}_c(\cdot)$. Focus on the first two dimensions, where we may think of fluid intelligence as dimension $j = 1$ and crystallized intelligence as dimension $j = 2$. Under the model, the relative supply of fluid intelligence

---

[17]To see this, start with an endowment $\mathring{\mu}_i$ with mean $\mathring{\mu}_c = \frac{\sum_{\{i:c(i)=c\}} \mathring{\mu}_i}{|\{i:c(i)=c\}|}$ in cohort c, where $\mathring{\mu}_c$ need not be zero. The problem of maximizing $\mathbf{P}'_{c(i)}\mathring{\mathbf{x}}_i$ subject to $\mathring{\mathbf{x}}_i \geq \mathring{\mu}_i$ and $S_{c(i)}(\mathring{\mathbf{x}}_i - \mathring{\mu}_i) \leq \overline{S}_{c(i)}$ is equivalent to the problem of maximizing $\mathbf{P}'_{c(i)}\mathbf{x}_i$ subject to (1.2) where $\mathbf{x}_i = \mathring{\mathbf{x}}_i - \mathring{\mu}_{c(i)}$ and $\mu_i = \mathring{\mu}_i - \mathring{\mu}_{c(i)}$. Here μ_i has mean zero within each cohort by construction.

[18]Specifically, for each skill $j \in \{1, \ldots, J\}$, we have

$$\tilde{x}_{cj}(\mathbf{P}_c) = \frac{P_{cj}^{\frac{1}{\rho-1}} K_{cj}^{-1}}{\left(\sum_{j'=1}^{J} P_{cj'}^{\frac{\rho}{\rho-1}} K_{cj'}^{-1} \right)^{\frac{1}{\rho}}} \overline{S}_c.$$

obeys

$$\ln\left(\frac{\tilde{x}_{c1}}{\tilde{x}_{c2}}\right) = \frac{1}{\rho - 1}\ln\left(\frac{P_{c1}}{P_{c2}}\right) - \ln\left(\frac{K_{c1}}{K_{c2}}\right). \tag{1.5}$$

A standard difficulty in learning the elasticity of substitution $\frac{1}{\rho-1}$ is that the unobserved costs $\mathbf{K}_c$ may affect both skill investments (via the workers' incentives) and skill premia (via the labor market). We assume that, on average, there is no trend in the relative costs of the two skill dimensions.

Assumption 1.1. *(Zero average relative supply shock.) We assume that*

$$\frac{1}{\overline{c} - \underline{c}}\sum_{c=\underline{c}}^{\overline{c}-1}\left[\ln\left(\frac{K_{c+1,1}}{K_{c+1,2}}\right) - \ln\left(\frac{K_{c1}}{K_{c2}}\right)\right] = 0.$$

Under Assumption 1.1, long-run improvements in the technology for producing skills are not systematically biased towards either fluid or crystallized intelligence.

Assumption 1.1 is sufficient for the identification of $\tilde{\mathbf{x}}_c(\cdot)$ under a regularity condition on $\mathbf{P}_c$.

Proposition 1.1. *Under Assumption 1.1, if* $\frac{P_{\overline{c}1}}{P_{\overline{c}2}} \neq \frac{P_{\underline{c}1}}{P_{\underline{c}2}}$*, then the skill supply function* $\tilde{\mathbf{x}}_c(\cdot)$ *for each cohort c is identified from data* $\{(\mathbf{P}_c, \tilde{\mathbf{x}}_c)\}_{c=\underline{c}}^{\overline{c}}$.

All proofs are in Appendix 1.A. The proof of Proposition 1.1 is constructive. Under Assumption 1.1, an explicit expression for ρ can be derived using equation (1.5). We can then learn the costs $\mathbf{K}_c$ and budget $\overline{S}_c$ up to suitable normalizations. The required regularity condition on $\mathbf{P}_c$ can in principle be checked in the data. Appendix 1.C presents conditions for the identification of $\tilde{\mathbf{x}}_c(\cdot)$ in the presence of a social multiplier in skill investment in the spirit of Dickens and Flynn (2001, equation 2").

Proposition 1.1 requires that the econometrician knows $\mathbf{P}_c$. This requirement can be relaxed to require only that $\mathbf{P}_c$ is known up to scale.

Corollary 1.1. *Under the conditions of Proposition 1.1, the skill supply function* $\tilde{\mathbf{x}}_c(\cdot)$ *for each cohort c is identified from data* $\{(\alpha\mathbf{P}_c, \tilde{\mathbf{x}}_c)\}_{c=\underline{c}}^{\overline{c}}$*, where the scalar* $\alpha > 0$ *may be unknown.*

Corollary 1.1 allows the econometrician to understate or overstate the lifetime skill premia, provided the error is proportional across dimensions j and the constant of proportionality does not differ across cohorts. An immediate implication is that if there are non-market returns to skill that evolve in proportion to market returns—say, because skills earn a premium on the marriage market only to the extent they improve a person's earning potential—then measurement of market returns is sufficient for identification of the skill supply function.

What remains is to establish conditions for the identification of $\tilde{\mathbf{x}}_c$ and $\mathbf{P}_c$. Recall that we assume that μ_i has mean zero within each cohort, implying that $\tilde{\mathbf{x}}_c = \overline{\mathbf{x}}_c$ for $\overline{\mathbf{x}}_c$ the mean skill of individuals in cohort c. Identification of $\tilde{\mathbf{x}}_c$ from the distribution of $\mathbf{x}_i$ is therefore trivial.

Recall also that $\mathbf{P}_c$ is the net present value of cohort-and-period-specific skill premia $\mathbf{p}_{t,a} = \mathbf{p}_{t,t-c}$. We identify $\mathbf{p}_{t,t-c}$, up to scale, from a Mincerian regression of the log of earnings on measured skills. To do this, we restrict the relationship between the unobserved determinants of earnings z_{it} and skill endowments μ_i, allowing that the econometrician may also observe a vector of covariates $\mathbf{d}_{it}$.

Assumption 1.2. *The values of z_{it} in each period t obey*

$$\mathrm{E}\left(\ln\left(z_{it}\right)|\mu_i = \mu,\ \mathbf{d}_{it} = \mathbf{d},\ c\left(i\right) = c\right) = \zeta_{t,t-c} + \tilde{\alpha}\mathbf{p}_{t,t-c}^{'}\mu + \mathbf{d}^{'}\beta_{t,t-c}$$

where $\zeta_{t,t-c}$ and $\beta_{t,t-c}$ are unknown parameters, and the scalar $\tilde{\alpha} \geq 0$ may also be unknown.

Assumption 1.2 allows that the unobserved determinants of earnings are linearly related both to the observed covariates $\mathbf{d}_{it}$ and to the market value of the skill endowment $\mathbf{p}_{t,t-c}^{'}\mu_i$. Such a relationship can arise if the market supplies inputs complementary to the worker's endowment.[19]

Assumption 1.2 is sufficient to identify the cohort-and-period-specific skill premia $\mathbf{p}_{t,t-c}$, and hence the lifetime skill premia $\mathbf{P}_c$, up to scale, from the

[19] Suppose, for example, that the efficiency units z_{it} of worker i at time t are given by $z_{it} = \tilde{z}_{it}\underline{z}_{t,a(i,t)}$ where $\tilde{z}_{it} \geq 1$ is the amount of some input and $\underline{z}_{t,a} = (\nabla F_{t,a}^{'}\mathbf{x}_{t,a})^{-1}$ is a scale factor that ensures that mean earnings in each period and experience level are unity if the minimum input is always supplied. Say that the input for worker i at time t is supplied competitively, with marginal product $\underline{z}_{t,a(i,t)}\nabla F_{t,a(i,t)}^{'}\mu_i$ given by the effect of an increase in $\tilde{z}_{it}$ on total output from the worker's skill endowment, and marginal cost $\tilde{\alpha}^{-1}\left(\ln\left(\tilde{z}_{it}\right) - \eta_{it}\right)$ for η_{it} a shock. From equating marginal product and marginal cost, it follows that

$$\ln\left(\tilde{z}_{it}\right) = \tilde{\alpha}\mathbf{p}_{t,t-c}^{'}\mu_i + \eta_{it}$$

conditional expectation function of the log of earnings.

Proposition 1.2. *Under Assumption 1.2, for some scalar $\alpha > 0$, a multiple $\alpha \mathbf{P}_c$ of the lifetime skill premia for each cohort c is identified from the conditional expectation function of the log of earnings,*

$$\mathrm{E}\left(\ln\left(w_{it}\right)|\mathbf{x}_i = \mathbf{x},\ \mathbf{d}_{it} = \mathbf{d},\ c\left(i\right) = c\right),$$

for each time period $t \in \{c+1, ..., c+A\}$.

Importantly, Proposition 1.2 does not require that all determinants of earnings are observed, or that unobserved determinants of earnings are independent of skills. Instead, Proposition 1.2 requires that unobserved determinants of earnings are related to the skill endowment only through its market value, with a coefficient that does not vary across cohorts or periods. Appendix 1.D presents alternative conditions for identification of $\mathbf{P}_c$ up to scale when skills are measured with error.

Although we identify $\mathbf{P}_c$ only up to an unknown multiple $\alpha > 0$, going forward we will for simplicity write as if $\alpha = 1$. Moreover, although for concreteness Assumption 1.2 requires that $\tilde{\alpha} \geq 0$, and hence that a regression of the log of earnings on skills will tend to overstate skill premia, the proofs of Corollary 1.1 and Proposition 1.2 make clear that $\tilde{\alpha} \neq -1$ is sufficient.

1.2.4 Discussion

Assumption 1.1 is violated if long-run improvements in skill production technology favor one skill dimension over the other. Testing this assumption is difficult because it imposes a restriction only on those changes in relative skill levels that would have occurred in the absence of changes in relative skill premia.[20]

---

and therefore that Assumption 1.2 holds if

$$\mathrm{E}\left(\eta_{it}|\mu_i = \mu,\ \mathbf{d}_{it} = \mathbf{d},\ c\left(i\right) = c\right) = \tilde{\zeta}_{t,t-c} + \mathbf{d}'\beta_{t,t-c}$$

in each period t for some $\tilde{\zeta}_{t,t-c}$.

[20]Following the proof of Proposition 1.1, any data $\{(\mathbf{P}_c, \tilde{\mathbf{x}}_c)\}_{c=\underline{c}}^{\overline{c}}$ such that $\mathbf{P}_c, \tilde{\mathbf{x}}_c > 0$ for all c, with $\mathrm{sgn}\left(\ln\left(\frac{\tilde{x}_{\overline{c}1}\tilde{x}_{\underline{c}2}}{\tilde{x}_{\overline{c}2}\tilde{x}_{\underline{c}1}}\right)\right) = \mathrm{sgn}\left(\ln\left(\frac{P_{\overline{c}1}P_{\underline{c}2}}{P_{\overline{c}2}P_{\underline{c}1}}\right)\right) \neq 0$, are compatible with our model and with Assumption 1.1.

However, it is possible to obtain some clues about the plausibility of this assumption from prior research in cognitive science and economics. Improvements in schooling are one potentially important cause of changes in skill production technology. Pietschnig and Voracek (2015, Table 2) argue that higher levels of education are linked especially to greater crystallized intelligence.[21] Improvements in health and nutrition are another potentially important cause of changes in skill production technology. Pietschnig and Voracek (2015, Table 2) argue that some factors in this category (e.g., blood lead levels) do not affect fluid and crystallized intelligence differently, but that some (e.g., nutrition) have larger effects on fluid than crystallized intelligence.[22] Other changes that may have improved skill production technology include increased availability of personal technology (e.g., video games) and a reduction in disease burden (Pietschnig & Voracek, 2015, Table 2).[23]

Thus there are factors that favor crystallized intelligence, factors that favor fluid intelligence, and factors that do not favor one or the other. We may think of Assumption 1.1 as describing a situation where the opposing factors wash out. To the extent that they do not, and that changes in skill production technology favor crystallized intelligence, we expect to understate the role of labor market returns in explaining trends in skills. To the extent that changes instead favor fluid intelligence, we expect to overstate the role of labor market returns.[24]

In our empirical analysis, we explore the sensitivity of our findings to departures from Assumption 1.1 and to accounting for measurable changes in schooling and health occurring at or before the ages at which we measure

[21] Cliffordson and Gustafsson (2008) and Carlsson et al. (2015) document stronger effects of schooling on crystallized than fluid intelligence using data from the same military enlistment battery that we study.

[22] In a review of the literature, Lam and Lawlis (2017) identify randomized trials showing evidence of effects of micronutrient interventions on both fluid and crystallized intelligence, though with larger effect sizes for fluid intelligence. See also Lynn (2009, pp. 253-254).

[23] Pietschnig and Voracek (2015, pp. 290-291) note that increased access to technology may have improved fluid more than crystallized intelligence, but also that gains in fluid intelligence have been observed in countries and time periods with lower levels of access to modern technology (see also Baker et al., 2015, p. 146). Simons et al. (2016) argue that there is limited evidence of effects of interventions such as video game playing on broader cognitive performance.

[24] Say that $\frac{P_{\bar{c}1}}{P_{\bar{c}2}} > \frac{P_{\underline{c}1}}{P_{\underline{c}2}}$. If $\frac{1}{\bar{c}-\underline{c}} \sum_{c=\underline{c}}^{\bar{c}-1} \left[\ln\left(\frac{K_{c+1,1}}{K_{c+1,2}}\right) - \ln\left(\frac{K_{c1}}{K_{c2}}\right) \right] > 0$, then our construction will understate the elasticity of substitution $\frac{1}{\rho-1}$. If $\frac{1}{\bar{c}-\underline{c}} \sum_{c=\underline{c}}^{\bar{c}-1} \left[\ln\left(\frac{K_{c+1,1}}{K_{c+1,2}}\right) - \ln\left(\frac{K_{c1}}{K_{c2}}\right) \right] < 0$, then our construction will overstate it.

skills. We also study skills measured at various ages and therefore at different points in a person's schooling.

Assumption 1.2 is violated if there are unmeasured factors that directly affect earnings and whose correlation with a person's skill endowment is not proportional to the endowment's market value. In our empirical analysis, we explore the sensitivity of our findings to including proxies for candidate factors in the covariate set $\mathbf{d}_{it}$.

1.3 Data

1.3.1 Linked Data on Test Scores and Earnings

Our main analysis uses data on scores from tests administered at military enlistment, typically at age 18 or 19, for the near-population of Swedish men born between 1962 and 1975 and who enlisted between 1980 and 1993 (War Archives, 2016). Across all cohorts, these men took identical tests that were part of a group of tests called Enlistment Battery 80. Carlstedt (2000), Rönnlund et al. (2013), and Gyllenram et al. (2015) describe the tests in more detail.

To extend our analysis to a broader set of birth cohorts and earlier testing ages, we also use data on scores from tests administered, typically at age 13, as part of the Evaluation Through Follow-up, a large survey of Swedish families (Härnqvist, 2000). These data cover around 10 percent of the birth cohorts 1948, 1953, 1967, 1972, and 1977. Härnqvist (2000) and Svensson (2011) describe the tests, which were unchanged across the cohorts, and the survey in more detail.[25] We focus on males to parallel the military enlistment sample. Appendix 1.E presents supplementary findings for females.

Both data sources include tests for logical reasoning and vocabulary knowledge. In the enlistment data, the logical reasoning test consisted of drawing correct conclusions based on statements that are made complex by distracting negations or conditional clauses and numerical operations (Carlstedt & Mårdberg, 1993; Gyllenram et al., 2015). The vocabulary knowledge test consisted of correctly identifying synonyms to a set of words (Gyllenram et al., 2015).

[25] Extensions of our analysis in Appendix 1.E include data for birth cohorts 1982 and 1992, for which we can measure skill levels but have more limited information on earnings. The test administered to the 1982 and 1992 cohorts differs slightly from the test administered to earlier cohorts in aspects such as the order of possible answers.

In the survey data, the logical reasoning test consisted of guessing the next in a sequence of numbers, and the vocabulary knowledge test consisted of recognizing antonyms (Svensson, 2011, Chapter 1). In both data sources, we observe the number of questions (out of a total of 40) that each person answered correctly on each test.[26]

We treat performance on the logical reasoning test as our main measure of fluid intelligence ($j = 1$). We treat performance on the vocabulary knowledge test as our main measure of crystallized intelligence ($j = 2$). Pietschnig and Voracek (2015, Table 1) list guessing the next number in a sequence as an example of a task that measures fluid intelligence, and a vocabulary test as an example of a task that measures crystallized intelligence.[27]

Enlistees were assigned to military positions in part based on a composite cognitive score that depended on the logical reasoning test, the vocabulary knowledge test, and other tests (Grönqvist & Lindqvist, 2016, pp. 873-874, 877, 880). We are not aware of any incentives attached to the individual cognitive test components (e.g., logical reasoning, vocabulary knowledge), as opposed to the composite cognitive score, or any reason why incentives to perform well on the tests would have differed by birth cohort. The test questions are classified so could not be practiced in advance, and the exact mapping from individual cognitive test components to the composite cognitive score was not publicly known at the time of the tests. We are not aware of any incentives attached to performance on the survey tests, which are not publicly available.

We include in our analysis only those individuals for whom we observe valid logical reasoning and vocabulary knowledge scores. For each data source and each dimension j, we let x_{ij} denote the percentile rank of individual i's score within the distribution of scores of those born in 1967.[28] The skill vector

[26]Both data sources also include a test of spatial reasoning, which we use in sensitivity analysis. Appendix Figure 1.1 shows trends in the level of and premium for technical skills, which are measured in the military enlistment data but not in the survey data. Appendix Figure 1.2 shows trends in the levels of and premia for skills in the military enlistment data for men born between 1954 and 1961, for which the format of the tests was different (War Archives, 2016).

[27]Carroll (1993) lists induction and sequential reasoning as two of the three factors most frequently associated with fluid intelligence, and verbal ability as the factor most frequently associated with crystallized intelligence, in a tabulation based on a hierarchical factor analysis (pp. 598-899; see also Flanagan and Dixon, 2014).

[28]Specifically, x_{ij} is equal to the average rank of sample individuals born in 1967 who have the same score as individual i on dimension j, multiplied by 100, divided by the number of

$\mathbf{x}_i = (x_{i1}, x_{i2})$ then measures the performance of individual i on each dimension j relative to the set of individuals born in 1967. Appendix Table 1.4 shows the number of individuals in each birth cohort for each data source.

We join both sources of test scores to information on labor market earnings for the universe of Swedish residents from the Income and Tax Register for the years 1968–2018.[29] For each individual i in each year t, we let w_{it} be the total gross labor market earnings.

Portions of our analysis use additional variables. From the enlistment data (War Archives, 2016), we obtain the date on which an individual took the enlistment tests,[30] the individual's height and weight as of enlistment, and a measure of non-cognitive skill that follows a standardized distribution.[31] From other sources we obtain administrative data on each individual's employment history (Statistics Sweden, 2020b, 2021) , foreign-born status (Statistics Sweden, 2014a), secondary schooling completion (Statistics Sweden, 2014c), region of birth (Statistics Sweden, 2021), family relations (Statistics Sweden, 2014a), and parental labor market earnings (Statistics Sweden, 2014b, 2021).

Appendix Table 1.1 presents sensitivity analyses with respect to many of the choices we have made in constructing the sample and variables for our analysis, including varying the set of included cohorts, measuring an individual's skill with the percent of the maximum possible score rather than with the percentile rank, combining logical and spatial reasoning skills into a single composite measure of fluid intelligence, and including business income in the measure of earnings. We summarize the quantitative implications of these choices in Section 1.4.2.

---

sample individuals born in 1967, and centered by adding a constant so that x_{ij} has an average value of 50 among those born in 1967.

[29] Data on labor market earnings for 1990–2018 are from Statistics Sweden (2021), where we define gross labor market earnings using the concept described in Statistics Sweden (2016a, pp. 137-138). Data for 1968–1989 are from Statistics Sweden (2014b), where we approximate the concept described in Statistics Sweden (2016a, pp. 137-138) using the available data fields. For sensitivity analysis we also obtain data on business income for 1990–2018 from Statistics Sweden (2021). We define a total income measure combining labor market earnings and business income using the concept described in Statistics Sweden (2016a, pp. 141-142).

[30] We match information on enlistment test date to our other data using information on parish of residence from Statistics Sweden (2016b).

[31] Non-cognitive skill is evaluated based on an interview and scored on a Stanine (1–9) scale. Lindqvist and Vestman (2011, pp. 107-109 and Appendix F) and Edin et al. (2022, p. 6) describe the measure in more detail.

1.3.2 Original Survey of Parents' Perceptions

We conducted an original survey to assess the importance that parents place on different types of skills. We hosted the survey on a Stockholm University survey platform. We recruited participants via Facebook ads from October 17 through October 24, 2020. During this time, 1,199 respondents began the survey and 983 completed it. We asked each respondent their own year of birth as well as the range of birth years of their children, if any. We include in our analysis the 716 respondents who reported that their first child was born at least 16 years after their own birth year.

We asked these respondents the following question:

> *As a parent, how much do you encourage (or did you encourage) your children to develop the qualities below while growing up?*
>
> *To be able to think critically and solve problems logically.*
>
> *To be able to remember facts, such as the definitions of difficult words.*

We intended the first quality to approximate the concept of fluid intelligence and the second to approximate the concept of crystallized intelligence. We also asked respondents about the importance of each quality in today's society, how much their own parents emphasized each quality, and how much their own primary school emphasized each quality. There were five possible answers ranging from "Not at all" to "Very much," and we classified each response according to whether the person rated the first quality as more important, the second quality as more important, or neither.

Appendix Figure 1.3 gives screenshots of the consent form and survey form. Appendix Figure 1.4 shows the distribution of year of birth, and year of birth of first child, among the respondents in our sample.

1.4 Results

1.4.1 Trends in Skills and Skill Premia

We let $c(i)$ be the year that worker i turns 29 and we let $A = 26$, so that the working life is from ages 30 through 55. Appendix Figure 1.5 shows that full-time work tends to be highest during these years.

We estimate the parameter $\mathbf{p}_{t,a}$ in equation (1.1) by ordinary least squares regression of the log of labor market earnings $\ln(w_{it})$ on the vector of percentile ranks $\mathbf{x}_i$, separately for each worker experience level (age) a and for each year t for which we measure earnings, excluding men with zero earnings. This yields an estimate of $\mathbf{p}_{c+a,a}$ for each c,a such that $c+a \leq T$, for T the most recent year of earnings data available. Appendix Figure 1.6 illustrates the fit of the regression model for three example cohorts at three different ages.

To estimate $\mathbf{p}_{c+a,a}$ for c,a such that $c+a > T$, we take the average estimate for the given cohort c for all ages $a > 10$ for which a regression estimate of $\mathbf{p}_{c+a,a}$ is available. Appendix Figure 1.7 illustrates this extrapolation for three example cohorts.

We plug the resulting estimates of $\mathbf{p}_{c+a,a}$ into equation (1.3), along with the value $\delta = 0.96$, to get an estimate of the lifetime skill premia $\mathbf{P}_c$ for the cohorts $c \in \{\underline{c},...,\overline{c}\}$. We obtain standard errors for $\mathbf{P}_c$ via a nonparametric bootstrap in which we sample individuals i with replacement.

Figure 1.1 depicts the average skill levels $\overline{\mathbf{x}}_c$ and the estimated lifetime skill premia $\mathbf{P}_c$ across cohorts in the enlistment data along with their 95 percent pointwise confidence intervals and uniform confidence bands. For convenience we label cohorts with their birth year, i.e., $c-29$. Figure 1.1 also depicts the lines of best fit through the plotted series.

Panel A of Figure 1.1 shows that logical reasoning skill rose, on average, by 4.4 percentile points, relative to the 1967 distribution, across the birth cohorts from 1962 to 1975. By contrast, vocabulary knowledge skill fell, on average, by 2.9 percentile points. Appendix Figure 1.8 depicts the cumulative distribution functions of skills in the 1962 and 1975 cohorts. Appendix Figure 1.9 compares trends in skill in our data to those measured in other countries.

Panel B of Figure 1.1 shows that the lifetime skill premium fell for both logical reasoning and vocabulary knowledge. The line of best fit indicates that the lifetime premium for a percentile point of logical reasoning skill fell from 0.48 to 0.40 log points across the birth cohorts from 1962 to 1975, and the lifetime premium for a percentile point of vocabulary knowledge fell from 0.16 to 0.09 log points. Thus, the lifetime premium for both skill dimensions fell, with a proportionately much greater decline for vocabulary knowledge.[32] Appendix

[32]Prior work finding evidence of declining returns to cognitive skill includes Castex and Dechter (2014) for the US, Markussen and Røed (2020) for Norway, and Edin et al. (2022) for

Figure 1.10 depicts estimated lifetime skill premia based on a generalization of equation (1.1) that allows interactions between the skill dimensions.

Panel A of Figure 1.2 depicts the evolution of the relative skill levels $\ln\left(\frac{\bar{x}_{c1}}{\bar{x}_{c2}}\right)$ and of the relative lifetime skill premia $\ln\left(\frac{P_{c1}}{P_{c2}}\right)$ across the two dimensions. The plot shows that both objects tend to increase with later birth cohorts and are fairly close to the line of best fit, evoking a movement along a relative linear supply curve as in equation (1.5). Figure 1.3 shows that a similar qualitative pattern obtains in our survey sample, which is smaller and for which estimates tend to be less precise. Appendix Figure 1.11 depicts the underlying estimates of skill levels and lifetime skill premia for men in the survey sample. Appendix Figure 1.12 depicts the evolution of relative skill levels and relative lifetime skill premia for women in the survey sample. Appendix Figure 1.13 depicts the evolution of relative skill levels and relative lifetime skill premia in the enlistment sample by region of birth.

Under the conditions in Appendix 1.D, our approach to identification and estimation of relative skill premia remains valid even in the presence of measurement error in skills. As an alternative exploration of the role of measurement error, requiring different assumptions from those in Appendix 1.D, Panel A of Appendix Table 1.5 shows estimates of the trend in skill premia computed using the individuals present in both the enlistment and survey data, instrumenting for skills measured at enlistment with skills measured in the survey. The sample is small and the instrumental variables estimates are imprecise. The confidence intervals on the estimated trends include zero and also include the slope of the linear fit from Panel B of Figure 1.1. Relative to the slope of the linear fit from Panel B of Figure 1.1, instrumental variables estimates tend to show growth in the premium to logical reasoning and more rapid decline in the premium to vocabulary knowledge, suggesting even stronger trends in labor-market incentives to invest in logical reasoning at the expense of vocabulary knowledge than in our baseline calculations. Panel B of Appendix Table 1.5 reports small and statistically insignificant trends in the correlation between skills measured in the survey data and those measured in the enlistment data.

Appendix Table 1.1 presents sensitivity analyses with respect to many of the choices we have made in constructing the sample and variables for our anal-

Sweden.

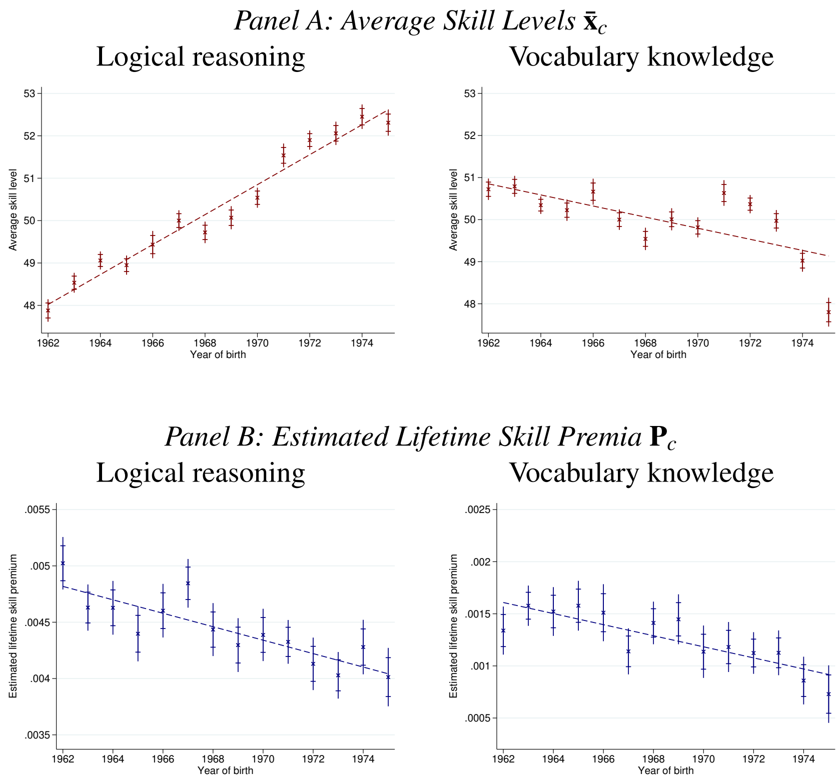
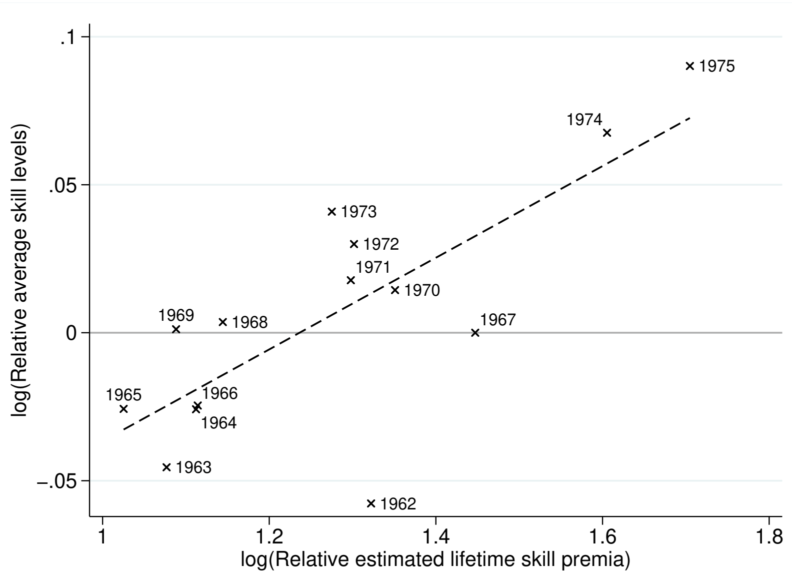


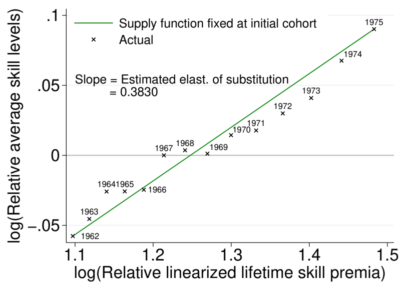
Figure 1.1: Trends in Skills and Skill Premia across Birth Cohorts 1962–1975, Military Enlistment Sample

Notes. Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. Panel A depicts the average skill $\bar{\mathbf{x}}_c$ for each birth cohort c. Skills are expressed as a percentile of the distribution for the 1967 birth cohort. Panel B depicts the estimated lifetime skill premia $\mathbf{P}_c$ for each birth cohort, constructed as described in Section 1.4.1. Each plot depicts both 95 percent pointwise confidence intervals (inner intervals, marked by dashes) and 95 percent uniform confidence bands (outer intervals, marked by line segments). Pointwise confidence intervals are based on standard errors from a nonparametric bootstrap with 50 replicates. Uniform confidence bands are computed as sup-t bands following Montiel Olea and Plagborg-Møller (2019). Each plot depicts the line of best fit through the estimated points.

Panel A: Relative Skill Levels and Relative Skill Premia



Panel B: Illustration of Relative Supply Function



Panel C: Differences in Skills and Differences in Skill Premia

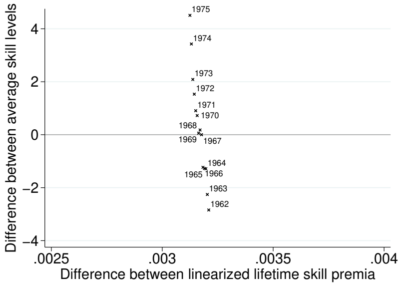


Figure 1.2: Evolution of Relative Skill Levels and Relative Skill Premia, Military Enlistment Sample

Notes. Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. Panel A shows a scatterplot of the natural logarithm of the relative average skill levels, $\ln\left(\frac{\bar{x}_{c1}}{\bar{x}_{c2}}\right)$, against the natural logarithm of the relative estimated lifetime skill premia, $\ln\left(\frac{P_{c1}}{P_{c2}}\right)$. The dashed line depicts the line of best fit. Panel B shows a scatterplot of the natural logarithm of the relative average skill levels, $\ln\left(\frac{\bar{x}_{c1}}{\bar{x}_{c2}}\right)$, against the natural logarithm of the relative estimated lifetime skill premia, $\ln\left(\frac{P_{c1}}{P_{c2}}\right)$, based on the linearized skill premia depicted in Panel B of Figure 1.1. The solid line shows the relative skill supply function estimated for the 1962 birth cohort, i.e., the relationship between $\ln\left(\frac{\bar{x}_{\underline{c},1}(\mathbf{P}_c)}{\bar{x}_{\underline{c},2}(\mathbf{P}_c)}\right)$ and $\ln\left(\frac{P_{c1}}{P_{c2}}\right)$. The slope of the solid line is equal to the estimated elasticity of substitution $\frac{1}{\rho-1}$. Panel C shows a scatterplot of the difference between average skill levels, $\bar{x}_{c1}-\bar{x}_{c2}$, against the difference between estimated lifetime skill premia, $P_{c1}-P_{c2}$, based on the linearized skill premia depicted in Panel B of Figure 1.1. The ratio of the x-axis range to the x-axis value for the 1962 birth cohort is equal to the analogous ratio in Panel B.

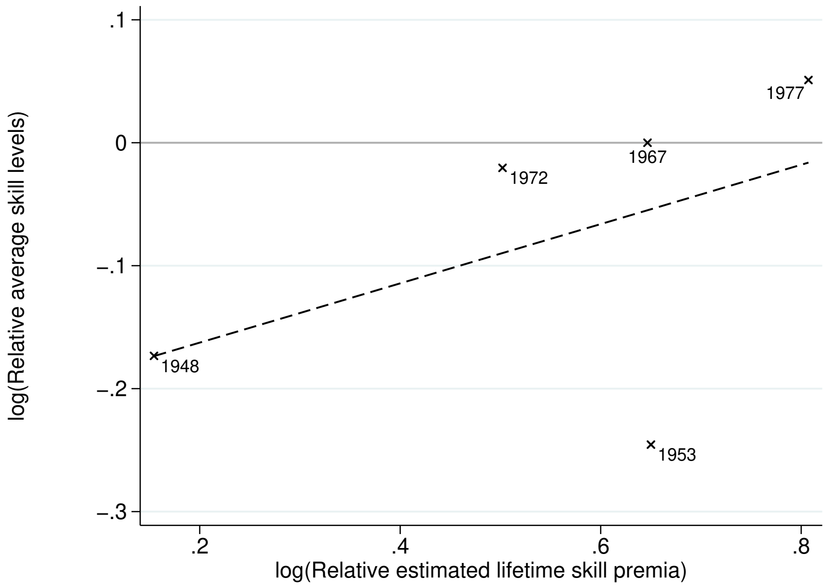


Figure 1.3: Evolution of Relative Skill Levels and Relative Skill Premia, Survey Sample

Notes. Data are from the survey sample covering birth cohorts 1948, 1953, 1967, 1972, and 1977, with tests typically taken at age 13. The plot shows a scatterplot of the natural logarithm of the relative average skill levels, $\ln\left(\frac{\bar{x}_{c1}}{\bar{x}_{c2}}\right)$, against the natural logarithm of the relative estimated lifetime skill premia, $\ln\left(\frac{P_{c1}}{P_{c2}}\right)$. The dashed line depicts the line of best fit.

ysis, including altering the assumed ages of working life, restricting to workers who are employed year-round in a typical year, averaging over a shorter or longer span of ages to extrapolate premia to working years we do not observe in the data, and varying the assumed value of δ. We summarize the quantitative implications of these choices in Section 1.4.2.

1.4.2 Model Estimates and Counterfactuals

We estimate the skill supply function $\tilde{\mathbf{x}}_c(\cdot)$ for each cohort in the enlistment sample following the construction in the proof of Proposition 1.1. We take $J = 2$. We take the average skill $\overline{\mathbf{x}}_c$ in each cohort as our estimate of $\tilde{\mathbf{x}}_c$. We take the linear fit in Panel B of Figure 1.1 as our estimate of the lifetime skill premia $\mathbf{P}_c$.[33] We may think of the linear fit either as a way of smoothing the sampling variation in the data, or as a way of approximating the forward-looking expectations of workers at the time the skill investment decision is made. Panel A of Table 1.1 reports estimates of key parameters.

Figure 1.4 shows the evolution of logical reasoning and vocabulary knowledge skill in the data and in the counterfactual scenario in which the lifetime skill premia $\mathbf{P}_c$ remain constant at their initial level $\mathbf{P}_{\underline{c}}$. In the counterfactual scenario, logical reasoning skill increases by 2.8 percentile points instead of 4.4 as in the actual data. Vocabulary knowledge skill increases by 3.0 percentile points rather than falling by 2.9 percentile points. In this sense, according to the model, changes in the lifetime skill premia $\mathbf{P}_c$ account for 36.8 percent of the increase in logical reasoning skill (with a standard error of 1.7 percent), and for more than the entire decline in vocabulary knowledge skill.

To unpack the findings in Figure 1.4, begin with estimation of the elasticity of substitution $\frac{1}{\rho-1}$. Under Assumption 1.1, all long-term change in relative skill levels across cohorts must be due to change in relative skill premia. In particular, the elasticity of substitution $\frac{1}{\rho-1}$ can be estimated as the ratio of the long-term change in relative skill levels to the long-term change in relative skill premia. Panel B of Figure 1.2 illustrates by plotting the log of the relative estimated average skill level $\ln\left(\frac{\bar{x}_{c1}}{\bar{x}_{c2}}\right)$ against the log of the relative estimated (linearized) skill premia $\ln\left(\frac{P_{c1}}{P_{c2}}\right)$. Under Assumption 1.1, the linear relative supply curve $\ln\left(\frac{\tilde{x}_{\underline{c}1}(\cdot)}{\tilde{x}_{\underline{c}2}(\cdot)}\right)$ defined by the estimated skill supply function $\tilde{\mathbf{x}}_{\underline{c}}(\cdot)$ for the 1962 birth cohort must pass through the points on the scatterplot for both the 1962 and 1975 birth cohorts. This implies an elasticity of substitution of $\frac{1}{\rho-1} = 0.383$, which is in turn the slope of the line $\ln\left(\frac{\tilde{x}_{\underline{c}1}(\cdot)}{\tilde{x}_{\underline{c}2}(\cdot)}\right)$ depicted on the plot.

Next, consider estimation of the remaining parameters of the skill supply function $\tilde{\mathbf{x}}_c(\cdot)$. Given the data, under any elasticity of substitution less than 0.97, the model implies that changes in relative premia alone are too small to explain the large increase in logical reasoning skill. We can therefore infer an upward shift in the first dimension of the skill supply function $\tilde{x}_{c1}(\cdot)$ across cohorts, i.e., growth in logical reasoning skill beyond what can be explained by

[33] Consistent with the regularity condition in Proposition 1.1, based on the linear fit we reject the null hypothesis that $\ln\left(\frac{P_{\underline{c}1}}{P_{\underline{c}2}}\right) = \ln\left(\frac{P_{\bar{c}1}}{P_{\bar{c}2}}\right)$ at conventional significance levels ($p = 0.0006$).

Table 1.1: Summary of data and model implications

Panel A: Baseline

	Logical reasoning	Vocabulary knowledge
Initial lifetime skill premium, 1962	0.0048	0.0016
$P_{\underline{c}j}$	(0.0001)	(0.0001)
Change in lifetime skill premium, 1962–1975	-0.0008	-0.0007
$P_{\bar{c}j} - P_{\underline{c}j}$	(0.0001)	(0.0001)
Initial average skill rank, 1962	47.88	50.72
$\bar{x}_{\bar{c}j}$	(0.14)	(0.13)
Change in average skill rank 1962–1975	4.43	-2.92
$\bar{x}_{\bar{c}j} - \bar{x}_{\underline{c}j}$	(0.22)	(0.21)
Under estimated model:		
Change in average skill rank, 1962–1975 at initial skill premia	2.80	2.97
$\tilde{x}_{\bar{c}j}\left(\mathbf{P}_{\underline{c}}\right) - \tilde{x}_{\underline{c}j}\left(\mathbf{P}_{\underline{c}}\right)$	(0.21)	(0.22)
Share of observed change explained by change in skill premia	0.3681	2.0151
$1 - \frac{\tilde{x}_{\bar{c}j}(\mathbf{P}_{\underline{c}}) - \tilde{x}_{\underline{c}j}(\mathbf{P}_{\underline{c}})}{\bar{x}_{\bar{c}j} - \bar{x}_{\underline{c}j}}$	(0.0175)	(0.1483)
Substitution parameter	3.61	
ρ	(0.76)	
[Implied elasticity of substitution $\frac{1}{\rho-1}$]	[0.3830]	

Panel B: Accounting for Non-Cognitive Skills

	Logical reasoning	Vocabulary knowledge
Initial lifetime skill premium, 1962	0.0037	0.0009
$P_{\underline{c}j}$	(0.0001)	(0.0001)
Change in lifetime skill premium, 1962–1975	-0.0009	-0.0006
$P_{\bar{c}j} - P_{\underline{c}j}$	(0.0001)	(0.0001)
Initial average skill rank, 1962	47.88	50.72
$\bar{x}_{\bar{c}j}$	(0.14)	(0.13)
Change in average skill rank 1962–1975	4.43	-2.92
$\bar{x}_{\bar{c}j} - \bar{x}_{\underline{c}j}$	(0.22)	(0.21)
Under estimated model:		
Change in average skill rank, 1962–1975 at initial skill premia	3.27	3.46
$\tilde{x}_{\bar{c}j}\left(\mathbf{P}_{\underline{c}};\tilde{\mathbf{x}}_{\bar{c},L+1:J}\right) - \tilde{x}_{\underline{c}j}\left(\mathbf{P}_{\underline{c}};\tilde{\mathbf{x}}_{\underline{c},L+1:J}\right)$	(0.22)	(0.24)
Share of observed change explained by change in skill premia	0.2617	2.1860
$1 - \frac{\tilde{x}_{\bar{c}j}(\mathbf{P}_{\underline{c}};\tilde{\mathbf{x}}_{\bar{c},L+1:J}) - \tilde{x}_{\underline{c}j}(\mathbf{P}_{\underline{c}};\tilde{\mathbf{x}}_{\underline{c},L+1:J})}{\bar{x}_{\bar{c}j} - \bar{x}_{\underline{c}j}}$	(0.0206)	(0.1636)
Substitution parameter	5.72	
ρ	(1.65)	
[Implied elasticity of substitution $\frac{1}{\rho-1}$]	[0.2120]	

Notes. Data are from the military enlistment sample covering birth cohorts 1962–1975. Standard errors in parentheses are obtained via a nonparametric bootstrap with 50 replicates. In Panel A, estimates of $\bar{\mathbf{x}}_c$ and $\mathbf{P}_c$ follow Figure 1.1 with the linear fit used as our estimate of $\mathbf{P}_c$. Estimates of $\tilde{\mathbf{x}}_c(\cdot)$ follow the proof of Proposition 1.1. The unknown parameters are ρ and $\left\{\mathbf{K}_c, \overline{S}_c\right\}_{c=\underline{c}}^{\bar{c}}$. Take $\bar{\mathbf{x}}_c$ as our estimate of $\tilde{\mathbf{x}}_c$. Then estimate the elasticity of substitution $\frac{1}{\rho-1}$ following equation (3.3). Next, estimate the relative cost parameters $\frac{K_{c2}}{K_{c1}}$ in each cohort c following equation (1.2). From the normalization used in the proof of Proposition 1.1, estimate K_{c1} following equation (1.3), from which estimate K_{c2} using the ratio $\frac{K_{c2}}{K_{c1}}$. Finally, estimate the skill budget $\overline{S}_c$ following equation (1.4). In Panel B, estimates follow Section 1.6, with $L = 2$ and $J = 3$. We estimate $\mathbf{P}_{c,1:L}$ from earnings regressions that control for a standardized measure of non-cognitive skill, excluding from the sample any worker missing information on non-cognitive skill. The rest of the analysis follows similarly to Panel A, following the logic in the proof of Proposition 1.3.

changes in premia alone. And, given Assumption 1.1, the model implies that there must also have been an upward shift in the second dimension of the skill supply function $\tilde{x}_{c2}(\cdot)$ across cohorts, i.e., that vocabulary knowledge would have risen absent changes in skill premia.

Following the constant elasticity form of the transformation function in equation (1.4) and the log-linear form of the relative supply function in equation (1.5), our discussion has focused on ratios of skill premia rather than on their differences. It seems likely that a model focusing instead on differences in premia would imply a different conclusion regarding the role of changes in premia in explaining cohort trends in skill levels. To illustrate why, Panel C of Figure 1.2 presents an analogue of the scatterplot in Panel B of Figure 1.2, but replacing log ratios of skill levels and skill premia with their differences. Panel C of Figure 1.2 shows that the difference in premia between logical reasoning and vocabulary knowledge did not rise across successive cohorts in the way that Panel B of Figure 1.2 shows that the ratio of premia did. Following Figure 1.1, we find it intuitive that as the premium to vocabulary knowledge fell to a very low level while the premium to logical reasoning skill remained nontrivial, individuals would substitute effort away from vocabulary knowledge, as implied by the constant elasticity form of the transformation function in equation (1.4).

Appendix Table 1.1 presents sensitivity analysis with respect to choices we have made in constructing the sample and variables for our analysis. Rows (b) and (c) concern the set of birth cohorts we include. Rows (d) and (e) concern the measurement of skills $\mathbf{x}_i$. Row (f) concerns the measurement of earnings w_{it}. Rows (g) through (i) concern the experience levels a and individuals i included in the analysis. Rows (j) through (m) concern the construction of estimates of lifetime skill premia $\mathbf{P}_c$ from estimates of period-specific premia $\mathbf{p}_{c+a,a}$. Rows (n) and (o) concern the smoothing of the estimated lifetime skill premia $\mathbf{P}_c$. Across these different sensitivity analyses, we estimate that changes in lifetime skill premia account for between 29.4 and 46.5 percent of the increase in logical reasoning skill, which can be compared to our baseline estimate of 36.8 percent. Appendix Figure 1.14 extends our analysis to a larger set of cohorts, and to women, using the survey sample. We estimate that changes in lifetime skill premia account for a larger share of the increase in logical reasoning skill than in our baseline estimate, though the estimates from

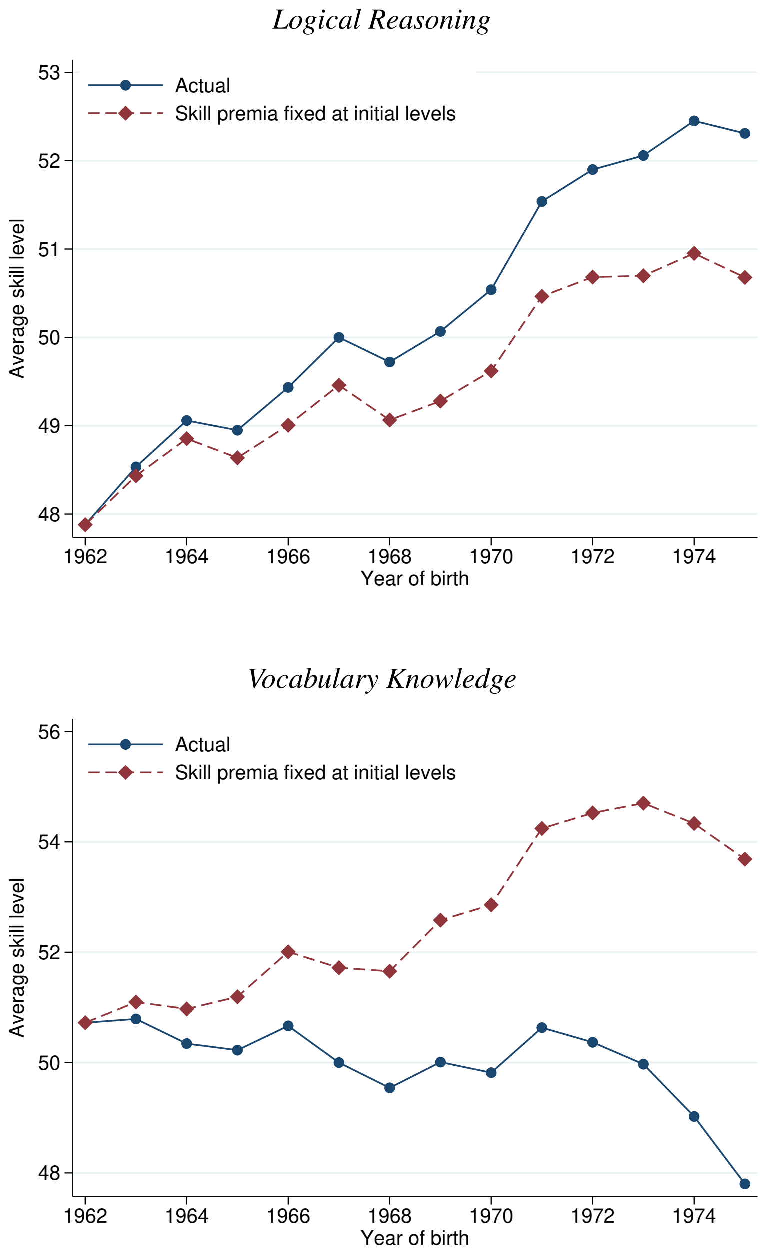


Figure 1.4: Decomposition of Change in Average Skill Level, Military Enlistment Sample

Notes. Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. Each plot depicts the average skill $\bar{\mathbf{x}}_c$ for each birth cohort c ("Actual") and the predicted average skill $\tilde{\mathbf{x}}_c\left(\mathbf{P}_{\underline{c}}\right)$ under the counterfactual in which lifetime skill premia remain at the level estimated for the 1962 birth cohort ("Skill premia fixed at initial levels"). Skills are expressed as a percentile of the distribution for the 1967 birth cohort.

the survey sample are less precise than our baseline estimate.

1.4.3 Sensitivity to Assumption 1.1

Figure 1.5 shows how our conclusions change as we depart from Assumption 1.1. The upper plot is for logical reasoning skill and the lower plot is for vocabulary knowledge. Each plot shows the relationship between the estimated share of the change in the given skill dimension explained by changes in the lifetime skill premia (y-axis) and the average relative shock to the supply of skill (x-axis). We measure the shock as a fraction of the observed change in relative skill levels. A positive shock implies that changes in skill-producing technology favored fluid intelligence over crystallized intelligence, on average across the cohorts that we study. A negative shock implies the reverse. A shock of zero corresponds to the case in which Assumption 1.1 holds, and thus to the estimates in Figure 1.4 and Panel A of Table 1.1.

A reader can use Figure 1.5 to gauge the effect of a given departure from Assumption 1.1 on our conclusions. Figure 1.5 thus improves transparency in the sense of Andrews et al. (2017, 2020) and Andrews and Shapiro (2021).

To illustrate the utility of Figure 1.5 with an example, consider the possibility that changes across cohorts in time spent in school shifted the relative supply of different skills. Carlsson et al. (2015) estimate that additional time in school improves performance on the vocabulary knowledge test that we study, and do not find evidence that additional time in school improves performance on the logical reasoning test. We estimate that, relative to the 1962 birth cohort, members of the 1975 birth cohort spent 0.40 more years in school as of the date of test-taking. If at least some of the increase in schooling time would have occurred absent changes in skill premia, then Carlsson et al.'s (2015) analysis implies that increased schooling time can be considered a positive shock to the relative supply of crystallized intelligence, or equivalently a negative shock to the relative supply of fluid intelligence. Figure 1.5 shows that if there is a negative shock to the relative supply of fluid intelligence, then our baseline estimates understate the share of the change in skill levels that can be accounted for by changes in skill premia. If we take the entire increase in schooling time as a supply shock, and assume no other shocks to the relative supply of the two skill dimensions, we can use the estimates in Carlsson et al. (2015) in

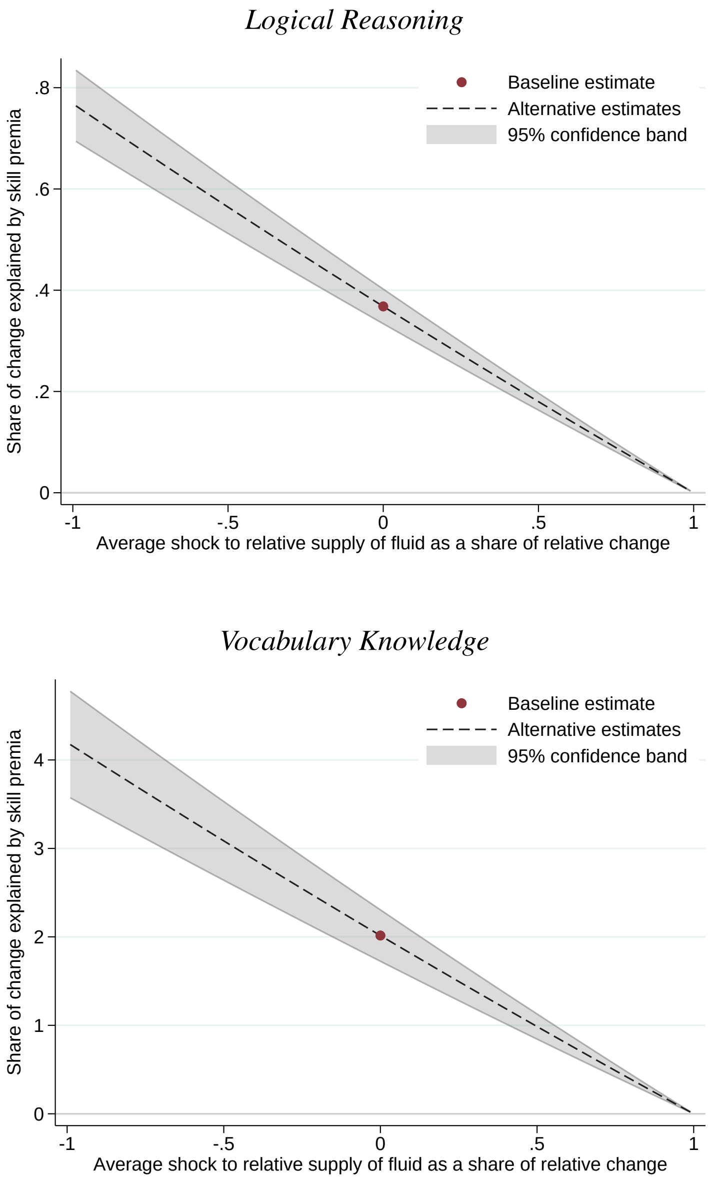


Figure 1.5: Sensitivity to Departures From Zero Average Relative Supply Shock

Notes. Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. In each plot, the curve labeled "Alternative estimates" depicts the estimated share $1-\frac{\tilde{x}_{\bar{c}j}(\mathbf{P}_{\underline{c}})-\tilde{x}_{\underline{c}j}(\mathbf{P}_{\underline{c}})}{\bar{x}_{\bar{c}j}-\bar{x}_{\underline{c}j}}$ of the change in observed skills on dimension j explained by the change in skill premia (y-axis) as a function of the average relative supply shock $-\frac{1}{\bar{c}-\underline{c}}\sum_{c=\underline{c}}^{\bar{c}-1}\left[\ln\left(\frac{K_{c+1,1}}{K_{c+1,2}}\right)-\ln\left(\frac{K_{c1}}{K_{c2}}\right)\right]$ (x-axis). The average relative supply shock is expressed as a share of the estimated change $\ln\left(\frac{\bar{x}_{\bar{c}1}}{\bar{x}_{\bar{c}2}}\right)-\ln\left(\frac{\bar{x}_{\underline{c}1}}{\bar{x}_{\underline{c}2}}\right)$ in relative skill levels between the 1962 and 1975 birth cohorts, with positive values implying changes in skill-producing technology that favor fluid relative to crystallized intelligence. The shaded region collects pointwise 95% confidence intervals obtained via a nonparametric bootstrap with 50 replicates. The estimate labeled "Baseline estimate" corresponds to the estimate in Panel A of Table 1.1, obtained under Assumption 1.1.

tandem with Figure 1.5 to calculate that changes in lifetime skill premia explain 53.5 percent of the observed increase in logical reasoning skill, which is 16.7 percentage points more than our baseline estimate of 36.8 obtained under Assumption 1.1.[34]

A similar exercise is possible with respect to assumptions about the measurement of skill. To illustrate, Appendix Figure 1.15 depicts our findings regarding trends in actual and counterfactual skills under the assumption that a portion of the cohort trend in logical reasoning skill (upper panel) or vocabulary knowledge skill (lower panel) is spurious. One possible source of spurious trends is a general improvement in test-taking ability (e.g., Jensen, 1998; Neisser, 1997, pp. 332-333), though this would not by itself explain the simultaneous rise in logical reasoning skill and decline in vocabulary knowledge. Another possible source of spurious trends, specific to vocabulary knowledge, is greater test difficulty for later cohorts due to gradual obsolescence of the words on the test (e.g., Alwin & Pacheco, 2012; Hauser & Huang, 1997; Roivainen, 2014). Appendix Figure 1.15 shows that if a portion of the measured decline in vocabulary knowledge is spurious, our analysis will tend to overstate the role of labor market returns in explaining cohort trends in logical skill, though even if there were no trend in vocabulary knowledge we would still infer that 22.7 percent (SE = 0.6) of the trend in logical skill was due to changes in labor market returns. As more concrete evidence on trends in word usage, Appendix Figure 1.16 shows estimates of the exposure of each cohort to words on example synonym questions for a recent enlistment battery, measuring word exposure based on usage in a major Swedish newspaper. The hypothesis that words on the enlistment battery are more familiar to those

[34]Carlsson et al. (2015, Table 3, column 1) estimate that an additional 100 days of schooling increases performance in the vocabulary knowledge test by 0.112 standard deviations, relative to the population of test-takers in 1980–1994. Among individuals in our enlistment data, those born in 1975 completed on average 0.40 more years of schooling at enlistment than those born in 1962. As there are roughly 180 schooling days per year in Sweden (Carlsson et al., 2015, p. 538), this implies an increase of 0.0803 standard deviations in vocabulary knowledge skill. Interpolating around the median test score, we estimate that an increase of 0.0803 standard deviations in vocabulary test score is equivalent to an increase of 3.29 percentile points among those born in 1962. Based on the skill levels reported for the 1962 cohort in Panel A of Table 1.1, an increase of 3.29 percentile points in vocabulary knowledge skill would have reduced the log ratio of logical reasoning and vocabulary knowledge skills by 0.063, or by 0.426 of the observed change. Given a relative supply shock of -0.426, Figure 1.5 implies that changes in skill premia account for 53.5 percent of the observed increase in logical reasoning.

born closer to the time of the test design would predict an increasing trend in exposure. We do not find evidence of such a trend.

1.4.4 Sensitivity to Controls

We explore the sensitivity of our conclusions to adjusting for covariates. We adjust both the estimated trend in mean skills $\overline{\mathbf{x}}_c$ and the estimated trend in lifetime skill premia $\mathbf{P}_c$ with respect to individual-specific, time-invariant covariates $\mathbf{d}_i$ that are normalized to have mean zero among those born in 1967. We adjust the estimated trend in mean skills by estimating a regression of skills x_{ij} on cohort indicators and covariates $\mathbf{d}_i$, excluding the constant.[35] We then treat the coefficients on the cohort indicators as a covariate-adjusted measure of mean skills. We adjust the estimated trend in lifetime skill premia $\mathbf{P}_c$ by including the covariates $\mathbf{d}_i$ in the time-and-age-specific earnings regressions from which we estimate $\mathbf{p}_{t,a}$.

Selection of covariates for inclusion in this exercise is delicate. For adjusting the trend in mean skills, we wish to consider adjusting only for covariates whose cohort trends do not respond to skill premia $\mathbf{P}_c$. For example, if a trend in mean heights would have occurred even absent changes in $\mathbf{P}_c$, then it may be appropriate to adjust the trend in mean skills for the trend in mean heights, and thus to study the effect of skill premia $\mathbf{P}_c$ on the part of the trend in skills that cannot be accounted for by the trend in height. By contrast, if trends in the content of schooling occur in response to changes in skill premia $\mathbf{P}_c$, then these trends are part of the skill investment process that we model, and we do not want to study the effect of skill premia $\mathbf{P}_c$ on only the part of the trend in skills that cannot be accounted for by the trend in the content of schooling.[36] Likewise, for adjusting the trend in lifetime skill premia $\mathbf{P}_c$, we wish

[35] Within the model in Section 1.2, we may think of this exercise as re-normalizing the skill endowment μ_i to have cohort-specific mean $\Gamma\overline{\mathbf{d}}_c$ where $\overline{\mathbf{d}}_c$ is the cohort-specific mean of $\mathbf{d}_i$ and Γ is a matrix whose j^{th} row contains the coefficients on $\mathbf{d}_i$ in the regression of skills x_{ij} on cohort indicators and covariates $\mathbf{d}_i$.

[36] Trends in parents' skills may likewise be attributable to (earlier) trends in labor market returns. Suppose, for example, that for each cohort c and skill j, $K_{cj} = \underline{K}_{cj}\overline{x}_{c-g,j}^{-\phi}$ where $\underline{K}_{cj} > 0$ is a scalar, $\overline{x}_{c-g,j}$ is the mean skill level in the parental cohort born $g > 0$ years before cohort c, and $\phi \geq 0$ is a parameter governing the intergenerational transmission of skills. Then if we envision a counterfactual change to the time path of skill premia, the skill investment of cohort c will change both due to a direct effect on its incentives, and an indirect effect via the incentives of the parental cohort $c-g$.

to consider adjusting only for covariates that exert a direct effect on earnings independently of their relationship to skills.

Appendix Table 1.2 shows how our findings change when we adjust for age at enlistment, an indicator for having completed secondary school at the time of enlistment or at age 18, log(height) and log(weight) measured at the time of enlistment, and an indicator for being born outside of Sweden. Across these exercises, we find that changes in labor market returns consistently account for at least 35.5 percent of the increase in logical skill, and for more than the entire decline in vocabulary knowledge skill.

1.4.5 Heterogeneity

Appendix Table 1.3 shows how our findings change when we estimate the model separately for workers with below- vs. above-median parental earnings.[37] We estimate that changes in skill premia explain 1.3 percentage points more of the increase in logical reasoning skill for those whose parents have above-median earnings than for those whose parents have below-median earnings, though the difference is not statistically significant (SE = 4.3).

1.5 Trends In Emphasis Among Parents, Schools, And Occupations

Sections 1.5.1 and 1.5.2 explore whether parents and schools increasingly emphasize reasoning over knowledge. Section 1.5.3 explores whether changes in the occupation mix favor reasoning-intensive as opposed to knowledge-intensive occupations. Evidence that parents, schools, and occupations have shifted to emphasize reasoning over knowledge does not, on its own, establish that changes in production technology are driving changes in skill investment. Such evidence can, however, serve to make tangible some of the real-world processes that underlie the skill investment decision modeled in Section 1.2.2 and the production economy modeled in Section 1.2.1.

[37] To nest this exercise within the model in Section 1.2, we can suppose there are two distinct labor markets, one for each group of workers, with the two markets potentially linked by a common production function $F_t(\cdot)$.

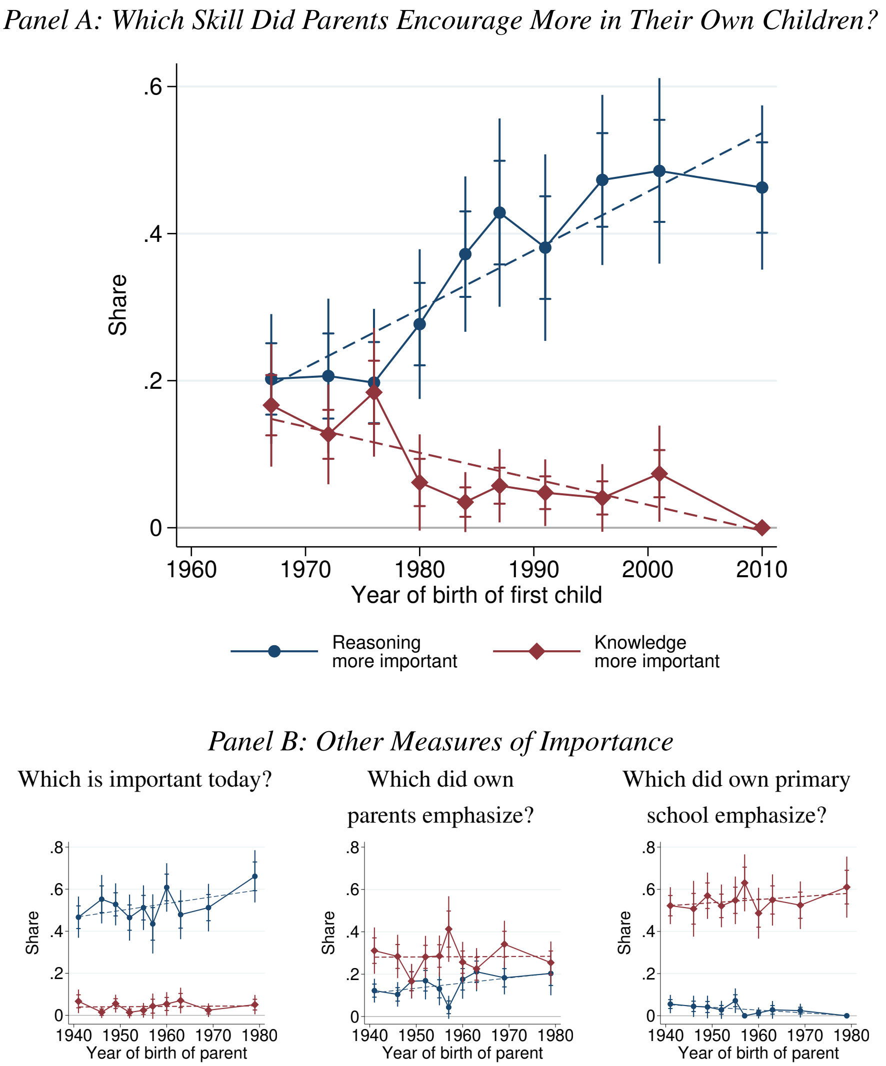


Figure 1.6: Trends in the Perceived Importance of Different Skills in the Survey of Parents' Perceptions

Notes. Data are from the original survey of parents' perceptions described in Section 1.3.2. Each figure shows the fraction of respondents rating reasoning as more important (circles) and the fraction rating knowledge as more important (diamonds), separately by decile of the birth cohort of the respondent's first child (Panel A) or of the respondent (Panel B), with deciles labeled by the integer-rounded mean year of birth within the decile. Each plot depicts both 95 percent pointwise confidence intervals (inner intervals, marked by dashes) and 95 percent uniform confidence bands (outer intervals, marked by line segments). Pointwise confidence intervals are based on standard errors from a nonparametric bootstrap with 50 replicates, stratified by birth cohort decile. Uniform confidence bands are computed as sup-t bands following Montiel Olea and Plagborg-Møller (2019). Each plot depicts the line of best fit through the estimated points.

1.5.1 Parents

Panel A of Figure 1.6 depicts trends in the perceived importance of different skills among parents, as reported in the survey described in Section 1.3.2. Parents of more recent birth cohorts place more emphasis on reasoning skills and less emphasis on knowledge, compared to parents of earlier birth cohorts. Panel B depicts trends in respondents' perception of the importance of different skills in today's society, how much their own parents emphasized each skill, and how much their own primary school emphasized each skill. There is some visual evidence that younger parents perceive logical skills to be more important than do older parents. Parents' perceptions of what skills were emphasized by their own parents and primary schools do not show a clear trend.

1.5.2 Schools

We can also investigate changes in school curricula over the period we study. We focus on primary schooling because Figure 1.3 suggests that the trends in skill levels that we study emerge at young ages. The primary school curriculum in Sweden is summarized in an official Curriculum ("Läroplan") that is revised from time to time. Meeting society's demands is an explicit goal of the primary schooling system,[38] and although vocational training is not given in primary school, the needs of the workplace have sometimes played a direct role in the development of the Curriculum.[39]

Scholars of pedagogy in Sweden have noted a trend in the Curricula towards greater emphasis over time on problem solving and critical thinking. For example, in an investigation of long-term trends in the teaching of scientific inquiry, Johansson and Wickman (2012) conclude that, "The early Curricula of 1962 and 1969 lack the goal that students should learn to ask questions, formulate hypotheses or participate in the planning of investigations. That students should learn to formulate questions is first described in the 1980 Curriculum"

---

[38]For example, the first paragraph of the first section of the 1962 Curriculum states a goal of helping students develop into "capable and responsible members of society" (Skolöverstyrelsen, 1962, p. 13). The 1980 Curriculum repeats this language, quoting it as part of the Education Act (Skolöverstyrelsen, 1980, p. 13).

[39]For example, the 1962 Curriculum partly reflected the findings from systematic interviews of supervisors and employees regarding the knowledge demands of the workplace (Thavenius, 1999, p. 43; Statens offentliga utredningar, 1960, pp. 500-508).

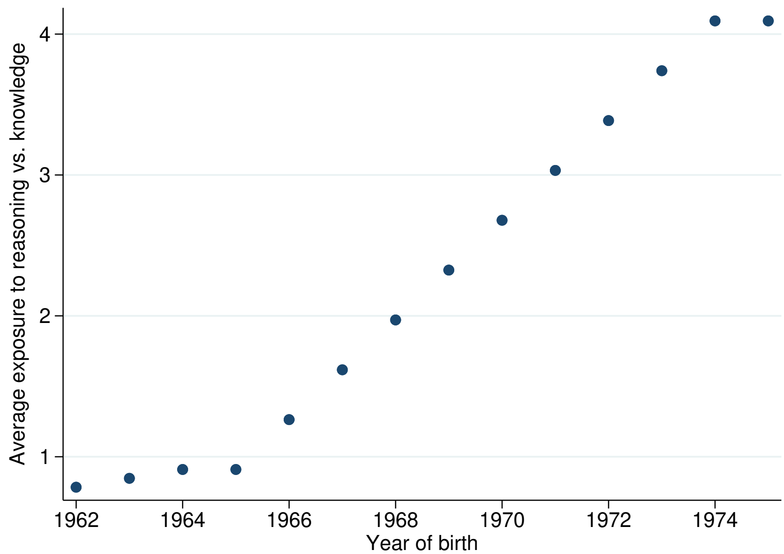


Figure 1.7: Trends in Emphasis on Reasoning vs. Knowledge in Swedish Primary School Curricula

Notes. The plot shows the trend across birth cohorts in the emphasis on reasoning relative to knowledge in the Swedish primary school Curricula (Läroplan for grundskolan) prevailing during each cohort's primary schooling. We construct the series as follows. First, we associate each school year from 1963 through 1991 with the prevailing Curriculum, treating the 1962 Curriculum (Skolöverstyrelsen, 1962) as prevailing from 1963 through 1971, the 1969 Curriculum (Skolöverstyrelsen, 1969) as prevailing from 1972 through 1981, and the 1980 Curriculum (Skolöverstyrelsen, 1980) as prevailing from 1982 through 1991. Second, for each Curriculum we obtain the ratio of the number of appearances of keywords related to reasoning to the number of appearances of keywords related to knowledge. We choose these keywords based on a close reading of the Curricula; see Appendix Figure 1.17 for details. Third, for each cohort, we define the average exposure to reasoning vs. knowledge as the average of the ratio of keyword appearances over the cohort's primary school years, which we take to be the school years beginning in the fall of the year that members of the cohort turn age 7 and ending in the spring of the year that members of the cohort turn age 16.

(p. 205). Similar trends have been observed in other areas of study.[40] These trends seem consistent with a greater emphasis on reasoning as compared to knowledge,[41] though we note that, in our survey, parents' perceptions of their own primary schooling experience do not reflect such a trend (see Panel B of Figure 1.6).

Figure 1.7 presents an original quantitative analysis of trends in emphasis in the Curricula. Based on a close reading of the Curricula we selected a set of keywords related to reasoning and knowledge. For each cohort, we calculate the relative frequency of keywords related to reasoning vs. knowledge during the cohort's primary school years. The figure shows a trend across cohorts toward greater emphasis on reasoning relative to knowledge. Appendix Figure 1.17 lists the set of keywords we study and provides more details on data construction.

1.5.3 Occupations

Figure 1.8 shows trends across cohorts in the average reasoning vs. knowledge intensity of occupations. We construct the series as follows. First, we measure the relative reasoning vs. knowledge intensity of occupations in Sweden by matching occupations to those in the US and taking data on the importance of different abilities and knowledge from the O*NET 25.0 database (U.S. Department of Labor, Employment and Training Administration, 2020). Second, we compute for each occupation the percentile rank in the distribution of reasoning vs. knowledge intensity of occupations for the 1967 cohort. Finally, we take the weighted average across occupations within each cohort using as weights either total employment or total earnings among the men in the enlistment sample.

[40] Löfdahl (1987) studies the physics Curriculum but also describes a more general evolution from 1962 to 1980 towards more emphasis on creativity and critical thinking (see also Johansson & Wickman, 2012, p. 199). Prytz (2015, p. 317) studies the mathematics Curriculum and notes a trend since the 1960s towards less emphasis on performing calculations. Dahlbäck and Lyngfelt (2017, pp. 167-168) study the evolution of the Curriculum and note that, compared to the 1969 Curriculum, the 1980 Curriculum places greater emphasis on the creative use of language.

[41] Larsson (2011) situates these trends in a transition from realism to progressivism in education. Trends toward greater emphasis on critical thinking and less emphasis on rote knowledge have been noted in many contexts, not only Sweden (see, e.g., Darling-Hammond et al., 2020). Bietenbeck (2014) finds using test score data from the US that modern teaching practices promote reasoning skills whereas traditional teaching practices promote factual knowledge.

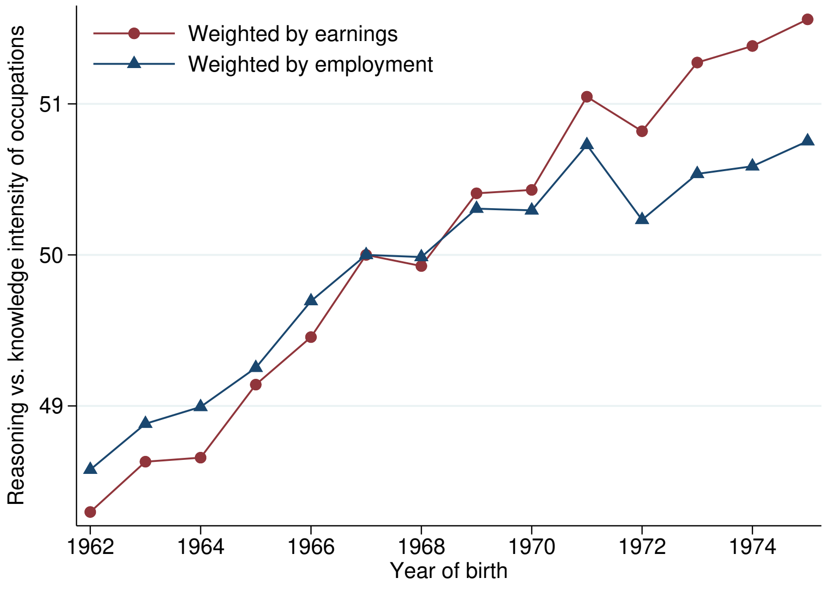


Figure 1.8: Trends in the Reasoning vs. Knowledge Intensity of Men's Occupations in Sweden

Notes. The plot shows the trend across birth cohorts in the reasoning vs. knowledge intensity of occupations in the Swedish Occupational Register, measured as the mean percentile rank of the reasoning vs. knowledge intensity of the given cohort's occupations in the distribution of either total employment ("weighted by employment") or total earnings ("weighted by earnings") for the cohort 1967. We measure the distribution of employment and earnings across occupations in the Swedish Occupational Register using data on employment histories from 2004 onwards from Statistics Sweden (2021), using 4-digit Swedish Standard Classification of Occupations 96 (SSYK 96) codes, and taking each individual's occupation to be the one observed in the available year closest to the year the individual turns 40. For each O*NET 25.0 (2020) occupation we define the total importance of reasoning abilities by summing the importance scores of Inductive, Deductive, and Mathematical Reasoning abilities and dividing by the highest possible sum. Similarly, we define the total importance of knowledge by summing the importance scores of all knowledge categories and dividing by the highest possible sum. We then define the reasoning vs. knowledge intensity of each O*NET 25.0 (2020) occupation by taking the log of the ratio of the total importance of reasoning abilities to the total importance of knowledge. We define the reasoning vs. knowledge intensity of each Standard Occupational Classification 2010 (SOC 2015) occupation by taking the unweighted average reasoning vs. knowledge intensity of all corresponding occupations in O*NET 25.0 (2020). We match the occupations in the Swedish Occupational Register to SOC 2010 occupations by using the crosswalks from Statistics Sweden (2016c) and BLS (2015), manually excluding some matches to improve accuracy. We define the reasoning vs. knowledge intensity of each occupation in the Swedish Occupational Register by taking the employment-weighted mean reasoning vs. knowledge intensity of all corresponding SOC 2010 occupations, using May 2018 OES employment estimates (BLS 2019) as weights. Each series is normalized by adding a constant so that its value for the 1967 cohort is 50. This figure includes information from the O*NET 25.0 Database by the U.S. Department of Labor, Employment and Training Administration (USDOL/ETA). Used under the CC BY 4.0 license. O*NET® is a trademark of USDOL/ETA. We have modified all or some of this information. USDOL/ETA has not approved, endorsed, or tested these modifications.

Figure 1.8 shows evidence of a trend towards relatively more reasoning-intensive occupations. The average man born in 1975 is employed in an occupation that is 2.2 percentile points more reasoning-intensive (relative to knowledge-intensive) than the average man born in 1962. The average krona earned by a man born in 1975 is earned by a man in an occupation 3.3 percentile points more reasoning-intensive than the average krona earned by a man born in 1962. Appendix Figure 1.18 shows trends in shares of total employment and total earnings separately for each occupation.

It is important to caveat that the concepts of reasoning and knowledge we measure here do not correspond exactly to those measured by the enlistment tests we study, that the join between Swedish and US occupation codes is imperfect, and that the O*NET scores are static, so the scores do not reflect changes over time in the demands of different occupations. Still, we find the pattern in Figure 1.8 interesting in light of the growth in the relative premium to fluid intelligence that we document in Section 1.4.

1.6 Non-cognitive Skills

There is evidence of rising labor-market returns to non-cognitive skill (e.g., Deming, 2017; Edin et al., 2022). We can extend our analysis to incorporate non-cognitive skills. Suppose that dimensions $j \in \{1, ..., L\}$, for $2 \leq L < J$ are dimensions of cognitive skill, and the remaining dimensions $j \in \{L+1, ..., J\}$ are dimensions of non-cognitive skill. Suppose further that

$$S_c(\tilde{\mathbf{x}}) = s_c\left(\left(\sum_{j=1}^{L} K_{cj}^{\rho-1}\tilde{x}_j^{\rho}\right)^{\frac{1}{\rho}}, \tilde{\mathbf{x}}_{L+1:J}\right) \tag{1.6}$$

where $\tilde{\mathbf{x}}_{L+1:J} = (\tilde{x}_{L+1}, ..., \tilde{x}_J)$ is the non-cognitive skill investment and $s_c(\cdot)$ is an aggregator strictly increasing in its first argument.[42] We suppose conditions

[42] An example is the two-level constant elasticity function (e.g., Goldin & Katz, 2008; Sato, 1967, Chapter 8, equations 1 and 2):

$$s_c\left(\left(\sum_{j=1}^{L} K_{cj}^{\rho-1}\tilde{x}_j^{\rho}\right)^{\frac{1}{\rho}}, \tilde{\mathbf{x}}_{L+1:J}\right) = \left(\lambda\left(\sum_{j=1}^{L} K_{cj}^{\rho-1}\tilde{x}_j^{\rho}\right)^{\frac{\sigma}{\rho}} + (1-\lambda)\left(\sum_{j=L+1}^{J} K_{cj}^{\nu-1}\tilde{x}_j^{\nu}\right)^{\frac{\sigma}{\nu}}\right)^{\frac{1}{\sigma}}$$

where ν, σ, and λ are parameters.

on $s_c(\cdot)$ sufficient to ensure a unique, strictly positive solution $\tilde{\mathbf{x}}_c(\mathbf{P}_c)$ to the worker's skill investment problem for any $\mathbf{P}_c > 0$. We define a cognitive skill supply function $\tilde{\mathbf{x}}_{c,1:L}(\cdot;\mathbf{x}_{L+1:J})$ that describes the optimal level of cognitive skill investment $\tilde{\mathbf{x}}_{c,1:L} = (\tilde{x}_{c,1},...,\tilde{x}_{c,L})$ for workers in cohort c given any lifetime skill premia $\mathbf{P}_c > 0$ and any level $\mathbf{x}_{L+1:J}$ of non-cognitive skill investment.

For each worker i we observe

$$\hat{\mathbf{x}}_i = \left(\mathbf{x}_{i,1:L}, \mathbf{A}_{c(i)}\left(\mathbf{x}_{i,L+1:J}\right)\right)$$

where $\mathbf{A}_c(\cdot)$ is a cohort-specific, possibly unknown affine map. The presence of the map $\mathbf{A}_c(\cdot)$ reflects the fact that, in our data, the measure of non-cognitive skill is standardized and thus not directly comparable across cohorts.[43]

Analogous to our baseline analysis, from data on each cohort's cognitive skill premia $\mathbf{P}_{c,1:L}$ and mean observed skill levels $\hat{\mathbf{x}}_c$, it is possible to identify the cognitive skill supply function $\tilde{\mathbf{x}}_{c,1:L}(\cdot;\tilde{\mathbf{x}}_{c,L+1:J})$ where non-cognitive skill $\tilde{\mathbf{x}}_{c,L+1:J} = \tilde{\mathbf{x}}_{c,L+1:J}(\mathbf{P}_c)$ is fixed at its equilibrium value for each cohort.

Proposition 1.3. *Under Assumption 1.1, if* $\frac{P_{\bar{c}1}}{P_{\bar{c}2}} \neq \frac{P_{\underline{c}1}}{P_{\underline{c}2}}$*, then the cognitive skill supply function*
$\tilde{\mathbf{x}}_{c,1:L}(\cdot;\tilde{\mathbf{x}}_{c,L+1:J})$ *for each cohort c is identified from data* $\{(\alpha\mathbf{P}_{c,1:L}, \hat{\mathbf{x}}_c)\}_{c=\underline{c}}^{\bar{c}}$*, where the scalar* $\alpha > 0$ *may be unknown.*

Our assumptions are also sufficient to identify the lifetime cognitive skill premia up to scale.

Proposition 1.4. *Under Assumption 1.2, for some scalar* $\alpha > 0$*, a multiple* $\alpha\mathbf{P}_{c,1:L}$ *of the lifetime cognitive skill premia for each cohort c is identified from the conditional expectation function of the log of earnings,*

$$\mathrm{E}\left(\ln(w_{it})\,|\hat{\mathbf{x}}_i = \hat{\mathbf{x}},\ \mathbf{d}_{it} = \mathbf{d},\ c(i) = c\right),$$

for each time period $t \in \{c+1,...,c+A\}$.

Notice that our assumptions are not generally sufficient to identify the lifetime non-cognitive skill premia $\mathbf{P}_{c,L+1:J}$ up to scale due to the presence of the map $\mathbf{A}_c(\cdot)$.

[43] Edin et al. (2022, Appendix A1.2) discuss the implications of standardization for the estimation of returns to non-cognitive skill.

Following the logic of Propositions 1.3 and 1.4 and their proofs, we estimate the cognitive skill supply function as follows. First, we re-estimate lifetime skill premia $\mathbf{P}_c$ following the procedure outlined in Section 1.4.1, but including the standardized measure of non-cognitive skill as an additional covariate in each earnings regression. Second, we estimate the cognitive skill supply function $\tilde{\mathbf{x}}_{c,1:L}\left(\cdot;\tilde{\mathbf{x}}_{c,L+1:J}\right)$ following the steps we used to estimate the skill supply function in Section 1.4.2, but using the re-estimated lifetime skill premia.

Panel B of Table 1.1 presents our estimates. The estimated cognitive skill supply function implies that, fixing the level of non-cognitive skill at its equilibrium level, changes in labor market returns account for 26.2 percent of the increase in logical skill (with a standard error of 2.1 percent), and for more than the entire decline in vocabulary knowledge skill. The estimated role of changing labor market returns reported in Panel B is meaningfully smaller than in our baseline analysis reported in Panel A, as is the estimated elasticity of substitution.

1.7 Conclusions

We develop a quantitative economic model of the evolution of multidimensional skills across cohorts. We estimate the model using administrative data from Sweden. The estimated model implies that a significant portion of the puzzling "Flynn effect" of rising fluid intelligence is due to substitution in investment across different dimensions of skill. The model also explains the decline in crystallized intelligence across cohorts in our setting. The model is consistent with evidence of a trend towards greater emphasis on reasoning relative to knowledge among parents, schools, and occupations. We extend our analysis to incorporate non-cognitive skill. We conclude that it is fruitful to incorporate market-driven incentives into the analysis of cohort trends in measured intelligence.

We treat the labor demand side of our model abstractly and do not offer a detailed account of the causes of cohort trends in measured labor market returns to skill. Our analysis does, however, suggest some possible explanations for trends in labor market returns to skill. We estimate an increase in the overall supply of skill across cohorts. All else equal, an increase in the supply of skill would tend to lower its return, consistent with our finding of declining returns to cognitive skill across cohorts. Likewise, our finding of an increase in the relative return to reasoning, as compared to knowledge, seems consistent with the trends in occupational composition that we document. We think that developing a more detailed model of skill demand that can be combined with our model of skill supply to explain cohort trends in returns to skill is an interesting direction for future work.

References

Acemoglu, D., & Autor, D. (2011). Skills, Tasks and Technologies: Implications for Employment and Earnings. In O. Ashenfelter & D. Card (Eds.), *Handbook of Labor Economics* (pp. 1043–1171, Vol. 4). North Holland.

Agostinelli, F., & Wiswall, M. (2020). *Estimating the Technology of Children's Skill Formation* (NBER Working Paper No. 22442). National Bureau of Economic Research.

Alwin, D. F., & Pacheco, J. (2012). Population Trends in Verbal Intelligence in the United States. In P. V. Marsden (Ed.), *Social Trends in American Life: Findings from the General Social Survey Since 1972*. Princeton University Press.

Andrews, I., Gentzkow, M., & Shapiro, J. M. (2017). Measuring the Sensitivity of Parameter Estimates to Estimation Moments. *Quarterly Journal of Economics*, *132*, 1553–1592.

Andrews, I., Gentzkow, M., & Shapiro, J. M. (2020). Transparency in Structural Research. *Journal of Business & Economic Statistics*, *38*, 711–722.

Andrews, I., & Shapiro, J. M. (2021). A Model of Scientific Communication. *Econometrica*, *89*, 2117–2142.

Baker, D. P., Eslinger, P. J., Benavides, M., Peters, E., Dieckmann, N. F., & Leon, J. (2015). The Cognitive Impact of the Education Revolution: A Possible Cause of the Flynn Effect on Population IQ. *Intelligence*, *49*, 144–158.

Bietenbeck, J. (2014). Teaching Practices and Cognitive Skills. *Labour Economics*, *30*, 143–153.

Bishop, J. H. (1989). Is the Test Score Decline Responsible for the Productivity Growth Decline? *American Economic Review*, *79*, 178–197.

Carlsson, M., Dahl, G., Öckert, B., & Rooth, D.-O. (2015). The Effect of Schooling on Cognitive Skills. *Review of Economics and Statistics*, *97*, 533–547.

Carlstedt, B. (2000). *Cognitive Abilities: Aspects of Structure, Process and Measurement* [Doctoral dissertation, University of Gothenburg].

Carlstedt, B., & Mårdberg, B. (1993). Construct Validity of the Swedish Enlistment Battery. *Scandinavian Journal of Psychology*, *34*, 353–362.

Carroll, J. B. (1993). *Human Cognitive Abilities*. Cambridge University Press.

Castex, G., & Dechter, E. K. (2014). The Changing Roles of Education and Ability in Wage Determination. *Journal of Labor Economics*, *32*, 685–710.

Cattell, R. B. (1943). The Measurement of Adult Intelligence. *Psychological Bulletin*, *40*, 153–193.

Cliffordson, C., & Gustafsson, J.-E. (2008). Effects of Age and Schooling on Intellectual Performance: Estimates Obtained from Analysis of Continuous Variation in Age and Length of Schooling. *Intelligence*, *36*, 143–152.

Cunha, F., Elo, I., & Culhane, J. (forthcoming). Maternal Subjective Expectations about the Technology of Skill Formation Predict Investments in Children One Year Later. *Journal of Econometrics*.

Cunha, F., Heckman, J. J., Lochner, L., & Masterov, D. V. (2006). Interpreting the Evidence on Life Cycle Skill Formation. In E. A. Hanushek & F. Welch (Eds.), *Handbook of the Economics of Education* (pp. 697–812, Vol. 1). North Holland.

Cunha, F., Heckman, J. J., & Schennach, S. M. (2010). Estimating the Technology of Cognitive and Noncognitive Skill Formation. *Econometrica*, *78*, 883–931.

Dahlbäck, K., & Lyngfelt, A. (2017). Estetiska dimensioner i svenskämnets kursplaner från Lgr 69 till Lgr 11. *Educare*, *1*, 152–182.

Darling-Hammond, L., Flook, L., Cook-Harvey, C., Barron, B., & Osher, D. (2020). Implications for Educational Practice of the Science of Learning and Development. *Applied Developmental Science*, *24*, 97–140.

Deary, I. J. (2020). *Intelligence: A Very Short Introduction* (2nd). Oxford University Press.

Deming, D. J. (2017). The Growing Importance of Social Skills in the Labor Market. *Quarterly Journal of Economics*, *132*, 1593–1640.

Dickens, W. T., & Flynn, J. R. (2001). Heritability Estimates Versus Large Environmental Effects: The IQ Paradox Resolved. *Psychological Review*, *108*, 346–369.

Doepke, M., Sorrenti, G., & Zilibotti, F. (2019). The Economics of Parenting. *Annual Review of Economics*, *11*, 55–84.

Edin, P.-A., Fredriksson, P., Nybom, M., & Öckert, B. (2022). The Rising Return to Non-cognitive Skill. *American Economic Journal: Applied Economics*, *14*(2), 78–100.

Falk, A., Becker, A., Dohmen, T., Enke, B., Huffman, D., & Sunde, U. (2018). Global Evidence on Economic Preferences. *Quarterly Journal of Economics*, *133*, 1645–1692.

Flanagan, D. P., & Dixon, S. G. (2014). The Cattell-Horn-Carroll Theory of Cognitive Abilities. In H. S. Davis, H. L. Hatton, & F. E. Vannest (Eds.), *Encyclopedia of Special Education*. John Wiley; Sons.

Flynn, J. R. (2007). *What is Intelligence? Beyond the Flynn Effect*. Cambridge University Press.

Flynn, J. R. (2012). *Are We Getting Smarter? Rising IQ in the Twenty-First Century*. Cambridge University Press.

Flynn, J. R. (2018). Reflections About Intelligence Over 40 Years. *Intelligence*, *70*, 73–83.

Flynn, J. R., & Shayer, M. (2018). IQ Decline and Piaget: Does the Rot Start at the Top? *Intelligence*, *66*, 112–121.

Goldin, C., & Katz, L. F. (2008). *The Race Between Education and Technology*. Harvard University Press.

Grönqvist, E., & Lindqvist, E. (2016). The Making of a Manager: Evidence from Military Officer Training. *Journal of Labor Economics*, *34*, 869–898.

Gyllenram, A., Hellström, J., & Hanes, N. (2015). Förmåga att hantera stress och individers beslut att äga aktier. *Ekonomisk Debatt*, *43*(5), 7–15.

Hanushek, E. A., & Woessmann, L. (2008). The Role of Cognitive Skills in Economic Development. *Journal of Economic Literature*, *46*, 607–668.

Hauser, R. M., & Huang, M.-H. (1997). Verbal Ability and Socioeconomic Success: A Trend Analysis. *Social Science Research*, *26*, 331–376.

Heckman, J. J., Lochner, L., & Taber, C. R. (1998). Explaining Rising Wage Inequality: Explorations with a Dynamic General Equilibrium Model of Labor Earnings with Heterogeneous Agents. *Review of Economic Dynamics*, *1*, 1–58.

Heckman, J. J., & Mosso, S. (2014). The Economics of Human Development and Social Mobility. *Annual Review of Economics*, *6*, 689–733.

Härnqvist, K. (2000). Evaluation Through Follow-up. A Longitudinal Program for Studying Education and Career Development. In C.-G. Janson (Ed.), *Seven Swedish Longitudinal Studies in Behavioral Science* (pp. 76–114). Forskningsrådsnämnden.

Jensen, A. (1998). *The g Factor: The Science of Mental Ability*. Praeger.

Johansson, A.-M., & Wickman, P.-O. (2012). Vad ska elever lära sig angående naturvetenskaplig verksamhet? En analys av svenska läroplaner för grundskolan under 50 år. *Nordic Studies in Science Education*, *8*, 197–212.

Jokela, M., Pekkarinen, T., Sarvimäki, M., Terviö, M., & Uusitalo, R. (2017). Secular Rise in Economically Valuable Personality Traits. *Proceedings of the National Academy of Sciences*, *114*, 6527–6532.

Katz, L. F., & Murphy, K. M. (1992). Changes in Relative Wages, 1963–1987: Supply and Demand Factors. *Quarterly Journal of Economics*, *107*, 35–78.

Lam, L. F., & Lawlis, T. R. (2017). Feeding the Brain – The Effects of Micronutrient Interventions on Cognitive Performance Among School-Aged Children: A Systematic Review of Randomized Controlled Trials. *Clinical Nutrition*, *36*, 1007–1014.

Larsson, H. A. (2011). *Mot bättre vetande: En svensk skolhistoria*. SNS Förlag.

Lindquist, M. J. (2005). Capital–Skill Complementarity and Inequality in Sweden. *Scandinavian Journal of Economics*, *107*, 711–735.

Lindqvist, E., & Vestman, R. (2011). The Labor Market Returns to Cognitive and Noncognitive Ability: Evidence from the Swedish Enlistment. *American Economic Journal: Applied Economics*, *3*(1), 101–128.

Lynn, R. (2009). Fluid Intelligence but Not Vocabulary Has Increased in Britain, 1979–2008. *Intelligence*, *37*, 249–255.

Löfdahl, S. E. (1987). *Fysikämnet i svensk realskola och grundskola: kartläggning och alternativ ur fysikdidaktisk synvinkel* [Doctoral dissertation, Uppsala University].

Maguire, E. A., Woollett, K., & Spiers, H. J. (2006). London Taxi Drivers and Bus Drivers: A Structural MRI and Neuropsychological Analysis. *Hippocampus*, *16*, 1091–1101.

Markussen, S., & Røed, K. (2020). Economic Mobility Under Pressure. *Journal of the European Economic Association*, *18*, 1844–1885.

Montiel Olea, J. L., & Plagborg-Møller, M. (2019). Simultaneous Confidence Bands: Theory, Implementation, and an Application to SVARs. *Journal of Applied Econometrics*, *34*, 1–17.

Neisser, U. (1997). Rising Scores on Intelligence Tests: Test Scores Are Certainly Going up All over the World, but Whether Intelligence Itself Has Risen Remains Controversial. *American Scientist*, *85*(5), 440–47.

Okagaki, L., & Sternberg, R. J. (1993). Parental Beliefs and Children's School Performance. *Child Development*, *64*(1), 36–56.

Pietschnig, J., & Voracek, M. (2015). One Century of Global IQ Gains: A Formal Meta-analysis of the Flynn Effect (1909–2013). *Perspectives on Psychological Science*, *10*, 282–306.

Potrafke, N. (2019). Risk Aversion, Patience and Intelligence: Evidence Based on Macro Data. *Economics Letters*, *178*, 116–120.

Prytz, J. (2015). Swedish Mathematics Curricula, 1850-2014: An Overview. *Proceedings of the Third International Conference on the History of Mathematics Education*, 309–326.

Rindermann, H., Becker, D., & Coyle, T. R. (2017). Survey of Expert Opinion on Intelligence: The FLynn Effect and the Future of Intelligence. *Personality and Individual Differences*, *106*, 242–247.

Roivainen, E. (2014). Changes in Word Usage Frequency May Hamper Intergenerational Comparisons of Vocabulary Skills: An Ngram Analysis of Wordsum, WAIS, and WISC Test Items. *Journal of Psychoeducational Assessment*, *32*, 83–87.

Roys, N. A., & Taber, C. R. (2020). *Skill Prices, Occupations, and Changes in the Wage Structure for Low Skilled Men* (NBER Working Paper No. 26453). National Bureau of Economic Research.

Rönnlund, M., Carlstedt, B., Blomstedt, Y., Nilsson, L.-G., & Weinehall, L. (2013). Secular Trends in Cognitive Test Performance: Swedish Conscript Data 1970–1993. *Intelligence*, *41*, 19–24.

Sanders, C., & Taber, C. R. (2012). Life-Cycle Wage Growth and Heterogeneous Human Capital. *Annual Review of Economics*, *4*, 399–425.

Sato, K. (1967). A Two-Level Constant-Elasticity-of-Substitution Production Function. *Review of Economic Studies*, *34*, 201–218.

Schaie, K. W., Willis, S. L., & Pennak, S. (2005). An Historical Framework for Cohort Differences in Intelligence. *Research in Human Development*, *2*, 43–67.

Simons, D. J., Boot, W. R., Charness, N., Gathercole, S. E., Chabris, C. F., Hambrick, D. Z., & Stine-Morrow, E. A. (2016). Do "Brain-training" Programs Work? *Psychological Science in the Public Interest*, *17*, 103–186.

Skolöverstyrelsen. (1962). *Läroplan för grundskolan*. Sö-förlaget.

Skolöverstyrelsen. (1969). *Läroplan för grundskolan. Allmän del*. Liber.

Skolöverstyrelsen. (1980). *Läroplan för grundskolan*. Liber.

Statens offentliga utredningar. (1960). *1957 års skolberedning. 3, Kursplaneundersökningar i matematik och modersmålet: empiriska studier över kursinnehållet i den grundläggande skolan, av Urban Dahllöf* (SOU 1960:15). Stockholm, Ecklesiastikdepartementet.

Statistics Sweden. (2014a). Flergenerationsregistret [Received on October 13, 2014].

Statistics Sweden. (2014b). Inkomst- och taxeringsregistret (IoT) [Received on October 13, 2014].

Statistics Sweden. (2014c). Registret över slutbetyg från gymnasieskolan [Received on October 13, 2014].

Statistics Sweden. (2016a). *Longitudinell integrationsdatabas för Sjukförsäkrings- och Arbetsmarknadsstudier (LISA) 1990–2013* [Accessed on June 14, 2021].

Statistics Sweden. (2016b). Mikrodata för Folk- och bostadsräkning 1990 [Received on April 20, 2016].

Statistics Sweden. (2016c). Översättningsnycklar [Accessed on January 6, 2021].

Statistics Sweden. (2020b). Registerbaserad arbetsmarknadsstatistik (RAMS) [Received on April 8, 2020].

Statistics Sweden. (2021). Longitudinell integrationsdatabas för sjukförsäkrings- och arbetsmarknadsstudier (LISA) [Received on April 8, 2020. Last updated on April 9, 2021].

Svensson, A. (2011). *Utvärdering genom uppföljning. Longitudinell individforskning under ett halvsekel*. Acta Universitatis Gothoburgensis.

Thavenius, J. (Ed.). (1999). *Svenskämnets historia*. Studentlitteratur.

Trahan, L., Stuebing, K. K., Hiscock, M. K., & Fletcher, J. M. (2014). The Flynn Effect: A Meta-Analysis. *Psychological Bulletin*, *140*, 1332–1360.

U.S. Bureau of Labor Statistics. (2015). Crosswalk between the 2008 International Standard Classification of Occupations to the 2010 SOC [Accessed on December 28, 2020].

U.S. Bureau of Labor Statistics. (2019). May 2018 National Occupational Employment and Wage Estimates, United States [Accessed on November 14, 2020].

U.S. Department of Labor, Employment and Training Administration. (2020). O*NET 25.0 Database [Accessed on October 14, 2020].

Wai, J., & Putallaz, M. (2011). The Flynn Effect Puzzle: A 30-year Examination from the Right Tail of the Ability Distribution Provides Some Missing Pieces. *Intelligence*, *39*, 443–455.

War Archives. (2016). Krigsarkivets Mönstringsregister [Received on October 27, 2016].

Appendices

Appendix 1.A Proofs

Proof of Proposition 1.1

From Assumption 1.1 and equation (1.5) we have that

$$\frac{1}{\rho-1}=\frac{\ln\left(\frac{\tilde{x}_{\bar{c}1}}{\tilde{x}_{\bar{c}2}}\right)-\ln\left(\frac{\tilde{x}_{\underline{c}1}}{\tilde{x}_{\underline{c}2}}\right)}{\ln\left(\frac{P_{\bar{c}1}}{P_{\bar{c}2}}\right)-\ln\left(\frac{P_{\underline{c}1}}{P_{\underline{c}2}}\right)} \tag{1.1}$$

where the existence of the ratio on the right is guaranteed because $\frac{P_{\bar{c}1}}{P_{\bar{c}2}} \neq \frac{P_{\underline{c}1}}{P_{\underline{c}2}}$. Thus ρ is identified.

Because $\mathbf{P}_c > 0$, an analogue of equation (1.5) holds for any pair of dimensions $(1,j)$. Thus given ρ the ratio $\frac{K_{cj}}{K_{c1}}$ is identified for all c and j via the relation

$$\ln\left(\frac{K_{cj}}{K_{c1}}\right)=\ln\left(\frac{\tilde{x}_{c1}}{\tilde{x}_{cj}}\right)-\frac{1}{\rho-1}\ln\left(\frac{P_{c1}}{P_{cj}}\right). \tag{1.2}$$

From the budget constraint in (1.2) and the transformation function in (1.4), observe that multiplying $\mathbf{K}_c$ by any positive constant κ is equivalent to multiplying $\overline{S}_c$ by $\kappa^{\frac{1-\rho}{\rho}}$. Therefore fix the scale of $\mathbf{K}_c$ by supposing that its average element equals one, i.e., $\sum_{j=1}^J K_{cj}=J$. Then $\sum_{j=1}^J K_{cj}=\sum_{j=1}^J\left(\frac{K_{cj}}{K_{c1}}\right)K_{c1}=K_{c1}\sum_{j=1}^J\left(\frac{K_{cj}}{K_{c1}}\right)=J$, which from (1.2) implies

$$K_{c1}=\frac{J}{\sum_{j=1}^J\frac{\tilde{x}_{c1}}{\tilde{x}_{cj}}\left(\frac{P_{cj}}{P_{c1}}\right)^{\frac{1}{\rho-1}}}. \tag{1.3}$$

Thus $\mathbf{K}_c$ is identified for each cohort c given ρ and the ratios $\frac{K_{cj}}{K_{c1}}$.

Finally, $\overline{S}_c$ is identified for all c given ρ and $\mathbf{K}_c$ because, from the solution to the worker's problem,

$$\overline{S}_c=\frac{\tilde{x}_{c1}\left(\sum_{j=1}^J P_{cj}^{\frac{\rho}{\rho-1}}K_{cj}^{-1}\right)^{\frac{1}{\rho}}}{P_{c1}^{\frac{1}{\rho-1}}K_{c1}^{-1}}. \tag{1.4}$$

Proof of Corollary 1.1

Let $\hat{\mathbf{P}}_c = |\alpha \mathbf{P}_c| = |\alpha| \mathbf{P}_c$ for $\alpha \neq 0$. Because $\frac{\hat{P}_{c1}}{\hat{P}_{cj}} = \frac{P_{c1}}{P_{cj}}$ for all c and j, the arguments in the proof of Proposition 1.1 directly establish identification of ρ and identification of $\mathbf{K}_c$ up to a normalization. Then $\overline{S}_c$ is identified for all c given ρ and $\mathbf{K}_c$ because

$$\overline{S}_c = \frac{\tilde{x}_{c1} \left(\sum_{j=1}^{J} P_{cj}^{\frac{\rho}{\rho-1}} K_{cj}^{-1} \right)^{\frac{1}{\rho}}}{P_{c1}^{\frac{1}{\rho-1}} K_{c1}^{-1}} = \frac{\tilde{x}_{c1} \left(\sum_{j=1}^{J} \hat{P}_{cj}^{\frac{\rho}{\rho-1}} K_{cj}^{-1} \right)^{\frac{1}{\rho}}}{\hat{P}_{c1}^{\frac{1}{\rho-1}} K_{c1}^{-1}}.$$

Proof of Proposition 1.2

From equation (1.1) we have that for each period t

$$\begin{aligned}
&\mathrm{E}\left(\ln\left(w_{it}\right)|\mathbf{x}_i = \mathbf{x}, \mathbf{d}_{it} = \mathbf{d}, c\left(i\right) = c\right) = \\
&\mathrm{E}\left(B_{t,a(i,t)} + \mathbf{p}_{t,a(i,t)}^{'}\mathbf{x}_i + \ln\left(z_{it}\right)|\mathbf{x}_i = \mathbf{x}, \mathbf{d}_{it} = \mathbf{d}, c\left(i\right) = c\right) = \\
&B_{t,t-c} + \mathbf{p}_{t,t-c}^{'}\mathbf{x} + \mathrm{E}\left(\ln\left(z_{it}\right)|\mathbf{x}_i = \mathbf{x}, \mathbf{d}_{it} = \mathbf{d}, c\left(i\right) = c\right).
\end{aligned}$$

Because $\mathbf{x}_i = \tilde{\mathbf{x}}_{c(i)} + \mu_i$ for all i, we also have that

$$\begin{aligned}
\mathrm{E}\left(\ln\left(z_{it}\right)|\mathbf{x}_i = \mathbf{x}, \mathbf{d}_{it} = \mathbf{d}, c\left(i\right) = c\right) &= \mathrm{E}\left(\ln\left(z_{it}\right)|\tilde{\mathbf{x}}_c + \mu_i = \mathbf{x}, \mathbf{d}_{it} = \mathbf{d}, c\left(i\right) = c\right) \\
&= \mathrm{E}\left(\ln\left(z_{it}\right)|\mu_i = \mathbf{x} - \tilde{\mathbf{x}}_c, \mathbf{d}_{it} = \mathbf{d}, c\left(i\right) = c\right) \\
&= \zeta_{t,t-c} + \tilde{\alpha}\mathbf{p}_{t,t-c}^{'}\left(\mathbf{x} - \tilde{\mathbf{x}}_c\right) + \mathbf{d}^{'}\beta_{t,t-c}
\end{aligned}$$

where the last equality uses Assumption 1.2. It follows that

$$\mathrm{E}\left(\ln\left(w_{it}\right)|\mathbf{x}_i = \mathbf{x}, \mathbf{d}_{it} = \mathbf{d}, c\left(i\right) = c\right) = \tilde{B}_{t,t-c} + \alpha\mathbf{p}_{t,t-c}^{'}\mathbf{x} + \mathbf{d}^{'}\beta_{t,t-c}$$

where $\tilde{B}_{t,t-c} = \left(B_{t,t-c} + \zeta_{t,t-c} - \tilde{\alpha}\mathbf{p}_{t,t-c}^{'}\tilde{\mathbf{x}}_c\right)$ and $\alpha = 1 + \tilde{\alpha}$. Since $\tilde{\alpha} \neq -1$, we have $\alpha \neq 0$. Identification of $\mathbf{p}_{t,t-c}$ up to scale is then immediate, from which identification of $\mathbf{P}_c$ up to scale follows directly from equation (1.3).

Proof of Proposition 1.3

Recall that the worker maximizes $\mathbf{P}_{c(i)}^{'}\tilde{\mathbf{x}}_i$ subject to $\tilde{\mathbf{x}}_i \geq 0$ and $S_{c(i)}\left(\tilde{\mathbf{x}}_i\right) \leq \overline{S}_{c(i)}$, where $\tilde{\mathbf{x}}_i = \mathbf{x}_i - \mu_i$. Fixing non-cognitive skill investment at $\tilde{\mathbf{x}}_{i,L+1:J} =$

$\tilde{\mathbf{x}}_{c(i),L+1:J} \geq 0$, and taking account of the form of the transformation function in (1.6), we can rewrite the worker's problem as maximizing $\mathbf{P}^{'}_{c(i),1:L}\tilde{\mathbf{x}}_{i,1:L}$ subject to $\tilde{\mathbf{x}}_{i,1:L} \geq 0$ and $\left(\sum_{j=1}^{L} K_{c(i)j}^{\rho-1}\tilde{x}_j^{\rho}\right)^{\frac{1}{\rho}} \leq s_{c(i)}^{-1}\left(\overline{S}_{c(i)}, \tilde{\mathbf{x}}_{c(i),L+1:J}\right)$, where $s_{c(i)}^{-1}\left(\overline{S}_{c(i)}, \tilde{\mathbf{x}}_{c(i),L+1:J}\right)$ solves $s_{c(i)}\left(s_{c(i)}^{-1}\left(\overline{S}_{c(i)}, \tilde{\mathbf{x}}_{c(i),L+1:J}\right), \tilde{\mathbf{x}}_{c(i),L+1:J}\right) = \overline{S}_{c(i)}$, is unique by the strict monotonicity of $s_c(\cdot)$ in its first argument, and is strictly positive because the worker's problem is assumed to have a strictly positive solution. We have demonstrated that the worker's problem of choosing cognitive skills given non-cognitive skills is equivalent to the worker's problem in Section 1.2.3, replacing J with L and $\overline{S}_{c(i)}$ with $s_{c(i)}^{-1}\left(\overline{S}_{c(i)}, \tilde{\mathbf{x}}_{c(i),L+1:J}\right)$. The results of Proposition 1.1 and Corollary 1.1 thus apply given Assumption 1.1.

Proof of Proposition 1.4

We have that

$$
\begin{aligned}
&\mathrm{E}\left(\ln\left(w_{it}\right)|\hat{\mathbf{x}}_i = \hat{\mathbf{x}},\ \mathbf{d}_{it} = \mathbf{d},\ c\left(i\right) = c\right) = \\
&\mathrm{E}\left(\mathrm{E}\left(\ln\left(w_{it}\right)|\mathbf{x}_i = \mathbf{x}, \mathbf{d}_{it} = \mathbf{d}, c\left(i\right) = c\right)|\hat{\mathbf{x}}_i = \hat{\mathbf{x}}\right) = \\
&\mathrm{E}\left(\tilde{B}_{t,t-c} + \alpha\mathbf{p}^{'}_{t,t-c}\mathbf{x} + \mathbf{d}^{'}\beta_{t,t-c}|\hat{\mathbf{x}}_i = \hat{\mathbf{x}}\right) = \\
&\tilde{B}_{t,t-c} + \alpha\mathbf{p}^{'}_{t,t-c,1:L}\mathbf{x}_{1:L} + \alpha\mathbf{p}^{'}_{t,t-c,L+1:J}\mathbf{A}_c^{-1}\left(\hat{\mathbf{x}}_{L+1:J}\right) + \mathbf{d}^{'}\beta_{t,t-c}
\end{aligned}
$$

where the first step follows from the law of total expectation, the second from the proof of Proposition 1.2, and the third from the invertibility of $\mathbf{A}_c$. Because $\mathbf{A}_c^{-1}(\cdot)$ is linear in $\hat{\mathbf{x}}_{L+1:J}$, identification of $\mathbf{p}^{'}_{t,t-c,1:L}$ up to scale is immediate, from which identification of $\mathbf{P}_{c,1:L}$ up to scale follows directly from equation (1.3).

Appendix 1.B Sensitivity and Heterogeneity Analysis

Appendix Table 1.1: Sensitivity of main results to different specifications

Specification	Initial lifetime skill premium, 1962 $P_{\underline{c}j}$		Change in lifetime skill premium $P_{\bar{c}j} - P_{\underline{c}j}$		Initial average skill level $\bar{x}_{\bar{c}j}$		Change in average skill level $\tilde{x}_{\bar{c}j} - \tilde{x}_{\underline{c}j}$		Share of observed change explained by change in skill premia $1 - \frac{\tilde{x}_{\bar{c}j}(\mathbf{P}_{\underline{c}}) - \tilde{x}_{\underline{c}j}(\mathbf{P}_{\underline{c}})}{\tilde{x}_{\bar{c}j} - \tilde{x}_{\underline{c}j}}$	
	Logical reasoning	Vocabulary knowledge	Logical reasoning	Vocabulary knowledge	Logical reasoning	Vocabulary knowledge	Logical reasoning	Vocabulary knowledge	Logical reasoning	Vocabulary knowledge
(a) Baseline	0.0048	0.0016	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3681	2.0151
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0175)	(0.1483)
(b) Exclude birth cohort 1962	0.0047	0.0016	-0.0007	-0.0008	48.53	50.79	4.09	-3.24	0.3940	1.8008
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.12)	(0.13)	(0.21)	(0.25)	(0.0250)	(0.1294)
(c) Exclude birth cohort 1975	0.0048	0.0016	-0.0008	-0.0006	47.88	50.72	4.95	-1.84	0.3153	2.9520
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.23)	(0.19)	(0.0114)	(0.3006)
(d) Replace percentile rank with	0.0074	0.0025	-0.0009	-0.0009	59.96	61.07	3.30	-1.04	0.2936	3.2777
percent of maximum score attained	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.09)	(0.07)	(0.14)	(0.11)	(0.0122)	(0.3969)
(e) Replace logical reasoning skill with	0.0050	0.0020	-0.0010	-0.0006	46.42	50.72	5.10	-2.92	0.4545	2.0408
logical-spatial composite	(0.0001)	(0.0000)	(0.0001)	(0.0001)	(0.13)	(0.13)	(0.21)	(0.21)	(0.0174)	(0.1486)
(f) Include business income in	0.0049	0.0016	-0.0009	-0.0007	47.88	50.72	4.43	-2.92	0.3587	2.0302
earnings measure	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0168)	(0.1486)
(g) Age range 35–55	0.0049	0.0019	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.4393	1.9008
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0196)	(0.1396)
(h) Age range 30–60	0.0048	0.0016	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3780	1.9992
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0177)	(0.1471)
(i) Restrict to modal full-year workers	0.0043	0.0016	-0.0006	-0.0006	49.07	51.61	3.74	-3.52	0.4653	1.5987
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.14)	(0.23)	(0.21)	(0.0258)	(0.0982)
(j) Extrapolate from ages 35+	0.0048	0.0016	-0.0008	-0.0008	47.88	50.72	4.43	-2.92	0.3591	2.0296
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0174)	(0.1497)
(k) Extrapolate from oldest available	0.0048	0.0016	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3703	2.0115
age	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0179)	(0.1486)
(l) NPV with discount factor 0.93	0.0048	0.0015	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3396	2.0609
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0169)	(0.1519)
(m) NPV with discount factor 0.99	0.0048	0.0017	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3931	1.9750
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0182)	(0.1452)
(n) Quadratic smoothing for estimated	0.0048	0.0016	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3680	2.0154
skill premium series	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0175)	(0.1483)
(o) No smoothing for estimated skill	0.0050	0.0013	-0.0010	-0.0006	47.88	50.72	4.43	-2.92	0.3085	2.1108
premium series	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.14)	(0.13)	(0.23)	(0.20)	(0.0308)	(0.1648)

Notes. This table summarizes the sensitivity of our main results to different specifications. Standard errors in parentheses are obtained via a nonparametric bootstrap with 50 replicates. In each replicate, for each cohort c, we draw men with replacement from the population in that cohort, and recalculate all data-dependent objects. We exclude three and five bootstrap replicates from the calculation of standard errors for rows (e) and (o), respectively, due to values inconsistent with the model. Row (a) reproduces our baseline estimates from Panel A of Table 1.1. Row (b) changes the initial birth cohort $\underline{c}$ to be those born in 1963, and row (c) changes the final birth cohort $\bar{c}$ to be those born in 1974. Both rows (b) and (c) multiply estimated changes by $\frac{13}{12}$ for comparability with the other specifications. Row (d) replaces the logical reasoning and vocabulary knowledge skill measures with the percent of the maximum possible score attained by the individual. Row (e) replaces the logical reasoning skill measure with the first component from a principal component analysis of logical reasoning and spatial reasoning skill measures for the 1967 birth cohort. Spatial reasoning skills are measured using a task in which individuals are asked to identify a three-dimensional object that corresponds to an unfolded piece of metal (Carlstedt, 2000; Carlstedt & Mårdberg, 1993). Row (f) incorporates business income into our measure of earnings for the years 1990–2018. Rows (g) and (h) vary the ages of working life that we consider for estimating the lifetime skill premia $\mathbf{P}_c$. Row (i) excludes individuals for whom the greatest mode of annual months worked in sample years is less than 12. We define an individual as being employed in a given month if that month falls between the first and last month of employment for at least one of the jobs he holds during the year. Rows (j) and (k) vary the ages over which we average the estimated premia $\mathbf{p}_{c+a,a}$ in order to infer premia for ages for which earnings data are unavailable. Rows (l) and (m) vary the discount factor δ that we use in the calculation of $\mathbf{P}_c$ via equation (1.3). Rows (n) and (o) use, respectively, a quadratic fit (second-order polynomial) and no smoothing at all, instead of a linear fit, for the relationship between estimated lifetime skill premia and cohort.

Appendix Table 1.2: Sensitivity of main results to adjusting for control variables

Specification	Initial lifetime skill premium, 1962 $P_{\underline{c}j}$		Change in lifetime skill premium, 1962–1975 $P_{\bar{c}j} - P_{\underline{c}j}$		Initial average skill rank, 1962 $\bar{x}_{\underline{c}j}$		Change in average skill rank, 1962–1975 $\bar{x}_{\bar{c}j} - \bar{x}_{\underline{c}j}$		Share of observed change explained by change in skill premia $1 - \frac{\tilde{x}_{\bar{c}j}(\mathbf{P}_{\underline{c}}) - \tilde{x}_{\underline{c}j}(\mathbf{P}_{\underline{c}})}{\bar{x}_{\bar{c}j} - \bar{x}_{\underline{c}j}}$	
	Logical reasoning	Vocabulary knowledge	Logical reasoning	Vocabulary knowledge	Logical reasoning	Vocabulary knowledge	Logical reasoning	Vocabulary knowledge	Logical reasoning	Vocabulary knowledge
(a) Baseline (no controls)	0.0048 (0.0001)	0.0016 (0.0001)	-0.0008 (0.0001)	-0.0007 (0.0001)	47.88 (0.14)	50.72 (0.13)	4.43 (0.22)	-2.92 (0.21)	0.3681 (0.0175)	2.0151 (0.1483)
(b) Age at enlistment (indicators)	0.0047 (0.0001)	0.0016 (0.0001)	-0.0007 (0.0001)	-0.0007 (0.0001)	48.21 (0.13)	51.03 (0.13)	4.43 (0.23)	-2.84 (0.21)	0.3596 (0.0179)	2.0592 (0.1575)
(c) Completed secondary education at enlistment (indicator)	0.0048 (0.0001)	0.0016 (0.0001)	-0.0008 (0.0001)	-0.0007 (0.0001)	47.22 (0.14)	50.03 (0.13)	4.48 (0.23)	-2.86 (0.21)	0.3594 (0.0175)	2.0612 (0.1577)
(d) Completed secondary education at age 18 (indicator)	0.0048 (0.0001)	0.0016 (0.0001)	-0.0008 (0.0001)	-0.0007 (0.0001)	47.10 (0.14)	49.87 (0.13)	4.15 (0.22)	-3.20 (0.20)	0.3860 (0.0193)	1.8431 (0.1196)
(e) log(height) and log(weight) at enlistment	0.0047 (0.0001)	0.0015 (0.0001)	-0.0008 (0.0001)	-0.0006 (0.0001)	47.94 (0.14)	50.79 (0.13)	4.47 (0.22)	-2.96 (0.21)	0.3554 (0.0178)	2.0301 (0.1489)
(f) Born outside Sweden (indicator)	0.0048 (0.0001)	0.0016 (0.0001)	-0.0008 (0.0001)	-0.0007 (0.0001)	47.90 (0.13)	50.72 (0.13)	4.30 (0.23)	-3.03 (0.21)	0.3756 (0.0189)	1.9370 (0.1376)

Notes. This table summarizes the sensitivity of our main results to adjusting for different control variables. Standard errors in parentheses are obtained via a nonparametric bootstrap with 50 replicates. In each replicate, for each cohort c, we draw men with replacement from the population in that cohort, and recalculate all data-dependent objects. Row (a) reproduces our baseline estimates with no controls from Panel A of Table 1.1. Each subsequent row includes a different control variable or variables. Control variables are included when estimating cohort- and age-specific skill premia and are used to adjust the estimated average skill levels, following Section 1.4.4. In each row, we omit individuals with missing or invalid values of the relevant control variables. In specification (b), we control for indicators for the number of years between the person's year of enlistment and year of birth. In specification (c), we define a person as having completed secondary education at enlistment if the person's enlistment date occurs on or after June 1 of the year in which they complete secondary education.

Appendix Table 1.3: Model implications for different subsamples

Specification	Initial lifetime skill premium, 1962 $P_{\underline{c}j}$		Change in lifetime skill premium, 1962–1975 $P_{\bar{c}j} - P_{\underline{c}j}$		Initial average skill rank, 1962 $\bar{x}_{\underline{c}j}$		Change in average skill rank, 1962–1975 $\tilde{x}_{\bar{c}j} - \tilde{x}_{\underline{c}j}$		Share of observed change explained by change in skill premia $1 - \frac{\tilde{x}_{\bar{c}j}(\mathbf{P}_{\underline{c}}) - \tilde{x}_{\underline{c}j}(\mathbf{P}_{\underline{c}})}{\bar{x}_{\bar{c}j} - \bar{x}_{\underline{c}j}}$	
	Logical reasoning	Vocabulary knowledge	Logical reasoning	Vocabulary knowledge	Logical reasoning	Vocabulary knowledge	Logical reasoning	Vocabulary knowledge	Logical reasoning	Vocabulary knowledge
(a) Baseline	0.0048	0.0016	-0.0008	-0.0007	47.88	50.72	4.43	-2.92	0.3681	2.0151
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.14)	(0.13)	(0.22)	(0.21)	(0.0175)	(0.1483)
(b) Below-median parental	0.0044	0.0012	-0.0006	-0.0006	43.59	46.73	3.51	-3.39	0.3518	1.7200
earnings	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.17)	(0.21)	(0.29)	(0.29)	(0.0375)	(0.1457)
(c) Above-median parental	0.0048	0.0017	-0.0010	-0.0008	52.43	54.96	4.98	-2.81	0.3646	2.1799
earnings	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.21)	(0.20)	(0.31)	(0.30)	(0.0232)	(0.2213)

Notes. This table summarizes the model implications for different subsamples. Standard errors in parentheses are obtained via a nonparametric bootstrap with 50 replicates. In each replicate, for each cohort c, we draw men with replacement from the population in that cohort, and recalculate all data-dependent objects. We exclude one bootstrap replicate from the calculation of standard errors for row (b) due to values inconsistent with the model. Row (a) reproduces our baseline estimates from Panel A of Table 1.1. Rows (b) and (c) estimate the model for individuals with below- or above-median parental earnings, respectively, within their given cohort. For each sample individual, we define parental earnings as the average lifetime earnings of all biological or adoptive parents, with lifetime earnings given by the average earnings over all ages 30 through 55 observed in the data.

Appendix 1.C Identification of the Skill Supply Function with a Social Multiplier

Suppose that $K_{cj} = \overline{K}_{cj} \overline{x}_{cj}^{-\upsilon}$ where $\upsilon \in [0,1)$ is a parameter governing the strength of social spillovers in skill investment and $\overline{\mathbf{K}}_c \in \mathbb{R}^J_{>0}$ is a vector of cost parameters. Each worker chooses skill investment taking the average skill $\overline{\mathbf{x}}_{c(i)}$ of their cohort $c(i)$ as given.

Assumption 1.3. *(Zero average relative supply shock.) We assume that*

$$\frac{1}{\overline{c}-\underline{c}} \sum_{c=\underline{c}}^{\overline{c}-1} \left[\ln\left(\frac{\overline{K}_{c+1,1}}{\overline{K}_{c+1,2}}\right) - \ln\left(\frac{\overline{K}_{c1}}{\overline{K}_{c2}}\right)\right] = 0.$$

Proposition 1.5. *Under Assumption 1.3, if $\frac{P_{\overline{c}1}}{P_{\overline{c}2}} \neq \frac{P_{\underline{c}1}}{P_{\underline{c}2}}$, then the skill supply function $\tilde{\mathbf{x}}_c(\cdot)$ for each cohort c is identified from data $\{(\mathbf{P}_c, \tilde{\mathbf{x}}_c)\}_{c=\underline{c}}^{\overline{c}}$.*

Proof of Proposition 1.5

In the model in Section 1.2.3 the skill supply function is given by

$$\tilde{x}_{cj}(\mathbf{P}_c) = \frac{P_{cj}^{\frac{1}{\rho-1}} K_{cj}^{-1}}{\left(\sum_{j'=1}^{J} P_{cj'}^{\frac{\rho}{\rho-1}} K_{cj'}^{-1}\right)^{\frac{1}{\rho}}} \overline{S}_c \tag{1.5}$$

for each skill $j \in \{1,...,J\}$. Recalling that $K_{cj} = \overline{K}_{cj}\overline{x}_{cj}^{-\upsilon}$ and imposing the equilibrium condition that $\tilde{\mathbf{x}}_c = \overline{\mathbf{x}}_c$ we have that

$$\tilde{x}_{cj}(\mathbf{P}_c) = \frac{P_{cj}^{\frac{1}{\rho-1}} \overline{K}_{cj}^{-1} \left(\tilde{x}_{cj}(\mathbf{P}_c)\right)^{\upsilon}}{\left(\sum_{j'=1}^{J} P_{cj'}^{\frac{\rho}{\rho-1}} \overline{K}_{cj'}^{-1} \left(\tilde{x}_{cj'}(\mathbf{P}_c)\right)^{\upsilon}\right)^{\frac{1}{\rho}}} \overline{S}_c \tag{1.6}$$

for each skill $j \in \{1,...,J\}$. Define $\tilde{\mathbf{K}}_c$ such that $\tilde{K}_{cj} = \overline{K}_{cj}^{\frac{1}{1-\upsilon}}$ and notice that $\tilde{\mathbf{K}}_c \in \mathbb{R}^J_{>0}$ and that

$$\frac{1}{\overline{c}-\underline{c}} \sum_{c=\underline{c}}^{\overline{c}-1} \left[\ln\left(\frac{\tilde{K}_{c+1,1}}{\tilde{K}_{c+1,2}}\right) - \ln\left(\frac{\tilde{K}_{c1}}{\tilde{K}_{c2}}\right)\right] = 0$$

by Assumption 1.3. Define $\tilde{\rho}$ such that

$$\frac{1}{\tilde{\rho}-1}=\frac{1}{(\rho-1)(1-\upsilon)}$$

and notice that $\tilde{\rho}>1$. Define $\tilde{S}_c=\overline{S}_c^{\frac{\rho}{\tilde{\rho}}}$ and notice that $\tilde{S}_c>0$. Then the unique solutions to the J equations in (1.6) are given by

$$\tilde{x}_{cj}(\mathbf{P}_c)=\frac{P_{cj}^{\frac{1}{\tilde{\rho}-1}}\tilde{K}_{cj}^{-1}}{\left(\sum_{j'=1}^{J}P_{cj'}^{\frac{\tilde{\rho}}{\tilde{\rho}-1}}\tilde{K}_{cj'}^{-1}\right)^{\frac{1}{\tilde{\rho}}}}\tilde{S}_c \tag{1.7}$$

for each skill $j\in\{1,\ldots,J\}$. Because (1.7) is isomorphic to (1.5), replacing $\mathbf{K}_c$ with $\tilde{\mathbf{K}}_c$, ρ with $\tilde{\rho}$, and $\overline{S}_c$ with $\tilde{S}_c$, and because an analogue of Assumption 1.1 in the main text holds for $\tilde{\mathbf{K}}_c$, Proposition 1.1 in the main text applies directly.

Appendix 1.D Identification of Lifetime Skill Premia with Mismeasured Skills

Let $\hat{\mathbf{x}}_i$ denote a measurement of $\mathbf{x}_i$. For simplicity we set aside the role of covariates $\mathbf{d}_{it}$.

Assumption 1.4. *The measurement error in each cohort c obeys*

$$\mathrm{E}\left(\hat{\mathbf{x}}_i - \mathbf{x}_i | \mu_i = \mu, c(i) = c\right) = 0 \tag{1.8}$$

and

$$\mathrm{Var}\left(\hat{\mathbf{x}}_i - \mathbf{x}_i | c(i) = c\right) = \hat{\alpha}\,\mathrm{Var}\left(\hat{\mathbf{x}}_i | c(i) = c\right), \tag{1.9}$$

where the scalar $\hat{\alpha} \in [0, 1)$ *may be unknown.*

Assumption 1.4 implies that the measurement error in $\hat{\mathbf{x}}_i$ has mean zero conditional on true skills and has variance proportional to both measured and true skills.

Assumption 1.5. *The values of* z_{it} *in each period t obey*

$$\begin{aligned} \mathrm{E}\left(\ln(z_{it}) | \hat{\mathbf{x}}_i - \mathbf{x}_i = \xi, \mu_i = \mu, c(i) = c\right) &= \mathrm{E}\left(\ln(z_{it}) | \mu_i = \mu, c(i) = c\right) \\ &= \zeta_{t,t-c} + \tilde{\alpha}\mathbf{p}_{t,t-c}^{\prime}\mu, \end{aligned} \tag{1.10}$$

where the scalars $\zeta_{t,t-c}$ *and* $\tilde{\alpha} \geq 0$ *may be unknown.*

Assumption 1.5 implies that a version of Assumption 1.2 in the main text holds, and that unobserved determinants of log earnings are mean-independent of the measurement error in skills.

Assumptions 1.4 and 1.5 are sufficient to identify the cohort-and-period-specific skill premia $\mathbf{p}_{t,t-c}$, and hence the lifetime skill premia $\mathbf{P}_c$, up to scale, from the conditional expectation function of the log of earnings given measured skills.

Proposition 1.6. *Under Assumptions 1.4 and 1.5, for some scalar* $\alpha > 0$*, a multiple* $\alpha\mathbf{P}_c$ *of the lifetime skill premia for each cohort c is identified from the conditional expectation function of the log of earnings given measured skills,*

$$\mathrm{E}\left(\ln(w_{it}) | \hat{\mathbf{x}}_i = \hat{\mathbf{x}}, c(i) = c\right),$$

for each time period $t \in \{c+1,...,c+A\}$.

Proof of Proposition 1.6

Fix a cohort c and period t. From (1.8) and (1.9) we have that

$$\operatorname{Var}(\hat{\mathbf{x}}_i|c(i)=c) = (1-\hat{\alpha})^{-1}\operatorname{Var}(\mathbf{x}_i|c(i)=c).$$

From (1.1) in the main text, (1.8), and (1.10) we have that

$$\begin{aligned}\operatorname{Cov}(\hat{\mathbf{x}}_i, \ln(w_{it})|c(i)=c) &= \operatorname{Cov}\left(\hat{\mathbf{x}}_i, \mathbf{p}'_{t,t-c}\mathbf{x}_i + \ln(z_{it})|c(i)=c\right) \\ &= \operatorname{Cov}\left(\mathbf{x}_i, (1+\tilde{\alpha})\mathbf{p}'_{t,t-c}\mathbf{x}_i|c(i)=c\right) \\ &= (1+\tilde{\alpha})\operatorname{Var}(\mathbf{x}_i|c(i)=c)\mathbf{p}_{t,t-c}.\end{aligned}$$

The population regression of $\ln(w_{it})$ on $\hat{\mathbf{x}}_i$ and a constant therefore yields coefficients

$$\operatorname{Var}(\hat{\mathbf{x}}_i|c(i)=c)^{-1}\operatorname{Cov}(\hat{\mathbf{x}}_i, \ln(w_{it})|c(i)=c) = \alpha\mathbf{p}_{t,t-c}$$

for $\alpha = (1-\hat{\alpha})(1+\tilde{\alpha}) > 0$. Because the population regression is available from the conditional expectation function, identification of $\mathbf{p}_{t,t-c}$ up to scale is then immediate, from which identification of $\mathbf{P}_c$ up to scale follows directly from equation (1.3) in the main text.

Appendix 1.E Additional Tables and Figures

Appendix Table 1.4: Number of individuals by birth cohort, military enlistment and survey samples

(a) *Military Enlistment Data*

Birth cohort	Number of individuals
1962	52,317
1963	55,526
1964	58,639
1965	55,018
1966	39,056
1967	47,767
1968	49,965
1969	48,850
1970	48,815
1971	51,108
1972	50,824
1973	47,353
1974	47,923
1975	38,069
Total	691,230

(b) *Survey Data*

Birth cohort	Number of individuals
1948	5,361
1953	4,699
1967	3,907
1972	3,899
1977	1,966
Total	19,832

Notes. Each panel shows the number of individuals in each birth cohort for whom we measure valid logical reasoning and vocabulary knowledge test scores. Panel (a) shows counts for the military enlistment data. Panel (b) shows counts for the survey data.

Appendix Table 1.5: Trends in lifetime skill premia using survey test scores as instruments

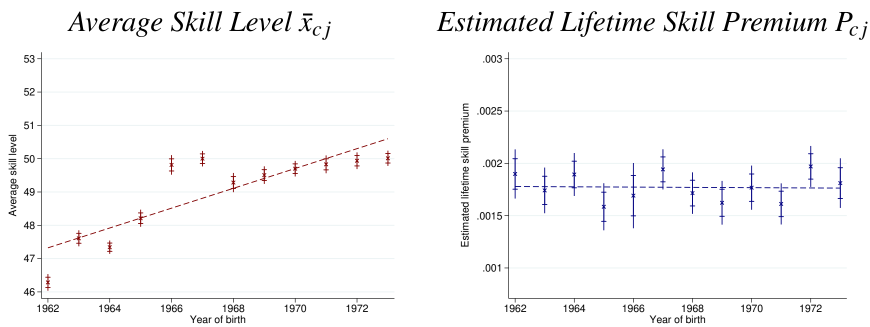
Panel A: Trends in Lifetime Skill Premia

	Enlistment data	Enlistment + survey data	
	Linear trend	OLS	IV
Change from 1967 to 1972 in lifetime premium to:			
Logical reasoning skill (P_{c1})	-0.000298	0.000622	0.002109
	(0.000040)	(0.000709)	(0.001870)
Vocabulary knowledge skill (P_{c2})	-0.000266	-0.000353	-0.001547
	(0.000042)	(0.000740)	(0.001903)
Number of individuals			
1967 cohort	42,427	2,927	2,927
1972 cohort	45,397	3,451	3,451

Panel B: Correlations in Skill Measures

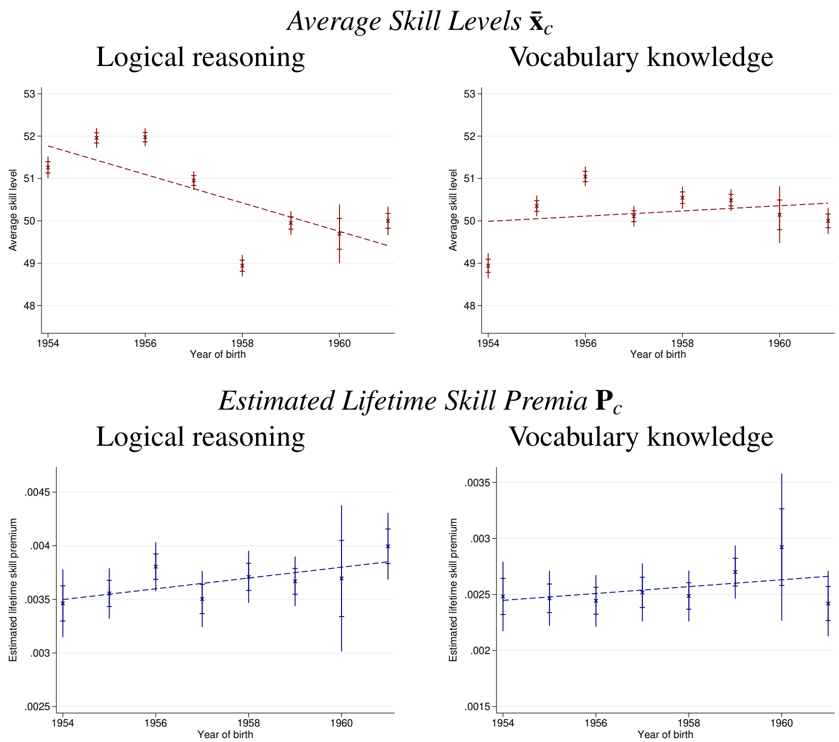
	Cohort		
	1967	1972	Difference
Correlation between survey and enlistment data in:			
Logical reasoning skill (x_{i1})	0.6557	0.6795	0.0237
	(0.0119)	(0.0085)	(0.0157)
Vocabulary knowledge skill (x_{i2})	0.6738	0.6910	0.0172
	(0.0106)	(0.0080)	(0.0129)
Number of individuals	2,927	3,451	

Notes. Panel A compares the estimated change in lifetime skill premia between birth cohorts 1967 and 1972 based on different estimation methods. The first column is based on the linear trend fitted to the series of estimated lifetime skill premia for the enlistment data, where tests were typically taken at age 18 or 19, as shown in Panel B of Figure 1.1 in the main text. The second and third columns are the differences between the lifetime skill premia for the two cohorts, as estimated on the set of individuals who have valid logical reasoning and vocabulary knowledge test scores in both the enlistment and survey data, where tests were typically taken at age 13. In the second (OLS) column, we estimate the lifetime skill premia for each cohort as the net present value of age-specific skill premia estimated via OLS, following the approach in Section 1.4.1 in the main text. In the third (IV) column, we estimate the lifetime skill premia for each cohort as the net present value of age-specific skill premia estimated via IV, treating age-13 test scores as instruments for age-18/19 test scores. Panel B compares, between birth cohorts 1967 and 1972, the Pearson correlation of skills measured in the survey data with skills measured in the enlistment data. In both panels, standard errors in parentheses are obtained via a nonparametric bootstrap with 50 replicates.



Appendix Figure 1.1: Trends in Technical Knowledge and Technical Knowledge Premia Across Birth Cohorts 1962–1973, Military Enlistment Sample

Notes. Data are from the military enlistment sample for birth cohorts 1962–1973. We exclude birth cohorts 1974 and 1975 because of significant amounts of missing data on technical knowledge test scores for these cohorts. The left plot depicts the average technical knowledge skill $\bar{x}_{cj}$ for each birth cohort c. Skills are expressed as a percentile of the distribution for the 1967 birth cohort. The right plot depicts the estimated lifetime skill premium P_{cj} for technical knowledge for each birth cohort, constructed as described in Section 1.4.1 in the main text. These skill premia are estimated controlling for logical reasoning and vocabulary knowledge skills. Each plot depicts both 95 percent pointwise confidence intervals (inner intervals, marked by dashes) and 95 percent uniform confidence bands (outer intervals, marked by line segments). Pointwise confidence intervals are based on standard errors from a nonparametric bootstrap with 50 replicates. Uniform confidence bands are computed as sup-t bands following Montiel Olea and Plagborg-Møller (2019). Each plot depicts the line of best fit through the estimated points.



Appendix Figure 1.2: Trends in Skills and Skill Premia across Birth Cohorts 1954–1961, Military Enlistment Sample

Notes. Data are from the military enlistment sample covering Swedish men born between 1954 and 1961 and who enlisted before 1980. For these birth cohorts, information on logical reasoning and vocabulary knowledge skills is based on scores from tests administered at military enlistment, called the Enlistment Battery 67. The first row of plots depicts the average skill $\bar{\mathbf{x}}_c$ for each birth cohort c. Skills are expressed as a percentile of the distribution for the 1961 birth cohort. The second row of plots depicts the estimated lifetime skill premia $\mathbf{P}_c$ for each birth cohort, constructed as described in Section 1.4.1 in the main text. Each plot depicts both 95 percent pointwise confidence intervals (inner intervals, marked by dashes) and 95 percent uniform confidence bands (outer intervals, marked by line segments). Pointwise confidence intervals are based on standard errors from a nonparametric bootstrap with 50 replicates. Uniform confidence bands are computed as sup-t bands following Montiel Olea and Plagborg-Møller (2019). Each plot depicts the line of best fit through the estimated points.

Panel A: Consent Form

Stockholms universitet

Samtyckesblankett för Enkät-undersökning alt. Elektroniska val

Stockholms universitet är personuppgiftsansvarig för den behandling av personuppgifter som sker i Survey and Report.

Den lagliga grunden för behandlingen är att du gett samtycke till behandlingen i någon enkät.
För att projektet ska kunna utföras kommer ansvarig för studien och ansvarig för projektet ha tillgång till personuppgifterna. Uppgifterna kommer att behandlas så att inte obehöriga kan ta del av dem.

Om detta material bedöms ha ett bestående värde enligt de riktlinjer som anges i 6-8 §§ i RA-FS 1999:1 kommer det att bevaras för framtiden.

Enligt EU:s dataskyddsförordning samt nationell kompletterande lagstiftning har du rätt att:

återkalla ditt samtycke utan att det påverkar lagligheten av behandling som skett i enlighet med samtycket innan det återkallades,

begära tillgång till dina personuppgifter,

få dina personuppgifter rättade,

få dina personuppgifter raderade,

få behandlingen av dina personuppgifter begränsad.

Under vissa omständigheter medger dataskyddsförordningen samt kompletterande nationell lagstiftning undantag för dessa rättigheter, som kan komma att tillämpas.

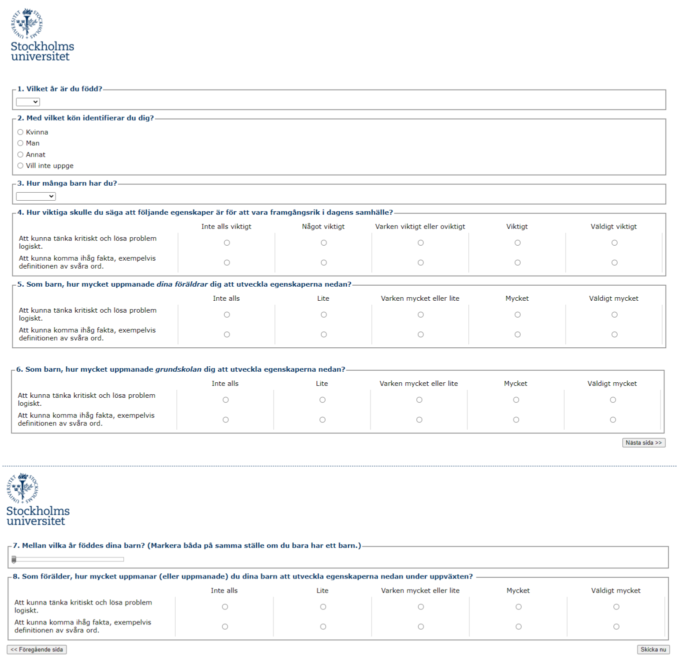
Om du vill åberopa någon av dessa rättigheter Kontakta Dataskyddsombudet vid Stockholms universitet (dso@su.se).

Mer information om detta finns på Datainspektionens hemsida. https://www.datainspektionen.se/

☐ Jag har läst och accepterar villkoren

Jag nekar samtycke | Jag samtycker

Panel B: Survey Form



Stockholms universitet

1. Vilket år är du född?

2. Med vilket kön identifierar du dig?

- Kvinna
- Man
- Annat
- Vill inte uppge

3. Hur många barn har du?

4. Hur viktiga skulle du säga att följande egenskaper är för att vara framgångsrik i dagens samhälle?

	Inte alls viktigt	Något viktigt	Varken viktigt eller oviktigt	Viktigt	Väldigt viktigt
Att kunna tänka kritiskt och lösa problem logiskt.	○	○	○	○	○
Att kunna komma ihåg fakta, exempelvis definitionen av svåra ord.	○	○	○	○	○

5. Som barn, hur mycket uppmanade *dina föräldrar* dig att utveckla egenskaperna nedan?

	Inte alls	Lite	Varken mycket eller lite	Mycket	Väldigt mycket
Att kunna tänka kritiskt och lösa problem logiskt.	○	○	○	○	○
Att kunna komma ihåg fakta, exempelvis definitionen av svåra ord.	○	○	○	○	○

6. Som barn, hur mycket uppmanade *grundskolan* dig att utveckla egenskaperna nedan?

	Inte alls	Lite	Varken mycket eller lite	Mycket	Väldigt mycket
Att kunna tänka kritiskt och lösa problem logiskt.	○	○	○	○	○
Att kunna komma ihåg fakta, exempelvis definitionen av svåra ord.	○	○	○	○	○

Nästa sida >>

Stockholms universitet

7. Mellan vilka år föddes dina barn? (Markera båda på samma ställe om du bara har ett barn.)

8. Som förälder, hur mycket uppmanar (eller uppmanade) du dina barn att utveckla egenskaperna nedan under uppväxten?

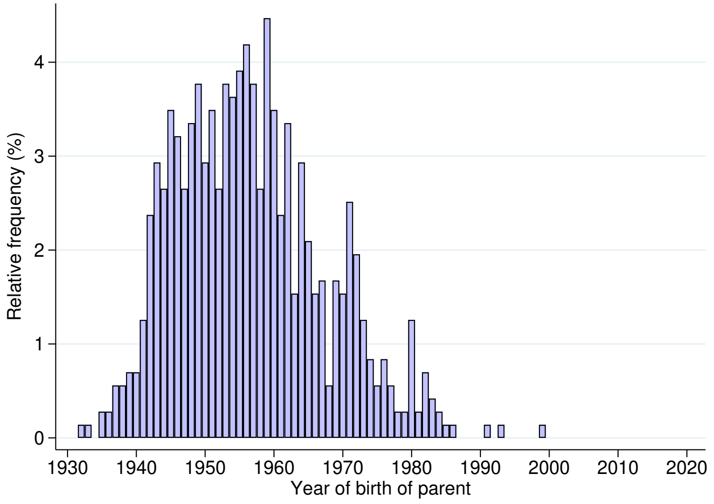
	Inte alls	Lite	Varken mycket eller lite	Mycket	Väldigt mycket
Att kunna tänka kritiskt och lösa problem logiskt.	○	○	○	○	○
Att kunna komma ihåg fakta, exempelvis definitionen av svåra ord.	○	○	○	○	○

<< Föregående sida | Skicka nu

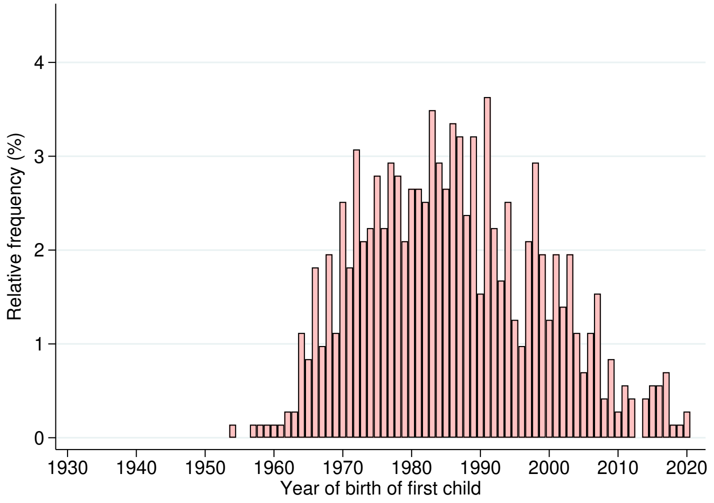
Appendix Figure 1.3: Structure of the Survey of Parents' Perceptions

Notes. This figure shows the content and structure of the survey on parents' perceptions described in Section 1.3.2 in the main text. Panel A displays the consent form and Panel B displays the survey form, both in the original Swedish.

Panel A: Respondent

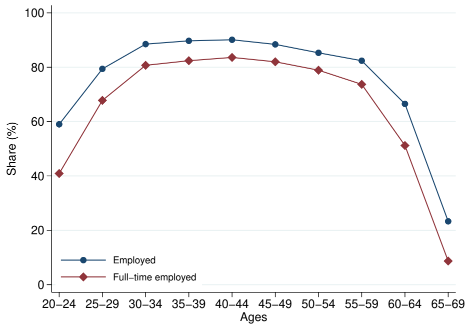


Panel B: Respondent's First Child

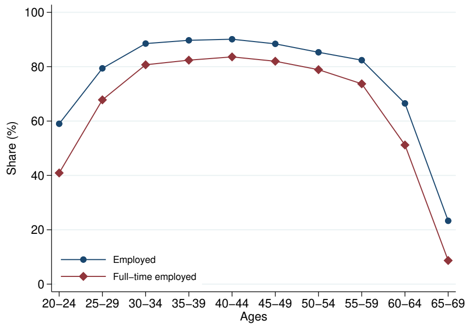


Appendix Figure 1.4: Distributions of Year of Birth of Respondent and First Child in the Survey of Parents' Perceptions

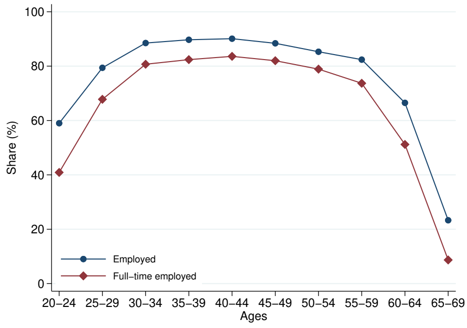
Notes. Data come from the survey of parents' perceptions described in Section 1.3.2 in the main text. Panel A shows the distribution of the year of birth of the respondent. Panel B shows the distribution of the year of birth of the respondent's first child.



(a) 2010



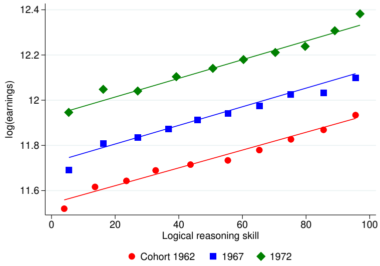
(b) 2015



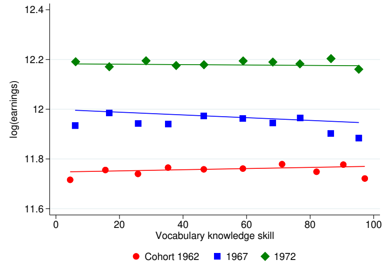
(c) 2019

Appendix Figure 1.5: Male Employment Rates by Age Group for Selected Years

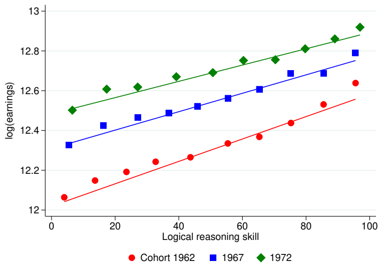
Notes. This figure shows the rates of employment and full-time employment among men in Sweden in 2010, 2015, and 2019, separately by age group, based on data from the Swedish Labour Force Surveys (Statistics Sweden, 2020a). We define an individual as employed if he meets the definition of employment used by the International Labor Organization (see, e.g. Eurostat, 2021). We define an employed individual as full-time employed if he reports working full-time in the survey.



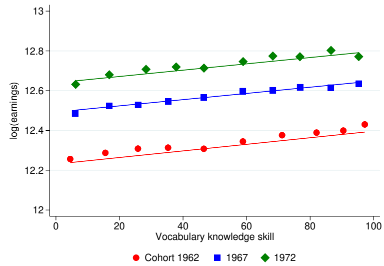
(a) Logical Reasoning, Age 30



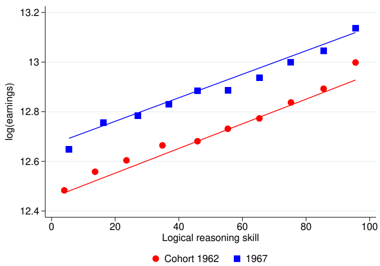
(b) Vocabulary Knowledge, Age 30



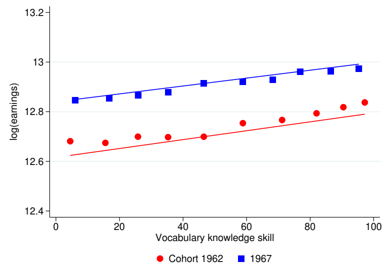
(c) Logical Reasoning, Age 40



(d) Vocabulary Knowledge, Age 40



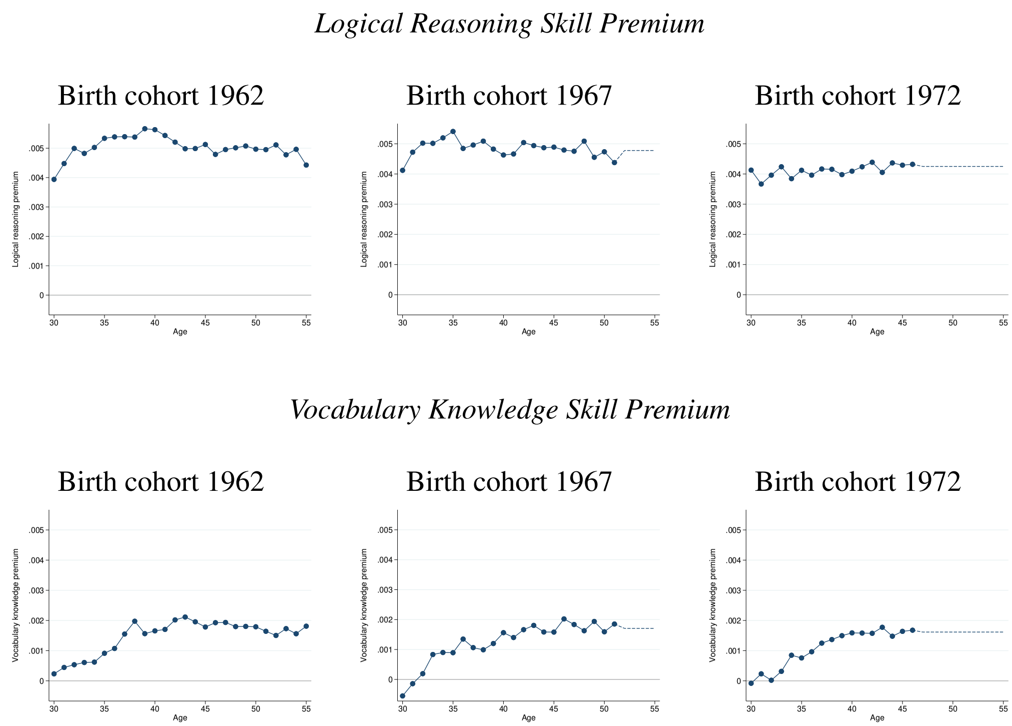
(e) Logical Reasoning, Age 50



(f) Vocabulary Knowledge, Age 50

Appendix Figure 1.6: Illustrating the Relationship Between Log(Earnings) and Skill Percentile, Military Enlistment Sample

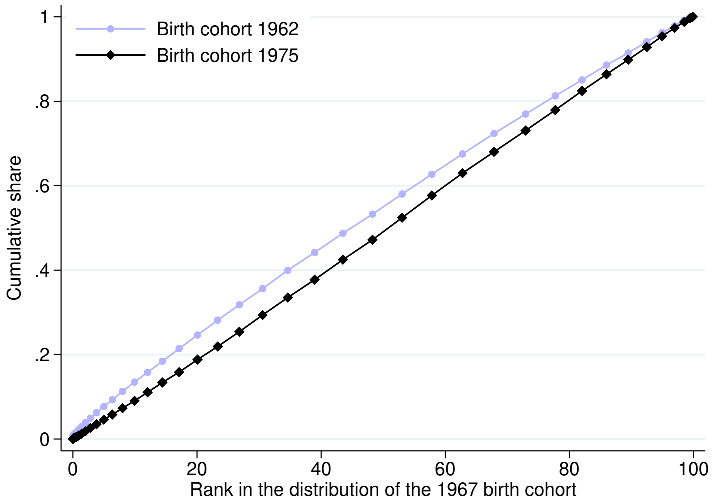
Notes. Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. This figure illustrates the relationship between the mean of log annual earnings and logical reasoning and vocabulary knowledge skill for birth cohorts 1962, 1967, and 1972, at ages 30, 40, and 50. For each cohort, age, and skill dimension, we estimate a regression of log(earnings) on indicators for decile of skill. We plot the coefficients on the decile indicators, shifted by a constant so that their mean value coincides with the sample mean of log(earnings), against the average value of the given skill within the decile. We also plot a line whose slope is equal to the estimated premium $p_{c+a,a,j}$ of the given skill dimension, estimated from a regression of log(earnings) on skills $\mathbf{x}_i$, and whose intercept is chosen so that the line coincides with the decile coefficient at the fifth decile.



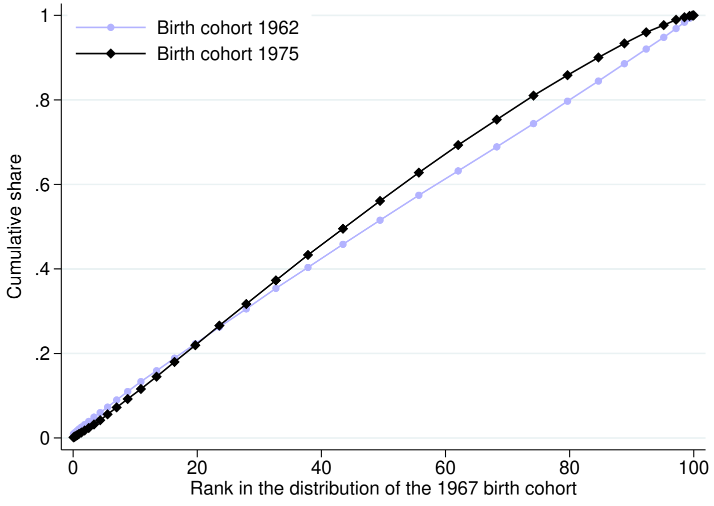
Appendix Figure 1.7: Illustrating the Extrapolation of Skill Premia to Ages with No Earnings Data, Military Enlistment Sample

Notes. Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. The plots illustrate how we estimate skill premia for ages of working life for which we do not observe earnings. The upper row of plots illustrates for logical reasoning and the lower row of plots illustrates for vocabulary knowledge. Each row includes plots for birth cohorts 1962, 1967, and 1972. For each cohort, we estimate the skill premia $\mathbf{p}_{c+a,a}$ in ages for which we do not observe earnings (dashed line) by taking the average estimated skill premia across all ages 40+ for which we do observe earnings (markers).

Logical Reasoning

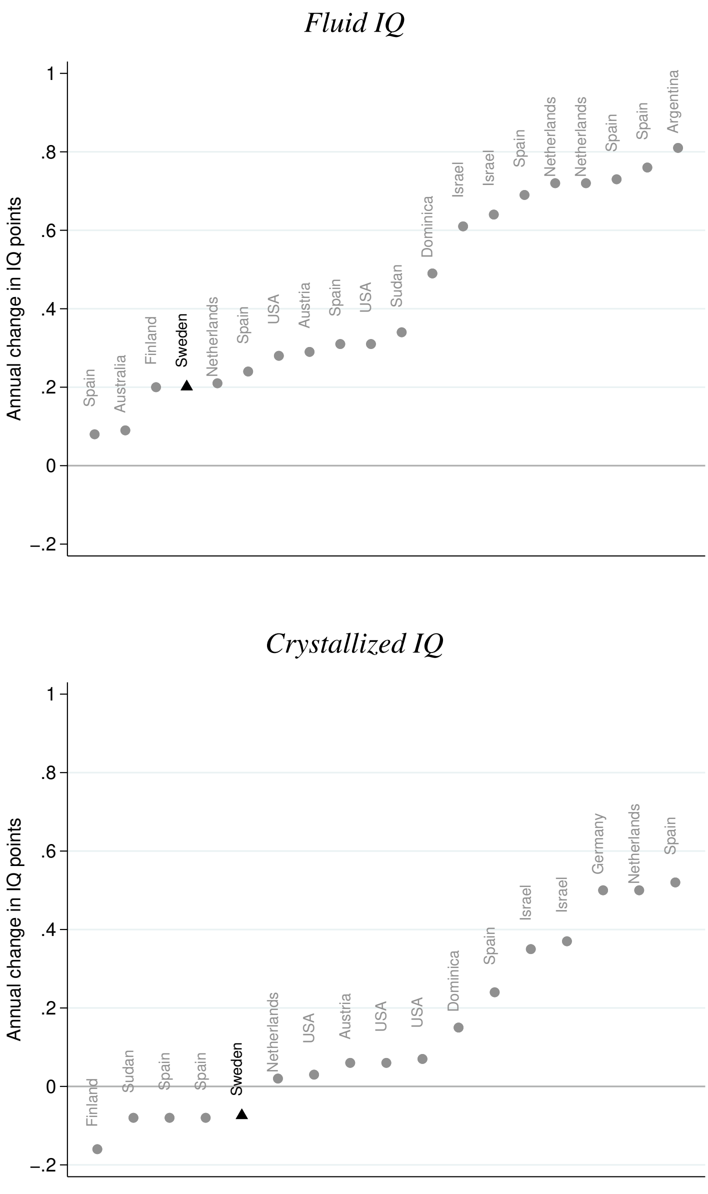


Vocabulary Knowledge



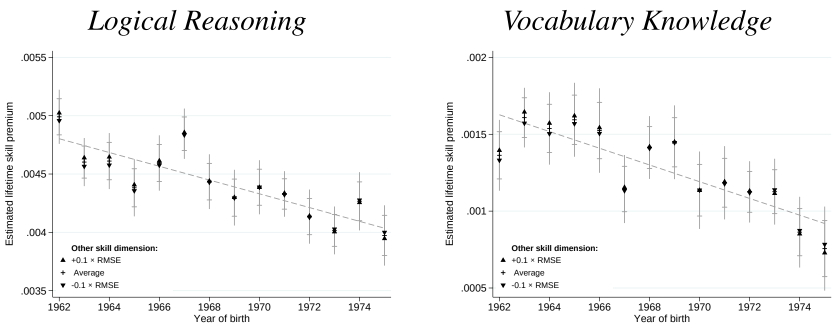
Appendix Figure 1.8: Distributions of Skills in the 1962 and 1975 Birth Cohorts, Military Enlistment Sample

Notes. Data are from the military enlistment sample covering birth cohorts 1962 and 1975, with tests typically taken at age 18 or 19. Each plot depicts the empirical cumulative distribution function of skills x_{ij} for a given dimension j for members i of the 1962 and 1975 birth cohorts. Skills are expressed as a percentile of the distribution for the 1967 birth cohort.



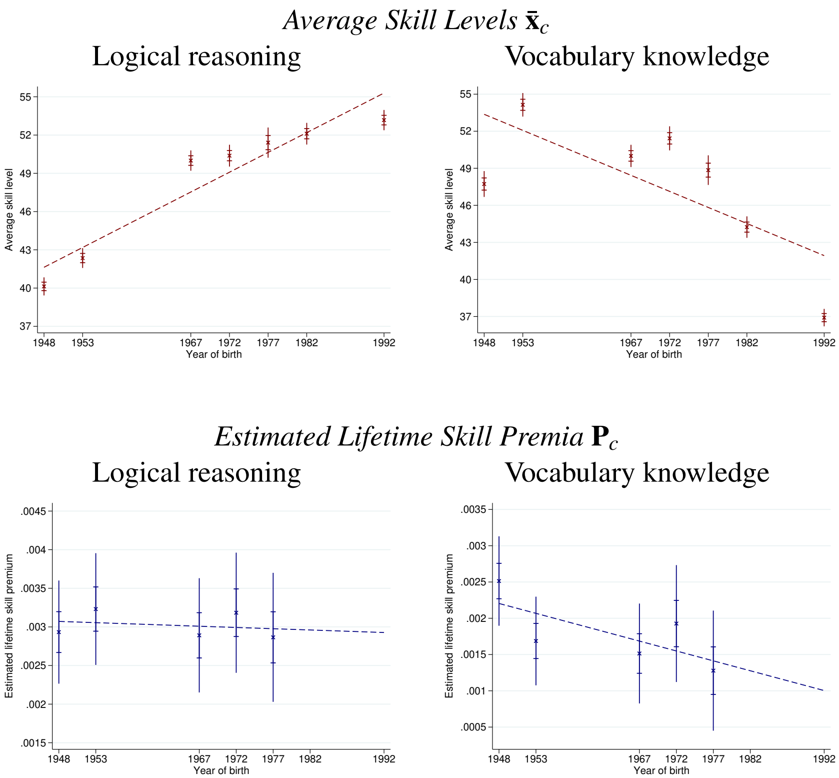
Appendix Figure 1.9: Measured Trends in Fluid and Crystallized IQ

Notes. Data are from Pietschnig and Voracek (2015, Table S1, circles) or from the military enlistment sample covering birth cohorts 1962–1975 (triangles). We select from Pietschnig and Voracek's meta-analysis (2015, Table S1) all single-country studies of fluid or crystallized intelligence covering healthy adults with a sample size of at least 100 and a study period ending in 1980 or later. We classify studies of PIQ as fluid and studies of VIQ or verbal as crystallized. We plot the annual IQ gain in each study, labeling each study with the country in which the sample was obtained. For comparison, we also plot the annual IQ gain in the enlistment sample, which we calculate by standardizing the raw score on the logical reasoning (fluid) and vocabulary knowledge (crystallized) tests to have a mean of 100 and a standard deviation of 15 in the 1967 cohort.



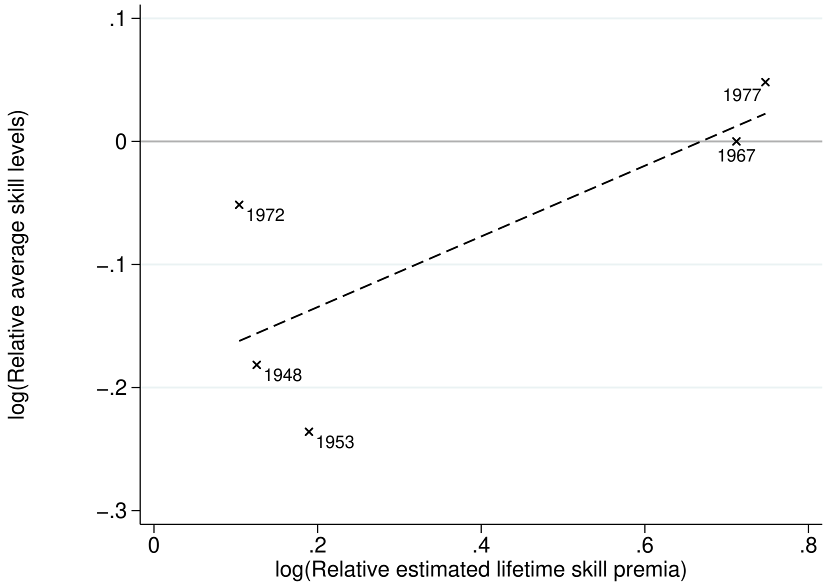
Appendix Figure 1.10: Trends in Skill Premia across Birth Cohorts 1962–1975, Allowing for Interactions, Military Enlistment Sample

Notes. Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. We construct the plots as follows. For each cohort c and each year t for which we measure earnings, we estimate a generalization of equation (1.1) in the main text that includes an interaction $x_{i1}x_{i2}$ between the two skill dimensions. From these estimates we calculate cohort-and-year-specific skill premia for each skill dimension j, evaluated at three different levels of skill on the other dimension $j' \neq j$: the cohort average, 0.1 root mean squared error (RMSE) above the cohort average, and 0.1 RMSE below the cohort average, where the RMSE is calculated from a cohort-specific regression of skill $x_{ij'}$ on indicators for skill x_{ij}. We then follow the approach described in Section 1.4.1 in the main text to estimate the cohort-and-year-specific premia for years outside of our sample, and we compute lifetime premia following equation (1.3) in the main text. For each dimension j, the plot depicts the lifetime premium for an individual in each cohort c whose skill on the other dimension $j' \neq j$ is equal to the cohort average ("Average"), an individual whose skill on the other dimension is 0.1 RMSE above the cohort average ("$+0.1 \times$ RMSE"), and an individual whose skill on the other dimension is 0.1 RMSE below the cohort average ("$-0.1 \times$ RMSE"). Each plot includes a line of best fit, 95 percent pointwise confidence intervals (inner grey intervals, marked by dashes), and uniform confidence bands (outer grey intervals, marked by line segments) corresponding to the "Average" series. Pointwise confidence intervals are based on standard errors from a nonparametric bootstrap with 50 replicates. Uniform confidence bands are computed as sup-t bands following Montiel Olea and Plagborg-Møller (2019).



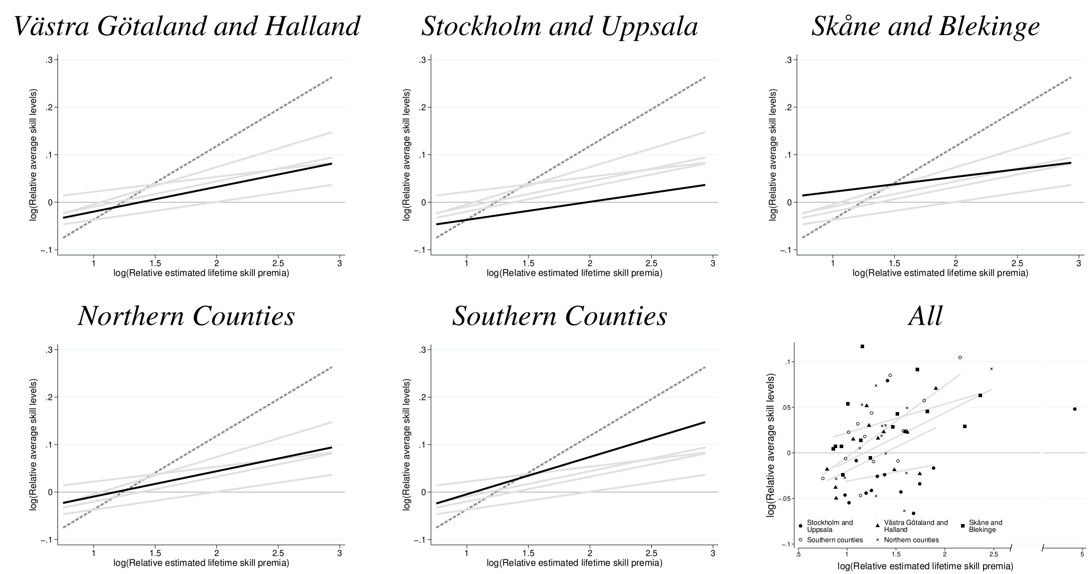
Appendix Figure 1.11: Trends in Skills and Skill Premia across Birth Cohorts, Survey Sample

Notes. Data are from the survey sample covering birth cohorts 1948, 1953, 1967, 1972, 1977, 1982, and 1992. The first row of plots depicts the average skill $\bar{\mathbf{x}}_c$ for each birth cohort c. Skills are expressed as a percentile of the distribution for the 1967 birth cohort. The second row of plots depicts the estimated lifetime skill premia $\mathbf{P}_c$ for each birth cohort c in 1948, 1953, 1967, 1972, and 1977, constructed as described in Section 1.4.1 in the main text. Each plot depicts both 95 percent pointwise confidence intervals (inner intervals, marked by dashes) and 95 percent uniform confidence bands (outer intervals, marked by line segments). Pointwise confidence intervals are based on standard errors from a nonparametric bootstrap with 50 replicates. Uniform confidence bands are computed as sup-t bands following Montiel Olea and Plagborg-Møller (2019). Each plot depicts the line of best fit through the estimated points.



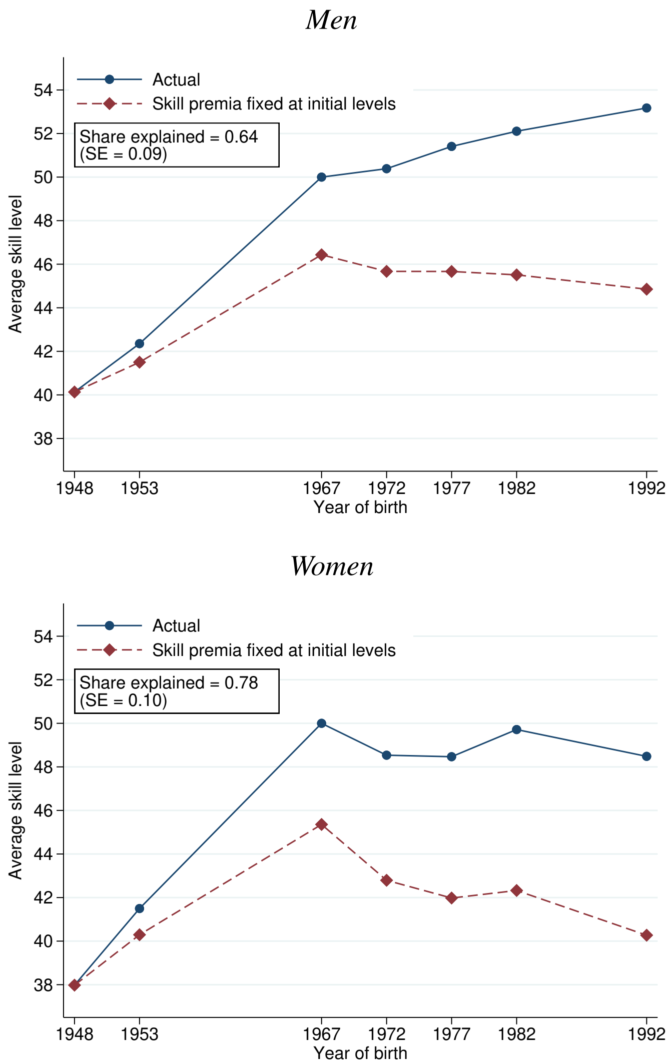
Appendix Figure 1.12: Evolution of Relative Skill Levels and Relative Skill Premia, Women in Survey Sample

Notes. Data are from the survey sample covering birth cohorts 1948, 1953, 1967, 1972, and 1977, with tests typically taken at age 13, for female respondents. The plot shows a scatterplot of the natural logarithm of the relative average skill levels, $\ln\left(\frac{\bar{x}_{c1}}{\bar{x}_{c2}}\right)$, against the natural logarithm of the relative estimated lifetime skill premia, $\ln\left(\frac{P_{c1}}{P_{c2}}\right)$. The dashed line depicts the line of best fit.



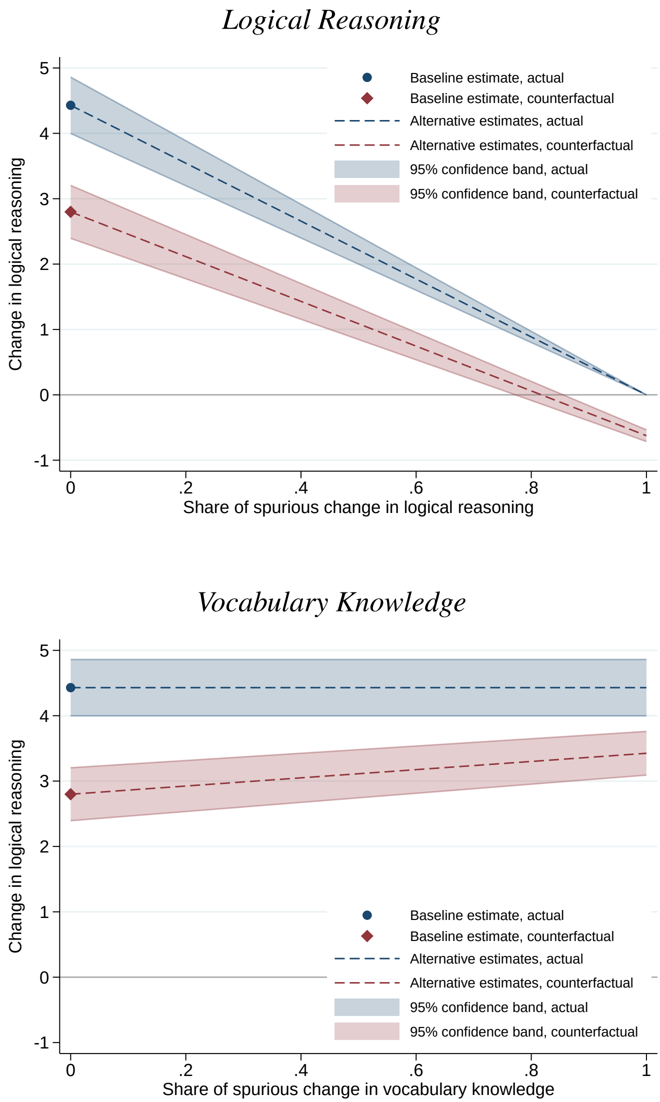
Appendix Figure 1.13: Evolution of Relative Skill Levels and Relative Skill Premia by Region, Military Enlistment Sample

Notes. Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. The lower right plot shows a scatterplot of the natural logarithm of the relative average skill levels, $\ln\left(\frac{\bar{x}_{c1}}{\bar{x}_{c2}}\right)$, against the natural logarithm of the relative estimated lifetime skill premia, $\ln\left(\frac{P_{c1}}{P_{c2}}\right)$, separately by birth region. Each marker corresponds to a cohort and region. The light gray lines depict the lines of best fit for each region. Each of the other plots shows the line of best fit for the given region (black line), the lines of best fit for the other regions (light gray lines), and the line of best fit for the full sample (dashed line). We classify individuals into mutually exclusive and exhaustive regions according to their county of birth, excluding those born outside of Sweden, using data from Statistics Sweden (2016a). We classify Värmland, Örebro, Västmanland, Dalarna, Gävleborg, Västernorrland, Jämtland, Västerbotten, and Norrbotten as Northern counties, Södermanland, Östergötland, Jönköping, Kronoberg, Kalmar, and Gotland as Southern counties, and the remaining counties eponymously.



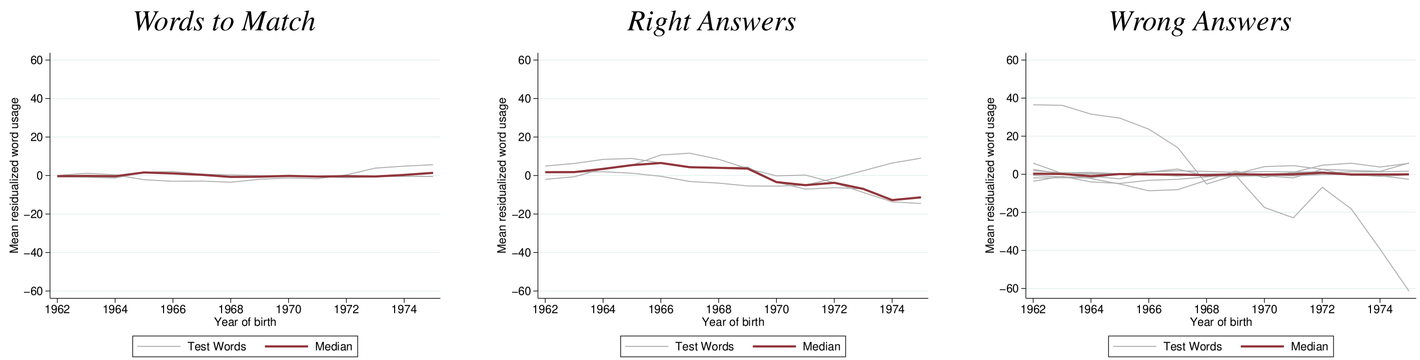
Appendix Figure 1.14: Decomposition of Change in Average Logical Reasoning Skill, Survey Sample

Notes. Data are from the survey sample of male respondents (upper panel) and female respondents (lower panel) covering birth cohorts 1948, 1953, 1967, 1972, 1977, 1982, and 1992, with tests typically taken at age 13. Each plot depicts the average logical reasoning skill $\bar{x}_{c1}$ for each birth cohort c ("Actual") and the predicted average skill $\tilde{x}_{c1}\left(\overline{\mathbf{P}}_{\underline{c}}\right)$ under the counterfactual in which lifetime skill premia remain at the level estimated for the 1948 birth cohort ("Skill premia fixed at initial levels"). Skills are expressed as a percentile of the distribution for the 1967 birth cohort. We fit the model as in Figure 1.4 in the main text, separately for men and women, taking the linear fit for the cohorts through 1977 (depicted for men in Appendix Figure 1.11) as our estimate of the lifetime skill premia $\mathbf{P}_c$ for all cohorts. The text box in each plot shows the estimated share $1-\frac{\tilde{x}_{\bar{c}1}\left(\mathbf{P}_{\underline{c}}\right)-\tilde{x}_{\underline{c}1}\left(\mathbf{P}_{\underline{c}}\right)}{\bar{x}_{\bar{c}1}-\bar{x}_{\underline{c}1}}$ of the observed change from 1948 through 1992 that is accounted for by changes in skill premia ("Share explained"). The standard errors in parentheses are obtained via a nonparametric bootstrap with 50 replicates. We exclude seven and three bootstrap replicates from the calculation of standard errors for the upper and lower plots, respectively, due to values inconsistent with the model.



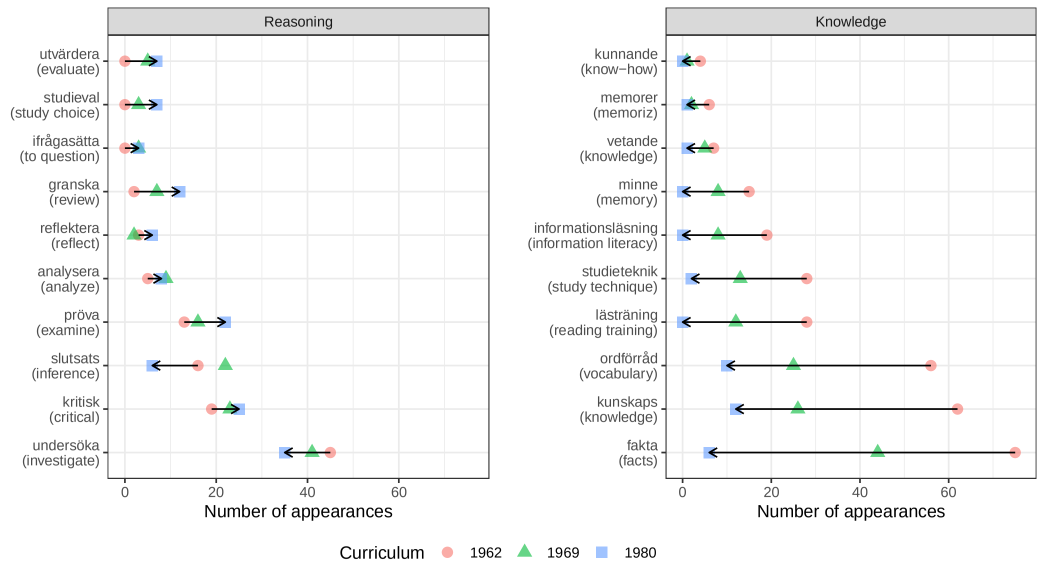
Appendix Figure 1.15: Sensitivity to Spurious Cohort Trends in Skills

Notes. Data are from the military enlistment sample covering birth cohorts 1962–1975, with tests typically taken at age 18 or 19. To construct each plot, we assume that the true mean skill level $\tilde{x}_{\bar{c}j}$ on dimension j in the 1975 birth cohort $\bar{c}$ is given by $\omega_j \bar{x}_{\underline{c}j} + \left(1-\omega_j\right)\bar{x}_{\bar{c}j}$, such that $\omega_j \in [0,1]$ denotes the fraction of the observed change $\bar{x}_{\bar{c}j} - \bar{x}_{\underline{c}j}$ that is spurious. We then re-estimate our model following the methods in Table 1.1 in the main text and calculate, for each ω_j, the implied actual change in logical reasoning skill $\tilde{x}_{\bar{c}1}\left(\mathbf{P}_{\bar{c}}\right) - \tilde{x}_{\underline{c}1}\left(\mathbf{P}_{\underline{c}}\right)$ and the implied counterfactual change in logical reasoning skill $\tilde{x}_{\bar{c}1}\left(\mathbf{P}_{\underline{c}}\right) - \tilde{x}_{\underline{c}1}\left(\mathbf{P}_{\underline{c}}\right)$ if skill premia had remained constant at their level for the 1962 birth cohort. Each plot depicts the actual and counterfactual change in logical reasoning skill (y-axis) as a function of the fraction of the observed change that is spurious (x-axis). The upper plot depicts the implications of a spurious change in logical reasoning skill ($\omega_1 \in [0,1]$, $\omega_2 = 0$). The lower plot depicts the implications of a spurious change in vocabulary knowledge ($\omega_1 = 0$, $\omega_2 \in [0,1]$). For each depicted series, the shaded region collects pointwise 95% confidence intervals obtained via a nonparametric bootstrap with 50 replicates. The estimates labeled "Baseline estimate" correspond to the estimates in Panel A of Table 1.1 in the main text, i.e., the case in which $\omega_1 = \omega_2 = 0$.



Appendix Figure 1.16: Trends in Usage of Words in Example Vocabulary Questions

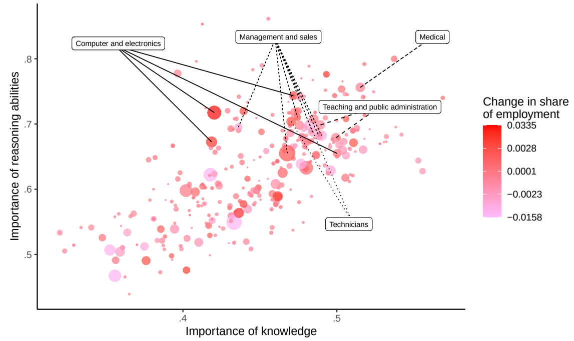
Notes. Data are from Kungliga biblioteket (2022). We construct the plots as follows. We begin with the example questions for Enlistment Battery 2000 posted at Rekryteringsmyndigheten (2010). We classify the words of interest in the example synonym questions (Rekryteringsmyndigheten, 2012) into the words the test-taker is asked to match to a synonym ("words to match"), the correct synonyms ("right answers"), and the incorrect synonyms ("wrong answers"). We specify a set of reference words consisting of the names of the days of the week. From Kungliga biblioteket (2022) we obtain the frequency with which each word of interest, as well as each reference word, was used in the newspaper *Dagens Nyheter* in each year from 1962 to 1993. For each word of interest we estimate a regression of the frequency of the word's use on the total frequency across all reference words, where the unit of observation is the word-year. We take the residual from this regression and compute, for each word of interest and each birth cohort 1962–1975, the average residual over the first 19 years of the cohort's life. We subtract the mean of this series and take the result as our measure of the cohort's exposure to the given word of interest. In each plot, each lighter line shows the exposure of each cohort to a given word of interest, and the darker line shows the median exposure of each cohort across all words of interest in the given category. We use example questions for Enlistment Battery 2000 because we are not aware of example questions for Enlistment Battery 1980 that are in the public domain.



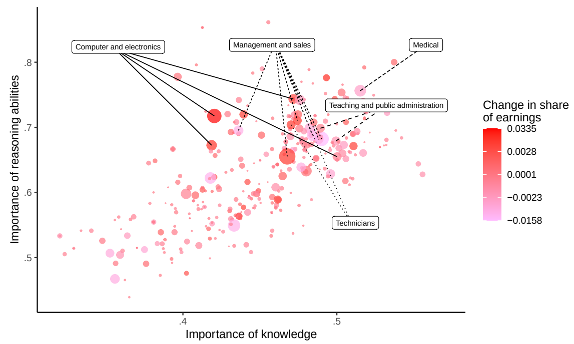
Appendix Figure 1.17: Selected Word Families Related to Reasoning vs. Knowledge in Swedish Primary School Curricula

Notes. The plot shows the number of appearances of selected word families related to fluid intelligence ("Reasoning", left panel) and crystallized intelligence ("Knowledge", right panel) in the 1962, 1969, and 1980 revisions of the Swedish primary school Curricula (Läroplan for grundskolan; Skolöverstyrelsen, 1962, 1969, 1980). Translations of word families are in parenthesis under the original Swedish. In both panels, word families are listed in ascending order based on their number of appearances in the 1962 Curriculum. The arrows point from the number of appearances in the 1962 Curriculum to the number of appearances in the 1980 Curriculum. We chose a set of keywords based on a close reading of the Curricula and categorized them in word families. We counted the number of appearances of each word family as follows. For the "analysera," "granska," "ifrågasätta," "informationsläsning," "kunnande," "ordförråd," "pröva," "reflektera," "slutsats," "studieval," and "utvärdera" families, we searched for all words that start with the same characters. For the "fakta," "kritisk," "kunskaps," "memorer," "minne," "undersöka," and "vetande" families, we searched for all words that start with the same characters plus a few alternate forms. For the "lästräning" and "studieteknik" families, we searched for the exact word only. We conducted searches automatically. A Swedish speaker then reviewed search results and excluded cases, such as negations, where usage did not match our intent.

Panel A: Changes in Shares of Total Employment



Panel B: Changes in Shares of Total Earnings



Appendix Figure 1.18: Growth of Occupations in Sweden by Their Reasoning and Knowledge Intensity

Notes. The figure shows scatterplots of the knowledge and reasoning intensity as well as growth of each occupation in the Swedish Occupational Register. Panel A measures occupation growth with the change in the occupation's share of total employment between cohorts 1962 and 1975, with marker sizes proportional to the occupation's share of total employment in cohort 1962. Panel B measures occupation growth with the change in the share of total earnings between cohorts 1962 and 1975, with marker sizes proportional to the occupation's share of total earnings in cohort 1962. In both panels, the y-axis depicts the total importance of reasoning abilities, and the x-axis depicts the total importance of knowledge. We measure the distribution of employment and earnings across occupations in the Swedish Occupational Register using data on employment histories from 2004 onwards from Statistics Sweden (2021), using 4-digit Swedish Standard Classification of Occupations 96 (SSYK 96) codes, and taking each individual's occupation to be the one observed in the available year closest to the year the individual turns 40. For each O*NET 25.0 (2020) occupation we define the total importance of reasoning abilities by summing the importance scores of Inductive, Deductive, and Mathematical Reasoning abilities and dividing by the highest possible sum. Similarly, we define the total importance of knowledge by summing the importance scores of all knowledge categories and dividing by the highest possible sum. We compute the total importance of reasoning abilities and knowledge of each SOC 2010 occupation by taking unweighted averages across all corresponding occupations in O*NET 25.0 (2020). We match the occupations in the Swedish Occupational Register to occupations in the Standard Occupational Classification 2010 (SOC 2010) by using the crosswalks from Statistics Sweden (2016c) and BLS (2015), manually excluding some matches to improve accuracy. We define the total importance of reasoning abilities and knowledge for each occupation in the Swedish Occupational Register by taking employment-weighted averages across all corresponding SOC 2010 occupations, using May 2018 OES employment estimates (BLS 2019) as weights. Heuristic descriptions, written by us, are applied to all occupations with total importance of reasoning above 0.65 and a share of earnings in 1962 of least 0.01. This figure includes information from the O*NET 25.0 Database by the U.S. Department of Labor, Employment and Training Administration (USDOL/ETA). Used under the CC BY 4.0 license. O*NET® is a trademark of USDOL/ETA. We have modified all or some of this information. USDOL/ETA has not approved, endorsed, or tested these modifications.

Appendix References

Eurostat. (2021). *Employed Person – LFS*. Retrieved February 27, 2021, from https://ec.europa.eu/eurostat/statistics-explained/index.php/Glossary:Employed_person_-_LFS

Kungliga biblioteket. (2022). *Svenska dagstidningar*. Retrieved March 31, 2022, from https://tidningar.kb.se/

Rekryteringsmyndigheten. (2010). *Teoretiska prov (December 30, 2010)*. Retrieved October 28, 2021, from https://web.archive.org/web/20120326205641/http://www.rekryteringsmyndigheten.se/malgruppsnavigering/om-du-skall-testas/tester-och-undersokningar/teoretiska-prov

Rekryteringsmyndigheten. (2012). *Prova på inskrivningsprovet (October 5, 2012)*. Retrieved October 28, 2021, from https://web.archive.org/web/20121005001305/http://rekryteringsmyndigheten.se/trmPublic/IProvet/inskrivningsprovet.htm

Statistics Sweden. (2020a). Labour Force Surveys (LFS) [Received on October 23, 2020].

Chapter 2

Adoption of Medical Innovations Across Hospitals and Socioeconomic Groups: Evidence from Sweden*

*This chapter is co-authored with Fabian Sinn. We are indebted to Arash Nekoei and David Seim for their encouragement, guidance, and support throughout this project. We thank David Seim for providing access to the data. We thank Maija Kaartinen, Arizo Karimi, Peter Nilsson, Mårten Palme, Torsten Persson, Dominik Wehr, and Jens Wikström, and seminar audiences at Harvard University and Stockholm University for helpful comments and discussions. We thank Viktor Granvald and Åsa Wresros at the National Board of Health and Welfare for providing statistics on inpatient and outpatient visits, costs per DRG point, and retrospective DRG weights and guidance on their use. We thank the World Management Survey research team for granting us special access to the WMS data with hospital identifiers for Sweden. All errors are ours.

2.1 Introduction

This paper studies technology adoption in healthcare. There can be substantial variation in the adoption patterns of medical innovations, such as new medicines (Skinner & Staiger, 2015). As a striking example, consider the adoption in the United States of beta blockers, inexpensive drugs for treating high blood pressure. A meta-analysis from 1985 concluded that long-term use of beta blockers can reduce one-year mortality rate after a heart attack by 25 percent (Yusuf et al., 1985, p. 366). Yet, in 2000–2001, the share of Medicare fee-for-service beneficiaries who received beta blockers at hospital discharge after a heart attack ranged from 57 to 95 percent across states (Jencks et al., 2003, Tables 2 and 3).[1]

Understanding to what extent the adoption of medical innovations varies across hospitals and patient groups is important because slow adoption is costly when new treatments substantially improve over existing ones. The resulting costs can be health-related, due to increased mortality and morbidity, and fiscal due to e.g. reduced work capacity and increased take-up of social benefits. Differences between socioeconomic groups in the adoption to medical innovations may also contribute to disparities in access to high-quality healthcare and widen health inequalities (e.g., Chetty et al., 2016; Finkelstein et al., 2021; Mackenbach, 2012; Zhang et al., 2010). Differences in adoption may also help explain geographic differences in healthcare spending (e.g., Badinski et al., 2023; Finkelstein et al., 2016).

In this paper, we use Swedish register data to show that there is sizable variation in the adoption patterns of medical innovations across hospitals and socioeconomic groups.[2] For a set of 58 novel medicines introduced between

[1]Large variations in technology adoption are not unique to the healthcare sector. Similar patterns hold within and across countries and in contexts such as agriculture, manufacturing, and transportation. Pioneering studies on technology adoption include Coleman et al. (1957), Griliches (1957), and Mansfield (1961). Surveys include Skinner and Staiger (2007), Foster and Rosenzweig (2010), Comin and Mestieri (2014), and Miraldo et al. (2019).

[2]By "medical innovations", we refer to clinical innovations, specifically new anatomical therapeutic chemical (ATC) codes. Studies focusing on organizational innovations, such as electronic medical records and remote healthcare services, include e.g. Miller and Tucker (2011), Zhou et al. (2021), and Dahlstrand (2023).

January 2005 and January 2014 related to 47 health conditions, we document large variation in adoption patterns hospitals and socioeconomic groups for diverse health conditions, ranging from cardiovascular diseases to lung diseases to attention deficit hyperactivity disorder (ADHD). We then use a novel antiplatelet medicine for the treatment of heart attacks that saw widespread adoption during our analysis period as a case study to argue that disparities in adoption rates of novel medicines across socioeconomic groups may contribute to health disparities.

Our analysis combines individual-level Swedish register data on inpatient care visits, outpatient care visits, and prescription drug purchases with register data on socioeconomic background and labor market histories. For each healthcare visit between 1998–2014, we observe the healthcare provider, admission and discharge dates, as well as diagnosis and procedure codes associated with the visit. For each drug purchase between 2005–2014, we observe the active substance (ATC code) of the drug, dates of purchase and prescription, and the total cost (including subsidies) of the drug.

Using the data on drug purchases and the marketing authorization dates of medical products, we identify a set of 58 novel medicines (new ATC codes) introduced between January 2005 and January 2014. Our set of drugs covers 47 distinct health conditions, including prominent ones such as cardiovascular conditions (e.g., heart attacks and atrial fibrillation), lung diseases (e.g., COPD), and diabetes. For each drug, we approximate the target patient group by mapping the indications of each to diagnosis and procedure codes included in the register data. We then measure adoption by matching dates of healthcare visits with the prescription dates of purchased drugs. We measure adoption at the hospital level by tracking the share of patients visiting a given a hospital who purchase novel medicines.

Our analysis proceeds in three steps. First, we document large heterogeneity across hospitals in the adoption rates of novel medicines. For example, at the end of our analysis period, the adoption rate of novel medicines for hospitals at the 90th percentile was roughly three times as large as the adoption rate for hospitals at the 10th percentile. Similar patterns hold when looking at specific patient groups, such as heart attack, atrial fibrillation, or chronic obstructive pulmonary disease (COPD) patients.

Second, we document a positive association between the patient's income rank (measured before the healthcare visit) and the adoption rate of novel medicines for a diverse set of health conditions. Pooling across all our novel medicines, we find that moving from the bottom to the top income percentile increases the probability of purchasing a novel medicine by roughly 0.1 percentage points, or 10 percent relative to the average adoption rate. This pattern holds across diverse health conditions, ranging from cardiovascular diseases to lung diseases to ADHD.

Third, we use a case study to study the potential consequences of the variation in adoption patterns on health disparities between socioeconomic groups. We focus on first-time heart attack patients and a novel antiplatelet drug (ticagrelor) that saw widespread adoption over our study period. Combining mortality estimates from the drug's pivotal clinical trial (Wallentin et al., 2009) together with our estimates, we find that equalizing adoption rates between the top and bottom income deciles could have reduced the gap in 12-month survival rates by 1.2 percent over the period we study.

Finally, we explore some factors that potentially related to the variation in adoption pattersn. In particular, we explore if delayed information in the form of old guidelines causes delays. Using an event study exploiting variation in when the antiplatelet drug is included in regional guidelines, we do not find evidence of the guidelines affecting the prescription share of the drug. We also investigate whether faster-adopting hospitals have better management practices using survey data from Bloom et al. (2014), but do not find evidence of a correlation. This leaves open the question of the what causes the variation in adoption patterns.

Although our analysis is subject to important caveats, we see our findings as suggesting that differences in the adoption rates of novel medicines can contribute to health disparities at least in the context of cardiovascular diseases, which have been identified as one of the main drivers of the increase in the life expectancy gap between the rich and poor in Sweden (Fors et al., 2021;

Hederos et al., 2018; Åström, Franks, & Sundquist, 2018; Åström, Sundquist, & Sundquist, 2018) as well as other countries such as Denmark (Dahl et al., 2021) and Norway (Kinge et al., 2019).

Related literature. Our paper makes two main contributions. First, we add to the literature on the adoption of medical innovations and on technology diffusion in general. Relative to existing studies that typically focus on a relatively small number of innovations (e.g., Agha & Molitor, 2018; Arrow et al., 2020; Korda et al., 2011; Skinner & Staiger, 2015), we contribute by studying the adoption patterns of a wide set of medical innovations. In particular, we show that adoption rates vary across hospitals and socioeconomic groups for a wide set of health conditions.

Second, our work relates to the large literature on socioeconomic disparities in health in developed countries (e.g., Banks et al., 2021; Bosworth, 2018; Chetty et al., 2016). A prominent theory for why these health inequalities persist is that individuals with high socioeconomic status have better access to medical innovations and are faster to adopt them (e.g., Lleras-Muney & Lichtenberg, 2005; Mackenbach, 2012). We contribute by documenting disparities between socioeconomic groups in the adoption of novel medicines and using a case study to evaluate how important these disparities can be for explaining health inequalities between socioeconomic groups.

Outline. This paper proceeds as follows. Section 3.2 describes the institutional setting. Section 2.3 describes our data sources, explains how we choose the set of novel medicines for the analysis, and how we measure their adoption. Section 3.5 describes the adoption of the novel medicines and highlights variation in adoption patterns across hospitals and socioeconomic groups. Section 2.5 uses a novel antiplatelet medicine as a case study to evaluate the potential the health costs and effects on health disparities of the variation in adoption patterns. Section 3.6 concludes.

2.2 Context

We first describe relevant features of the Swedish healthcare system and then describe how new medicines are introduced and priced.

2.2.1 Healthcare System

Organization. Sweden has a universal healthcare system. The system is mainly financed by taxes, but also by state subsidies and user fees. Total health expenditures grew from 7.9% to 11.5% of GDP between 2001 and 2020 (Statistics Sweden, 2022b). Roughly 80% of total health expenditures are public expenditures. Private health expenditures mostly consist of out-of-pocket user costs (Anell et al., 2012, pp. 49-58).

The healthcare system is decentralized with three independent levels: the national, the regional (21 regions), and the municipal (290 municipalities) levels (Björvang et al., 2023). Municipalities are responsible for providing nursing home care, social services, and elderly housing services. Regions are responsible for healthcare provision. They finance this mainly by levying taxes, but also with state subsidies and revenue from user fees.

Healthcare provision. Primary care and specialized outpatient care (care by medical specialists, such as surgeons, orthopedics, and physiotherapists) are provided by roughly 1,100 primary care units (in 2020), roughly 40% of which are private (Sveriges Kommuner och Regioner, 2021, p. 30). Inpatient care is typically provided by university hospitals (7 in total), regional hospitals (around 20), and local hospitals (more than 40). Most hospitals are public, but regions can have contracts with private hospitals. In both cases, the patient fees are subject to the same rules.[3]

Patient fees are low, being at most 100 SEK for inpatient visits and 350 SEK for specialized outpatient visits, and varying from 100–250 SEK across counties for primary care visits in 2017 (Pontén et al., 2017). All residents

[3]There are also a small number of private hospitals without a contract with the state, regions, or municipalities, at which patients pay for the cost of care and treatment in full.

are automatically covered by a public and uniform prescription drug insurance scheme (see e.g., Wikström, 2023). Both patient and prescription drug fees have ceilings for out-of-pocket expenditures that reset after 12 months of the first visit/purchase of the coverage period. In 2017, the ceiling for prescription drug expenditures was 2200 SEK while the ceiling for patient fees ranged from 900–1100 SEK across counties (Pontén et al., 2017).

Patient choice. Patients have been allowed to choose any public or private primary healthcare provider, and a specific general practitioner if they want, accredited by their region of residence since 2010. Before 2010, patients' ability to choose a provider was not codified into law but instead varied by region and municipality (Anell et al., 2012, pp. 44-45, 110–112).

2.2.2 Introduction and Pricing of New Medicines

The introduction of new pharmaceutical products proceeds in two steps.

Marketing authorization. First, the Swedish Medical Products Agency (SMPA, "Läkemedelsverket"), a government agency under the Ministry of Health and Social Affairs, decides on the market authorization of new pharmaceutical products and determines whether the product requires a prescription, and determines which products are substitutable with each other (Läkemedelsverket, 2024a, 2024b).[4] These processes are harmonized with European Union (EU) legislation, and the SMPA works in coordination with the European Medicines Agency (EMA).[5]

[4]Substitutable products must have the same active ingredients in the same amounts, have the same dosage form, and be therapeutically equivalent. Pharmacies are required to offer to replace a prescribed drug with its lowest-price substitute covered by the subsidy system (Läkemedelsverket, 2024b).

[5]The European Medicines Agency on average recommends approval for 38 novel drugs per year, a figure similar to the average number (34) of novel drugs the Food and Drug Administration (FDA) approves per year in the United States. Market authorization in the EU is granted by the European Commission, mostly following EMA's recommendations with a delay of some months. For the EMA, the above figure refers to the average number of positive opinions for market authorization issued by EMA for new non-orphan medicinal products from 2011–2021 (European Medicines Agency [EMA], 2013, 2017, 2021). For the U.S., the above figure refers to the annual average for the period 1993–2022, see Mullard (2022).

Pricing and subsidization. Second, the Dental and Pharmaceutical Benefits Agency ("Tandvårds- och läkemedelsförmånsverket", TLV) decides on the pricing and reimbursement of pharmaceutical products. For new products, these decisions are made by the Pharmaceutical Benefits Board ("Läkemedelsförmånsnämnden"), a separate government-appointed board of experts within TLV.[6]

When applying for a new medication to be included in the reimbursement (high-cost protection) system, a company needs to state the retail price at which they wish to sell the drug, specify the patient group(s) for which they wish the reimbursement coverage to apply, and provide a health economic analysis. According to the guidelines (TLV, 2003), the health economic analysis needs to identify relevant comparison treatments, cost-effectiveness estimates, and a description of analyses leading to these estimates.

Decisions of the Pharmaceutical Benefits Board over which new medications to include in the reimbursement system have to abide by three criteria, namely that the decisions do not discriminate based on age, gender, race, etc. (the human value principle), that patients with more severe diseases are given priority (the need and solidarity principle), and that the cost of using the medicine is reasonable (the cost-effectiveness principle).

2.3 Data and Measurement

2.3.1 Data Sources

We combine register data on healthcare visits and drug purchases with data on socioeconomic characteristics as described below.

Health records. We use register data on inpatient and outpatient care visits from the *National Patient Register* (Patientregistret; Socialstyrelsen, 2022a,

[6]Processing times of pricing and reimbursement decisions for new medications at TLV are by law not allowed to exceed 180 days. The average processing time for new original medications was 129 in 2022 (TLV, 2022, Table 1.2), up from 90 days in 2005 (TLV, 2005, Table 2). The number of decisions increased from 41 in 2005 to 61 in 2022.

2022b) provided by the Swedish National Board of Health and Welfare (Socialstyrelsen). Data on inpatient care are available from 1998 to 2014. Data on outpatient care are available from 2001 to 2014 and are limited to specialized outpatient care visits only. Primary care visits are not included.

For each visit, we observe dates of admission (for all visits) and discharge (for inpatient visits), an identifier for the healthcare provider, the primary and up to 29 secondary International Classification of Diseases 10 (ICD-10) diagnosis codes[7] and up to 30 surgical and non-surgical procedure codes associated with the visit.[8] The data do not contain information about physicians.

Drug purchases. We use individual-level data on drug purchases from outpatient pharmacies from the *National Prescribed Drug Register* (Läkemedelsregistret; Socialstyrelsen, 2022c) maintained by the Swedish National Board of Health and Welfare. These data are available for the period from July 2005 to October 2015. For each purchased product, we observe the date of purchase, date of prescription, the price, and the active substance recorded as a 7-digit Anatomical Therapeutic Chemical (ATC) code. We do not observe unfilled prescriptions.

Socioeconomic characteristics. We obtain information on socioeconomic characteristics from several register data sets provided by Statistics Sweden. From the *Total Population Register* (Statistics Sweden, 2023b), we obtain information on gender, year of birth, whether the individual was born in Sweden or not, as well as identifiers for the individual's parents. From the *Longitudinal Integrated Database for Health Insurance and Labour Market Studies* (LISA;

[7]To account for revisions to ICD-10 codes over time, we collect data from Centers for Disease Control and Prevention (2021), WHO Collaborating Centre for Drug Statistics Methodology (2022a, 2023), and Nordic Casemix Centre (2023a).

[8]Procedures are recorded using the Classification of Healthcare Procedures system (Klassifikation av vårdåtgärder, KVÅ) and are categorized into medical procedures (using the Classification of medical procedures system; Klassifikation av medicinska åtgärder, KMÅ) and surgical procedures (using the Classification of surgical procedures system; Klassifikation av kirurgiska åtgärder, KKÅ). Surgical procedure codes are based on the NOMESCO Classification of Surgical Procedures (NCSP) system used in the Nordic countries. We collect information on revisions to and names of procedure codes from Nordic Casemix Centre (2023b) and Socialstyrelsen (2023a, 2023b).

Statistics Sweden, 2022a), we obtain annual information on earnings and pensions as well as the region of residence and the highest level of completed education for the period 1990–2014. From the *Register-Based Labour Market Statistics* (RAMS; Statistics Sweden, 2023a), we obtain annual information on the individual's salary and self-employment income.

Predicted mortality. To obtain a proxy for the individual's health status at the time of a healthcare visit, we use our register data to train a prediction model for the probability that the individual dies within 12 months of the admission date of the healthcare visit. Appendix 2.A discusses the construction and out-of-sample performance of the model in detail. The prediction model is an ensemble of three prediction models (LASSO, random forest, and gradient-boosted regression tree). Our prediction algorithm (summarized in Appendix Figure 2.1) closely follows Mueller and Spinnewijn (2023) and choices of predictors are similar to Makar et al. (2015) and Einav et al. (2018). The model achieves an out-of-sample performance similar to existing mortality prediction models (Einav et al., 2018; Makar et al., 2015) as measured by the area under the receiver operating classification curve (AUC), see Appendix Figure 2.2.

Measuring socioeconomic status. As the measure of socioeconomic status, we use the individual's income rank. Specifically, we use the percentile rank in the national income distribution measured in year $t-2$, where t is the year of admission of the healthcare visit. By measuring the income rank before the admission year, we avoid the income rank being spuriously correlated with health shocks (cf. Chetty et al., 2016). We define percentile ranks separately by gender and year of birth. Because we can only measure income from 1990 onwards, we use the sum of old-age pensions and labor market earnings over all available ages from age 30 onwards as our income measure. We exclude individuals for whom we observe income for less than three years.

2.3.2 Choosing Novel Medicines and Measuring Adoption

Definition of a novel medicine. We define a novel medicine as a new Anatomical Therapeutic Chemical (ATC) code. The ATC classification system groups active substances according to the organ or system they act on and their therapeutic use. A novel drug has been typically assigned an ATC code by the time it is granted marketing authorization in at least one country.

Two things about the ATC system are worth noting. First, an active substance may have more than one ATC code if the substance has multiple different uses. Second, if a medicine uses a combination of active substances, this combination can have its own ATC code. Therefore, a new ATC code refers either to a new active substance, a new therapeutic use of an existing active substance, or a new combination of existing active substances (see WHO Collaborating Centre for Drug Statistics Methodology, 2022b). Note that this definition of novel medicines excludes e.g. new generic versions of existing branded drugs.

Measuring adoption. A limitation of our data is that we do not observe information on who prescribed a purchased medicine. However, we can still measure the adoption of novel medicines separately by hospital by merging the data on inpatient and outpatient visits with the data on drug purchases based on admission and discharge dates and drug prescription dates.

Specifically, for each visit, we merge all prescription drug purchases such that the prescription date falls between the admission and discharge date and the purchase date is either during the visit or within 7 days of the discharge date. For each drug, we measure the adoption of the drug at a given hospital by measuring the number of purchases by patients who were admitted to the hospital.

To make sure patients in principle have a chance to purchase the drug, we restrict attention to patients who survive for at least 7 days after being discharged from the hospital.

Choosing novel medicines and relevant patients. We collect information on the marketing authorization dates of medicines from the *National Repository for Medicinal Products* (Nationellt produktregister för läkemedel, NPL) of the Swedish Medical Products Agency (Läkemedelsverket, 2023). We define the marketing authorization date of an ATC code as the earliest marketing authorization date for a product with the ATC code as its active substance.[9]

Given our definition of a novel medicine, we focus on novel medicines that are introduced during the coverage of our data (marketing authorization date between January 2005 and January 2014), for which we can follow adoption in a meaningful way (observe purchases associated with in-/outpatient care visits in ≥ 6 months, with at least one month having ≥ 50 purchases associated with in-/outpatient care visits), and for which we can determine the patients for which the medicine is indicated reliably in our data.

To determine the relevant group of patients for a given novel medicine, we first determine the medicine's therapeutic indications by consulting the drug's "European Public Assessment Report" (EPAR) published by the European Medicines Agency as well as information on which patients the drug is subsidized for by the Dental and Pharmaceutical Benefits Agency.

After determining the therapeutic indications for the drug, we then map these descriptions as closely as possible to ICD-10 diagnosis codes and procedure codes included in the Patient Register data (Socialstyrelsen, 2022a, 2022b). Whenever possible, we choose these codes by following sources (reports, websites) published by the National Board of Health and Welfare or published research articles that explicitly mention how specific health conditions are mapped to diagnosis and procedure codes.

Overview of included medicines. Starting from the full list of unique ATC codes included in the *National Prescribed Drug Register* (Socialstyrelsen, 2022c), Appendix Table 2.1 summarizes the steps we use to choose the set of novel medicines included in the analysis as well as the remaining number of novel

[9]Similarly, we collect information on subvention dates of medicines from TLV (2023) and define the subvention date for an ATC code as the earliest subvention date for a product with the ATC code as its active substance.

medicines after each step.

Our analysis includes 58 novel medicines related to 47 patient groups (health conditions or procedures). Appendix Table 2.2 gives a summary of all the included medicines (ATC code, name, product names, etc.) while Appendix Table 2.3 gives a summary of the patient groups included in the analysis and how we define them. Appendix Figure 2.3 shows a breakdown of the novel medicines included in the analysis by the year they received marketing authorization (were approved for sale) in Sweden.[10]

Our set of novel medicines covers diverse health conditions, ranging from cardiovascular diseases (e.g., heart attack, atrial fibrillation, unstable angina) to type-1 and type-2 diabetes, to chronic obstructive pulmonary disease (COPD).[11] Several of the medicines — such as the direct oral anticoagulants apixaban, rivaroxaban, and dabigatran — have also been recognized as important as they appear on the List of Essential Medicines maintained by the World Health Organization (World Health Organization [WHO], 2024).

2.4 Results

2.4.1 Overall Adoption Patterns

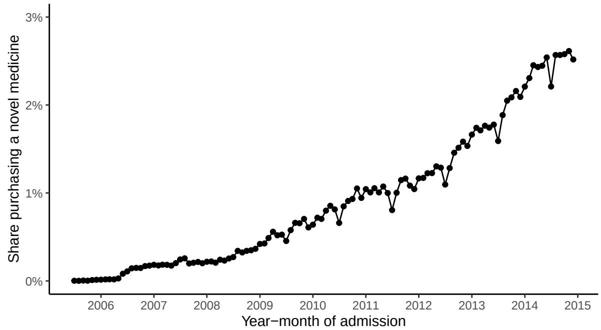
Figure 2.1A shows the overall adoption rate of the 58 novel medicines included in our analysis by month of hospital admission, pooling all the patient groups. The share of patients purchasing a novel medicine increases steadily over the period for which we have data, reaching a share of roughly 2.6 percent at the end of our data coverage.

The adoption rate picks up over time, which not only reflects the fact that more novel medicines are introduced over time but also the fact that some of the more prominent drugs included in the analysis were introduced around the

[10] Note that there is typically a gap of roughly 5-7 months between the month of marketing authorization and when we start to see the drug being adopted, which is typically after the drug receives subvention from the Dental and Pharmaceutical Benefits Agency.

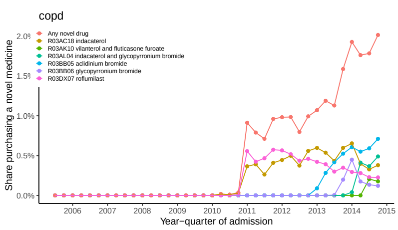
[11] Note that the share of patients we drop with when restricting to patients who survive for at least 7 days after hospital discharge varies by novel medicine and patient group, but typically we exclude at most 1–2 percent of patients. However, for cancers, cardiovascular conditions, and COPD the share is higher, roughly varying between 5-10 percent.

Panel A: Overall Adoption Rate

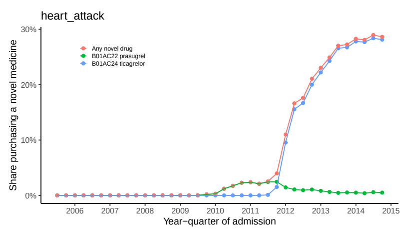


Panel B: Adoption Rate in Selected Patient Groups

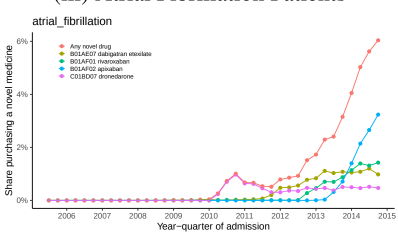
(i) COPD Patients



(ii) Heart Attack Patients



(iii) Atrial Fibrillation Patients



(iv) Type-2 Diabetes Patients

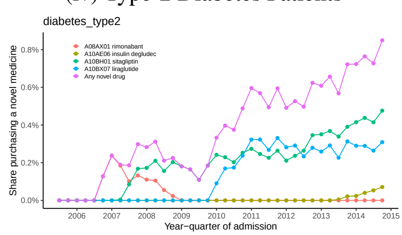


Figure 2.1: Overall Adoption Patterns of Novel Medicines

Notes. This figure shows the overall adoption rate of the novel medicines included in our analysis, see Appendix Table 2.2. Panel A shows the adoption rate of all novel medicines, separately by year-month of hospital admission. We define adoption as the patient purchasing at least one novel drug with a prescription date between the admission and discharge dates of the healthcare visit within 7 days of discharge. We only keep individuals who survive for at least 7 days after the discharge date. Panel B shows the adoption rate of novel medicines separately for four patient groups (see Appendix Table 2.3), separately by year-quarter of hospital admission.

years 2009–2011, such e.g. the antiplatelet medicine ticagrelor, and the direct oral anticoagulants apixaban, rivaroxaban, and dabigatran (see Appendix Table 2.2).

However, this overall pattern masks substantial variation in the adoption rate of novel medicines across different patient groups. While there are patient groups, such as COPD patients (Figure 2.1B, panel i), for which the adoption rate of novel medicines reaches a level similar to the overall adoption rate, there are patient groups for which the adoption rate is considerably higher. For example, panel (ii) of Figure 2.1B shows that for heart attack patients, one of the new drugs included in our data (ticagrelor) saw widespread adoption during our analysis period. This medicine is the one for which we see the highest adoption rate during our study period, and we will study it in more detail in Section 2.5.

Another large patient group for which we see a considerable adoption rate of new drugs is atrial fibrillation (Figure 2.1B, panel iii). For a long time, the typical treatment, especially for those with a high stroke risk, has been the anticoagulant drug warfarin. However, during our analysis period, a new class of drugs called "direct oral anticoagulants", in particular apixaban, overtook warfarin as the preferred choice of treatment for these patients (A. Chen et al., 2020; Joglar et al., 2024).

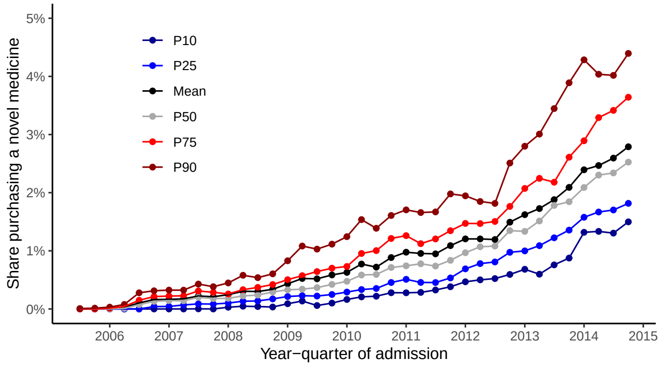
In contrast, we see relatively low adoption of novel medicines in another large patient group, namely type-2 diabetes patients (see Figure 2.1B, panel (iv)). This might in part be driven by the fact that TLV subsidizes the new drugs only for patients who have first tried other drugs, such as metformin or insulin, or patients for whom these other drugs are not suitable.

2.4.2 Heterogeneity in Adoption Patterns

We see large heterogeneity in the adoption of novel medicines across hospitals.

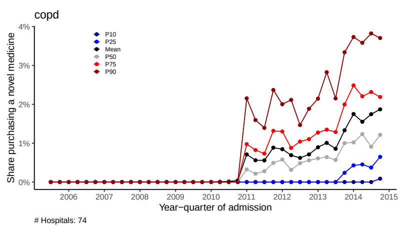
Figure 2.2A shows the mean and percentiles ranging between the 10th and the 90th percentile of hospital-specific adoption rates of novel medicines drugs, separately by year-quarter of the hospital visit. We focus on hospitals that we

Panel A: Overall Adoption Rate

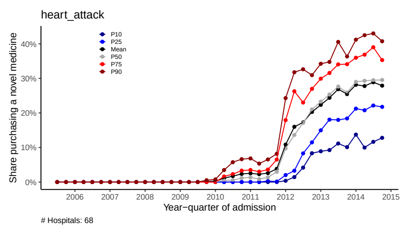


Panel B: Adoption Rate in Selected Patient Groups

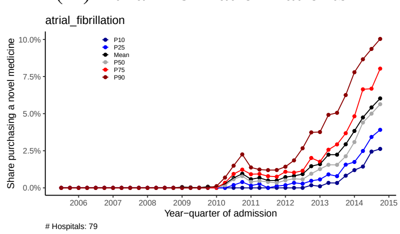
(i) COPD Patients



(ii) Heart Attack Patients



(iii) Atrial Fibrillation Patients



(iv) Type-2 Diabetes Patients

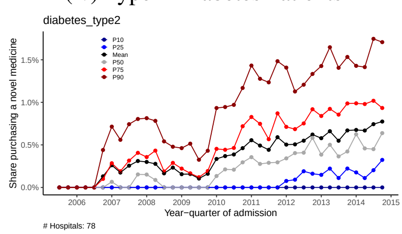


Figure 2.2: Heterogeneity in the Adoption Rate of Novel Medicines Across Hospitals

Notes. This figure shows the distribution of hospital-specific adoption rates of the novel medicines included in our analysis, see Appendix Table 2.2. Panel A shows the mean adoption rate of all novel medicines as well as percentiles of adoption rates, ranging from the 10th to 90th percentile, separately by year-quarter of hospital admission. We only include hospitals that we observe in the data throughout our analysis period and that have at least 100 visits related to the health conditions associated with our set of novel drugs, leaving us with 92 hospitals. Panel B shows the same moments of the hospital-specific adoption rates of novel medicines separately for four patient groups (see Appendix Table 2.3), separately by year-quarter of hospital admission. In each figure in Panel B, we only include hospitals that appear in the data in the data throughout our analysis period and have at least 10 visits related to the patient group in each quarter.

observe in the data throughout our study period and that have at least 100 visits related to the health conditions associated with our set of novel drugs. This gives us 92 hospitals.

We see large variation of adoption patterns between hospitals. For example, in the last quarter of our study period, a hospital at the 90th percentile had an adoption rate of novel medicines of roughly 4.5 percent, which is nearly twice as large as the median hospital and roughly three times the adoption rate of a hospital at the 10th percentile. Rather than merely reflecting heterogeneity in the patient mix, we see in Figure 2.2B that similar degrees of variation in adoption patterns exist when looking separately at the same patient groups as in Figure 2.1B.

2.4.3 Correlates of Novel Drug Adoption

Next, in Table 2.1 we look at to what extent the adoption of novel medicines is correlated with patient characteristics. We ask whether these matter even after accounting for hospital fixed effects as well as patient group fixed effects.

Two interesting things are worth noting in Table 2.1. First, the coefficient on the income rank is positive and significant once hospital fixed effects are included (columns 2–3). This suggests a positive income gradient in the adoption of novel medicines, implying that moving from the bottom to the top percentile of the income distribution increases the adoption rate of novel medicines by 0.1 percentage points, or 9.7 percent relative to the mean.

Table 2.1 also indicates a positive association between the adoption of novel medicines and whether the patient is a medical doctor, as well as whether s/he has a doctor in the family.[12] These findings are interesting given recent work using Swedish data documenting that having a doctor in the family is related to higher investments in preventive healthcare (Y. Chen et al., 2022) and a lower adherence rate to medical guidelines (Finkelstein et al., 2022).

[12] We define a person as having a doctor in the family if s/he has a parent, child, or sibling who is a medical doctor.

Table 2.1: Correlates of the Adoption of Novel Medicines

Dependent Variable:	Purchases a Novel Medicine (mean = 0.01028)		
Model:	(1)	(2)	(3)
Variables			
Female	-0.0010*** (0.0002)	-0.001*** (0.0002)	9.5×10^{-5} (7.28×10^{-5})
Income Rank	-3.83×10^{-5} (0.0005)	0.001*** (0.0003)	0.001*** (0.0001)
Born Abroad	0.0003 (0.0003)	0.0005*** (0.0001)	0.0002** (9.06×10^{-5})
Treated at University Hospital	-0.004*** (0.001)		
Medical Doctor	0.0008** (0.0004)	0.0009** (0.0004)	0.001*** (0.0003)
Medical Doctor in the Family	-0.0002 (0.0002)	0.0002 (0.0002)	0.0003** (0.0002)
Fixed-effects			
Age FEs	Yes	Yes	Yes
Calendar Year-Quarter FEs	Yes	Yes	Yes
Hospital FEs		Yes	Yes
Patient Group FEs			Yes
Fit statistics			
Observations	29,635,227	29,635,227	29,635,227
R^2	0.00839	0.02504	0.06394

Clustered (Hospital FEs) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes. This table presents estimated coefficients from regressions of an indicator for purchasing a novel medicine drug against different sets of covariates along with standard errors clustered at the hospital level. The sample pools all inpatient and outpatient care visits with an admission date on or after July 1, 2005 related to the patient groups associated with the novel medicines included in the analysis, see Appendix Table 2.3. The unit of observation is an inpatient or outpatient care visit. We exclude individuals who die within 7 days of the discharge date of the healthcare visit, see Section 2.3 for details. We define a person as having a medical doctor in the family if s/he has a parent, child, or sibling who is a medical doctor. We define a person as being treated at a university hospital if his/her healthcare visit is at one of the following hospitals: Karolinska universitetssjukhuset Solna, Karolinska universitetssjukhuset Huddinge, Universitetssjukhuset i Linköping, Universitetssjukhuset Örebro, Sahlgrenska universitetssjukhuset, Skånes universitetssjukhus Malmö, Skånes universitetssjukhus Lund, Skånes universitetssjukhus Lund, Norrlands universitetssjukhus, Akademiska sjukhuset.

2.4.4 Adoption Across Socioeconomic Groups

Figure 2.3 focuses on the association between the patient's income rank and the adoption of novel medicines documented in Table 2.1. Panel (i) of Figure 2.3 shows that the slope of the income gradient remains roughly similar when excluding or including controls for age, gender, year-quarter of hospital admission, and hospital fixed effects. With controls, the relationship between income rank and the adoption rate of new drugs is also roughly linear.

Panel (ii) of Figure 2.3 shows the slopes of the income gradients separately for each patient group included in our analysis.[13] Two patterns stand out from the figure. First, we see that the positive association between income rank and the adoption of novel medicines is not driven by a particular set of health conditions, but instead can be seen across diverse conditions ranging from cardiovascular conditions (heart attacks, atrial fibrillation, deep vein thrombosis, etc.) to lung diseases (asthma, COPD) to ADHD. However, there are some exceptions among prominent health conditions, such as for type-1 and type-2 diabetes for which we do not find evidence for an income gradient.

Second, we see that the largest income gradients are found for cardiovascular conditions. Given that cardiovascular conditions have been identified as one of the drivers behind increasing disparities in life expectancy between socioeconomic groups in Sweden and other Nordic countries (Dahl et al., 2021; Fors et al., 2021; Hederos et al., 2018; Kinge et al., 2019; Åström, Franks, & Sundquist, 2018; Åström, Sundquist, & Sundquist, 2018), we next zoom in on this patient group and turn to analyze the potential consequences of differences in adoption rates of novel medicines for health disparities.

2.5 Potential Consequences: A Case Study

In this section, we assess the potential consequences of the observed differences in the adoption of novel medicines between hospitals and socioeconomic groups. We present a case study that focuses on *ticagrelor*, the novel an-

[13] Appendix Figure 2.4 shows the slopes of the income gradients separately for each drug.

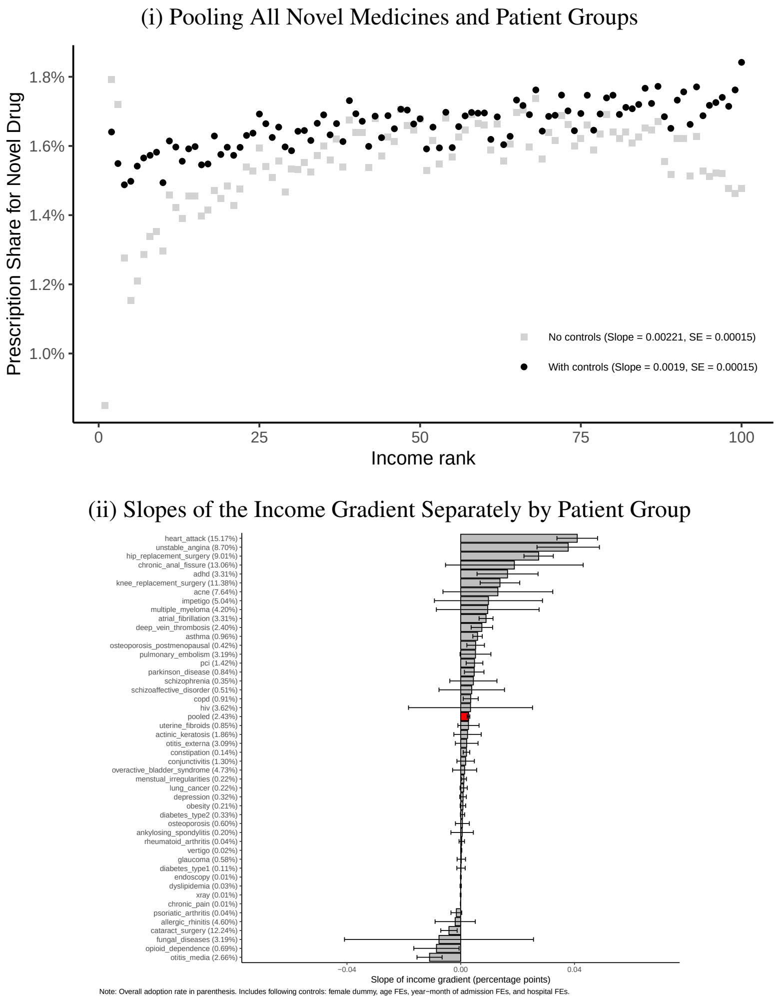


Figure 2.3: Adoption Rate of Novel Medicines by Income Rank

Notes. Panel (i) shows estimates for the adoption rate of novel medicines separately by percentile rank in the income distribution. The estimates are based on linear regressions of an indicator for purchasing a novel medicine against income percentile rank dummies and other covariates. The gray square series shows estimates without additional controls, while the black square series includes a female dummy, age FEs, admission year-month FEs, and hospital FEs. The reported slopes of the income gradient are based on the estimated coefficients for the income percentile rank from linear regressions of an indicator for purchasing a novel medicine against the same sets of covariates as above. Panel (ii) shows estimated slope coefficients for the income gradient separately for each of the patient groups associated with the novel medicines, see Appendix Table 2.3. The figure also shows the 95 percent pointwise confidence intervals for the slope estimates, using standard errors clustered at the hospital level. The percentage share next to the name of the patient group gives the mean adoption rate of novel medicines in the patient group. The red bar highlights the estimated slope when pooling all patient groups.

tiplatelet medicine for which we saw both the highest rate of adoption and the largest differences in adoption rates between socioeconomic groups in Section 3.5 and Appendix Figure 2.4.

We start by providing some context in Section 2.5.1 and then ask two questions. First, in Section 2.5.2 we estimate how many life-years would have been saved had all hospitals adopted the drug at the rate of the fastest-adopting hospital. Second, in Section 2.5.3 we estimate how much the gap in survival rates between low-income and high-income first-time heart attack patients would shrink if low-income individuals adopted the drug at the same rate as high-income individuals.

Due to data limitations, we focus on the first 12 months following hospital discharge after a first heart attack. Our analyses are subject to several caveats that we highlight in Section 2.5.4. We therefore view our findings merely as suggestive.

2.5.1 Context

Heart attacks. Heart attacks are among the most common causes of death for both men and women in Sweden as well as most other developed countries. Cardiovascular diseases have been identified as one of the main drivers of the increase in the life expectancy gap between rich and poor in Sweden (Fors et al., 2021; Hederos et al., 2018; Åström, Franks, & Sundquist, 2018; Åström, Sundquist, & Sundquist, 2018) as well as other countries such as Denmark (Dahl et al., 2021) and Norway (Kinge et al., 2019).

About Ticagrelor. Ticagrelor (ATC B01AC24) is an antiplatelet medicine that prevents strokes and heart attacks by inhibiting the formation of blood clots. During the period we study, the medicine was sold in Sweden by AstraZeneca under the brand name Brilique.

In comparison to the previous standard of care clopidogrel (ATC B01AC04), ticagrelor is direct-acting, reversible, and has a stronger, more consistent, and faster effect of preventing blood platelet formation. The main drawback reported in the pivotal clinical trial (Wallentin et al., 2009) was a higher bleeding

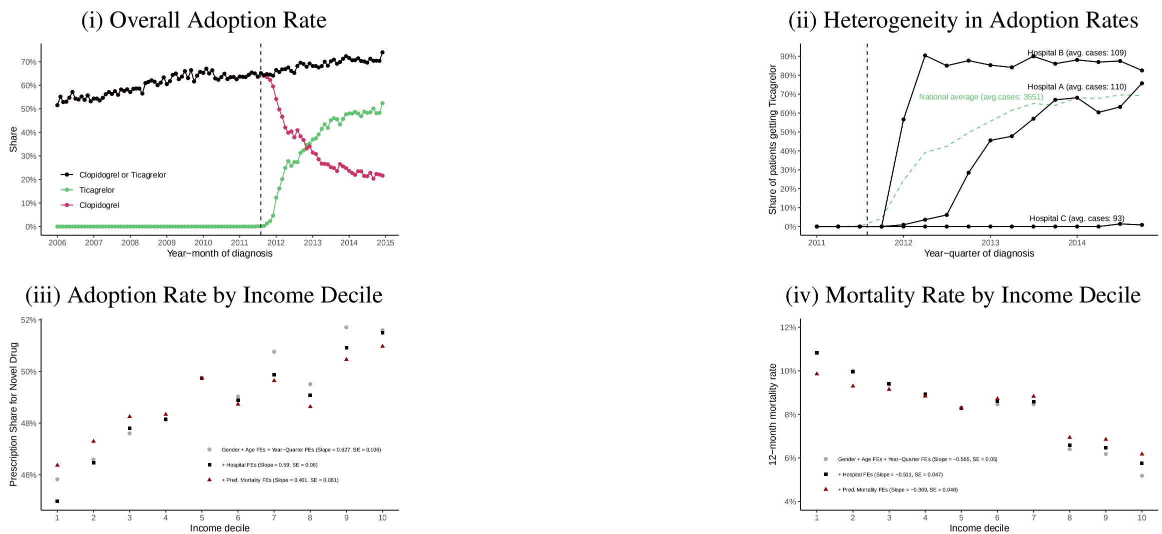


Figure 2.4: Case Study Using a Novel Antiplatelet Medicine (Ticagrelor)

Notes. This figure provides descriptive evidence on the adoption patterns for ticagrelor, the case study medicine in Section 2.5. Panel (i) shows the share of first-time heart attack patients (see Section 2.5.2) who purchase up ticagrelor or clopidogrel (black series), ticagrelor (green series), or clopidogrel (pink series) within 7 days of hospital discharge, separately by month of hospital admission. Panel (ii) compares the national adoption rate of ticagrelor relative to clopidogrel (green dashed series) with the adoption rates for three different hospitals. Numbers in parentheses next to the labels for each series indicate the average number (rounded to the nearest integer) of first-time heart attack patients per month. Panel (iii) shows estimates for the adoption rate of ticagrelor separately by decile in the income distribution. The estimates are based on linear regressions of an indicator for purchasing ticagrelor against income decile dummies and other covariates. The gray circle series includes fixed effects for age and year-quarter of hospital admission, the black square series adds hospital fixed effects, and the red triangle series further adds dummies for tercile of predicted probability of death within 12 months of hospital admission. The reported slopes are based on the estimated coefficients for income decile from linear regressions of an indicator for purchasing ticagrelor against the same sets of covariates as above. The standard errors for the slope estimates clustered at the hospital level are given in parentheses. Panel (iv) shows the same types of estimates as in Panel (iii), but where the outcome is now an indicator for dying within 12 months of hospital admission.

risk. Ticagrelor is currently recommended over clopidogrel for treatment after a heart attack by the European Society of Cardiology (Byrne et al., 2024) and the American College of Cardiology (Lawton et al., 2022).

The Dental and Pharmaceutical Benefits Agency added Brilique to the high-cost protection scheme in June 2011. They motivated this decision by the higher 12-month survival rate of patients who received ticagrelor relative to patients receiving clopidogrel in the pivotal Phase-III clinical trial (Wallentin et al., 2009).

2.5.2 Aggregate Consequences

We focus our analysis on individuals aged 18 or older with a first observed inpatient care visit with heart attack (myocardial infarction, ICD-10 I21) as the main diagnosis who purchase either Ticagrelor or Clopidogrel within 7 days of hospital discharge, mimicking the approach of the clinical trial conducted by Wallentin et al. (2009).[14] This allows us to assess the potential impact of the observed differences in ticagrelor adoption on health outcomes, with the clinical trial providing us with a direct measure of the benefits of ticagrelor relative clopidogrel in reducing mortality and other adverse outcomes after a heart attack.

We supplement the clinical trial outcomes using an event study design to estimate the effect of receiving ticagrelor over clopidogrel on drug purchases and healthcare use (see Appendix Figure 2.5). Specifically, we examine how the total costs of drug purchases and the number of days in inpatient and outpatient care change over time for individuals who receive ticagrelor compared to those who receive clopidogrel after a first heart attack. This allows us to capture the potential downstream health and cost impacts of the difference between socioeconomic groups in adoption rates of the medicine. The details on the event study are in Appendix 2.B.

We present the average benefits and costs of using ticagrelor relative to clopidogrel in Table 2.2.

[14] Following Wallentin et al. (2009), we exclude individuals using anticoagulants.

Table 2.2: Cost and Benefits of Using Ticagrelor vs. Clopidogrel – 1-year Horizon

Measure	Value	Monetary Value
Medication Cost	-4,200 SEK	-4,200 SEK
Mortality	1.4%	+16,800 SEK
Inpatient Days	-0.21	+2,368 SEK
Sum of Benefits		+14,968 SEK

While ticagrelor is more expensive than clopidogrel, with no other cost savings from prescribing clopidogrel, individuals prescribed ticagrelor spend more on medications on average. However, being prescribed ticagrelor has benefits – it reduces the 1-year mortality rate by 1.4 percentage points, which we value at 1.4 percent of the value of a year of life. This is a conservative estimate, as individuals may live for many years beyond the first year. Furthermore, in Appendix 2.B we show that receiving ticagrelor over clopidogrel reduces inpatient nights by 0.21, which we have also incorporated into our cost-benefit calculation. Converting these benefits into monetary values[15] translates into cumulative benefits of 14,968 SEK per person.

Next, we conduct a counterfactual analysis to determine the potential benefits if all hospitals were to adopt ticagrelor at the same rate as the fastest-adopting hospital. In this leading hospital, 91.2% of heart attack patients receive ticagrelor. If other hospitals matched this prescription rate, an additional 14,382 individuals would receive ticagrelor, resulting in benefits totaling 215,269,776 SEK.

2.5.3 Consequences on Health Disparities

Next, we consider the potential consequences for health disparities of closing the income gradient in the adoption rate of ticagrelor relative to clopidogrel. By how much would the gap in 12-month mortality rates between the top and bottom income deciles shrink?

Figure 2.4(iv) shows that the 12-month mortality rates, adjusted by con-

[15] Sources for the valuation of the different components are given in Appendix 2.C.

trolling for gender, age, and year-quarter of heart attack, are 10.8% for the bottom and 5.2% for the top income decile, respectively, a gap of 5.6 percentage points.[16] At the same time, Figure 2.4(iii) shows that the similarly-adjusted adoption rate of ticagrelor is 5.8 percentage points higher in the top income decile (51.6% vs. 45.8%).

Using the estimated effect of receiving ticagrelor over clopidogrel from Wallentin et al. (2009), we find that equalizing the adoption rates in the top and bottom deciles would shrink the adjusted gap in survival rates by 0.07 percentage points, or by 1.2 percent.

2.5.4 Caveats

Our analysis in this section is subject to two important caveats and the findings should therefore be interpreted with caution. First, we assume that the estimate of the medicine's effect on 12-month mortality reported in the clinical trial (Wallentin et al., 2009) is valid for the group of patients who were not prescribed ticagrelor. Although Wallentin et al. (2009) do not find significant heterogeneity in the effectiveness of the medicine across subgroups, the effects of the medicine may differ from those in the clinical trial for example if patients do not adhere as well to the treatment protocol as in the trial.[17]

Second, we have only focused on one determinant of the health-income gradient, namely the gradient in health outcomes (here, survival rate) conditional on a given health shock (here, heart attack). A comprehensive analysis of the effects of differences in the adoption of medical innovations on health disparities is very challenging as it needs to account for differences in the incidence of different health shocks between socioeconomic groups as well as

[16] Note that it is important to adjust the income gradient in mortality rates by controlling for gender and age fixed effects. As shown Appendix Figure 2.6, the *unadjusted* income gradient in mortality rates is positive, but this is because individuals in higher income deciles experience their first heart attacks at significantly older ages (see panel iv).

[17] For example, using the same Swedish administrative data as we do, Finkelstein et al. (2022, Appendix Table A3) report an adherence rate of only 53 percent to the guideline recommending heart attack patients to take statins for 12-18 months after the diagnosis. For comparison, the adherence rate to the treatment (ticagrelor) and control (clopidogrel) protocols reported by Wallentin et al. (2009) is 83 percent.

the potential spillover effects of reducing differences in morbidity and mortality from one health condition on other health conditions (cf. Dow et al., 1999; Murphy & Topel, 2006). Such a comprehensive analysis is beyond the scope of this paper.

We view our results merely as suggesting that differences in the adoption of medical innovations may contribute to health disparities, even in countries with extensive and highly subsidized healthcare systems such as Sweden.

2.6 Correlates of Hospital Level Differences

In this section we explore two possible sources for the differences in adoption rates of novel medicines across hospitals: differences in management quality and differences in medical guidelines.

2.6.1 Management Quality of Hospitals

The first potential source of the differences in adoption rates is differences in the management quality of hospitals. To measure the quality of management practices, we use the management quality scores for Swedish hospitals from the World Management Survey (Bloom et al., 2014, 2020, 2021). These survey measures the adoption of best practices over operations, monitoring, targets, and people management. These management quality scores range from 1 to 5, with higher scores indicating better management practices. The survey was conducted in 2009 and the data include 56 hospitals.

Figure 2.5 shows no significant correlation between the management quality scores and the overall adoption rate of novel medicines. Figure 2.6 shows a positive but statistically insignificant correlation between the management quality score and the adoption rate of novel medicines for heart attack patients (ticagrelor and prasurgel, cf. Figure 2.1 and Section 2.5).

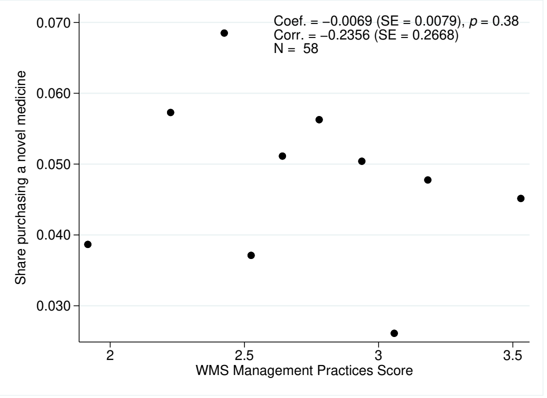


Figure 2.5: Management Scores and the Adoption Rates of Novel Medicines

Notes. This figure shows a binned scatterplot of the relationship between the prescription rate of novel medicines and hospital management practice scores from Bloom et al. (2020), using data for the 56 hospitals for which we have management quality scores. We group the hospitals to 10 bins due to data confidentiality reasons. We also report the slope from a linear regression and the bivariate correlation coefficient between the management scores and the adoption rate of novel medicines. Robust standard errors for both coefficients are shown in parentheses.

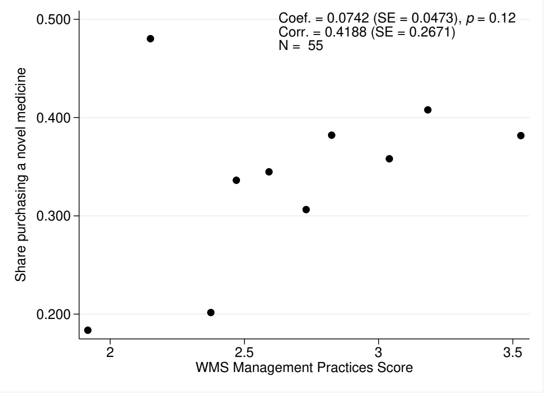


Figure 2.6: Management Scores and the Adoption Rates of Novel Medicines for Heart Attack Patients

Notes. This figure shows a binned scatterplot of the relationship between the prescription rate of novel medicines for heart attack patients and hospital management practice scores from Bloom et al. (2020), using data for the 56 hospitals for which we have management quality scores. We group the hospitals to 10 bins due to data confidentiality reasons. We also report the slope from a linear regression and the bivariate correlation coefficient between the management scores and the adoption rate of novel medicines. Robust standard errors for both coefficients are shown in parentheses.

2.6.2 Medical Guidelines

Sweden is divided into 21 different healthcare regions, each of which independently develops and publishes their own clinical guidelines on the recommended use of various medicines for different medical conditions. The level of detail in the lists, the time period covered, and the frequency with which lists are updated vary by region. Apart from minor corrections such as correcting factual or typographical errors, the lists are not substantially revised until the publication of the next edition of the list. The only cases where a medication might be removed from the list before a revision are when the medicine is discontinued (withdrawn from the market) and when the medicine is out of stock for an extended time period.

We hand collected the lists of recommended medicines from 15 out of 21 healthcare regions in Sweden.[18] Figure 2.7 presents estimated coefficients from an event study of the inclusion of ticagrelor into the regional guidelines. The event is the first mention of ticagrelor in the respective regional guidelines, while the outcome is the prescription share of ticagrelor among the hospitals within the region. Figure 2.7 shows that updating guidelines does not lead to hospitals increasing their prescription share.

The lack of an effect can have multiple reasons. For one, doctors might rely on different guidelines, such as the guidelines of the National Board of Health and Welfare, the American Heart Association, or the European Society of Cardiology. In particular, if the recommendation precedes inclusion of ticagrelor in the regional guidelines, the effect of including the medicine in regional guidelines is muted.

2.7 Concluding Remarks

We study the consequences of differences in technology adoption patterns in healthcare using Swedish administrative data. For a set of 58 novel medicines related to a diverse set of 47 health conditions, we find large variation in the

[18]The remaining 6 regions did not respond to our request or had a continuously updating system that did not provide the older records we needed.

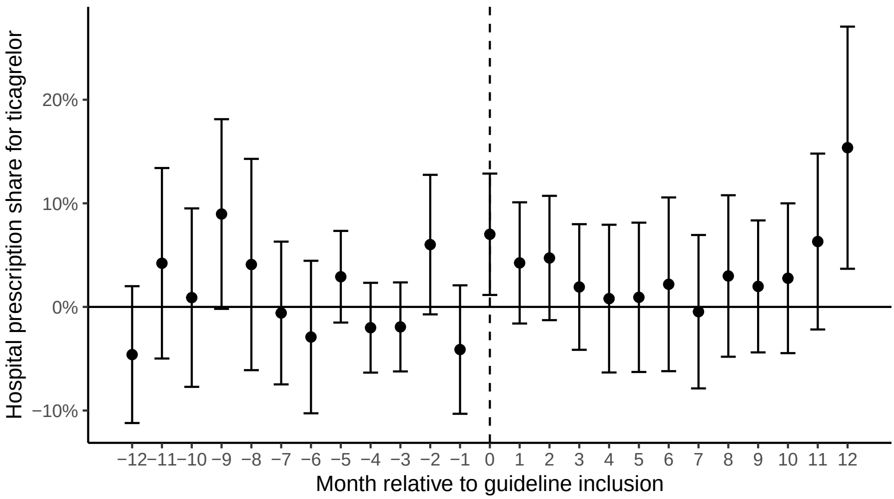


Figure 2.7: Event Study Estimates of the Introduction of Ticagrelor in Regional Guidelines

Notes. This figure shows the coefficients from an event study of the introduction of ticagrelor in regional medical guidelines. The guidelines are updated at $t = 0$. The outcome is the prescription share of ticagrelor averaged across all hospitals in the respective healthcare region. Appendix Figure 2.7 shows the prescription shares for ticagrelor separately for each region, along with when the drug first appears in the region's guidelines.

adoption patterns of the novel medicines across hospitals and socioeconomic groups. In particular, we document a positive association between a patient's income rank and the probability of getting a novel medicine for health conditions ranging from cardiovascular diseases to lung diseases to ADHD. Finally, we use a novel antiplatelet drug and first-time heart attack patients in a case study to argue that differences in the adoption rates of novel medicines between socioeconomic groups can contribute to health disparities.

We close by highlighting three important avenues for future work. First, it would be interesting to study the joint adoption patterns of novel drugs at the hospital level. For example, is there a positive correlation between the adoption patterns of different drugs, or are adoption patterns driven by comparative advantage (as argued by Chandra & Staiger, 2007, 2020, in the context of heart attack treatments)? It would also be interesting to expand the analysis to also cover new medical procedures as well, such as laparoscopic surgeries.

Second, it would be important to analyze the fiscal effects of medical innovations since such effects are not typically accounted for when authorities decide whether to subsidize novel drugs. Some papers, such as Jeon and Pohl (2019) and Lazuka (2022), have attempted to estimate the effects of medical innovation on labor market outcomes using indirect measures for the state of medical knowledge (such as the number of new molecular entities), but such intention-to-treat estimates are difficult to interpret.

Third, it is important to distinguish between the relative importance of demand, supply, and institutional factors in explaining the variation in the adoption of novel medicines. To distinguish physician-specific factors, such as practice style (e.g., Molitor, 2018) or incorrect beliefs (e.g., Cutler et al., 2019), from patient-specific and institutional factors, it would be important to add information on which physicians treat which patients.[19] Using such matched physician-patient data, it could be possible to use a "mover design" that exploits physician moves between hospitals to distinguish physician-specific factors from other factors, along the lines of recent work by Molitor (2018) and Badinski et al. (2023).

[19]For Sweden, such data exist in various National Quality Registers, such as the *Swedeheart* register for cardiology (Jernberg et al., 2010). These registers are maintained by several organizations of healthcare professionals and patient representatives.

References

Agha, L., & Molitor, D. (2018). The Local Influence of Pioneer Investigators on Technology Adoption: Evidence from New Cancer Drugs. *Review of Economics and Statistics*, *100*(1), 29–44.

Anell, A., Glenngård, A. H., & Merkur, S. (2012). Sweden: Health System Review. *Health Systems in Transition*, *14*(5), 1–159.

Arrow, K. J., Bilir, L. K., & Sorensen, A. (2020). The Impact of Information Technology on the Diffusion of New Pharmaceuticals. *American Economic Journal: Applied Economics*, *12*(3), 1–39.

Badinski, I., Finkelstein, A., Gentzknow, M., Hull, P., & Williams, H. (2023). *Geographic Variation in Healthcare Utilization: The Role of Physicians* [Working paper, Brown University].

Banks, J., Currie, J., Krutikova, S., Salvanes, K. G., & Schwandt, H. (2021). The Evolution of Mortality Inequality in 11 OECD Countries: Introduction. *Fiscal Studies*, *42*(1), 9–23.

Björvang, C., Pontén, J., Rönnholm, G., & Skiöld, P. (2023). *PPRI Pharma Profile 2023*. Dental; Pharmaceutical Benefits Agency (TLV).

Bloom, N., Lemos, R., Sadun, R., Scur, D., & Van Reenen, J. (2014). JEEA-FBBVA Lecture 2013: The New Empirical Economics of Management. *Journal of the European Economic Association*, *12*(4), 835–876.

Bloom, N., Lemos, R., Sadun, R., Scur, D., & Van Reenen, J. (2021). *World Management Survey – Manufacturing (V1)* [Harvard Dataverse]. Retrieved July 31, 2024, from https://doi.org/10.7910/DVN/OY6CBK

Bloom, N., Lemos, R., Sadun, R., & Van Reenen, J. (2020). Healthy Business? Managerial Education and Management in Health Care. *Review of Economics and Statistics*, *102*(3), 506–517.

Bosworth, B. (2018). Increasing Disparities in Mortality by Socioeconomic Status. *Annual Review of Public Health*, *39*(1), 237–251.

Byrne, R. A., Rossello, X., Coughlan, J., Barbato, E., Berry, C., Chieffo, A., Claeys, M. J., Dan, G.-A., Dweck, M. R., Galbraith, M., et al. (2024). 2023 ESC Guidelines for the Management of Acute Coronary Syndromes: Developed by the Task Force on the Management of Acute Coronary Syndromes of the European Society of Cardiology (ESC). *European Heart Journal: Acute Cardiovascular Care*, *13*(1), 55–161.

Centers for Disease Control and Prevention. (2021). *ICD-10-CM Codes in 2022*. Retrieved December 14, 2021, from https://ftp.cdc.gov/pub/Health_Statistics/NCHS/Publications/ICD10CM/2022/icd10cm_order_2022.txt

Chandra, A., & Staiger, D. O. (2007). Productivity Spillovers in Health Care: Evidence from the Treatment of Heart Attacks. *Journal of Political Economy*, *115*(1), 103–140.

Chandra, A., & Staiger, D. O. (2020). Identifying Sources of Inefficiency in Healthcare. *Quarterly Journal of Economics*, *135*(2), 785–843.

Chen, A., Stecker, E., & A. Warden, B. (2020). Direct Oral Anticoagulant Use: A Practical Guide to Common Clinical Challenges. *Journal of the American Heart Association*, *9*(13), e017559.

Chen, Y., Persson, P., & Polyakova, M. (2022). The Roots of Health Inequality and the Value of Intrafamily Expertise. *American Economic Journal: Applied Economics*, *14*(3), 185–223.

Chetty, R., Stepner, M., Abraham, S., Lin, S., Scuderi, B., Turner, N., Bergeron, A., & Cutler, D. (2016). The Association Between Income and Life Expectancy in the United States, 2001-2014. *Journal of the American Medical Association*, *315*(16), 1750–1766.

Coleman, J., Katz, E., & Menzel, H. (1957). The Diffusion of an Innovation among Physicians. *Sociometry*, *20*(4), 253–270.

Comin, D., & Mestieri, M. (2014). Technology Diffusion: Measurement, Causes, and Consequences. In P. Aghion & S. N. Durlauf (Eds.), *Handbook of Economic Growth* (pp. 565–622, Vol. 2). Elsevier.

Cutler, D. M., Skinner, J. S., Stern, A. D., & Wennberg, D. (2019). Physician Beliefs and Patient Preferences: A New Look at Regional Variation in Health Care Spending. *American Economic Journal: Economic Policy*, *11*(1), 192–221.

Dahl, G. B., Kreiner, C. T., Nielsen, T. H., & Serena, B. L. (2021). Understanding the Rise in Life Expectancy Inequality. *Review of Economics and Statistics*, forthcoming.

Dahlstrand, A. (2023). *Defying Distance? The Provision of Services in the Digital Age* [Working paper, London School of Economics].

Dow, W. H., Philipson, T. J., & Sala-i-Martin, X. (1999). Longevity Complementarities Under Competing Risks. *American Economic Review*, *89*(5), 1358–1371.

Einav, L., Finkelstein, A., Mullainathan, S., & Obermeyer, Z. (2018). Predictive Modeling of U.S. Health Care Spending in Late Life. *Science*, *360*(6396), 1462–1465.

European Medicines Agency. (2013). *Annual Report 2013 – The European Medicines Agency's Contribution to Science, Medicines, and Health in 2013*. Retrieved June 1, 2024, from https://www.ema.europa.eu/en/documents/annual-report/annual-report-european-medicines-agency-2013_en.pdf%5C#page=42

European Medicines Agency. (2017). *Annual Report 2017 – The European Medicines Agency's Contribution to Science, Medicines, and Health in 2017*. Retrieved June 1, 2024, from https://www.ema.europa.eu/en/documents/annual-report/2017-annual-report-european-medicines-agency_en.pdf%5C#page=63

European Medicines Agency. (2021). *Annual Report 2021 – The European Medicines Agency's Contribution to Science, Medicines, and Health in 2021*. Retrieved June 1, 2024, from https://www.ema.europa.eu/en/documents/annual-report/2021-annual-report-european-medicines-agency_en.pdf%5C#page=95

Finkelstein, A., Gentzkow, M., & Williams, H. (2016). Sources of Geographic Variation in Health Care: Evidence from Patient Migration. *Quarterly Journal of Economics*, *131*(4), 1681–1726.

Finkelstein, A., Gentzkow, M., & Williams, H. (2021). Place-Based Drivers of Mortality: Evidence from Migration. *American Economic Review*, *111*(8), 2697–2735.

Finkelstein, A., Persson, P., Polyakova, M., & Shapiro, J. M. (2022). A Taste of Their Own Medicine: Guideline Adherence and Access to Expertise. *American Economic Review: Insights*, *4*(4), 507–526.

Fors, S., Wastesson, J. W., & Morin, L. (2021). Growing Income-Based Inequalities in Old-Age Life Expectancy in Sweden, 2006–2015. *Demography*, *58*(6), 2117–2138.

Foster, A. D., & Rosenzweig, M. R. (2010). Microeconomics of Technology Adoption. *Annual Review of Economics*, *2*(1), 395–424.

Griliches, Z. (1957). Hybrid Corn: An Exploration in the Economics of Technological Change. *Econometrica*, *25*(4), 501–522.

Hederos, K., Jäntti, M., Lindahl, L., & Torssander, J. (2018). Trends in Life Expectancy by Income and the Role of Specific Causes of Death. *Economica*, *85*(339), 606–625.

Jencks, S. F., Huff, E. D., & Cuerdon, T. (2003). Change in the Quality of Care Delivered to Medicare Beneficiaries, 1998-1999 to 2000-2001. *Journal of the American Medical Association*, *289*(3), 305–312.

Jeon, S.-H., & Pohl, R. V. (2019). Medical Innovation, Education, and Labor Market Outcomes of Cancer Patients. *Journal of Health Economics*, *68*, 102228.

Jernberg, T., Attebring, M. F., Hambraeus, K., Ivert, T., James, S., Jeppsson, A., Lagerqvist, B., Lindahl, B., Stenestrand, U., & Wallentin, L. (2010). The Swedish Web-system for Enhancement and Development of Evidence-Based Care in Heart Disease Evaluated According to Recommended Therapies (SWEDEHEART). *Heart*, *96*(20), 1617–1621.

Joglar, J. A., Chung, M. K., Armbruster, A. L., Benjamin, E. J., Chyou, J. Y., Cronin, E. M., Deswal, A., Eckhardt, L. L., Goldberger, Z. D., Gopinathannair, R., et al. (2024). 2023 ACC/AHA/ACCP/HRS Guideline for the Diagnosis and Management of Atrial Fibrillation: A Report of the American College of Cardiology/American Heart Association Joint Committee on Clinical Practice Guidelines. *Circulation*, *149*(1), e1–e156.

Kinge, J. M., Modalsli, J. H., Øverland, S., Gjessing, H. K., Tollånes, M. C., Knudsen, A. K., Skirbekk, V., Strand, B. H., Håberg, S. E., & Vollset, S. E. (2019). Association of Household Income with Life Expectancy and Cause-Specific Mortality in Norway, 2005-2015. *Journal of the American Medical Association*, *321*(19), 1916–1925.

Korda, R. J., Clements, M. S., & Dixon, J. (2011). Socioeconomic Inequalities in the Diffusion of Health Technology: Uptake of Coronary Procedures as an Example. *Social Science & Medicine*, *72*(2), 224–229.

Lawton, J. S., Tamis-Holland, J. E., Bangalore, S., Bates, E. R., Beckie, T. M., Bischoff, J. M., Bittl, J. A., Cohen, M. G., DiMaio, J. M., et al. (2022). 2021 ACC/AHA/SCAI Guideline for Coronary Artery Revascularization: A Report of the American College of Cardiology/American Heart

Association Joint Committee on Clinical Practice Guidelines. *Journal of the American College of Cardiology*, *79*(2), e21–e129.

Lazuka, V. (2022). *Household and Individual Economic Responses to Different Health Shocks: The Role of Medical Innovations* (Working paper). University of Southern Denmark.

Lleras-Muney, A., & Lichtenberg, F. R. (2005). Are the More Educated More Likely to Use New Drugs? *Annales d'Économie et de Statistique*, 671–696.

Läkemedelsverket. (2023). *Nationellt produktregister för läkemedel (NPL), version 4.3*. Retrieved September 19, 2023, from https://npl.mpa.se/MpaProductExport/4.3/

Läkemedelsverket. (2024a). *Marketing authorisation*. Retrieved June 1, 2024, from https://www.lakemedelsverket.se/en/permission-approval-and-control/marketing-authorisation

Läkemedelsverket. (2024b). *Substitutable drugs*. Retrieved June 1, 2024, from https://www.lakemedelsverket.se/en/treatment-and-prescription/substitutable-drugs

Mackenbach, J. P. (2012). The Persistence of Health Inequalities in Modern Welfare States: The Explanation of a Paradox. *Social Science & Medicine*, *75*(4), 761–769.

Makar, M., Ghassemi, M., Cutler, D. M., & Obermeyer, Z. (2015). Short-Term Mortality Prediction for Elderly Patients Using Medicare Claims Data. *International Journal of Machine Learning and Computing*, *5*(3), 192.

Mansfield, E. (1961). Technical Change and the Rate of Imitation. *Econometrica*, *29*(4), 741–766.

Miller, A. R., & Tucker, C. E. (2011). Can Health Care Information Technology Save Babies? *Journal of Political Economy*, *119*(2), 289–324.

Miraldo, M., Hauck, K., Vernet, A., & Wheelock, A. (2019). Variations in the Adoption of Healthcare Innovation: A Literature Review. *Oxford Research Encyclopedia of Economics and Finance*.

Molitor, D. (2018). The Evolution of Physician Practice Styles: Evidence from Cardiologist Migration. *American Economic Journal: Economic Policy*, *10*(1), 326–56.

Mueller, A. I., & Spinnewijn, J. (2023). *The Nature of Long-Term Unemployment: Predictability, Heterogeneity and Selection* (NBER Working Paper No. 30979). National Bureau of Economic Research.

Mullard, A. (2022). 2021 FDA Approvals. *Nature Reviews Drug Discovery*, *21*(2), 83–88.

Murphy, K. M., & Topel, R. H. (2006). The Value of Health and Longevity. *Journal of Political Economy*, *114*(5), 871–904.

Nordic Casemix Centre. (2023a). *NordDRG ICD-10+ Version 2014*. Retrieved October 20, 2023, from https://nordcase.org/app/uploads/2023/04/icd10_plus_2014.xlsx

Nordic Casemix Centre. (2023b). *NordDRG NCSP+ Version 2014*. Retrieved October 20, 2023, from https://nordcase.org/app/uploads/2023/04/ncsp_plus_2014.xlsx

Pontén, J., Rönnholm, G., & Skiöld, P. (2017). *PPRI Pharma Profile 2017*. Dental; Pharmaceutical Benefits Agency (TLV).

Skinner, J., & Staiger, D. (2007). Technology Adoption from Hybrid Corn to Beta-Blockers. In E. R. Berndt & C. R. Hulten (Eds.), *Hard-to-Measure Goods and Services: Essays in Honor of Zvi Griliches* (pp. 545–570). University of Chicago Press.

Skinner, J., & Staiger, D. (2015). Technology Diffusion and Productivity Growth in Health Care. *Review of Economics and Statistics*, *97*(5), 951–964.

Socialstyrelsen. (2022a). *National Patient Register, Inpatient care (*Patientregister*), 1998–2014*. Retrieved February 12, 2022, from https://www.socialstyrelsen.se/statistik-och-data/register/alla-register/patientregistret/

Socialstyrelsen. (2022b). *National Patient Register, Outpatient care (*Patientregister*), 2001–2014*. Retrieved February 12, 2022, from https://www.socialstyrelsen.se/statistik-och-data/register/alla-register/patientregistret/

Socialstyrelsen. (2022c). *National Prescribed Drug Register (*Läkemedelsregistret*), 2005–2015*. Retrieved February 4, 2022, from https://www.socialstyrelsen.se/statistik-och-data/register/alla-register/lakemedelsregistret/

Socialstyrelsen. (2023a). *Klassifikation av vårdåtgärder (KVÅ)*. Retrieved November 9, 2023, from https://www.socialstyrelsen.se/statistik-och-data/klassifikationer-och-koder/kva/

Socialstyrelsen. (2023b). *Ändringar i Socialstyrelsens klassifikationer*. Retrieved November 9, 2023, from https://www.socialstyrelsen.se/statistik-och-data/klassifikationer-och-koder/senaste-uppdateringar/d

Statistics Sweden. (2022a). *Longitudinal Integrated Database for Health Insurance (LISA), 2000–2018*. Retrieved February 4, 2022, from https://www.scb.se/contentassets/f0bc88c852364b6ea5c1654a0cc90234/lisa-bakgrundsfakta-1990-2017.pdf

Statistics Sweden. (2022b). *Total Health Care Expenditures in Relation to GDP 2001–2020*. Retrieved March 21, 2023, from https://www.statistikdatabasen.scb.se/pxweb/en/ssd/START__NR__NR0109/HCBNP/

Statistics Sweden. (2023a). *Mikrodata för Registerbaserad arbetsmarknadsstatistik (RAMS)* [2023-08-02]. https://www.scb.se/vara-tjanster/bestall-data-och-statistik/bestalla-mikrodata/vilka-mikrodata-finns/individregister/registerbaserad-arbetsmarknadsstatistik-rams/

Statistics Sweden. (2023b). *Mikrodata för Registret över totalbefolkningen (RTB)* [2023-08-02]. https://www.scb.se/vara-tjanster/bestall-data-och-statistik/bestalla-mikrodata/vilka-mikrodata-finns/individregister/registret-over-totalbefolkningen-rtb/

Sveriges Kommuner och Regioner. (2021). *Statistik om hälso- och sjukvård samt regional utveckling, 2020* [2023-03-21.]. https://skr.se/download/18.4a782d2d17c01ffabdde51aa/1634305850273/Statistik-om-halso-och-sjukvard-samt-regional-utveckling-202009.pdf

TLV. (2003). *General Guidelines for Economic Evaluations from the Pharmaceutical Benefits Board (LFNAR 2003:2)*. Retrieved March 22, 2023, from https://www.tlv.se/download/18.2e53241415e842ce95514e9/1510316396792/Guidelines-for-economic-evaluations-LFNAR-2003-2.pdf

TLV. (2005). *Årsredovisning 2005*. Retrieved March 22, 2023, from https://www.tlv.se/download/18.60aa8fdb16a8476d9f8ea50b/1557823144782/arsredovisning_lfn_2005.pdf

TLV. (2022). *Årsredovisning 2022*. Retrieved March 22, 2023, from https://www.tlv.se/download/18.1dddeb291864a62e81bcdcb1/1677221954062/arsredovisning_tlv_2022.pdf

TLV. (2023). *Sök priser och beslut i databasen*. Retrieved November 9, 2023, from https://www.tlv.se/beslut/sok-i-databasen.html

Wallentin, L., Becker, R. C., Budaj, A., Cannon, C. P., Emanuelsson, H., Held, C., Horrow, J., Husted, S., James, S., Katus, H., Mahaffey, K. W., Scirica, B. M., Skene, A., Steg, P. G., Storey, R. F., & Harrington, R. A. (2009). Ticagrelor Versus Clopidogrel in Patients with Acute Coronary Syndromes. *New England Journal of Medicine*, *361*(11), 1045–1057.

WHO Collaborating Centre for Drug Statistics Methodology. (2022a). *ATC Alterations From 2005-2023*. Retrieved September 21, 2023, from https://www.whocc.no/atc_ddd_alterations__cumulative/atc_alterations/

WHO Collaborating Centre for Drug Statistics Methodology. (2022b). *ATC Classification: Structure and Principles* [Last updated: November 10, 2022.]. Retrieved November 10, 2023, from https://www.whocc.no/atc/structure_and_principles/

WHO Collaborating Centre for Drug Statistics Methodology. (2023). *ATC/DDD Index 2023* [2023-09-21]. https://www.whocc.no/atc_ddd_index/

Wikström, J. (2023). *Liquidity Effects in Non-Linear Health Insurance Contracts* [Working paper, Stockholm University].

World Health Organization. (2024). *Model List of Essential Medicines*. Retrieved June 1, 2024, from https://list.essentialmeds.org

Yusuf, S., Peto, R., Lewis, J., Collins, R., & Sleight, P. (1985). Beta Blockade During and After Myocardial Infarction: an Overview of the Randomized Trials. *Progress in Cardiovascular Diseases*, *27*(5), 335–371.

Zhang, Y., Baicker, K., & Newhouse, J. P. (2010). Geographic Variation in the Quality of Prescribing. *New England Journal of Medicine*, *363*(21), 1985–1988.

Zhou, M., Li, X., & Burtch, G. (2021). *Healthcare Across Boundaries: Urban-Rural Differences in the Financial and Healthcare Consequences of Telehealth Adoption* (Boston University Questrom School of Business Research Paper No. 3807577).

Åström, D. O., Franks, P. W., & Sundquist, K. (2018). Neighborhoods and Mortality in Sweden: Is Deprivation Best Assessed Nationally or Regionally? *Demographic Research*, *38*, 429–450.

Åström, D. O., Sundquist, J., & Sundquist, K. (2018). Differences in Declining Mortality Rates Due to Coronary Heart Disease by Neighbourhood Deprivation. *Journal of Epidemiology and Community Health*, *72*(4), 314–318.

Appendices

Appendix 2.A Details on the Mortality Prediction Model

This appendix provides details on our machine-learning mortality prediction model. For a given inpatient healthcare visit, the model estimates the probability that the individual dies within 12 months of the admission date. Section 2.A.1 describes in detail our prediction algorithm and which covariates we choose. The prediction model is an ensemble of three prediction models (LASSO, random forest, and gradient-boosted regression tree).[20] Our prediction algorithm closely follows Mueller and Spinnewijn (2023) and choices for predictors are similar to Makar et al. (2015) and Einav et al. (2018). Section 2.A.3 evaluates the out-of-sample performance of the model.

2.A.1 Approach

Data and predictors. We use data on all inpatient care visits with an admission date on or after July 1, 2005. We keep visits for which we can follow the patient for at least 12 months after admission and drop individuals with missing information on any of the covariates used to train the prediction models. Our data contain 10,080,364 inpatient visits for which we observe a mortality rate of 5.43% within 12 months of the admission date.

We use the following covariates as predictors:

- *Demographic information:* Age at diagnosis (in years), indicator for being female, and 282 indicators for the municipality (*kommun*) of residence in the year of the inpatient care visit.

- *Healthcare use over the previous 12 months:*

  – Number of days spent in inpatient care in each of the previous 12 months before hospitalization (12 covariates).

  – Number of days spent in outpatient care in each of the previous 12 months before hospitalization (12 covariates).

[20] See e.g. Athey and Imbens (2019, Section 2.7) for a discussion of ensemble methods.

- *Health measures:* 1,638 indicator variables for 3-digit ICD-10 diagnosis codes associated with the inpatient visit.

Prediction algorithm. Our prediction algorithm splits the data into four mutually exclusive random samples. Panel A of Appendix Figure 2.1 summarizes the prediction algorithm and provides the sample size and observed mortality rate in each sample.

- *Test sample:* We hold out 92.5 percent of the data and use this sample to evaluate the out-of-sample performance of the prediction models. The test sample is not used for training, tuning, or calibrating the prediction models.
- *Training sample:* We use 7.5 percent of the data to train the prediction models. We use a lower percentage share of the data to form the training sample than is typically used in the machine learning literature (e.g., 50–99%) to ensure training the prediction models is computationally feasible. We further split the training sample into three mutually exclusive subsamples as follows.
  - *Tuning sample:* We use 80 percent of the training sample (i.e. 6 percent of the overall data) to train and tune parameters for the LASSO, random forest, and gradient-boosted regression tree prediction models (see below).
  - *Weights sample:* We use 10 percent of the training sample (i.e. 0.75 percent of the overall data) to estimate weights associated with the ensemble prediction model. The weights are estimated with OLS using a linear regression model where an indicator for dying within 12 months of inpatient care admission is regressed on the fitted values from the LASSO, random forest, and gradient-boosted regression tree models, omitting the intercept.
  - *Calibration sample:* We use 10 percent of the training sample (i.e. 0.75 of the overall data) to calibrate the fitted values obtained using the weights associated with fitted values from each prediction model to observed mortality rates (see below). The calibration en-

sures that the predicted values from the ensemble model lie between $[0,1]$.

2.A.2 Training the Prediction Models

We use the tuning sample to train three prediction models that are then combined to construct the ensemble prediction model. For each prediction model, we tune relevant parameters with the R package caret (Kuhn, 2008) using 5-fold cross-validation and the area under the receiver operating classification (ROC) curve (AUC) as the performance metric. AUC is a commonly used metric in the machine learning literature in general and in the mortality prediction literature in particular (e.g., Einav et al., 2018; Makar et al., 2015).

The three prediction models we train are:

- A LASSO logistic regression model (called "LASSO" below) using the R package `glmnet` (Friedman et al., 2010), for which we tune the regularization parameter λ (option “lambda"). Our preferred model uses $\lambda = 0.00081$.

- A gradient-boosted regression tree model (called “GB” below) using the R package `xgboost` (T. Chen & Guestrin, 2016), for which we tune the number of prediction trees (option "rounds"), the maximum depth of each prediction tree (option “max_depth”), and the learning rate used to update between trees η (option “eta”). Our preferred model uses 200 prediction trees (nrounds = 200), a maximum prediction tree depth of 3 (max_depth = 3), and a learning rate of $\eta = 0.3$ (eta = 0.3).

- A random forest model (called “RF below) using the R package `ranger` (Wright & Ziegler, 2017), for which we tune the number of predictors considered for each split within a tree (option "mtry") and the minimum number of observations in each split of a prediction tree (option “min.node.size”). Our preferred model uses 20 predictors for each split within a tree (mtry = 20), and a minimum of 40 observations in each split of a tree (min.node.size = 40).

Constructing the ensemble model. After training the three prediction models, we construct the ensemble model in four steps:

1. First, using the weights sample, we estimate weights associated with fitted values from each prediction model. Specifically, the weight $\hat{p}^m$ on prediction model $m \in \{LASSO, RF, GB\}$ is the OLS estimate for the coefficient p^m in the linear regression model

$$y_i = p^{LASSO}\hat{y}_i^{LASSO} + p^{RF}\hat{y}_i^{RF} + p^{GB}\hat{y}_i^{GB} + \varepsilon_i,$$

   where y_i is the outcome for observation i, $\hat{y}_i^m$ is the fitted value for observation i from prediction model m, and ε_i is an error term. The estimated weights are $\hat{p}^{LASSO} = 0.191928$ for the LASSO model, $\hat{p}^{RF} = 0.517278$ for the random forest model, and $\hat{p}^{GB} = 0.440330$ for the gradient boosted regression tree model.

2. Second, using the calibration sample, we compute for each observation a fitted value using fitted values from each prediction model and the weights associated with each prediction model. We call the resulting fitted value $\hat{y}_i^{raw} = \hat{p}^{LASSO}\hat{y}_i^{LASSO} + \hat{p}^{RF}\hat{y}_i^{RF} + \hat{p}^{GB}\hat{y}_i^{GB}$ the "raw fitted value" for observation i.

3. Third, we use the calibration sample to group observations to 250 equal-frequency bins (i.e. 250 quantile groups) based on the rank of raw fitted values $\hat{y}_i^{raw}$. We then calibrate the mean raw fitted value in each bin to the observed mortality rate for observations in that bin. This is illustrated by the black dots in Panel B of Appendix Figure 2.1.

4. Fourth, we calibrate the raw fitted values lying between the bin-specific mean raw fitted values by linearly interpolating the observed mortality rates of each bin. This is illustrated by the dashed lines between black dots in Panel B of Appendix Figure 2.1. For observations with a raw fitted value below the mean raw fitted value of the lowest-ranked bin, we calibrate the raw fitted values to the observed mortality rate in the lowest-ranked bin. For observations with a raw fitted value above the mean raw fitted value of the highest-ranked bin, we calibrate the raw fitted values to the observed mortality rate in the highest-ranked bin. This is illustrated by the arrows in Panel B of Appendix Figure 2.1.

2.A.3 Out-of-Sample Performance

Panel A of Appendix Figure 2.2 shows a binned scatterplot of the actual 12-month mortality rate against the predicted 12-month mortality rate using the ensemble prediction model in the test sample. The panel also shows the line of best fit from regressing a dummy for dying within 12 months of inpatient care admission against the predicted mortality rate in the test sample. The ensemble model seems well calibrated for both lower and higher predicted mortality rates, although mortality rates are slightly underestimated for individuals with a higher predicted mortality rate since the line of best fit lies above the 45-degree line (dashed gray line).

Panel B of Appendix Figure 2.2 shows the receiver operating classification (ROC) curve and the area under the ROC curve (AUC) for the ensemble prediction model in the test sample. The model achieves an AUC of 0.88, which is in line with other mortality prediction models in the literature (e.g., Einav et al., 2018; Makar et al., 2015). Intuitively, an AUC of 0.88 means that if we randomly choose from the data an inpatient care visit where the individual dies within 12 months of admission date and another visit where the individual does not die with 12 months, the ensemble model predicts a higher mortality risk for the individual who died with a probability of 0.88.

Appendix 2.B Event Studies to Estimate Downstream Costs and Benefits

This section presents the event studies we use to estimate three downstream effects of receiving ticagrelor over clopidogrel. We estimate the effects on (i) the number of days in inpatient care, (ii) the number of days in outpatient care, and (iii) the total costs of drug purchases over the first 12 months following a first observed heart attack. Total costs of drug purchases include both out-of-pocket expenditures and costs covered by prescription drug insurance.

Consider individual i who has a first observed heart attack in year-quarter t at hospital g. Our goal is to estimate the effects of receiving ticagrelor over clopidogrel. The equation of interest is

$$y_{i,g,t} = \alpha + \beta \, \text{NovelDrug}_{i,g,t} + X'_{i,g,t}\gamma + \delta_t + \psi_g + \varepsilon_{i,g,t} \tag{2.1}$$

where $\text{NovelDrug}_{i,g,t}$ is a dummy equal to one if individual i picks up Ttcagrelor rather than clopidogrel after the heart attack, δ_t denotes year-quarter fixed effects, ψ_g denotes hospital fixed effects, $X_{i,g,t}$ is a vector of individual-level characteristics[21], and $\varepsilon_{i,g,t}$ is an error term. The coefficient of interest is β, which gives the effect of receiving ticagrelor on the outcome of interest $y_{i,g,t}$ (say, days in inpatient care).

The concern is that an estimate of β in (2.1) using OLS will be biased, even conditional on the included covariates, because who gets ticagrelor is decided by the physician, who may also take the patient's preferences into account. To address this endogeneity problem, we instrument $\text{NovelDrug}_{i,g,t}$ using $\overline{\text{NovelDrug}}_{-i,g,t}$, the leave-one-out prescription share for ticagrelor among all heart attack patients in hospital g at time t, except individual i.[22]

For the leave-one-out prescription share $\overline{\text{NovelDrug}}_{-i,g,t}$ to be a valid instrument for $\text{NovelDrug}_{i,g,t}$, it needs to be uncorrelated with any unobserved factors that are correlated with the outcome of interest, conditional on hospital quality and the included individual-level characteristics.

Denote calendar time (year-month) by c and denote event time, i.e. time relative to the time of the health shock, by $e = c - t$. Since our outcomes are measured at multiple points in time, we estimate Equation (2.1) separately for each of the 12 months (specifically, 30-day intervals) before and after the heart attack. This provides a way to assess the validity of the exclusion restriction by looking at estimates for β in Equation (2.1) in months before the heart attack (that is, for $e < 0$). If these "placebo" coefficients differ meaningfully from zero, it suggests the leave-one-out prescription share is an invalid instrument.

Appendix Figure 2.5 shows IV estimates of β from Equation (2.1) for the three outcomes of interest, separately for event times $e \in \{-12, \ldots, 12\} \setminus \{0\}$.

---

[21] We include the following covariates: dummies for age at the time of the heart attack, female dummy, dummy for undergoing percutaneous coronary intervention (PCI) during the heart attack visit (ICD-10 code Z95.5), dummies for symptoms and other factors related to health status (ICD-10 codes R[0-9][0-9] and Z[0-9][0-9]), and dummies for having a diagnosis code for diabetes (E10, E11), COPD (J44), smoking (F17, Z72), and alcohol use (F10) associated with the heart attack visit.

[22] Formally, the leave-one-out prescription share for is defined as

$$\overline{\text{NovelDrug}}_{-i,g,t} = \frac{1}{|N_{g,t}| - 1} \sum_{j \in N_{g,t} \setminus \{i\}} \text{NovelDrug}_{j,g,t},$$

where $N_{g,t}$ denotes the set of individuals experiencing a heart attack at hospital g at time t.

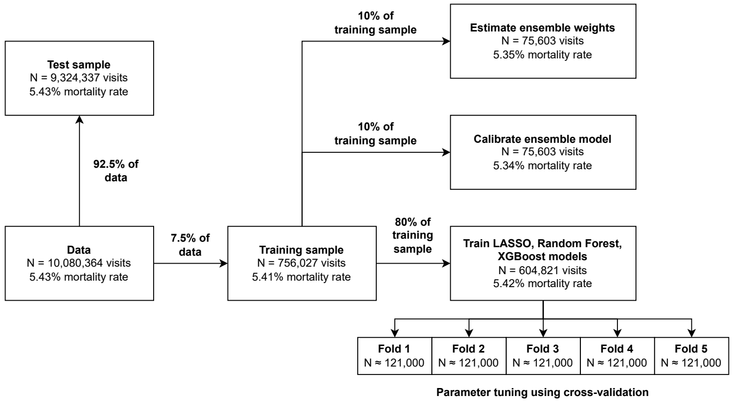
Appendix Table 2.4 presents the estimated coefficients.

Appendix 2.C Sources for the Valuation of Costs and Benefits

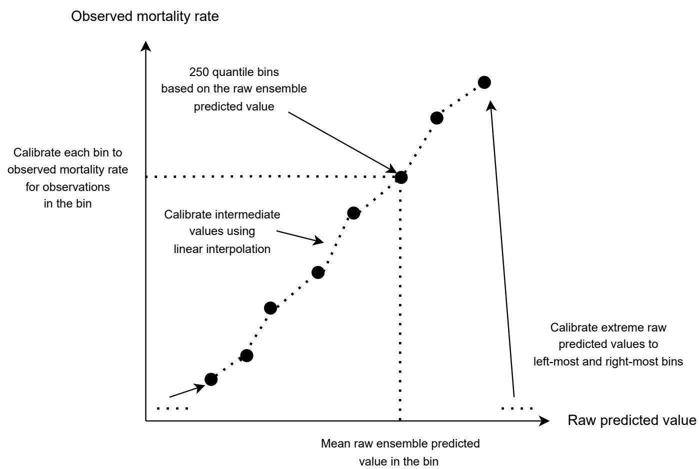
See Appendix Table 2.5.

Appendix 2.D Supplementary Figures and Tables

Panel A: Schematic of the Prediction Algorithm



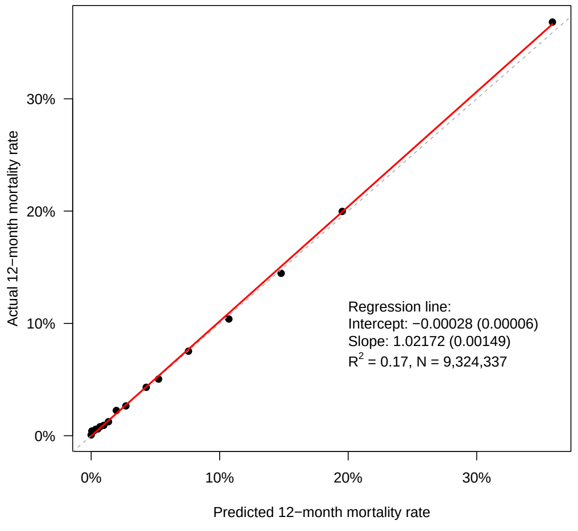
Panel B: Calibration of the Prediction Model



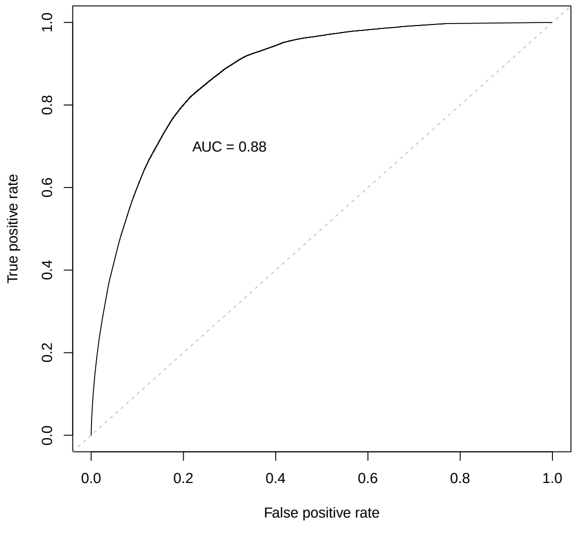
Appendix Figure 2.1: Construction of the Prediction Model

Notes: This figure describes the algorithm we use to construct the ensemble prediction model for mortality. Panel A illustrates how we split the data on inpatient care visits into four mutually exclusive samples to construct the ensemble prediction model. We set aside 92.5 percent of the data as a test sample that we use to evaluate the out-of-sample performance of the model. We use 7.5 percent of the data as a training sample to train and calibrate the ensemble prediction model. We use 80 percent of the training sample to train the three prediction models (LASSO, random forest, and gradient-boosted regression tree) that are used to construct the ensemble model. We use 10 percent of the training sample to estimate the weights associated with predicted values from the three prediction models for the ensemble model. Finally, we use 10 percent of the training sample to calibrate the ensemble model. Panel B illustrates how we calibrate the ensemble model. See Appendix Section 2.A.1 for details.

Panel A: Predicted vs. Actual Mortality Rates

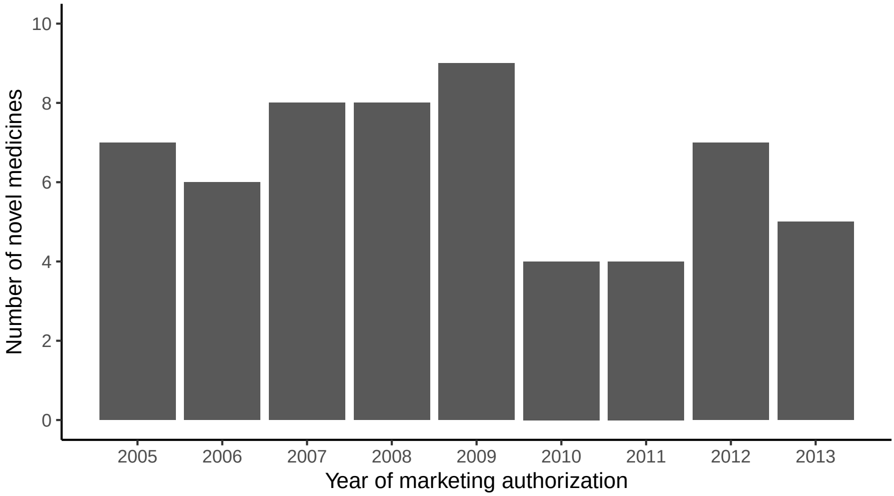


Panel B: ROC Curve



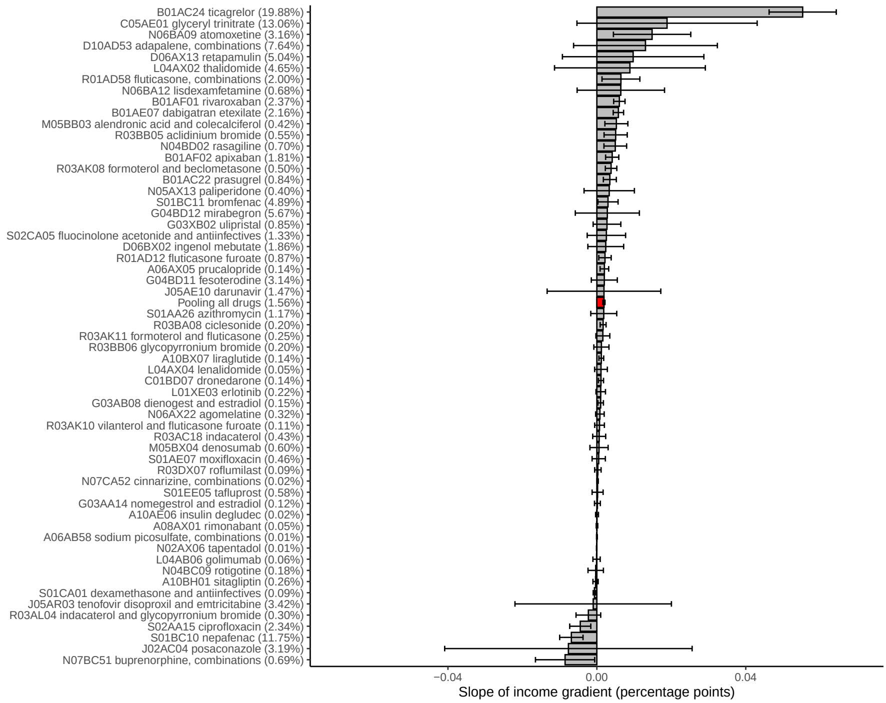
Appendix Figure 2.2: Out-of-Sample Performance of the Prediction Model

Notes. Panel A shows a binned scatterplot of predicted 12-month mortality rates against the actual 12-month mortality rate for the ensemble prediction model using the test sample. The red line shows the line of best fit from regressing a dummy for dying within 12 months of inpatient care admission against the predicted 12-month mortality rate using the test sample. Panel B shows the receiver operating classification (ROC) curve and the area under the ROC curve (AUC) for the ensemble prediction model based on the test sample. In both panels, the dashed gray line indicates the 45-degree line.



Appendix Figure 2.3: Novel Medicines by Year of Marketing Authorization

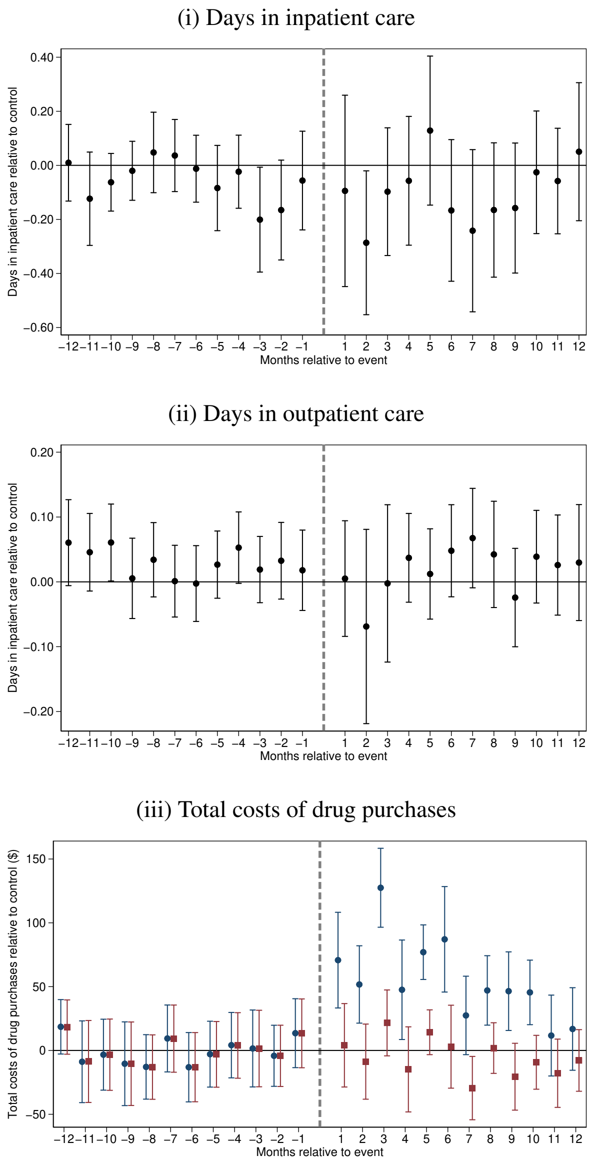
Notes. This figure shows a histogram of the number of novel medicines included in our analysis, separately by the year of marketing authorization, that is, the year when the medicine was approved for sale in Sweden.



Note: Overall adoption rate in parenthesis. Includes following controls: female dummy, age FEs, year−month of admission FEs, and hospital FEs.

Appendix Figure 2.4: Slopes of the Income Gradient for Each Novel Medicine

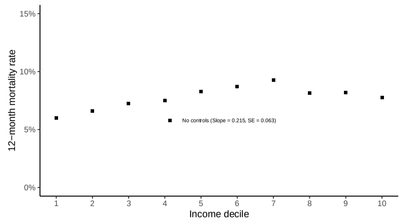
Notes. This figure shows estimated slope coefficients, along with their 95 percent confidence intervals, for the income gradient separately for each of the novel medicines included in the analysis, see Appendix Table 2.2. The confidence intervals are based on standard errors clustered at the hospital level. The percentage share next to the name of the drug gives the mean adoption rate of the drug. The red bar highlights the estimated slope when pooling all drugs.



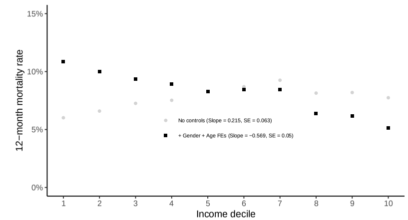
Appendix Figure 2.5: Event Study Estimates of Ticagrelor on Health-Related Outcomes

Notes. This figure shows estimates for the effect of ticagrelor relative to clopidogrel among first-time heart attack patients (see Section 2.5 and Appendix 2.B) based on estimates of Equation (2.1). Panels (i) and (ii) use as outcomes the number of days spent in inpatient care and outpatient care in event month $e \in \{-12,\ldots,12\} \setminus \{0\}$. Panel (iii) uses as outcomes the total monthly expenditures (in USD) on all drugs (blue circle series) and all drugs except ticagrelor and clopidogrel (red square series) in event month $e \in \{-12,\ldots,12\} \setminus \{0\}$. Horizontal line segments in each panel show the 95 percent pointwise confidence intervals based on standard errors clustered at the hospital level.

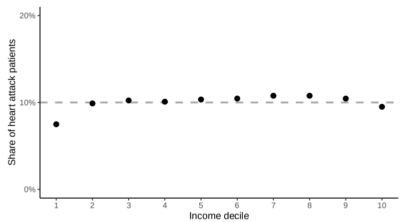
(i) Mortality Rate by Income Decile – Without Controls



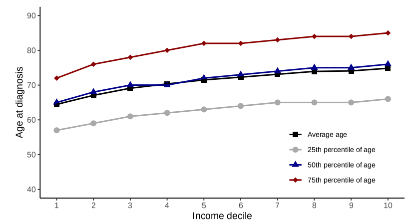
(ii) Mortality Rate by Income Decile – With Controls



(iii) Share of Heart-Attack Patients by Income Decile

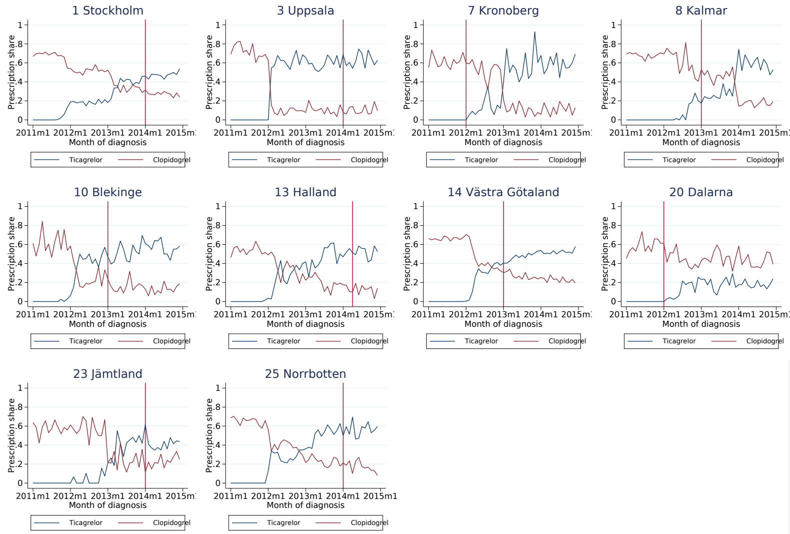


(iv) Age at First Heart Attack by Income Decile



Appendix Figure 2.6: Additional Background for Case Study Using a Novel Antiplatelet Medicine (Ticagrelor)

Notes. This figure provides additional context for the case study in Section 2.5 and Figure 2.4. Panels (i) and (ii) show estimates for the adoption rate of ticagrelor separately by decile of the income distribution. The estimates are based on linear regressions of an indicator for purchasing ticagrelor against income decile dummies when omitting other covariates (panel i) and when adding a female dummy and age fixed effects as additional covariates (panel ii). Panel (iii) shows for each income decile their share of all first-time heart attack patients. The gray dashed horizontal line highlights a share of 10 percent so that dots above (below) the line indicate that the income decile is over-represented (under-represented) among first-time heart attack patients. Panel (iv) helps explain why the sign of the income gradient switches between Panels (i) and (ii) by showing the average as well as the 25th, 50th, and 75th percentiles of the age at first heart attack separately by income decile. The panel shows that individuals in higher decile groups experience their first heart attacks at an older age.



Vertical line shows month when ticagrelor first appears in list of recommendations.

Appendix Figure 2.7: Ticagrelor Prescription Shares and Guideline Inclusions by Healthcare Region

Notes. This figure shows for each healthcare region the prescription share for ticagrelor before and after the guidelines were updated (red line).

Appendix Table 2.1: Choosing the Set of Novel Medicines Considered for the Analysis

Step	Description	Remaining ATC codes
	Unique ATC codes in Prescribed Drug Register	1,622
1.	Drop if ATC code has later been split, deleted, or updated to too coarse ATC codes*	1,611
2.	Marketing authorization between 2005-01-01 and 2014-01-01	238
3.	At least 6 months of purchases associated with in-/outpatient visits	179
4.	At least 50 purchases in at least one month	64
5.	Exclude because of difficulties with determining relevant patients or because there was an existing drug with the same ATC before marketing authorization.**	58
	Novel drugs considered for analysis	58

Notes. This table summarizes the steps we use to choose the set of novel medicines (ATC codes) that we consider for the analysis. We define a purchase as being associated with an inpatient or outpatient care visit if the prescription date falls between the admission and discharge date of the visit and the purchase date is either during the visit or within 7 days of discharge.

* Uses data on revisions to ATC codes from WHO Collaborating Centre for Drug Statistics Methodology (2022a).

** We exclude an HPV vaccine and a smoking cessation medicine due to difficulties mapping the indicated patients to ICD-10 diagnosis codes, and we exclude four medicines because of the latter reason. The latter case could happen when more than one combination of active substances are coded under the same ATC code or when the ATC code was changed at some point to a different one.

Appendix Table 2.2: Summary of Included Novel Medicines

ATC	Name	Brand name(s)	Marketing authorization	Subvention	Patient groups	Notes
A06AB58	sodium picosulfate, combinations	CitraFleet; Picoprep	2008-01-25	2011-02-02	Patients undergoing an x-ray or endoscopy.	
A06AX05	prucalopride	Resolor	2009-10-15	2012-06-28	Constipation	
A08AX01	rimonabant	Acomplia, Zimulti	2006-06-19; 2009-01-16 (withdrawal)	2006-11-09	Type 2 diabetes; Obesity; Dyslipidemia	Withdrawn in November 2008 due to serious psychiatric side effects.
A10AE06	insulin degludec	Tresiba	2013-01-21	2013-06-19	Type 1 & 2 diabetes	
A10BH01	sitagliptin	Januvia	2007-03-21	2007-06-06	Type 2 diabetes	
A10BX07	liraglutide	Victoza	2009-06-30		Type 2 diabetes	
B01AC22	prasugrel	Efient	2009-02-25	2009-09-12	Unstable angina; Heart attack + Undergoing PCI	
B01AC24	ticagrelor	Brilique	2010-12-03	2011-06-09	Unstable angina; Heart attack	
B01AE07	dabigatran etexilate	Pradaxa	2008-03-18	2008-10-01	Pulmonary embolism; Atrial fibrillation; Deep vein thrombosis; Knee/hip replacement surgery	
B01AF01	rivaroxaban	Xarelto	2008-09-30	2009-02-04	Pulmonary embolism; Atrial fibrillation; Deep vein thrombosis; Knee/hip replacement surgery	
B01AF02	apixaban	Eliquis	2011-05-18	2011-12-09	Pulmonary embolism; Atrial fibrillation; Deep vein thrombosis; Knee/hip replacement surgery	
C01BD07	dronedarone	Multaq	2009-11-26	2010-05-18	Atrial fibrillation	
C05AE01	glyceryl trinitrate	Rectogesic	2006-08-18	2007-02-07	Chronic anal fissure	
D06AX13	retapamulin	Altargo	2007-05-24	2010-09-23	Impetigo	Withdrawn in February 2019 for commercial reasons.

Appendix Table 2.2: Summary of Included Novel Medicines (continued)

ATC	Name	Brand name(s)	Marketing authorization	Subvention	Patient groups	Notes
D06BX02	ingenol mebutate	Picato	2012-11-15	2013-06-27	Actinic keratosis	Withdrawn by firm in February 2020 due to potentially increased risk of skin cancer.
D10AD53	adapalene, combinations	Epiduo	2008-01-11	2015-11-21	Acne	
G03AA14	nomegestrol and estradiol	Zoely	2011-07-27	N/A	Menstrual irregularities; Contraception	
G03AB08	dienogest and estradiol	Qlaira	2009-03-06	2011-11-22	Menstrual irregularities; Contraception	
G03XB02	ulipristal	Esmya	2012-02-23	2013-02-22	Uterine fibroids	
G04BD11	fesoterodine	Toviaz	2007-04-20	2008-04-16	Overactive bladder syndrome	
G04BD12	mirabegron	Betmiga	2012-12-20	2013-05-23	Overactive bladder syndrome	
J02AC04	posaconazole	Noxafil	2005-10-25	2006-02-03	Fungal diseases	
J05AE10	darunavir	Prezista	2007-02-12	2007-03-29	HIV	
J05AR03	tenofovir disoproxil and emtricitabine	Truvada	2005-02-21	2005-10-29	HIV	
L01XE03	erlotinib	Tarceva	2005-09-19	2005-10-29	Lung cancer	
L04AB06	golimumab	Simponi	2009-10-01	2010-05-06	Rheumatoid arthritis; Psoriatic arthritis; Ankylosing spondylitis	
L04AX02	thalidomide	Thalidomide Pharmion; Thalidomide Celgene; Thalidomide BMS	2008-04-16	2008-12-19	Multiple myeloma	Orphan drug

Appendix Table 2.2: Summary of Included Novel Medicines (continued)

ATC	Name	Brand name(s)	Marketing authorization	Subvention	Patient groups	Notes
L04AX04	lenalidomide	Revlimid	2007-06-14	2008-03-14	Multiple myeloma	Orphan drug
M05BB03	alendronic acid and colecalciferol	Fosavance	2005-08-24	2006-05-09	Postmenopausal osteoporosis	
M05BX04	denosumab	Prolia	2010-05-26	2010-12-18	Osteoporosis	
N02AX06	tapentadol	Palexia	2010-09-10	2011-06-10	Chronic pain	
N04BC09	rotigotine	Neupro	2006-02-15	2007-11-20	Parkinson's disease	
N04BD02	rasagiline	Azilect	2005-02-21	2006-05-17	Parkinson's disease	
N05AX13	paliperidone	Invega	2007-06-25	2008-09-09	Schizophrenia; Schizoaffective disorder	
N06AX22	agomelatine	Valdoxan	2009-02-19	2010-10-29	Depression	
N06BA09	atomoxetine	Strattera	2006-04-21	2006-05-09	ADHD	
N06BA12	lisdexamfetamine	Elvanse	2013-07-22	2013-12-19	ADHD	
N07BC51	buprenorphine, combinations	Suboxone	2006-09-26	2006-11-21	Opioid dependence	
N07CA52	cinnarizine, combinations	Arlevert	2007-11-23	2014-02-28	Vertigo	
R01AD12	fluticasone furoate	Avamys, Alisade	2008-01-11	2009-04-02	Allergic rhinitis; Asthma	
R01AD58	fluticasone, combinations	Dymista	2013-02-21	2013-05-23	Allergic rhinitis; Asthma	
R03AC18	indacaterol	Onbrez Breezhaler, Oslif Breezhaler, Hirobriz Breezhaler	2009-11-30	2010-12-11	COPD	

Appendix Table 2.2: Summary of Included Novel Medicines (continued)

ATC	Name	Brand name(s)	Marketing authorization	Subvention	Patient groups	Notes
R03AK08	formoterol and beclometasone	Innovair	2008-07-03	2008-09-03	Asthma	
R03AK10	vilanterol and fluticasone furoate	Relvar Ellipta; Revinty Ellipta	2013-11-13	2014-06-20	COPD, Asthma	
R03AK11	formoterol and fluticasone	Flutiform, Iffera	2012-08-23	2012-12-12	Asthma	
R03AL04	indacaterol and glycopyrronium bromide	Ultibro Breezhaler; Ulunar Breezhaler; Xoterna Breezhaler	2013-09-19	2014-02-28	COPD	
R03BA08	ciclesonide	Alvesco	2005-01-14	2012-02-02	Asthma	
R03BB05	aclidinium bromide	Eklira Genuair, Bretaris Genuair	2012-07-20	2013-02-22	COPD	
R03BB06	glycopyrronium bromide	Tovanor Breezhaler; Enurev Breezhaler; Seebri Breezhaler	2012-09-28	2013-06-19	COPD	
R03DX07	roflumilast	Daxas	2010-07-05	2010-12-18	COPD	
S01AA26	azithromycin	Azyter	2011-04-15	2014-08-29	Conjunctivitis	

Appendix Table 2.2: Summary of Included Novel Medicines (continued)

ATC	Name	Brand name(s)	Marketing authorization	Subvention	Patient groups	Notes
S01AE07	moxifloxacin	Vigamox	2009-07-03	2009-12-19	Conjunctivitis	
S01BC10	nepafenac	Nevanac	2007-12-11	2012-12-12	Cataract surgery	
S01BC11	bromfenac	Yellox	2011-05-18	N/A	Cataract surgery	
S01CA01	dexamethasone and antiinfectives	Tobrasone	2006-03-31	2006-06-22	Cataract surgery	
S01EE05	tafluprost	Taflotan, Saflutan	2008-07-03	2008-12-03	Glaucoma	
S02AA15	ciprofloxacin	Ciloxan, Cetraxal	2005-06-23	2006-05-12	Otitis externa; Otitis media	
S02CA05	fluocinolone acetonide and antiinfectives	Cetraxal Comp	2012-11-16	2019-02-22	Otitis externa	

Notes. This table lists the novel medicine included in our analysis. For each medicine, the first column gives its 7-digit Anatomical Therapeutic Chemical (ATC) code, the second column gives its name, the third column lists the names of the product(s) sold in Sweden where the medicine works as the active substance, the fourth column gives the date when the medicine was given marketing authorization (i.e., approval for sale in Sweden), the fifth gives the date when the Dental and Pharmaceutical Benefits Agency (TLV) included the medicine in the high-cost protection scheme, the sixth column gives the relevant patient groups for the medicine. The last column provides additional notes, such as if the medicine was withdrawn from sale during the time period we study or if the medicine is an orphan drug. See Appendix Table 2.3 for the definitions of each patient group.

Appendix Table 2.3: Definitions of Patient Groups

Patient group	Diagnosis codes (ICD-10)	Procedure codes (KMÅ/KKÅ)	Notes
Acne	L70 Acne		
Actinic keratosis	L57.0 Actinic keratosis		
ADHD	F90 Hyperkinetic disorders		Follows Socialstyrelsden (2022)
Allergic rhinitis	J30.1 Allergic rhinitis due to pollen; J30.2 Other seasonal allergic rhinitis; J30.3 Other allergic rhinitis; J30.4 Allergic rhinitis, unspecified		
Ankylosing spondylitis	M45 Ankylosing spondylitis		
Arterial embolism and thrombosis	I74 Arterial embolism and thrombosis		
Asthma	J45 Asthma; J46 Status asthmaticus (incl. Acute severe asthma)		Follows Socialstyrelsden (2009a)
Atrial fibrillation	I48 Atrial fibrillation and flutter		
Cataract surgery		CJE (10, 15, 20, 25, 99), CJF (00, 10, 20, 30, 40, 45, 50, 55, 99) CJG (00, 05, 10, 15, 20, 25, 99)	Follows Hokkinen et al. (2022)
Chronic anal fissure	K60.1 Chronic anal fissure		
Chronic pain	Set of 149 3-digit ICD-10 codes in Gustavsson et al. (2012, Appendix A)		Follows Gustavsson et al. (2012, Appendix A)
Conjunctivitis	H10 Conjunctivitis		
Constipation	K59.0 Constipation		
COPD	J44 Other chronic obstructive pulmonary disease		Follows Socialstyrelsden (2007)
Deep vein thrombosis (DVT)	I80 Phlebitis and thrombophlebitis		Follows Socialstyrelsden (2009c)
Diabetes, type 1	E10 Type 1 diabetes mellitus		
Diabetes, type 2	E11 Type 2 diabetes mellitus		

Appendix Table 2.3: Definitions of Patient Groups (continued)

Patient group	Diagnosis codes (ICD-10)	Procedure codes (KMÅ/KKÅ)	Notes
Dyslipidemia	E78 Disorders of lipoprotein metabolism and other lipidaemias		
Fungal diseases	B38 Coccidioidomycosis; B43 Chromomycosis and phaeomycotic abscess; B44 Aspergillosis; B47 Mycetoma; B48.7 Opportunistic mycoses		Diagnosis codes related to J02AC04 posaconazole [Noxafil], see European Medicines Agency (2024)
Endoscopy		See table notes.	
Glaucoma	H40 Glaucoma; H42 Glaucoma in diseases classified elsewhere		
Heart attack	I21 Acute myocardial infarction	*Percutaneous coronary intervention (PCI)*: FNG Expansion and recanalisation of coronary artery	Follows Wallentin et al. (2009) and European Medicines Agency (2023).
Hip replacement surgery		*Primary total hip replacements:* NFB29, NFB39, NFB49, NFB62, NFB99 *Primary hemi hip replacements:* NFB09, NFB19 *Revisions of hip replacements:* NFC, NFU09, NFU19	Follows Swedish Arthroplasty Register (2022)
HIV	B20-B24 HIV disease		
Knee replacement surgery		*Primary knee replacements:* NGB09, NGB19, NGB29, NGB39, NGB49, NGB53, NGB59, NGB99 *Revision of knee replacements:* NGC, NGU03, NGU09, NGU19, NGU59	Follows Swedish Arthroplasty Register (2022)

Appendix Table 2.3: Definitions of Patient Groups (continued)

Patient group	Diagnosis codes (ICD-10)	Procedure codes (KMÅ/KKÅ)	Notes
Lung cancer	C34 Malignant neoplasm of bronchus and lung; C78.0 Secondary malignant neoplasm of lung		Follows McGuire et al. (2015)
Major depression	F32 Depressive episode; F33 Recurrent depressive disorder		
Menstrual irregularities	N91 Absent, scanty and rare menstruation; N92 Excessive, frequent and irregular menstruation; N93 Other abnormal uterine and vaginal bleeding; N94 Pain and other conditions associated with female genital organs and menstrual cycle		Follows Costa-Ramón et al. (2023)
Multiple myeloma	C90.0 Multiple myeloma		
Obesity	E66 Obesity		
Opioid dependence	F11.2 Mental and behavioural disorders due to use of opioids – Dependence syndrome		Follows WHO (2019)
Osteoporosis	M80 Osteoporosis with pathological fracture; M81 Osteoporosis without pathological fracture; M82 Osteoporosis in diseases classified elsewhere		
Otitis externa	H60 Otitis externa		
Otitis media	H65 Nonsuppurative otitis media; H66 Suppurative and unspecified otitis media; H67 Otitis media in diseases classified elsewhere		
Overactive bladder syndrome	R39.1 Other difficulties with micturition; N31.0 Uninhibited neuropathic bladder, not elsewhere classified; N31.2 Flaccid neuropathic bladder, not elsewhere classified; N31.9 Neuromuscular dysfunction of bladder, unspecified; N39.4 Other specified urinary incontinence		Follows Internetmedicin (2022)

Appendix Table 2.3: Definitions of Patient Groups (continued)

Patient group	Diagnosis codes (ICD-10)	Procedure codes (KMÅ/KKÅ)	Notes
Parkinson's disease	G20 Parkinson disease		Follows Socialstyrelsden (2009b)
Post-menopausal osteoporosis	M80.0 Postmenopausal osteoporosis with pathological fracture; M81.0 Postmenopausal osteoporosis		
Primary insomnia	F51.0 Nonorganic insomnia; G47.0 Disorders of initiating and maintaining sleep [insomnias]		Follows Socialstyrelsden (2010) and Sadeh and Gruber (1998)
Psoriatic arthritis	M07 Psoriatic and enteropathic arthropathies		Follows Socialstyrelsden (2020a)
Pulmonary embolism (PE)	I26 Pulmonary embolism		Follows Zöller et al. (2017)
Rheumatoid arthritis	M05 Seropositive rheumatoid arthritis: M06 Other rheumatoid arthritis		Follows Socialstyrelsden (2020b)
Schizophrenia	F20 Schizophrenia		
Schizoaffective disorder	F25 Schizoaffective disorders		
Unstable angina	I20.0 Unstable angina	*Percutaneous coronary intervention (PCI)*: FNG Expansion and recanalisation of coronary artery	Follows Wallentin et al. (2009) and European Medicines Agency (2023).
Uterine fibroids	D25 Leiomyoma of uterus		Follows Uimari et al. (2016)
Vertigo	H81 Disorders of vestibular function; R42 Dizziness and giddiness		
X-ray		See table notes.	

Notes. This table lists the diagnosis and procedure codes we use to define each of the patient groups related to the novel medicines we include in our analysis. The first column gives the name of the patient group, the second and third columns list the diagnosis (ICD-10) codes and the surgical (KKÅ) and non-surgical (KVÅ) procedure codes we use to define the group. The fourth column provides references to the sources we have used to determine the relevant diagnosis and procedure codes.

Diagnosis codes for chronic pain are taken from Gustavsson et al. (2012, Appendix A): C00, C01, C02, C03, C04, C05, C06, C07, C08, C09, C10, C11, C13, C14, C15, C17, C20, C21, C22, C24, C25, C30, C31, C32, C34, C37, C38, C39, C40, C41, C45, C46, C47, C48, C49, C52, C55, C64, C65, C66, C70, C71, C72, C74, C75, C76, C77, C78, C79, C80, C81, C82, C83, C85, C88, C90, C92, D00, D01, D02, D38, D42, D43, D46, D47, D90, Z51, M43, M45, M46, M48, M49, M81, M82, M50, M51, M05, M06, M07, M08, M10, M11, M12, M13, M14, M15, M16, M17, M18, M19, M23, M24, M25, M36, M77, R26, S12, S22, S32, S42, S43, S53, T02, T08, T91, L89, L97, L98, M80, G43, G44, R51, G50, G52, G53, G54, G55, G56, G57, G58, G59, G60, G61, G62, G62, G63, G64, G82, G97, M89, R29, F45, G96, M47, M53, M54, M70, M75, M79, M99, R07, R10, R52, S13, T85, T88, T92, T93, T94.

Procedure codes for X-rays are based on a keyword search for "[R|r]öntgen" from the list of procedure codes (Nordic Casemix Centre, 2023b; Socialstyrelsen, 2023a, 2023b, cf.): AA066, AA067, AA068, AA069, AD027, AD028, AD029, AD030, AD031, AD032, AD033, AD034, AE007, AE008, AE009, AE010, AE011, AE012, AE013, AE014, AE015, AE016, AF053, AG036, AG037, AG038, AG039, AG040, AG041, AG042, AG043, AH003, AJ038, AJ039, AJ040, AJ041, AJ042, AJ043, AJ044, AJ045, AJ046, AJ047, AJ048, AJ049, AJ050, AK020, AK021, AK022, AK023, AK024, AK025, AM007, AN051, AN052, AN053, AN054, AN055, AN056, AN057, AN058, AN059, AN060, AN061, AN062, AN063, AN064, AN065, AN066, AN067, AN068, AN069, AN070, AN071, AN072, AN073, AN074, AN075, AN090, AV035, AV036, DA031, TJF30, ZXM10.

Procedure codes for X-rays are based on a keyword search for "[E|e]ndoskopi" from the list of procedure codes (Nordic Casemix Centre, 2023b; Socialstyrelsen, 2023a, 2023b, cf.): AD008, AD048, AJ076, AV014, AV057, DG018, DJ003, DJ012, AAA50, ABA20, ABC01, ABC04, ABC07, CHF12, DMB20, DNB20, DQB10, DQD50, ENC70, GAC01, GAC11, GAC21, GAC34, GAC37, GAC41, GAC44, GAC47, GAC97, GBA12, GBA22, GBA25, GBA28, GBA32, GBA35, GBA98, GBW98, GCA18, GCA32, GCA42, GCA98, GDA01, GEC03, GEC16, GEC26, GEC93, GWC01, GWE01, GWF01, GWW97, GWW98, JAB04, JAB14, JAB44, JAB84, JAC14, JAC44, JAC84, JAD84, JAE84, JAF84, JAG84, JCA05, JCA08, JCA12, JCA22, JCA32, JCA35, JCA38, JCA42, JCA45, JCA52, JCA55, JCA98, JCF12, JCW98, JDA05, JDA08, JDA12, JDA22, JDA32, JDA35, JDA38, JDA42, JDA45, JDA52, JDA55, JDF32, JDH05, JDH08, JDH15, JDH18, JDH22, JDH25, JDH28, JDH32, JDH35, JDH52, JDW98, JFA02, JFA05, JFA12, JFA15, JFA22, JFA25, JFA28, JFA32, JFA35, JFA38, JFA42, JFA45, JFA48, JFA52, JFA55, JFA58, JFA65, JFA68, JFA85, JFA98, JFW98, JGA02, JGA05, JGA22, JGA28, JGA32, JGA35, JGA52, JGA58, JGA68, JGA75, JGA98, JGW98, JKE02, JKE12, JKE15, JKE18, JKE22, JKE25, JKE32, JKE98, JKW98, JLB12, JLB22, JLB25, JLB28, JLB35, JLB38, JLB42, JLB98, JLD12, JLD22, JLW98, JWC01, JWE01, JWE02, JWF01, JWW97, JWW98, KAA97, KAB01, KAC01, KAC21, KAD01, KAD51, KAD97, KAE11, KAE97, KAF11, KAH31, KAH41, KAH54, KAH81, KAH97, KAJ11, KAJ97, KAS97, KAW97, KBA01, KBE01, KBE97, KBF01, KBH97, KBJ97, KBW97, KBW98, KCW97, KDG21, KDG31, KDG41,

KDG97, KDH98, KDV22, KDW98, KEC01, KWC01, KWE01, KWW97, LBA96, LBA98, LBC98, LBF98, LBW98, LCA98, LCB98, LCG98, LCW98, LGA98, LWC01, LWE01, LWE02, LWF01, LWW97, LWW98, MAW98, MWC01, MWE01, MWE02, MWF01, MWW97, MWW98, NAA01, NAA11, NAA21, NAA31, NAH01, NAH91, NBA01, NBA11, NBA21, NBA31, NBE01, NBE11, NBE21, NBE31, NBE41, NBE51, NBE91, NBF01, NBF11, NBF21, NBF31, NBF91, NBH01, NBH11, NBH21, NBH31, NBH41, NBH51, NBH71, NBH91, NCA01, NCA11, NCA21, NCA31, NCE11, NCE21, NCE31, NCE41, NCE51, NCE91, NCF01, NCF11, NCF21, NCF31, NCF91, NCH01, NCH31, NCH41, NCH51, NCH71, NCH91, NDA01, NDA11, NDA21, NDA31, NDE01, NDE11, NDE21, NDE41, NDE51, NDE91, NDF01, NDF11, NDF21, NDF31, NDF91, NDH01, NDH11, NDH21, NDH31, NDH41, NDH51, NDH71, NDH91, NFA01, NFA11, NFA21, NFA31, NFF01, NFF11, NFF21, NFF31, NFF91, NFH01, NFH21, NFH31, NFH41, NFH51, NFH71, NFH91, NGA01, NGA11, NGA21, NGA31, NGD01, NGD11, NGD21, NGD91, NGE01, NGE11, NGE21, NGE31, NGE41, NGE51, NGE91, NGF01, NGF11, NGF21, NGF31, NGF91, NGH01, NGH11, NGH21, NGH31, NGH41, NGH51, NGH71, NGH91, NHA01, NHA11, NHA21, NHA31, NHE01, NHE11, NHE21, NHE31, NHE41, NHE51, NHE91, NHF01, NHF11, NHF21, NHF91, NHH01, NHH11, NHH21, NHH31, NHH41, NHH51, NHH71, NHH91, PCS40, PCS99, PCU86, PDS10, PDS30, PDU86, PES10, PES11, PES12, PEU86, PFS10, PFU86, PGU86, PHS13, PHS14, PHS99, UGA02, UGA05, UJF52, UJF55, UJX00, UKB12, UKB15, UKC12, UKC15, XAA00, XAB00, XAW97, XGX96, XJD02, XJF02, XJG12, XJK02, XJL02, XJW98, YKA01, YWC01, YWE01, YWF01, YWW97, ZUJ00, ZXC85, ZXC90, ZXC91, ZXC95, ZXC97, ZXK00.

Appendix Table 2.4: Event Study Estimates of Ticagrelor on Health-Related Outcomes

	e = -12	e = -11	e = -10	e = -9	e = -8	e = -7	e = -6	e = -5	e = -4	e = -3	e = -2	e = -1
A. Inpatient and Outpatient Care (number of days)												
Estimated coefficient (SE)	0.0700	-0.0779	-0.00199	-0.0148	0.0817	0.0374	-0.0151	-0.0574	0.0292	-0.182	-0.133	-0.0384
	(0.0792)	(0.100)	(0.0642)	(0.0641)	(0.0822)	(0.0814)	(0.0753)	(0.0831)	(0.0784)	(0.104)	(0.105)	(0.106)
Outcome mean (SE)	0.345	0.366	0.359	0.346	0.362	0.369	0.352	0.364	0.361	0.391	0.410	0.568
	(0.00873)	(0.00929)	(0.00895)	(0.00879)	(0.00902)	(0.00940)	(0.00876)	(0.00893)	(0.00876)	(0.00963)	(0.00984)	(0.0111)
Inpatient Care												
Estimated coefficient (SE)	0.00956	-0.124	-0.0627	-0.0202	0.0477	0.0363	-0.0125	-0.0840	-0.0236	-0.201*	-0.165	-0.0563
	(0.0724)	(0.0881)	(0.0544)	(0.0557)	(0.0760)	(0.0681)	(0.0632)	(0.0805)	(0.0690)	(0.0989)	(0.0942)	(0.0931)
Outcome mean (SE)	0.184	0.206	0.198	0.181	0.194	0.201	0.186	0.196	0.188	0.217	0.231	0.350
	(0.00777)	(0.00844)	(0.00804)	(0.00783)	(0.00796)	(0.00841)	(0.00773)	(0.00797)	(0.00777)	(0.00868)	(0.00889)	(0.0100)
Outpatient Care												
Estimated coefficient (SE)	0.0605	0.0457	0.0607*	0.00539	0.0341	0.00108	-0.00260	0.0266	0.0528	0.0190	0.0326	0.0179
	(0.0338)	(0.0305)	(0.0303)	(0.0316)	(0.0292)	(0.0282)	(0.0298)	(0.0265)	(0.0281)	(0.0261)	(0.0302)	(0.0316)
Outcome mean (SE)	0.161	0.160	0.161	0.165	0.168	0.168	0.166	0.168	0.173	0.174	0.179	0.217
	(0.00352)	(0.00345)	(0.00350)	(0.00363)	(0.00367)	(0.00365)	(0.00364)	(0.00364)	(0.00363)	(0.00369)	(0.00373)	(0.00396)
B. Total Costs of All Drug Purchases (USD)												
Estimated coefficient (SE)	18.54	-8.845	-3.306	-10.39	-12.89	9.405	-13.13	-2.899	4.157	1.572	-4.178	13.54
	(10.88)	(16.34)	(14.18)	(16.74)	(12.89)	(13.38)	(13.85)	(13.16)	(13.07)	(15.37)	(12.21)	(13.77)
Outcome mean (SE)	76.37	77.30	79.09	77.42	78.40	78.91	77.95	78.05	78.18	78.69	78.13	81.59
	(1.553)	(1.575)	(1.708)	(1.530)	(1.731)	(1.676)	(1.628)	(1.523)	(1.659)	(1.639)	(1.516)	(1.664)
Only Ticagrelor												
Estimated coefficient (SE)	-0.0272	0.0116	0.0565	0.0144	0.151	0.111	-0.0872	-0.0335	0.0934	-0.0234	0.155	0.0356
	(0.104)	(0.150)	(0.126)	(0.0679)	(0.188)	(0.110)	(0.121)	(0.186)	(0.129)	(0.176)	(0.250)	(0.104)
Outcome mean (SE)	0.0534	0.0574	0.0722	0.0293	0.0659	0.0391	0.0528	0.0719	0.0617	0.0461	0.0377	0.0621
	(0.0214)	(0.0201)	(0.0235)	(0.0170)	(0.0227)	(0.0166)	(0.0200)	(0.0231)	(0.0220)	(0.0180)	(0.0160)	(0.0221)
Only Clopidogrel												
Estimated coefficient (SE)	0.241	-0.228	-0.0568	-0.0143	-0.0183	0.0273	0.0507	0.188	0.139	0.0762	-0.112	0.0924
	(0.161)	(0.274)	(0.219)	(0.133)	(0.179)	(0.228)	(0.163)	(0.158)	(0.127)	(0.176)	(0.164)	(0.115)
Outcome mean (SE)	0.211	0.239	0.221	0.210	0.195	0.229	0.198	0.163	0.167	0.189	0.159	0.139
	(0.0174)	(0.0251)	(0.0207)	(0.0185)	(0.0174)	(0.0236)	(0.0174)	(0.0165)	(0.0142)	(0.0186)	(0.0162)	(0.0119)
All Other Drugs												
Estimated coefficient (SE)	18.32	-8.628	-3.305	-10.39	-13.02	9.266	-13.09	-3.053	3.925	1.520	-4.221	13.41
	(10.86)	(16.38)	(14.25)	(16.69)	(12.88)	(13.43)	(13.83)	(13.13)	(13.11)	(15.29)	(12.25)	(13.76)
Outcome mean (SE)	76.11	77.01	78.80	77.18	78.14	78.64	77.70	77.82	77.96	78.46	77.94	81.39
	(1.551)	(1.574)	(1.707)	(1.529)	(1.729)	(1.675)	(1.627)	(1.523)	(1.659)	(1.638)	(1.515)	(1.663)
Kleibergen-Paap first-stage F-statistic	302.3	302.3	302.3	302.3	302.3	302.3	302.3	302.3	302.3	302.3	302.3	302.3
Number of observations	33244	33244	33244	33244	33244	33244	33244	33244	33244	33244	33244	33244

	e = 1	e = 2	e = 3	e = 4	e = 5	e = 6	e = 7	e = 8	e = 9	e = 10	e = 11	e = 12
A. Inpatient and Outpatient Care (number of days)												
Estimated coefficient (SE)	-0.0895	-0.355*	-0.0998	-0.0202	0.141	-0.119	-0.174	-0.123	-0.182	0.0130	-0.0321	0.0802
	(0.200)	(0.160)	(0.140)	(0.126)	(0.144)	(0.138)	(0.166)	(0.136)	(0.135)	(0.125)	(0.110)	(0.140)
Outcome mean (SE)	1.335	1.373	1.022	0.866	0.762	0.711	0.694	0.665	0.621	0.598	0.606	0.594
	(0.0182)	(0.0169)	(0.0157)	(0.0158)	(0.0151)	(0.0148)	(0.0148)	(0.0149)	(0.0145)	(0.0140)	(0.0145)	(0.0148)
Inpatient Care												
Estimated coefficient (SE)	-0.0946	-0.286*	-0.0974	-0.0572	0.129	-0.167	-0.242	-0.165	-0.158	-0.0258	-0.0580	0.0504
	(0.181)	(0.136)	(0.121)	(0.122)	(0.141)	(0.134)	(0.153)	(0.127)	(0.123)	(0.116)	(0.0996)	(0.130)
Outcome mean (SE)	1.033	0.759	0.591	0.549	0.490	0.457	0.442	0.429	0.391	0.374	0.385	0.377
	(0.0175)	(0.0163)	(0.0148)	(0.0148)	(0.0141)	(0.0139)	(0.0138)	(0.0139)	(0.0134)	(0.0128)	(0.0134)	(0.0136)
Outpatient Care												
Estimated coefficient (SE)	0.00506	-0.0689	-0.00238	0.0370	0.0122	0.0480	0.0675	0.0424	-0.0242	0.0388	0.0259	0.0297
	(0.0455)	(0.0764)	(0.0619)	(0.0349)	(0.0355)	(0.0362)	(0.0391)	(0.0418)	(0.0387)	(0.0365)	(0.0394)	(0.0456)
Outcome mean (SE)	0.302	0.614	0.430	0.317	0.272	0.254	0.251	0.236	0.230	0.223	0.221	0.218
	(0.00440)	(0.00499)	(0.00488)	(0.00466)	(0.00459)	(0.00459)	(0.00476)	(0.00477)	(0.00471)	(0.00479)	(0.00493)	(0.00511)
B. Total Costs of All Drug Purchases (USD)												
Estimated coefficient (SE)	70.77***	51.71***	127.5***	47.55*	77.00***	87.09***	27.45	47.04***	46.46**	45.49***	11.69	16.83
	(19.12)	(15.46)	(15.78)	(19.88)	(10.91)	(21.08)	(15.68)	(13.88)	(15.71)	(12.90)	(16.16)	(16.50)
Outcome mean (SE)	135.6	125.5	166.1	140.8	117.6	137.1	142.5	109.3	122.6	132.5	101.6	105.0
	(1.675)	(1.925)	(1.726)	(2.144)	(1.922)	(2.034)	(2.026)	(1.900)	(1.948)	(2.064)	(2.015)	(2.095)
Only Ticagrelor												
Estimated coefficient (SE)	74.70***	60.44***	110.1***	62.93***	55.11***	79.28***	62.10***	40.71***	61.96***	53.90***	25.35***	24.52***
	(7.249)	(5.117)	(6.356)	(5.405)	(3.289)	(5.339)	(6.027)	(3.270)	(5.178)	(5.808)	(3.206)	(3.140)
Outcome mean (SE)	35.16	29.95	51.32	33.94	26.16	33.85	32.12	19.43	25.48	23.02	12.64	10.15
	(0.490)	(0.426)	(0.585)	(0.487)	(0.426)	(0.489)	(0.481)	(0.379)	(0.425)	(0.408)	(0.300)	(0.259)
Only Clopidogrel												
Estimated coefficient (SE)	-4.352***	-1.774***	-6.309***	-5.634***	-1.390***	-3.206***	-4.972***	-1.278***	-1.666***	-3.277***	-1.014**	-0.539*
	(0.872)	(0.520)	(0.677)	(0.716)	(0.304)	(0.535)	(0.840)	(0.368)	(0.466)	(0.894)	(0.334)	(0.254)
Outcome mean (SE)	4.493	2.078	3.720	3.456	1.550	2.117	2.979	1.339	1.547	2.636	1.362	0.915
	(0.0737)	(0.0484)	(0.0551)	(0.0559)	(0.0395)	(0.0427)	(0.0470)	(0.0343)	(0.0361)	(0.0445)	(0.0343)	(0.0289)
All Other Drugs												
Estimated coefficient (SE)	4.059	-8.755	21.60	-14.81	14.27	2.954	-29.48*	1.880	-20.57	-9.260	-17.85	-7.835
	(16.67)	(15.02)	(13.17)	(17.01)	(8.956)	(16.58)	(12.66)	(10.12)	(13.36)	(10.77)	(13.65)	(12.32)
Outcome mean (SE)	95.02	88.65	101.4	91.91	77.40	84.21	86.95	70.22	72.09	78.21	63.51	66.34
	(1.564)	(1.781)	(1.482)	(1.879)	(1.639)	(1.689)	(1.649)	(1.503)	(1.492)	(1.556)	(1.490)	(1.514)
Kleibergen-Paap first-stage F-statistic	302.3	302.3	302.3	302.3	302.3	302.3	302.3	302.3	302.3	302.3	302.3	302.3
Number of observations	33244	33244	33244	33244	33244	33244	33244	33244	33244	33244	33244	33244

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes. This figure shows estimates along with standard errors clustered at the hospital level for the effect of ticagrelor relative to clopidogrel among first-time heart attack patients (see Section 2.5 and Appendix 2.B) based on IV estimates of Equation (2.1). Panel A uses as outcomes the number of days spent in inpatient care and outpatient care in event month $e \in \{-12,\ldots,12\} \setminus \{0\}$. Panel B uses as outcomes the total monthly expenditures (in USD) on all drugs, ticagrelor, clopidogrel, and all drugs except ticagrelor and clopidogrel in event month $e \in \{-12,\ldots,12\} \setminus \{0\}$. The table also presents the means of the outcome variables for each event month, number of observations used for estimations, and the Kleibergen-Paap F-statistic for the leave-one-out instrument from the first-stage specification.

Appendix Table 2.5: Sources for Costs of Various Health Outcomes

Outcomes	Value	Source
Ticagrelor effect on 12-month all-cause mortality (p.p.)	-1.17 (SE = 0.32)	Wallentin et al. (2009), Table 3 (row "Death from any cause").
Quality-adjusted life-year	1.2 million SEK	Hultkrantz and Svensson (2012)
Per-day cost of inpatient care	11,279 SEK	Own calculations using data for 2015 from Socialstyrelsen.

Appendix References

Athey, S., & Imbens, G. W. (2019). Machine Learning Methods that Economists Should Know About. *Annual Review of Economics*, *11*, 685–725.

Chen, T., & Guestrin, C. (2016). XGBoost: A Scalable Tree Boosting System. *Proceedings of the 22nd ACM SIGKDD International Conference on Knowledge Discovery and Data Mining*, 785–794.

Costa-Ramón, A., Daysal, N. M., & Rodríguez-González, A. (2023). *The Oral Contraceptive Pill and Adolescents' Mental Health* (tech. rep. No. 16288). Institute of Labor Economics (IZA).

European Medicines Agency. (2023). *Brilique* [Last updated: August 3, 2023.]. Retrieved October 4, 2023, from https://www.ema.europa.eu/en/medicines/human/EPAR/brilique

European Medicines Agency. (2024). *Noxafil* [Last updated: May 2, 2024.]. Retrieved May 31, 2024, from https://www.ema.europa.eu/en/medicines/human/EPAR/noxafil

Friedman, J., Tibshirani, R., & Hastie, T. (2010). Regularization Paths for Generalized Linear Models via Coordinate Descent. *Journal of Statistical Software*, *33*(1), 1–22.

Gustavsson, A., Bjorkman, J., Ljungcrantz, C., Rhodin, A., Rivano-Fischer, M., Sjolund, K.-F., & Mannheimer, C. (2012). Socio-Economic Burden of Patients with a Diagnosis Related to Chronic Pain – Register Data of 840,000 Swedish Patients. *European Journal of Pain*, *16*(2), 289–299.

Hokkinen, K., Kaarniranta, K., Jämsen, E., Lampela, P., Hartikainen, S., & Tolppanen, A.-M. (2022). Incidence of Cataract Surgeries in People With and Without Alzheimer's Disease. *Acta Ophthalmologica*, *100*(1), 68–73.

Hultkrantz, L., & Svensson, M. (2012). The Value of a Statistical Life in Sweden: A Review of the Empirical Literature. *Health Policy*, *108*(2-3), 302–310.

Internetmedicin. (2022). *Överaktiv blåsa* [Last updated: 2022-11-24]. Retrieved May 31, 2024, from https://www.internetmedicin.se/urologi/overaktiv-blasa

Kuhn, M. (2008). Building Predictive Models in R Using the caret Package. *Journal of Statistical Software*, *28*, 1–26.

McGuire, A., Martin, M., Lenz, C., & Sollano, J. (2015). Treatment Cost of Non-Small Cell Lung Cancer in Three European Countries: Comparisons across France, Germany, and England using Administrative Databases. *Journal of Medical Economics*, *18*(7), 525–532.

Sadeh, A., & Gruber, R. (1998). 5.24 - Sleep Disorders. In A. S. Bellack & M. Hersen (Eds.), *Comprehensive Clinical Psychology* (pp. 629–653). Pergamon.

Socialstyrelsden. (2007). *Kroniskt obstruktiv lungsjukdom (KOL)* [Last updated 2007-10-01.]. Retrieved May 31, 2024, from https://forsakringsmedicin.socialstyrelsen.se/beslutsstod-for-diagnoser/diagnoser/andningsorganens-sjukdomar/kroniskt-obstruktiv-lungsjukdom-kol/

Socialstyrelsden. (2009a). *Astma* [Last updated 2009-06-01.]. Retrieved May 31, 2024, from https://forsakringsmedicin.socialstyrelsen.se/beslutsstod-for-diagnoser/diagnoser/andningsorganens-sjukdomar/astma/

Socialstyrelsden. (2009b). *Parkinsons sjukdom* [Last updated: 2009-06-01]. Retrieved May 31, 2024, from https://forsakringsmedicin.socialstyrelsen.se/beslutsstod-for-diagnoser/diagnoser/nervsystemets-sjukdomar/parkinsons-sjukdom/

Socialstyrelsden. (2009c). *Perifera kärlsjukdomar* [Last updated 2009-06-01.]. Retrieved May 31, 2024, from https://forsakringsmedicin.socialstyrelsen.se/beslutsstod-for-diagnoser/diagnoser/hjart-karlsjukdomar/perifera-karlsjukdomar/

Socialstyrelsden. (2010). *Insomni* [Last updated: 2010-06-01]. Retrieved May 31, 2024, from https://forsakringsmedicin.socialstyrelsen.se/beslutsstod-for-diagnoser/diagnoser/nervsystemets-sjukdomar/insomni/

Socialstyrelsden. (2020a). *Psoriasisartrit* [Last updated: 2020-10-07]. Retrieved May 31, 2024, from https://forsakringsmedicin.socialstyrelsen.se/

beslutsstod - for - diagnoser / diagnoser / rorelseorganens - sjukdomar / psoriasisartrit/

Socialstyrelsden. (2020b). *Reumatoid artrit (RA)* [Last updated: 2020-10-07]. Retrieved May 31, 2024, from https://forsakringsmedicin.socialstyrelsen.se/beslutsstod-for-diagnoser/diagnoser/rorelseorganens-sjukdomar/reumatoid-artrit-ra/

Socialstyrelsden. (2022). *Nationella riktlinjer – Indikatorer för vård och stöd vid ADHD och autism*. Retrieved May 31, 2024, from https://www.socialstyrelsen.se/globalassets/sharepoint-dokument/artikelkatalog/nationella-riktlinjer/2022-10-8150.pdf

Swedish Arthroplasty Register. (2022). *Annual Report 2022 – The Swedish Arthroplasty Register*. Retrieved May 31, 2024, from https://registercentrum.blob.core.windows.net/sar/r/SAR-Annual-Report-2022_EN-HkgQE89Nus.pdf

Uimari, O., Auvinen, J., Jokelainen, J., Puukka, K., Ruokonen, A., Järvelin, M.-R., Piltonen, T., Keinänen-Kiukaanniemi, S., Zondervan, K., Järvelä, I., et al. (2016). Uterine Fibroids and Cardiovascular Risk. *Human Reproduction*, *31*(12), 2689–2703.

World Health Organization. (2019). *Chapter V – Mental and behavioural disorders (F00-F99)*. Retrieved May 31, 2024, from https://icd.who.int/browse10/2019/en#/F11.2

Wright, M. N., & Ziegler, A. (2017). ranger: A Fast Implementation of Random Forests for High Dimensional Data in C++ and R. *Journal of Statistical Software*, *77*, 1–17.

Zöller, B., Pirouzifard, M., Memon, A. A., Sundquist, J., & Sundquist, K. (2017). Risk of Pulmonary Embolism and Deep Venous Thrombosis in Patients with Asthma: A Nationwide Case-Control Study from Sweden. *European Respiratory Journal*, *49*(2).

Chapter 3

Unemployment Insurance Generosity and Health: Evidence from Sweden*

*This chapter is co-authored with Arash Nekoei and David Seim. We thank Maija Kaartinen, Arizo Karimi, Viktoria Kozek, Per Pettersson Lidbom, Dominik Wehr, and Jens Wikström for helpful comments and discussions. We thank Viktor Granvald and Åsa Wresros at the National Board of Health and Welfare for providing statistics on inpatient and outpatient visits, costs per DRG point, and retrospective DRG weights and guidance on their use. All errors are ours.

3.1 Introduction

An extensive literature in the social sciences documents that job displacement and unemployment are stressful events harmful to mental and physical health.[1] These adverse health consequences impose costs not only on the affected individuals but can also generate fiscal externalities through increased healthcare use. One of the rationales for social insurance programs, such as unemployment insurance (UI), is to financially support individuals facing unemployment and other adverse shocks (Chetty & Finkelstein, 2013; Diamond, 1977).

Although financial resources are strongly associated with health[2], it remains unclear whether UI can alleviate the negative health impacts of unemployment. Knowing whether UI affects healthcare use sheds light on whether the negative health impacts mainly reflect the decline in income after job loss or whether other factors that affect independently of income, such as social stigma or the loss of social contacts and identity (as emphasized by e.g., Jahoda, 1982) matter more. Any health-related fiscal externalities alleviated by UI should also be considered when determining the optimal level of unemployment insurance (e.g., Chetty, 2006). These fiscal externalities could be large because individuals typically only pay a small share of the total costs of the healthcare they receive and prescription drugs they purchase out-of-pocket.[3]

We study how the generosity of unemployment insurance affects benefit recipients' healthcare use using Swedish administrative data. Our research design uses variation in UI generosity created by benefit caps in a regression kink design. We find little evidence that more generous unemployment benefits affect healthcare use among people located close to kink points. This finding is robust to different specification choices, and we do not find heterogeneity in the effects on healthcare use across gender or age groups or between short-term and long-term UI recipients.

Our analysis builds on individual-level register data on unemployment spells,

[1] See e.g., Jahoda (1982), Dooley et al. (1996), Sullivan and Von Wachter (2009), Wanberg (2012), Brand (2015), and Gathmann et al. (2022). See Picchio and Ubaldi (2023) for a meta-analysis of the literature on health and unemployment.

[2] For reviews, see e.g. Cutler et al. (2012) and Lleras-Muney et al. (2024).

[3] For example, in 2016, out-of-pocket costs paid by households in OECD countries accounted for 6% of total inpatient care expenses, 18% of outpatient care expenses, and 25% of prescription drug expenditures (Organisation for Economic Co-operation and Development, 2019, Figure 2).

unemployment benefit payments, and healthcare use. Our sample consists of around 340,000 unemployment spells with a start date between March 5, 2005 and July 14, 2014. For each spell, we match information on weekly unemployment benefit payments and detailed register data on inpatient and outpatient care visits and prescription drug purchases.

Our primary outcome measure is the total cost of healthcare use, which captures the costs of inpatient and outpatient care visits and prescription drug purchases. For drug purchases, we observe out-of-pocket costs and total costs, including costs covered by prescription drug insurance. For inpatient and outpatient care use, we measure the total costs of the visits by combining information on the length and the Major Diagnostic Category (MDC) of the visit with data on the national average costs of inpatient and outpatient visits for each MDC. Our cost measure aims to capture the full costs of the resources used (medications, materials, operations, etc.) during the visit as well as underlying costs such as personnel and administrative costs.

To obtain exogenous variation in UI generosity, we use a regression kink (RK) design that exploits caps in the amount of daily benefits individuals can receive. This non-linear policy rule produces a kink in the relationship between daily benefits and the pre-unemployment daily wage at the point where the individual reaches the maximum benefit amount. Provided that individuals on both sides of the kink are similar in terms of other determinants of healthcare use, we can attribute any kinks in the relationship between healthcare use and daily wage to a causal effect of unemployment benefits on healthcare use. We provide evidence in favor of this assumption by showing that predicted outcomes, pre-determined covariates, healthcare use before unemployment, and the density of the daily wage evolve smoothly around kink points.

We find that the generosity of UI has little effect on the total costs of healthcare use among people located close to kink points. For example, over the first 100 days of the unemployment spell, the 95 percent confidence interval of our preferred specification can rule out changes in the total costs of healthcare use greater than 9 percent, changes in inpatient (outpatient) care costs greater than 13 (5) percent, and changes in total drug expenditures greater than 5 percent in response to a 1 percent increase in daily unemployment benefits. This conclusion is robust to different specification choices. Moreover, this lack of an effect on healthcare use holds for men and women, younger and older individuals, and short-term and long-term benefit recipients.

Related literature. Our findings contribute to a growing literature on the effects of social insurance programs on the health and healthcare use of the recipients and their family members (see, e.g. Levy & Meltzer, 2008; Sun et al., 2021, for reviews). Previous studies have analyzed the health and health expenditure effects of disability insurance[4], health insurance[5], pensions[6], and social assistance[7].

The literature on how UI affects the health of the unemployed is more limited. Closest to our paper, Kuka (2020) finds using between-state policy variation in the U.S. that more generous UI increased health insurance coverage and expenditures, while Ahammer and Packham (2023) find that a nine-week extension to potential UI benefit duration in Austria reduced opioid and antidepressant expenditures among women but not among men.

Relative to Kuka (2020) and Ahammer and Packham (2023), we use a more comprehensive healthcare cost measure that captures the full costs of inpatient and outpatient care use and drug purchases, including costs covered by the healthcare system. Our finding that more generous unemployment benefits have little effect on healthcare use contrasts with the findings of Kuka (2020) and Ahammer and Packham (2023), which could be because we use a different source of policy variation.

More broadly, our findings add to the large literature on the effects of job loss on labor market and health outcomes.[8] An important yet understudied question is to what extent unemployment insurance can mitigate the adverse health effects of job loss.[9] Our findings suggest that more generous unemployment benefits have little effect on recipients' healthcare use, at least in the Swedish context where the public healthcare system is relatively highly subsidized.

---

[4] See e.g., Black et al. (2024), Gelber et al. (2023), and Wikström (2024).

[5] See e.g., Card et al. (2009), Brot-Goldberg et al. (2017), and Goldin et al. (2021).

[6] See e.g., Salm (2011), Cheng et al. (2018), and Migliano et al. (2023).

[7] See e.g., Snyder and Evans (2006), Barham and Maluccio (2013), Aizer et al. (2016), and Hoynes et al. (2016).

[8] See e.g., Jacobson et al. (1993), Sullivan and Von Wachter (2009), Eliason and Storrie (2009), and Kuhn et al. (2009).

[9] An exception is Amorim et al. (2024) who find using a tenure-based RDD that access to UI partly offsets the effects of job loss on mortality and hospitalizations in Brazil.

Outline. The paper proceeds as follows. Section 3.2 gives an overview of the Swedish unemployment insurance and healthcare systems. Section 3.3 describes our data sources, analysis sample, and key variables. Section 3.4 discusses the research design. Section 3.5 presents the main findings. Section 3.6 concludes. Additional discussion and results are collected in an appendix.

3.2 Context

This section gives brief overviews of the unemployment insurance and healthcare systems in Sweden during our study period.

3.2.1 Unemployment Insurance

To qualify for unemployment insurance, individuals need to be registered at the Public Employment Service, fulfill a work history requirement, actively seek work, and be prepared to take a suitable offer for a job or a labor market program. Individuals can receive unemployment benefits for up to 300 days (60 weeks), after which they have to participate in active labor market programs to continue receiving benefits.

During our study period, statutory UI was provided by 27 UI funds that were typically affiliated with trade unions. Although contributions to the UI funds are voluntary, membership rates were relatively high, ranging from 70–83 percent of the labor force aged 16–64 (Inspektionen för arbetslöshetsförsäkringen [IAF], 2024b).[10]

Statutory UI consists of two parts. Basic benefits ("grundersättning") provide a fixed amount unrelated to the individual's pre-unemployment earnings. During our study period, basic benefits were equal to 320 SEK per day, or roughly 25–31 percent of the median wage.[11]

Our focus is on income-based benefits ("inkomstrelaterad ersättning") available to individuals who have contributed continuously to a UI fund in the

[10]Membership rates fell by more than 10 percentage points following reforms in 2007–2008 that first sharply increased membership premia and then introduced an additional "unemployment fee" that partly tied the premia of each fund to the average unemployment rate of members of that fund. Since 2007 membership rates have remained stable at around 70 percent, even though the unemployment fee was repealed in 2014. See Kolsrud (2018) and Landais et al. (2021).

[11]Own calculations based on data from Statistics Sweden (2024a).

twelve months before unemployment. Income-based benefits replace a fraction of the individual's pre-unemployment daily wage up to a cap. This policy rule creates a kink in the relationship between benefits and pre-unemployment earnings, which we use as the source of variation in UI generosity as discussed in Section 3.4.[12]

Appendix Table 3.1 summarizes the parameters of the income-based UI during our study period. The daily benefit amount was capped at 680 SEK per day, while the replacement rate was 80 percent for the first 200 payment days and 70 percent thereafter. The benefit cap was fairly low, replacing roughly 53–65 percent of the median monthly wage.[13]

3.2.2 Healthcare System

Sweden has a national healthcare system financed primarily by taxes and featuring a high degree of subsidization for healthcare visits and prescription drug purchases (e.g., Anell et al., 2012; Björvang et al., 2023).[14] For example, out-of-pocket costs paid by households accounted for 1 percent of total inpatient care expenditures, 14 percent of outpatient care expenditures, and 28 percent of prescription drug purchases in 2016 (Organisation for Economic Co-operation and Development, 2019, Figure 2).

Patient fees for healthcare visits are relatively low, being at most 100 SEK for inpatient visits and 350 SEK for specialized outpatient visits, and varying from 100–250 SEK across counties for primary care visits in 2017 (Pontén et al., 2017). All residents are automatically covered by a public and uniform prescription drug insurance scheme where the share of out-of-pocket costs declines with total yearly expenditures (see e.g., Wikström, 2023). Both patient and prescription drug fees have ceilings for out-of-pocket expenditures that reset 12 months after the first visit/purchase of the coverage period. In 2017, the

[12]In addition to statutory UI, most unions offer their own, non-statutory UI schemes that top up statutory UI for members who are eligible for statutory UI. Between 69 and 78 percent of the labor force belonged to a union during the period we study (Kjellberg, 2019, Table 3), while 70 percent of all union members were eligible for non-statutory UI in 2009 through their membership (Lindquist & Wadensjö, 2011, p. 17) Unfortunately, our data do not contain information on which union (if any) an individual belongs to or whether the individual is eligible for or receives non-statutory UI. We therefore only focus on statutory UI in our analysis.

[13]Own calculations based on data from Statistics Sweden (2024a).

[14]For over-the-counter drugs and drugs not covered by the reimbursement scheme, individuals generally pay the full price.

ceiling for prescription drug expenditures was 2200 SEK while the ceiling for patient fees ranged from 900–1100 SEK across counties (Pontén et al., 2017).

3.3 Data

This section describes our administrative data sources and how we define the analysis sample and key variables. Appendix 3.A describes how we measure key outcomes in more detail.

3.3.1 Administrative Data

Unemployment spells. Our analysis builds on administrative data on registered unemployment spells obtained from the *Hist_Aktso*, *Insper*, and *Sokatper* registers of the Swedish Public Employment Service (PES) (Arbetsförmedlingen [AF], 2024a, 2024b, 2024c). We observe information on the dates when the unemployment spell is registered and deregistered at the PES, transitions between different job seeker categories during the spell (open unemployment, participation in a given labor market program, etc.), and the reason for deregistering the spell.[15]

We define the start date of an unemployment spell as the date when the spell is registered by the PES. We consider the spell to end when the spell is deregistered by the PES (e.g., due to finding employment, exiting from the labor force or to another social insurance program, or starting an education program not offered by the PES).[16]

Unemployment benefit payments. To each spell, we merge data on weekly unemployment benefit payments from the *ASTAT* database of the Swedish Unemployment Insurance Inspectorate (IAF, 2024a). For each payment week, we observe the number of payment days, the daily benefit amount, the pre-unemployment daily wage used as the basis for the benefit payments, and the

[15] Appendix Table 3.2 summarizes how we map job seeker categories and deregistration codes to employment, unemployment, participation in a labor market program, other individuals registered at the PES, and individuals deregistered from the PES.

[16] How we define the end of an unemployment spell differs slightly from Kolsrud et al. (2018), who define a spell as ending if the person finds any type of employment (including subsidized employment) or begins an active labor market program while still receiving unemployment benefits and being registered at the PES.

scheme (basic vs. income-based benefits) under which the unemployment benefit payments were made.

We aggregate the resulting matched "unemployment spell $\times$ unemployment benefit payment" data to the unemployment spell level. For each spell, we keep information on the daily wage and calculate the average daily unemployment benefit amount and the average replacement rate (i.e. average daily benefits divided by the daily wage) for three different periods: (i) the first 100 payment days, (ii) payment days 101–200 (if the person still receives benefits), and (iii) payment days 201–300 (if the person still receives benefits).

Socioeconomic background. For each unemployment spell, we add information on the individual's socioeconomic background (age, gender, educational attainment, whether the person is married or cohabiting, having children under age 18 at home, county of residence, and industry of highest-paying employer if s/he had any) using data from the *Longitudinal Integrated Database for Health Insurance and Labour Market Studies* (LISA), *Total Population Register* (RTB), and *Register-Based Labor Market Statistics* (RAMS) databases of Statistics Sweden (2022, 2023a, 2023b). We measure these covariates at the end of the calendar year before the start of the unemployment spell and use them to assess the validity and robustness of our regression kink design in Section 3.5.

Healthcare utilization. Our measures of healthcare use come from two registers of the National Board of Health and Welfare (Socialstyrelsen). First, we obtain data on inpatient care and outpatient care visits from the *National Patient Register* (Socialstyrelsen, 2022b, 2022c). Primary care visits are not included. For each visit, we observe dates of admission and discharge (the latter only for inpatient care) and the associated Major Diagnostic Category (MDC). Second, we obtain data on prescription drug purchases from outpatient pharmacies from the *National Prescribed Drug Register* (Socialstyrelsen, 2022d). For each purchase, we observe the purchase date and the disaggregated total costs of the purchase.

3.3.2 Sample Definition

Our analysis uses data on the universe of unemployment spells with a start date between March 5, 2007 and July 14, 2014.[17] We restrict attention to spells where the individual had turned age 20-64 in the calendar year before the start of the spell because eligibility for income-based unemployment benefits begins after turning age 20 and ends after turning age 65. We exclude spells during which the individual only receives basic benefits, restrict attention to those with a pre-unemployment daily wage between 150 SEK and 1,800 SEK, and focus on the first 60 calendar weeks of the unemployment spell.[18] We exclude spells for which we are not able to match information on socioeconomic characteristics, except for employer industry which we allow to be missing.

To analyze whether the effects of UI generosity vary between short-term and long-term benefit recipients, we estimate the effects separately for three sub-periods: (i) during the first 100 payment days (using all spells), (ii) during the next 100 payment days (i.e. payment days 101–200, using the 44.3 percent of spells where individuals still receive benefits after 100 payment days), and (iii) during payment days 201–300 (using the 22.0 percent of spells where individuals still receive benefits after 200 payment days). For the first two sub-periods, the kink point at which individuals reach maximum daily benefits was at a daily wage of 850 SEK, while during the third sub-period, it was at 971.43 SEK (see Appendix Table 3.1). For each of these periods, we measure healthcare use over the entire period, regardless of whether the individual receives UI during the whole period.[19]

3.3.3 Variable Definitions

We use the administrative registers to construct the main variables for our analysis. Unless stated otherwise, we deflate all cost variables with the overall consumer price index (Statistics Sweden, 2024b) using 2020 as the reference year.

[17]We focus on this period because it is the longest one during which the rules of the income-based UI scheme remained unchanged and for which we can measure our main outcomes.

[18]As noted in Section 3.2, unemployment benefits are initially granted for a maximum of 300 payment days (60 payment weeks).

[19]That is, for someone who e.g. only receives UI for 20 days (4 weeks) and then exits unemployment, we measure their healthcare use over the first 100 days (20 weeks).

Daily wage and daily benefit. We observe the daily wage and daily benefits, two main variables used in our research design, directly in the administrative data. The daily wage is calculated by PES employees based on the individual's earnings history before the unemployment spell, after which the daily benefit amount is determined based on the daily wage and the number of payment days the individual has used up during the unemployment spell. We measure both variables in nominal terms because unemployment benefits are not indexed.

Inpatient and outpatient care use. Our first measure of healthcare use measures *the number and total costs of inpatient and outpatient care visits* that the individual has over a given period. We compute the costs of a visit by combining information on a visit's MDC code with information on the average per-day costs of inpatient and outpatient care visits associated with that MDC.[20] We measure these costs using data collected from the Swedish Association of Local Authorities and Regions (Sveriges Kommuner och Regioner [SKR], 2023) and the National Board of Health and Welfare (Socialstyrelsen, 2023), using 2020 as the reference year (see Appendix 3.A for details).[21] Appendix Table 3.3 shows all 29 MDC codes used during our study period along with their average per-day costs, separately for inpatient and outpatient care visits.

Our cost measure captures the broad costs of resources used during the healthcare visits that the individual has over a given period. National guidelines stress that regions should attribute costs as closely as possible to a unique patient and healthcare visit. Relevant costs include costs of operations and procedures (surgeries, intensive care unit, X-rays, radiology, anesthesia, etc.), lab examinations, and costs of medications and materials, but also underlying costs such as those related to management, administration, facilities, and other

[20]Major Diagnostic Categories are groupings of Diagnosis Related Groups (DRG), which in turn group healthcare visits to categories deemed similar in terms of resource use and hence costs based on diagnoses, operations, and patient characteristics such as age and gender. The DRG system is also used to monitor the cost-effectiveness and resource allocation of the healthcare system in many countries, including Sweden and the United States. See Socialstyrelsen (2022a).

[21]We calculate the average per-day costs using information on costs per DRG point (which measures the average overall costs of providing a unit of care), the average length of stay of inpatient and outpatient care visits for each MDC, and average weights for each MDC. We use the weights to scale costs per DRG point to get the average costs of inpatient and outpatient care visits for each MDC code and then divide by the average length of stay to arrive at average per-day costs. We calculate all averages at the national level. See Appendix 3.A for details.

support functions. See SKR (2020) for a detailed discussion of the principles and guidelines for the cost calculation.[22]

Drug purchases. As our second measure of healthcare use, we measure *the costs of drugs purchased* by the individual over a given period. We distinguish between the total costs of the purchased drugs, the out-of-pocket costs paid by the individual, and the costs covered by prescription drug insurance.

3.3.4 Summary Statistics

Table 3.1 presents descriptive statistics for the analysis sample as well as the Swedish population aged 20–64. The analysis sample includes 340,955 unemployment spells affecting 320,592 individuals. Relative to the population, individuals in our sample are younger, less likely to be married or cohabiting, less likely to have higher education, more likely to have worked in the manufacturing sector, and have similar gross earnings in the previous calendar year. However, their costs of healthcare utilization and drug purchases are somewhat lower than for the population.

3.4 Empirical Strategy

Intuition. We use a regression kink (RK) design that exploits variation in the generosity of unemployment benefits created by the non-linear relationship between benefits and pre-unemployment earnings. Under the income-based UI scheme, the daily benefit amount is a piecewise linear function of the pre-unemployment daily wage, replacing a constant fraction of the daily wage up to a maximum.

This non-linear policy rule produces a kink in the relationship between daily benefits and pre-unemployment daily wage at the wage at which the individual reaches the benefit cap.[23] Provided that individuals on either side of

[22] A drawback of our measure is that the MDC codes are coarse since they group the roughly 800 DRG codes used in inpatient care and the roughly 600 DRG codes used in outpatient care to only 29 categories. Unfortunately, we do not observe the DRG codes associated with inpatient and outpatient care visits in our data.

[23] Put another way, the replacement rate (daily benefits divided by daily wage) stays constant as a function of the daily wage until it starts falling once the daily wage exceeds the wage at which the benefit cap is reached.

Table 3.1: Descriptive Statistics

	Analysis sample					Population 20–64 yo.				
A. Socioeconomic status, previous calendar year	Mean	Std. Dev.	P5	P50	P95	Mean	Std. Dev.	P5	P50	P95
Age	39.07	11.93	22.00	38.00	60.00	41.87	12.91	22.00	42.00	62.00
Share female	0.45	0.50	0.00	0.00	1.00	0.49	0.50	0.00	0.00	1.00
Share married or cohabiting	0.35	0.48	0.00	0.00	1.00	0.41	0.49	0.00	0.00	1.00
Share with children under age 18	0.38	0.49	0.00	0.00	1.00	0.37	0.48	0.00	0.00	1.00
Share with higher education	0.28	0.45	0.00	0.00	1.00	0.37	0.48	0.00	0.00	1.00
Share in manufacturing sector	0.23	0.42	0.00	0.00	1.00	0.11	0.31	0.00	0.00	1.00
Gross earnings (kSEK)	257.00	127.27	2.18	272.53	446.17	247.43	248.32	0.00	251.64	599.10
B. Unemployment spell										
Spell duration (weeks)	41.56	20.68	6.00	52.57	60.00					
Avg. replacement rate	0.67	0.13	0.45	0.67	0.80					
C. Health-related outcomes, previous 12 months										
Total costs of healthcare use (SEK)	14124.21	68144.08	0.00	671.49	62733.14	18735.18	122126.54	0.00	722.12	75671.18
Inpatient and outpatient care										
Total costs (SEK)										
In-/outpatient care	12070.02	65016.16	0.00	0.00	54968.06	16136.83	118521.85	0.00	0.00	63464.38
Inpatient care	8141.18	61731.43	0.00	0.00	38489.71	12059.01	115542.91	0.00	0.00	52929.83
Outpatient care	3928.84	9374.83	0.00	0.00	19270.27	4077.82	12180.13	0.00	0.00	19746.09
Number of visits										
In-/outpatient care	1.38	4.54	0.00	0.00	6.00	1.60	8.58	0.00	0.00	7.00
Inpatient care	0.43	3.35	0.00	0.00	2.00	0.59	7.67	0.00	0.00	3.00
Outpatient care	0.96	2.30	0.00	0.00	5.00	1.00	2.84	0.00	0.00	5.00
Drug purchases (SEK)										
Total costs	2054.19	15864.70	0.00	228.13	7654.33	2598.35	22356.70	0.00	251.28	10059.63
Benefit costs	1367.84	14924.50	0.00	0.00	5442.91	1870.02	21532.45	0.00	0.00	7788.72
Out-of-pocket costs	686.63	4704.52	0.00	215.73	2164.10	728.65	5046.66	0.00	236.38	2252.09
Observations			340,955					44,059,580		
Individuals			320,592					6,745,753		

Notes. This table provides descriptive statistics of selected variables for the analysis sample and the Swedish population aged 20–64 for the years 2007–2014. For the analysis sample, the unit of observation is an unemployment spell. For the population, the unit of observation is a person-year. Panel A shows statistics for selected socioeconomic covariates, measured in the previous (analysis sample) or the same calendar year (population). Gross earnings refer to the sum of salary and self-employment income. Panel B shows statistics related to the unemployment spell, only for the analysis sample. We cap the duration of the unemployment spell at 60 weeks since we do not analyze healthcare use beyond the first 60 weeks since the start of the spell. The average replacement rate refers to the overall replacement rate over the first 60 weeks of the unemployment spell. Panel C shows statistics for healthcare use (inpatient and outpatient care visits and drug purchases) over the last 365 days before the start of the unemployment spell (analysis sample) or over the previous calendar year (population). Total costs of healthcare use refer to the sum of the total costs of inpatient and outpatient care visits and drug purchases. Total costs of drug purchases refer to the sum of out-of-pocket costs and costs covered by prescription drug insurance. Earnings and costs are deflated using the overall CPI with 2020 as the reference year.

the kink are "similar", we can attribute any kinks observed in the relationship between outcomes of interest (say, costs of healthcare use) and the daily wage to a causal effect of more generous unemployment benefits. We discuss the assumptions necessary for causal interpretation in detail below.

We use a fuzzy instead of a sharp RK design because actual unemployment benefit payments do not in practice perfectly align with payments predicted by the policy rule. Apart from measurement error, this non-compliance arises because individuals may be subject to sanctions (payment suspensions) due to e.g. inactive job search or failing to apply to a suitable job (Act 1997:238 §43, cf. Svensk författningssamling [SFS], 1997).[24]

Identification. Following Card et al. (2015), we consider the non-separable model

$$Y = y(B^*, W^*, U),$$

where Y is an outcome of interest, B^* is the observed daily benefit amount(the treatment variable), W^* is the observed pre-unemployment daily wage (the running variable), and U is a potentially multidimensional error term. We are interested in the causal effect of a small increase in benefits B^* on the outcome Y, that is, on the partial derivative $\frac{\partial y(B^*,W^*,U)}{\partial B^*}$ of Y with respect to B^*.

Under perfect compliance, received benefits B^* would be determined by the policy rule $\rho \min(W, \overline{w})$, where W is the actual daily wage, ρ is the replacement rate, and $\overline{w} = \overline{b}/\rho$ is the daily wage at which individuals reach the maximum benefit amount $\overline{b}$. However, due to potential non-compliance, observed benefit payments may differ from predicted payments,

$$B^* = b(W, \varepsilon),$$

where the vector ε allows for non-compliant behavior and is potentially correlated with U and hence Y. Similarly, we allow for measurement error in the daily wage, that is, $W^* = W + e$ for e an error term.

[24]The administrative data on benefit payments are drawn from a system where UI fund employees report payments made to the unemployed and the daily wage used as the basis for these payments, so measurement errors should be minimal. Payment suspensions and reductions are rare as well: 0.54 percent of the unemployment spells in our analysis data are such that the individual faces a suspension or reduction in unemployment benefit payments at least once.

Our parameter of interest is the fuzzy RK estimand

$$\tau = \frac{\beta^+ - \beta^-}{\kappa^+ - \kappa^-} = \frac{\lim_{w_0 \to \overline{w}^+} \frac{d\mathrm{E}[Y|W^*=w^*]}{dw^*}\Big|_{w^*=w_0} - \lim_{w_0 \to \overline{w}^-} \frac{d\mathrm{E}[Y|W^*=w^*]}{dw^*}\Big|_{w^*=w_0}}{\lim_{w_0 \to \overline{w}^+} \frac{d\mathrm{E}[B^*|W^*=w^*]}{dw^*}\Big|_{w^*=w_0} - \lim_{w_0 \to \overline{w}^-} \frac{d\mathrm{E}[B^*|W^*=w^*]}{dw^*}\Big|_{w^*=w_0}}, \tag{3.1}$$

where β^+ and β^- are the slopes of the conditional mean of Y to the right and to the left of the kink point $\overline{w}$, and κ^+ and κ^- are the slopes of the conditional mean of B^* to the right and the left of the kink point. That is, the RK estimand (3.1) is equal to the change in the slope of the conditional mean of the outcome Y at the kink point, divided by the change in the conditional mean of the treatment variable B at the kink point.

Card et al. (2015, Section 2.2.2. and Proposition 2) provide conditions sufficient for the RKD estimand (3.1) to identify a weighted average of marginal effects of B on Y, with larger weights on groups with larger kinks in B at the cutoff, groups more likely to be at the cutoff, and groups with less measurement error in the assignment variable B (i.e., less non-compliance). In addition to certain regularity conditions, identification relies on three key assumptions.

1. *First stage.* The average replacement rate (slope of the conditional mean of the treatment variable B) changes at the kink point and there is a non-negligible population at the kink point $\overline{w}$.

2. *Monotonicity.* The direction of the kink in the assignment variable is the same for the whole population, that is, $\lim_{w_0 \to \overline{w}^+} \frac{\partial b(w,e)}{\partial w} \leq \lim_{w_0 \to \overline{w}^-} \frac{\partial b(w,e)}{\partial w}$ for all e.

3. *Smooth density of W.* The density of the actual daily wage W, conditional on the vector of unobserved heterogeneity (U, ε), is continuously differentiable in a neighborhood of the kink point $\overline{w}$.

We test for the existence of a first-stage kink to assess the first two assumptions (Card et al. 2015, Remark 4). The third assumption rules out deterministic sorting just above or below the kink point $\overline{w}$. We assess the validity of this assumption in Section 3.5 by checking for discontinuities and kinks in the densities of the daily wage W^* and in the conditional means of pre-determined covariates around the cutoff $\overline{w}$ (Card et al. 2015, Corollaries 1–2). We also check for any kinks in our outcome variables when measured before the start of the unemployment spell.

Estimation and inference. Following the standard in the literature, we implement the fuzzy RK design via local polynomial estimation (e.g., Card et al., 2015, 2017; Gelber et al., 2023). The fuzzy RK estimator of τ in (3.1) is

$$\hat{\tau} = \frac{\hat{\beta}_1^+ - \hat{\beta}_1^-}{\hat{\kappa}_1^+ - \hat{\kappa}_1^-}, \tag{3.2}$$

where $\hat{\beta}_1^s$ and $\hat{\kappa}_1^s$ for $s \in \{+,-\}$ solve the least squares problems

$$\hat{\beta}^s = \min_{\{\tilde{\beta}_j^s\}} \sum_{i=1}^{n^s} \left\{ Y_i^s - \sum_{j=0}^{p} \tilde{\beta}_j^s (W_i^{*s} - \overline{w})^j \right\}^2 K\left(\frac{W_i^{*s} - \overline{w}}{h}\right),$$
$$\hat{\kappa}^s = \min_{\{\tilde{\beta}_j^s\}} \sum_{i=1}^{n^s} \left\{ B_i^{*s} - \sum_{j=0}^{p} \tilde{\beta}_j^s (W_i^{*s} - \overline{w})^j \right\}^2 K\left(\frac{W_i^{*s} - \overline{w}}{h}\right),$$

where $s = -$ denotes quantities to the left and $s = +$ to the right of the kink point, p is polynomial order, K is the kernel function, and h is the bandwidth.

Following Card et al. (2017) and Gelber et al. (2023), our baseline estimates are based on a local linear estimator[25] ($p = 1$), a uniform kernel[26] (i.e., $K(c) = \frac{1}{2}1\{|c| < 1\}$), and a mean squared error (MSE) optimal bandwidth (Calonico et al., 2014a, 2014b).[27] In Section 3.5 we probe the sensitivity of our estimates to the choice of bandwidth, polynomial order, and kernel. We also compare estimates with and without adjusting for pre-determined covariates following the approach of Calonico et al. (2019).[28]

[25]For example, Pei et al. (2022) find in Monte Carlo simulations using data on Austrian UI recipients that a local linear specification has a smaller asymptotic mean squared error than a local quadratic specification for sample sizes up to 86 million observations. Our sample size is considerably smaller than this.

[26]We prefer a uniform kernel over the boundary-optimal triangular kernel because the asymptotic bias and variance of the RK estimator (3.2) with a uniform kernel are not affected by imposing continuity (Card et al., 2012).

[27]We follow Gelber et al. (2023) and omit the regularization term of the Calonico et al. (2014a) MSE-optimal bandwidth selector since Card et al. (2015, 2017) argue it tends to pick too small bandwidths in RK settings.

[28]While covariate adjustment is not necessary for RK estimates to be consistent, Ando (2017) argues that including covariates can improve the efficiency when the relationship between the running variable and the dependent variable is non-linear.

We present both conventional RK estimates and bias-corrected RK estimates that correct for the asymptotic bias of the RK estimator under an MSE-optimal bandwidth (see Calonico et al., 2014b). We only rely on bias-corrected estimates for statistical inference. Bias-corrected estimates are based on a quadratic bias estimator and robust standard errors that account for sampling variation in the bias estimator. We cluster standard errors at the individual level.

Estimates of interest. The fuzzy RK estimator $\tilde{\tau}$ in (3.2) is equal to the estimated change in the slope of average outcomes at the kink point (the reduced form) divided by the estimated change in the slope of average received benefits at the kink point (the first stage). We report estimates for the fuzzy RK parameter $\hat{\beta}_1^+ - \hat{\beta}_1^-$ and the first stage $\hat{\kappa}_1^+ - \hat{\kappa}_1^-$. The first stage estimate tells how average daily benefits change in response to a 1 SEK increase in the daily wage. The fuzzy RK estimate $\hat{\tau}$ tells how the outcome changes on average in response to a 1 SEK increase in daily benefits B^*.

We also report the estimated elasticity of the outcome Y with respect to unemployment benefits B at the kink point k,

$$\hat{\varepsilon}_{Y,B} = \hat{\tau} \times \frac{\overline{B^*}}{\overline{Y}}, \tag{3.3}$$

where $\overline{Y}$ and $\overline{B^*}$ are the means of the outcome Y and observed benefits B^* around the kink (using observations with a daily wage within 10 SEK of the kink). The estimated elasticity gives the percent change in the outcome per a 1 percent increase in daily benefits. For elasticities, we compute standard errors via a non-parametric bootstrap with 100 replicates where we sample unemployment spells with replacement.

3.5 Results

3.5.1 First Stage

We start by verifying that the regression kink design works in our setting. Figure 3.1 plots the average replacement rate (left column) and average daily benefit (right column) as a function of the daily wage, using a bandwidth of 250

SEK and 5 SEK bins. Panel A shows these relationships during the first 100 payment days of the spell, Panel B for payment days 101–200 (among those still receiving benefits after the first 100 payment days), and Panel C for payment days 201–300 (among those still receiving benefits after the first 200 payment days). Red lines in each plot illustrate the relationship between the variables predicted by the policy rules (cf., Appendix Table 3.1). For all three groups, it is apparent from Figure 3.1 that observed average replacement rates and daily benefits closely follow those predicted by the policy rule, indicating that non-compliance and measurement error in daily benefits or the daily wage are not an issue.

3.5.2 Main Results

Total costs of healthcare use. We now turn to our main results. Panel A of Figure 3.2 shows how the total costs of the UI recipient's healthcare use evolve around the daily wage kink. The outcome of interest is the sum of the total costs of inpatient and outpatient care visits and drug purchases, measured separately during the first 100 payment days (left column), payment days 101–200 (middle column), and payment days 201–300 (right column).

None of the plots in Panel A indicate discontinuous changes in the slope of average total healthcare costs at the kink points. Panel A of Table 3.2 confirms this, showing that an increase in daily unemployment benefits has no statistically significant effect on total healthcare costs. For example, during the first 100 payment days, the 95 percent confidence intervals for the bias-corrected coefficient without covariate adjustment (Table 3.2, Panel A, column 3) rule out decreases or increases greater than 1.4 percent (9.3 percent) of the average total healthcare costs around the kink in response to a 1 SEK (1 percent) increase in daily unemployment benefits. While these bounds are admittedly wide, they still suggest that potential fiscal externalities, via reductions in recipients' healthcare use that could offset some of the costs from increasing unemployment benefits, are not substantial.

Panel B of Figure 3.2 and Appendix Table 3.4 further show that the lack of an effect on total healthcare use also holds when only focusing on the extensive margin, i.e. whether the recipients had any inpatient or outpatient visits or made drug purchases. It is hard to distinguish between non-linearities and

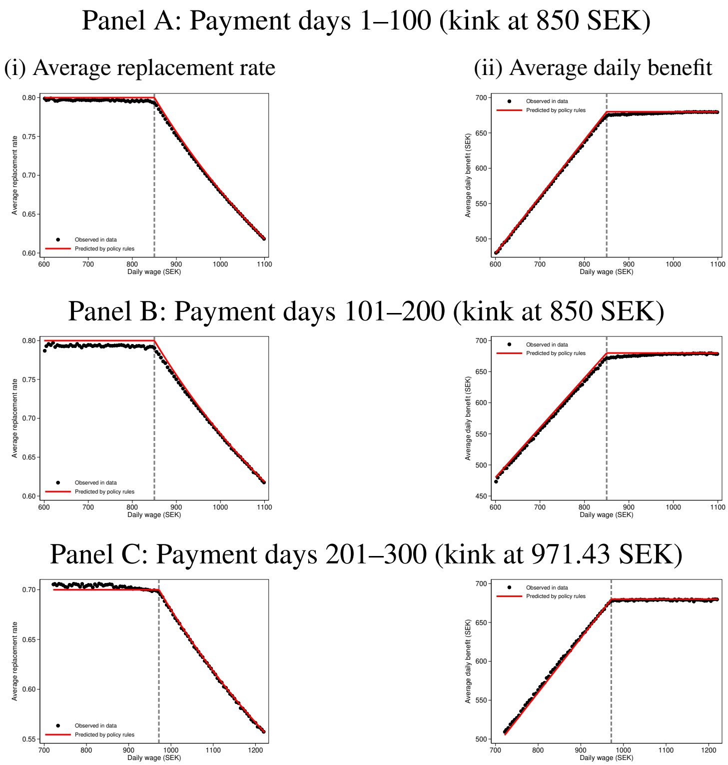


Figure 3.1: Replacement Rate and Daily Benefit as a Function of the Daily Wage

Notes. This figure illustrates our research design by showing the average replacement rate (left column) and average daily benefit (right column) as a function of the daily wage, our running variable. The figure uses our analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). Panel A shows these relationships during the first 100 unemployment benefit payment days of the spell, Panel B shows them during payment days 101–200, and Panel C shows them during payment days 201–300. Panels B and C only use individuals who still receive unemployment benefits after 100 and 200 payment days, respectively. In each panel, the unit of observation is an unemployment spell. The panels show binned scatterplots of the outcomes against the running variable using a bandwidth of 250 SEK and 5 SEK bins. Red kinked lines show the relationships between the daily wage, the replacement rate, and the daily benefit predicted by the policy rules (see Appendix Table 3.1).

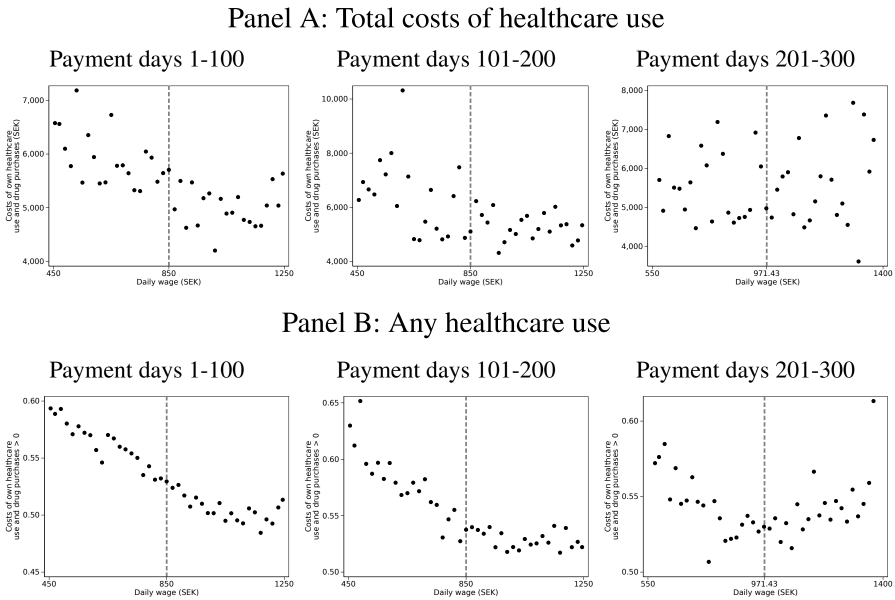


Figure 3.2: Total Healthcare Use Around Daily Wage Kinks

Notes. This figure shows binned scatterplots of total costs of healthcare use as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (Panel A) and an indicator for having any healthcare use, i.e., total costs of healthcare use being greater than zero (Panel B). We show plots separately for payment days 1-100 (left column), payment days 101–200 (middle column), and payment days 201–300 (right column) of the unemployment spell. In each column, the unit of observation is an unemployment spell.

actual kinks in the binned scatterplots from Figure 3.2B, and the estimates in Appendix Table 3.4 indeed do not indicate evidence for discontinuous slope changes. For example, for the first 100 payment days, the 95 percent confidence interval for bias-corrected estimates without covariate adjustment rules out changes greater than 0.2 percentage points in the probability of having any healthcare use in response to a 1 percent increase in daily benefits (Appendix Table 3.4, column 3).

Inpatient and outpatient care use. The lack of an effect on the total costs of healthcare use could be the result of opposing effects on inpatient care visits, outpatient care visits, and drug purchases that cancel out. However, Figure

Table 3.2: Effect of Unemployment Benefits on Costs of Healthcare Use

Panel A: Total costs of healthcare use

	Payment days 1–100				Payment days 101–200				Payment days 201–300			
	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction
First stage estimates												
Change in daily benefits per 1 SEK daily wage	-0.7651***	-0.7290***	-0.7730***	-0.7206***	-0.7200***	-0.6847***	-0.7191***	-0.6845***	-0.6476***	-0.6196***	-0.6493***	-0.6177***
	(0.00183)	(0.01817)	(0.00094)	(0.02648)	(0.00825)	(0.01535)	(0.00847)	(0.01561)	(0.00682)	(0.01497)	(0.00677)	(0.01388)
Fuzzy RK estimates												
Change in outcome per 1 SEK daily benefits	2.4876	-7.2740	2.0589	-14.6284	10.0280	5.8231	10.5558	11.8275	17.5724	-10.9905	21.6202	22.4608
	(4.18161)	(35.80053)	(2.32468)	(52.11864)	(15.76520)	(29.88501)	(16.22965)	(30.71649)	(23.45872)	(49.73411)	(21.55843)	(45.99221)
Implied elasticity												
% Change in outcome per 1% change in daily benefits	0.2930	-0.8567	0.2425	-1.7229	1.3142	0.7631	1.3834	1.5501	2.4570	-1.5367	3.0230	3.1405
	(0.48064)	(4.30989)	(0.26483)	(6.23017)	(1.81939)	(3.48526)	(1.87844)	(3.56436)	(3.04084)	(7.11332)	(2.66972)	(6.20263)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00	850.00	850.00	850.00	971.43	971.43	971.43	971.43
Covariates			✓	✓			✓	✓			✓	✓
Implied change (%)	0.044	-0.127	0.036	-0.256	0.196	0.114	0.206	0.231	0.363	-0.227	0.447	0.464
Outcome mean around kink	5716	5716	5716	5716	5117	5117	5117	5117	4840	4840	4840	4840
Bandwidth	193.6	193.6	297.7	297.7	103.6	103.6	102.1	102.1	101.7	101.7	106.2	106.2
Number of observations	154,927	154,927	221,254	221,254	36,855	36,855	36,356	36,356	23,701	23,701	24,661	24,661

Panel B: Total costs of inpatient and outpatient care visits

	Payment days 1–100				Payment days 101–200				Payment days 201–300			
	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction
First stage estimates												
Change in daily benefits per 1 SEK daily wage	-0.7653***	-0.7304***	-0.7645***	-0.7342***	-0.7198***	-0.6846***	-0.7256***	-0.6845***	-0.6484***	-0.6352***	-0.6489***	-0.6213***
	(0.00165)	(0.01980)	(0.00190)	(0.01833)	(0.00861)	(0.01612)	(0.00658)	(0.01556)	(0.00686)	(0.01594)	(0.00604)	(0.01501)
Fuzzy RK estimates												
Change in outcome per 1 SEK daily benefits	2.3915	-18.5870	4.7233	-15.3177	7.6006	6.4129	19.7918	15.6797	25.0616	-8.5124	17.4069	-11.7158
	(3.87616)	(38.46410)	(4.12044)	(35.47798)	(16.04975)	(30.77558)	(12.33141)	(29.34868)	(21.11762)	(54.68882)	(19.45588)	(48.68769)
Implied elasticity												
% Change in outcome per 1% change in daily benefits	0.3263	-2.5360	0.6445	-2.0900	1.2174	1.0271	3.1700	2.5114	4.2288	-1.4364	2.9372	-1.9769
	(0.53876)	(5.63139)	(0.53409)	(5.17813)	(2.34586)	(4.53634)	(1.80231)	(4.25023)	(3.10165)	(9.17508)	(2.89927)	(7.85229)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00	850.00	850.00	850.00	971.43	971.43	971.43	971.43
Covariates			✓	✓			✓	✓			✓	✓
Implied change (%)	0.048	-0.377	0.096	-0.310	0.182	0.153	0.473	0.375	0.625	-0.212	0.434	-0.292
Outcome mean around kink	4934	4934	4934	4934	4187	4187	4187	4187	4011	4011	4011	4011
Bandwidth	205.6	205.6	189.2	189.2	101.2	101.2	119.1	119.1	105.9	105.9	114.7	114.7
Number of observations	163,578	163,578	151,682	151,682	36,030	36,030	42,192	42,192	24,582	24,582	26,423	26,423

Panel C: Total costs of drug purchases

	Payment days 1–100				Payment days 101–200				Payment days 201–300			
	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction
First stage estimates												
Change in daily benefits per 1 SEK daily wage	-0.7572***	-0.7465***	-0.7560***	-0.7443***	-0.7148***	-0.6899***	-0.7215***	-0.6905***	-0.6529***	-0.6410***	-0.6407***	-0.6220***
	(0.00372)	(0.00691)	(0.00476)	(0.00882)	(0.01023)	(0.01374)	(0.00772)	(0.01278)	(0.00351)	(0.02182)	(0.00862)	(0.01509)
Fuzzy RK estimates												
Change in outcome per 1 SEK daily benefits	0.5637	1.6267	0.9223	3.2372	0.5664	2.4992	1.9713	3.4096	0.1240	-0.6230	1.1873	-0.1045
	(1.31760)	(2.44108)	(1.63324)	(3.18514)	(2.96802)	(4.03370)	(2.23202)	(3.70193)	(1.55773)	(11.40109)	(3.92863)	(7.36216)
Implied elasticity												
% Change in outcome per 1% change in daily benefits	0.4854	1.4009	0.7942	2.7878	0.4084	1.8019	1.4213	2.4582	0.1012	-0.5083	0.9688	-0.0853
	(1.03641)	(1.84737)	(1.26544)	(2.47315)	(1.98426)	(2.72556)	(1.69071)	(2.77881)	(1.32158)	(9.54101)	(3.25914)	(6.25653)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00	850.00	850.00	850.00	971.43	971.43	971.43	971.43
Covariates			✓	✓			✓	✓			✓	✓
Implied change (%)	0.072	0.208	0.118	0.414	0.061	0.269	0.212	0.367	0.015	-0.075	0.143	-0.013
Outcome mean around kink	782	782	782	782	930	930	930	930	829	829	829	829
Bandwidth	121.2	121.2	105.6	105.6	90.4	90.4	107.6	107.6	153.0	153.0	79.3	79.3
Number of observations	100,096	100,096	87,719	87,719	32,142	32,142	38,261	38,261	33,632	33,632	18,851	18,851

Notes. This table presents coefficients and standard errors of the effect of UI benefits on the costs of healthcare use. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification, a uniform kernel, and MSE-optimal bandwidths following Calonico et al. (2014b). We report conventional estimates (columns labeled "Local linear") and estimates with quadratic bias correction and robust standard errors (columns labeled "+ Bias correction"), with and without controlling for pre-determined covariates. Standard errors are clustered at the individual level. Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (Panel A), total costs of inpatient and outpatient care visits (Panel B), and total costs of drug purchases (Panel C). We show estimates separately for payment days 1–100 (columns 2-5), payment days 101–200 (columns 6-9), and payment days 201–300 (columns 10–13) of the unemployment spell. For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, and rows 5–6 show the implied elasticity. For elasticities, we obtain standard errors via a non-parametric bootstrap where we sample unemployment spells with replacement. Row 7 indicates the kink point, row 8 indicates whether covariates are included, and row 9 expresses the fuzzy RK estimate as a percent of the outcome mean around the kink. The last three rows show the outcome sample mean around the kink (using observations within 10 SEK of the kink), the MSE-optimal bandwidth, and the number of observations within the bandwidth.

3.3 indicates that more generous unemployment benefits do not affect the total costs of inpatient and outpatient care visits. The outcomes of interest are the total costs (Panel A) and the number (Panel B) of inpatient and outpatient care visits as well as an indicator for having any inpatient or outpatient care visits (Panel C), measured separately during the first 100 payment days (left column), payment days 101–200 (middle column), and payment days 201–300 (right column). None of the plots indicate clear discontinuous changes in the slopes of the outcomes at the kink points. The same can be seen from similar binned scatterplots that focus separately on inpatient care (Appendix Figure 3.1) and outpatient care (Appendix Figure 3.2).

Panel B of Table 3.2 and Appendix Tables 3.5, 3.6, and 3.7 corroborate the graphical evidence discussed above. Whether measured by total costs, number of visits, or at the extensive margin (having any visits), the bias-corrected estimates, with or without covariate adjustment, do not show statistically significant increases or decreases in inpatient or outpatient care use in response to an increase in unemployment benefits

Drug purchases. Figure 3.4 shows how drug purchases evolve around the daily wage kink points. The outcomes of interest are the total costs of drug purchases (Panel A) and an indicator for any drug purchases (Panel B), measured separately during the first 100 payment days (left column), payment days 101–200 (middle column), and payment days 201–300 (right column). As noted in Section 3.3, total costs include both out-of-pocket costs as well as costs covered by prescription drug insurance.

Although some kinks may be discernible in the binned scatterplots of Figure 3.4 (e.g., at the extensive margin for payment days 101–200, middle column of Panel B), the corresponding point estimates in Panel C of Table 3.2 and Appendix Table 3.8 and do not show statistically significant slope changes at the kinks, either in terms of total purchase costs or at the extensive margin (making any purchases).

3.5.3 Robustness and Validity Checks

Here we provide support for the validity of the RK design by summarizing results from a series of validity tests and sensitivity analyses.

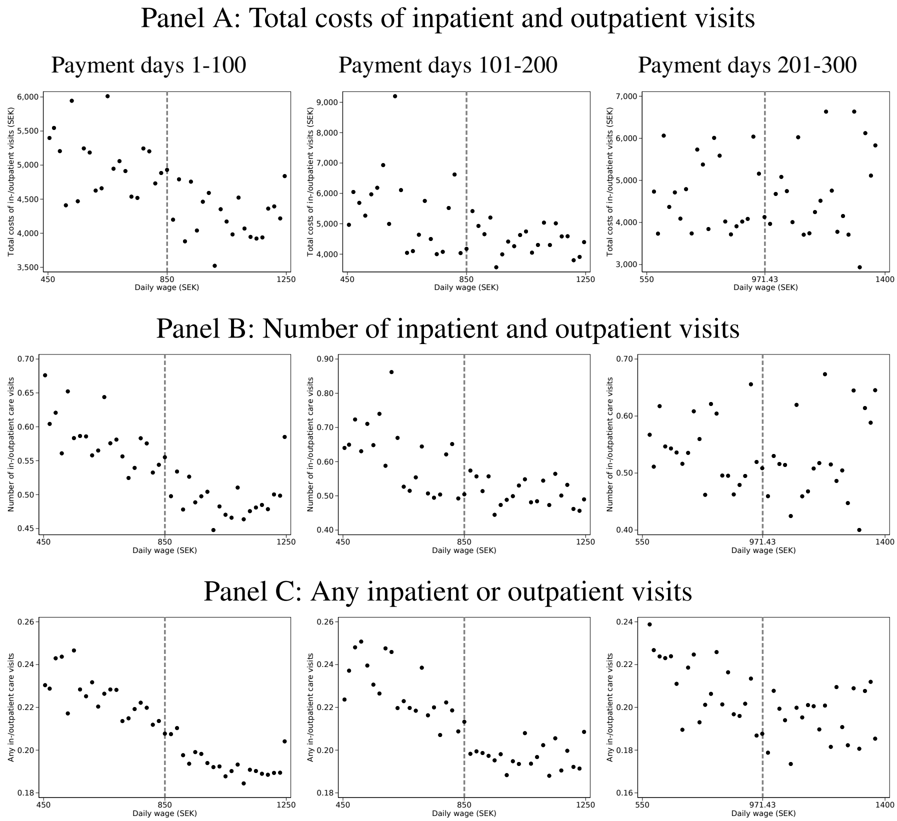


Figure 3.3: Inpatient and Outpatient Care Use Around Daily Wage Kinks

Notes. This figure shows binned scatterplots of inpatient and outpatient care use as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). Outcomes are the total costs of inpatient and outpatient care visits (Panel A), the total number of inpatient and outpatient care visits (Panel B), and an indicator for having any inpatient or outpatient care visits (Panel C). We show plots separately for payment days 1-100 (left column), payment days 101–200 (middle column), and payment days 201–300 (right column) of the unemployment spell. In each column, the unit of observation is an unemployment spell.

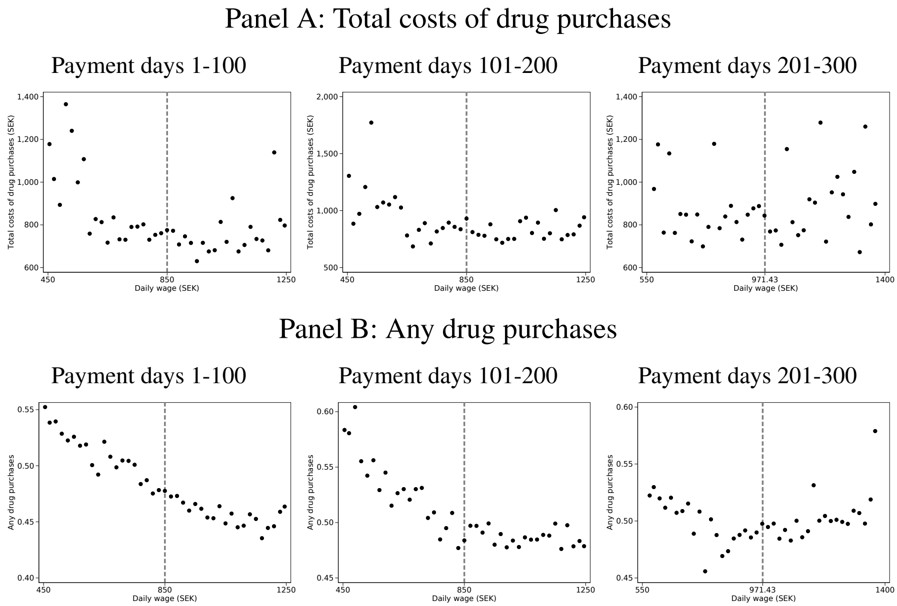


Figure 3.4: Drug Purchases Around Daily Wage Kinks

Notes. This figure shows binned scatterplots of drug purchases as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells starting between March 5, 2007 and July 14, 2015 (see Section 3.3). Outcomes are the total costs of all drug purchases (Panel A) and an indicator for making any drug purchases (Panel B). Total costs of drug purchases refer to the sum of out-of-pocket cost and costs covered by prescription drug insurance. We show plots separately for payment days 1-100 (left column), payment days 101–200 (middle column), and payment days 201–300 (right column) of the unemployment spell. In each column, the unit of observation is an unemployment spell.

Manipulation of running variable. Appendix Figure 3.3 shows the density function of the daily wage, separately for the first 100 payment days (Panel A), payment days 101–200 (Panel B), and payment days 201–300 (Panel C). Visually, it is not apparent that any of the densities would have discontinuous jumps (bunching) or kinks (slope changes). We formally test for the presence of discontinuities in two ways and report test statistics and associated p-values from the tests in each plot. Both McCrary (2008) tests for a discontinuous jump in the density function and tests similar to Card et al. (2015) and Landais (2015) for a kink in the density indicate that we cannot reject the null hypothesis of a lack of discontinuous jump or slope change at the kink points (see figure notes for details).

Smoothness of pre-determined covariates and placebo outcomes around kink. We provide three pieces of evidence supporting the assumption that other determinants correlated with health and healthcare use evolve smoothly around the kink points.

First, Appendix Figure 3.4 shows binned scatterplots of predicted healthcare use around the daily wage kinks, separately for the first 100 payment days (left column), payment days 101–200 (middle column), and payment days 201–300 (right column). Outcomes are the total costs of healthcare use (Panel A), total costs of inpatient and outpatient care visits (Panel B), and the total costs of drug purchases (Panel C).[29] Although some of the predicted outcomes evolve non-linearly around the kink points, the corresponding coefficients shown in Appendix Table 3.10 do not indicate the presence of kinks.

Second, Appendix Figure 3.5 presents binned scatterplots of the conditional means of selected covariates against the daily wage, separately for the same three sub-periods. While some of these conditional means evolve non-linearly as a function of the daily wage, the corresponding coefficients in Appendix Table 3.11 do not indicate the presence of kinks.[30]

[29] We form predictions, separately for each sub-period, as fitted values obtained from a linear regression of the outcome against the set of covariates measured in the calendar year before the start of the unemployment spell (see Section 3.3). Appendix Table 3.9 reports estimated coefficients from the regressions used to create these covariate indices.

[30] For age, the coefficients for two of the sub-periods (payment days 1–100 and 101–200) indicate statistically significant kinks. However, the corresponding binned scatterplots in Appendix Figure 3.5 suggest that these effects could stem from non-linearities in the relationship between the daily wage and age. For example, note that the optimal bandwidth for payment

Third, Appendix Figure 3.6 shows binned scatterplots of healthcare use in the last 12 months *before* the start of the unemployment spell against the daily wage, separately for the three sub-periods. The plots for these placebo outcomes show no evidence of discontinuities at the kink points, which the corresponding point estimates reported in Appendix Table 3.12 confirm.

Sensitivity to bandwidth choice. Appendix Figure 3.7 shows the coefficients and 95 percent confidence intervals for the effect of unemployment benefits on costs of healthcare use for varying bandwidths, separately for the first 100 payment days (left column), payment days 101–200 (middle column), and payment days 201–300 (right column). In each panel, the dashed vertical line indicates the MSE-optimal bandwidth of Calonico et al. (2014b) that we use for our main results.

For most of our cost measures and analysis sub-periods, the coefficients and confidence intervals remain stable and closely centered around zero for bandwidths much wider than the MSE-optimal bandwidths. The only exception is the total costs of drug purchases, for which bandwidths greater than 300 SEK would indicate that more generous unemployment benefits decrease drug expenditures. However, these coefficients are arguably small. For example, with a bandwidth of 400 SEK, the bias-corrected estimate without covariate adjustment for the first 100 payment days implies that total costs of drug purchases fall by 2.4 SEK (16.9 SEK), or by 0.3 (2.0) percent relative to the mean around the kink) in response to a 1 SEK (1 percent) increase in daily benefits.

Alternative specifications. Our main estimates are from a specification with a local linear estimator and a uniform kernel. Appendix Figure 3.8 compares these estimates to estimates from alternative specifications where we vary the polynomial order (linear vs. quadratic), the kernel function (uniform vs. triangular kernel), and whether we control for pre-determined covariates or not. The two leftmost coefficients in each plot correspond to our main estimates.

Although our main specification sometimes yields estimates with wider confidence intervals than the alternative ones (see e.g., the estimates for total healthcare costs during the first 100 payment days in Panel A) and some

---

days 1–100 (420 SEK), is close to the bandwidth used for the binned scatterplots in Appendix Figure 3.5.

alternative specifications suggest statistically significant effects (e.g., the local quadratic estimates for total drug purchase costs during payment days 101–200 in Panel C), our takeaway from Appendix Figure 3.8 is that none of the alternative specifications consistently provide more precise estimates or imply statistically significant effects compared to the other specifications.

3.5.4 Effect Heterogeneity

Our main findings indicate that UI generosity matters little for the total costs of recipients' healthcare use. As a last exercise, we investigate whether this lack of an effect for the whole sample masks heterogeneity by gender and age or between short-term and long-term benefit recipients. Looking at these groups is relevant in light of evidence that women's health is less affected by unemployment and that long-term unemployment spells are more detrimental to health than short-term spells (Picchio & Ubaldi, 2023).

Effects by gender and age group. Figure 3.5 shows coefficients and standard errors of the effect of unemployment benefits on healthcare use, separately by gender and age quartile. The youngest age quartile includes those aged 20–29 while the oldest includes those aged 49–64. As before, we measure effects separately during the first 100 payment days (left column), payment days 101–200 (middle column), and payment days 201–300 (right column).

Although estimates for some age groups are noisier than for others (in particular for the youngest age quartile) and for some groups point estimates would indicate statistically significant effects (which could happen by chance given the number of estimated coefficients), our overall takeaway from Figure 3.5 is that we do not see that more generous UI would systematically increase or decrease the costs of in-/outpatient use or drug purchases in any of the groups.

Effects over time. So far, we have estimated effects separately for three different sub-periods of the unemployment spell, but have measured the total healthcare use during each sub-period. Figure 3.6 instead presents estimates for the effects of UI on *weekly* healthcare use, starting from 52 calendar weeks *before* the start of the unemployment spell and going up to 60 weeks *after* the

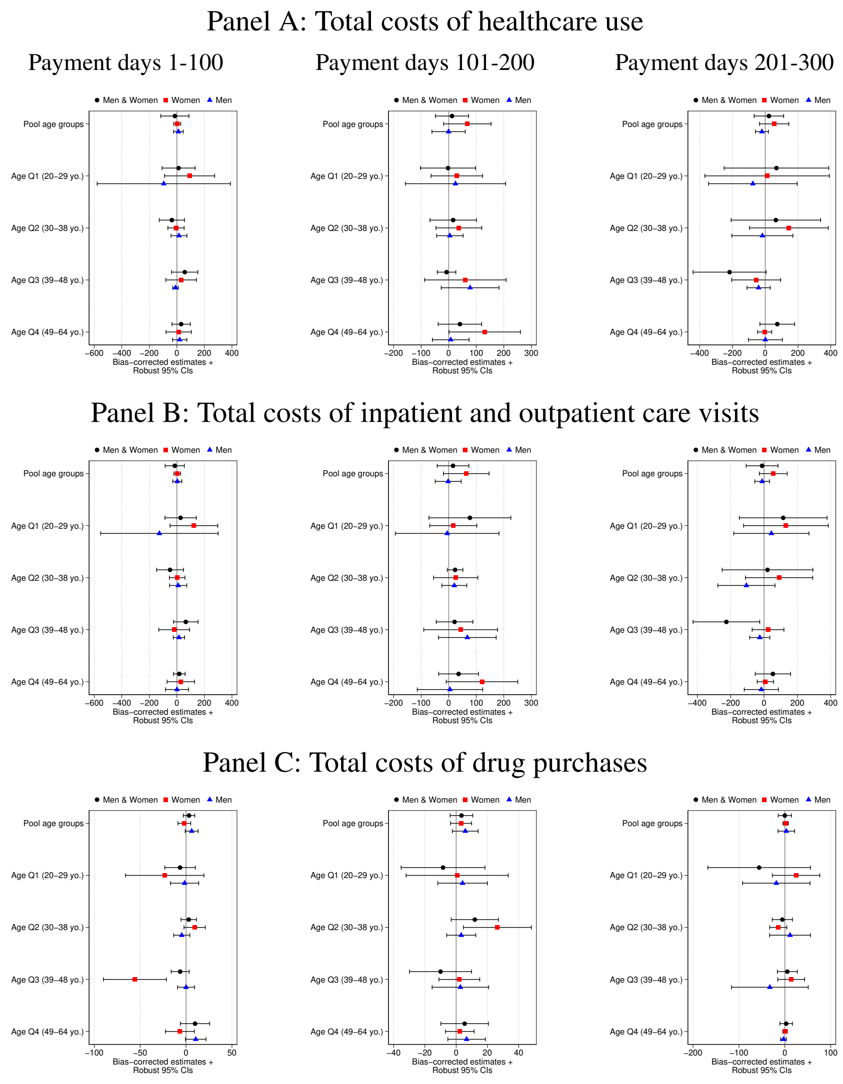


Figure 3.5: Effects on Healthcare Use by Gender and Age Quartile

Notes. This figure presents coefficients of the effect of UI benefits on the costs of healthcare use, along with their 95 percent pointwise confidence intervals, separately by gender and age quartile. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). We report local linear estimates with a uniform kernel, quadratic bias correction, MSE-optimal bandwidths, and robust pointwise 95 percent confidence intervals (Calonico et al., 2014a), and controlling for pre-determined covariates. Confidence intervals are based on standard errors clustered at the individual level. Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (Panel A), total costs of inpatient and outpatient care visits (Panel B), and total costs of drug purchases (Panel C). We show estimates separately for payment days 1–100 (left column), payment days 101–200 (middle column), and payment days 201–300 (right column) of the unemployment spell.

start of the spell. For all the weeks before the start of the spell and up to week 20 since the start of the spell, the estimation uses all the spells included in our analysis sample. For weeks 21–40 (41–60) since the start of the spell, the estimates are instead based on spells where individuals still receive benefits after the first 20 (40) weeks.

Figure 3.6 shows that the estimates for the effects of unemployment benefits on costs of in-/outpatient care visits and drug purchases remain stable and closely around zero, both before and after the start of the unemployment spell. We therefore conclude that the lack of an effect on healthcare use applies to both short-term and long-term UI recipients.[31]

3.6 Conclusion

We use Swedish administrative data on around 340,000 unemployment spells and a regression kink design to study how the generosity of unemployment insurance affects the recipients' healthcare use. Our measure of healthcare use covers inpatient and outpatient care visits and drug purchases and measures total costs to the healthcare system, not just out-of-pocket costs.

We find little evidence that more generous unemployment benefits affect recipients' healthcare use, a conclusion that is robust across specification choices and applies to men and women, older and younger individuals, and short-term and long-term benefit recipients. Our findings therefore suggest that, in the Swedish context, the detrimental impact of unemployment on health mainly reflects factors affecting independently of income, such as social stigma or loss of social contacts and identity (e.g., Jahoda, 1982), rather than the decline in income.

Our analysis has some limitations. First, our measure of healthcare use is incomplete because we do not observe primary care visits. Second, our estimates do not consider potential spillover effects on partners or children.[32]

[31] The weeks before the start of the spell also serve as additional placebo tests and rule out sorting around the kink points e.g. due to health shocks before the start of the unemployment spell (i.e. an Ashenfelter dip).

[32] On spillover effects of UI on partners, see e.g. Cullen and Gruber (2000) and Hendren (2017). Barr et al. (2022) and Bailey et al. (2024), among others, have recently highlighted the relevance of intergenerational spillover effects on children in the context of social insurance and transfer programs.

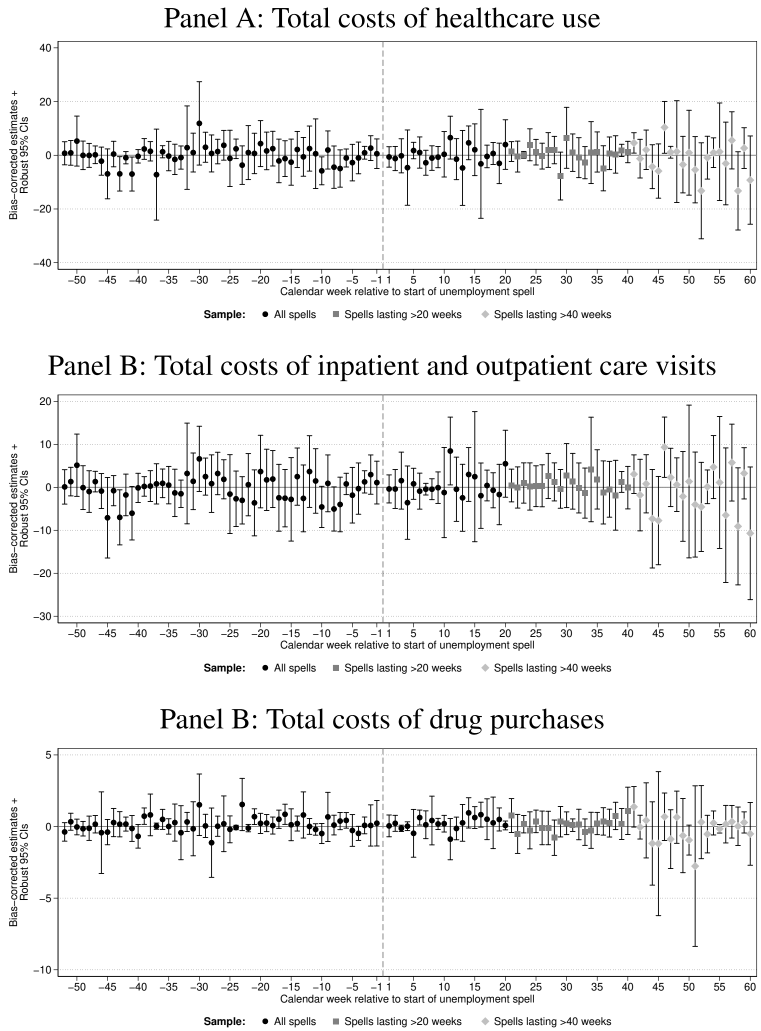


Figure 3.6: Effects on Healthcare Use Over the Unemployment Spell

Notes. This figure presents coefficients of the effect of UI benefits on the costs of healthcare use separately by calendar week relative to the start of the unemployment spell, along with their 95 percent pointwise confidence intervals. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). We present estimates from 52 weeks before the start of the spell up to 60 weeks after the start of the spell. For weeks up to week 20 since the start of the spell, the estimates use all the spells in our analysis sample. For weeks 21–40 since the start of the spell, the estimates only use the spells where individuals still receive unemployment benefits after the first 20 weeks. For weeks 41–60 since the start of the spell, the estimates only use the spells where individuals still receive unemployment benefits after the first 40 weeks. We report local linear estimates with a uniform kernel, quadratic bias correction, MSE-optimal bandwidths, and robust pointwise 95 percent confidence intervals (Calonico et al., 2014a), and controlling for pre-determined covariates. Confidence intervals are based on standard errors clustered at the individual level. Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (Panel A), total costs of inpatient and outpatient care visits (Panel B), and total costs of drug purchases (Panel C).

Third, due to data limitations, our measure of the costs of inpatient and outpatient care visits is based on a coarse categorization of healthcare visits to 29 groups. Fourth, our research design only allows us to estimate the effects of a small increase in the generosity of unemployment benefits for the subgroup of individuals located close to kink points.

We close by highlighting two directions for future work. First, although more generous unemployment benefits do not appear to affect healthcare use in the Swedish setting, this does not preclude such effects being present in settings where the out-of-pocket costs of healthcare are high and consumption smoothing is costly (cf., Chetty & Looney, 2006, 2007).

Second, it is important to study whether the generosity of benefits affects recipients' healthcare use in the context of other social insurance programs, such as disability insurance (see Gelber et al., 2023, for an example). Since public healthcare systems in developed economies are typically heavily subsidized, the fiscal externalities created by such effects on healthcare use could be sizable and matter for the optimal design of social insurance programs. To detect such fiscal externalities, it is important to use a comprehensive measure of the costs of healthcare use, similar to the one used in this paper.

References

Ahammer, A., & Packham, A. (2023). Effects of Unemployment Insurance Duration on Mental and Physical Health. *Journal of Public Economics*, *226*, 104996.

Aizer, A., Eli, S., Ferrie, J., & Lleras-Muney, A. (2016). The Long-Run Impact of Cash Transfers to Poor Families. *American Economic Review*, *106*(4), 935–971.

Amorim, G., Britto, D. G. C., Fonseca, A., & Sampaio, B. (2024). *Job Loss, Unemployment Insurance, and Health: Evidence from Brazil* [Unpublished manuscript].

Ando, M. (2017). How Much Should We Trust Regression-Kink-Design Estimates? *Empirical Economics*, *53*, 1287–1322.

Anell, A., Glenngård, A. H., & Merkur, S. (2012). Sweden: Health System Review. *Health Systems in Transition*, *14*(5), 1–159.

Arbetsförmedlingen. (2024a). *Arbetsförmedlingens datalager, Hist_Aktso*. Retrieved February 12, 2022, from https://arbetsformedlingen.se/other-languages/english-engelska

Arbetsförmedlingen. (2024b). *Arbetsförmedlingens datalager, Insper*. Retrieved February 12, 2022, from https://arbetsformedlingen.se/other-languages/english-engelska

Arbetsförmedlingen. (2024c). *Arbetsförmedlingens datalager, Sokatper*. Retrieved February 12, 2022, from https://arbetsformedlingen.se/other-languages/english-engelska

Bailey, M. J., Hoynes, H., Rossin-Slater, M., & Walker, R. (2024). Is the Social Safety Net a Long-Term Investment? Large-Scale Evidence from the Food Stamps Program. *Review of Economic Studies*, *91*(3), 1291–1330.

Barham, T., & Maluccio, J. A. (2013). Living Longer: The Effect of the Mexican Conditional Cash Transfer Program on Elderly Mortality. *Journal of Development Economics*, *105*, 66–76.

Barr, A., Eggleston, J., & Smith, A. A. (2022). Investing in Infants: The Lasting Effects of Cash Transfers to New Families. *Quarterly Journal of Economics*, *137*(4), 2539–2583.

Björvang, C., Pontén, J., Rönnholm, G., & Skiöld, P. (2023). *PPRI Pharma Profile 2023*. Dental; Pharmaceutical Benefits Agency (TLV).

Black, B., French, E., McCauley, J., & Song, J. (2024). The Effect of Disability Insurance Receipt on Mortality. *Journal of Public Economics*, *229*, 105033.

Brand, J. E. (2015). The Far-Reaching Impact of Job Loss and Unemployment. *Annual Review of Sociology*, *41*, 359–375.

Brot-Goldberg, Z. C., Chandra, A., Handel, B. R., & Kolstad, J. T. (2017). What Does a Deductible Do? The Impact of Cost-Sharing on Health Care Prices, Quantities, and Spending Dynamics. *Quarterly Journal of Economics*, *132*(3), 1261–1318.

Calonico, S., Cattaneo, M. D., Farrell, M. H., & Titiunik, R. (2019). Regression Discontinuity Designs using Covariates. *Review of Economics and Statistics*, *101*(3), 442–451.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014a). Robust Data-driven Inference in the Regression-Discontinuity Design. *The Stata Journal*, *14*(4), 909–946.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014b). Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica*, *82*(6), 2295–2326.

Card, D., Dobkin, C., & Maestas, N. (2009). Does Medicare Save Lives? *Quarterly Journal of Economics*, *124*(2), 597–636.

Card, D., Lee, D. S., Pei, Z., & Weber, A. (2012). *Nonlinear Policy Rules and the Identification and Estimation of Causal Effects in a Generalized Regression Kink Design* (NBER Working Paper No. 18564). National Bureau of Economic Research.

Card, D., Lee, D. S., Pei, Z., & Weber, A. (2015). Inference on Causal Effects in a Generalized Regression Kink Design. *Econometrica*, *83*(6), 2453–2483.

Card, D., Lee, D. S., Pei, Z., & Weber, A. (2017). Regression Kink Design: Theory and Practice. In *Regression Discontinuity Designs* (pp. 341–382, Vol. 38). Emerald Publishing Limited.

Cheng, L., Fang, H., Tan, K. S. M., Wang, Y., Wang, P., Yang, X., & Yu, S. (2018). The Health Implications of Social Pensions: Evidence from

China's New Rural Pension Scheme. *Journal of Comparative Economics*, *46*(1), 53–77.

Chetty, R. (2006). A General Formula for the Optimal Level of Social Insurance. *Journal of Public Economics*, *90*(10-11), 1879–1901.

Chetty, R., & Finkelstein, A. (2013). Social Insurance: Connecting Theory to Data. In A. J. Auerbach, R. Chetty, M. Feldstein, & E. Saez (Eds.), *Handbook of Public Economics* (pp. 111–193, Vol. 5). Elsevier.

Chetty, R., & Looney, A. (2006). Consumption Smoothing and the Welfare Consequences of Social Insurance in Developing Economies. *Journal of Public Economics*, *90*(12), 2351–2356.

Chetty, R., & Looney, A. (2007). Income Risk and the Benefits of Social Insurance: Evidence from Indonesia and the United States. In T. Ito & A. K. Rose (Eds.), *Fiscal Policy and Management in East Asia* (pp. 99–121). University of Chicago Press.

Cullen, J. B., & Gruber, J. (2000). Does Unemployment Insurance Crowd Out Spousal Labor Supply? *Journal of Labor Economics*, *18*(3), 546–572.

Cutler, D. M., Lleras-Muney, A., & Vogl, T. (2012). Socioeconomic Status and Health: Dimensions and Mechanisms. In S. Glied & P. C. Smith (Eds.), *The Oxford Handbook of Health Economics*. Oxford University Press.

Diamond, P. A. (1977). A Framework for Social Security Analysis. *Journal of Public Economics*, *8*(3), 275–298.

Dooley, D., Fielding, J., & Levi, L. (1996). Health and Unemployment. *Annual Review of Public Health*, *17*, 449–465.

Eliason, M., & Storrie, D. (2009). Does Job Loss Shorten Life? *Journal of Human Resources*, *44*(2), 277–302.

Gathmann, C., Huttunen, K., Jernström, L., Sääksvuori, L., & Stitzing, R. (2022). *In Sickness and in Health: Job Displacement and Health Spillovers in Couples* [Working paper, University of Luxembourg].

Gelber, A., Moore, T., Pei, Z., & Strand, A. (2023). Disability Insurance Income Saves Lives. *Journal of Political Economy*, *131*(11), 3156–3185.

Goldin, J., Handel, B. R., Kolstad, J. T., & Magne, N. P. (2021). Health Insurance and Mortality: Experimental Evidence from Taxpayer Outreach. *Quarterly Journal of Economics*, *136*(1), 1–47.

Hendren, N. (2017). Knowledge of Future Job Loss and Implications for Unemployment Insurance. *American Economic Review*, *107*(7), 1778–1823.

Hoynes, H., Schanzenbach, D. W., & Almond, D. (2016). Long-Run Impacts of Childhood Access to the Safety Net. *American Economic Review*, *106*(4), 903–934.

Inspektionen för arbetslöshetsförsäkringen. (2024a). *ASTAT databas, Tabell Utbetalning*. Retrieved February 13, 2024, from https://www.iaf.se/globalassets/statistik/databaserna/astatdokumentation.pdf#page=137

Inspektionen för arbetslöshetsförsäkringen. (2024b). *Sju av tio personer inom arbetskraften var med i en a-kassa 2023* [Last updated: 2024-02-23.]. Retrieved May 23, 2024, from https://www.iaf.se/statistikdatabasen/arsstatistik/sju-av-tio-personer-inom-arbetskraften-var-med-i-en-a-kassa-2023/

Jacobson, L. S., LaLonde, R. J., & Sullivan, D. G. (1993). Earnings Losses of Displaced Workers. *American Economic Review*, 685–709.

Jahoda, M. (1982). *Employment and Unemployment: A Social-Psychological Analysis*. Cambridge University Press.

Kjellberg, A. (2019). *Kollektivavtalens täckningsgrad samt organisationsgraden hos arbetsgivarförbund och fackförbund*. Department of Sociology, Lund University.

Kolsrud, J. (2018). Sweden: Voluntary Unemployment Insurance. In *The Future of Social Protection: What Works for Non-standard Workers?* (pp. 197–224). OECD Publishing.

Kolsrud, J., Landais, C., Nilsson, P., & Spinnewijn, J. (2018). The Optimal Timing of Unemployment Benefits: Theory and Evidence From Sweden. *American Economic Review*, *108*(4-5), 985–1033.

Kuhn, A., Lalive, R., & Zweimüller, J. (2009). The Public Health Costs of Job Loss. *Journal of Health Economics*, *28*(6), 1099–1115.

Kuka, E. (2020). Quantifying the Benefits of Social Insurance: Unemployment Insurance and Health. *Review of Economics and Statistics*, *102*(3), 490–505.

Landais, C. (2015). Assessing the Welfare Effects of Unemployment Benefits Using the Regression Kink Design. *American Economic Journal: Economic Policy*, *7*(4), 243–278.

Landais, C., Nekoei, A., Nilsson, P., Seim, D., & Spinnewijn, J. (2021). Risk-Based Selection in Unemployment Insurance: Evidence and Implications. *American Economic Review*, *111*(4), 1315–1355.

Levy, H., & Meltzer, D. (2008). The Impact of Health Insurance on Health. *Annual Review of Public Health*, *29*, 399–409.

Lindquist, G. S., & Wadensjö, E. (2011). *Avtalsbestämda ersättningar, andra kompletterande ersättningar och arbetsutbudet*. Finansdepartementet, Regeringskansliet.

Lleras-Muney, A., Schwandt, H., & Wherry, L. (2024). *Poverty and Health* (NBER Working Paper No. 32866). National Bureau of Economic Research.

McCrary, J. (2008). Manipulation of the Running Variable in the Regression Discontinuity Design: a Density Test. *Journal of Econometrics*, *142*(2), 698–714.

Migliano, G., Gallego, F., Huneeus, C., & Cerda, R. A. (2023). Health Effects of Increasing Income for the Elderly: Evidence from a Chilean Pension Program. *American Economic Journal: Economic Policy*, *15*(2), 276–308.

Organisation for Economic Co-operation and Development. (2019). *Out-of-Pocket Spending: Access to Care and Financial Protection*. Retrieved May 27, 2024, from https://www.oecd.org/health/health-systems/OECD-Focus-on-Out-of-Pocket-Spending-April-2019.pdf

Pei, Z., Lee, D. S., Card, D., & Weber, A. (2022). Local Polynomial Order in Regression Discontinuity Designs. *Journal of Business & Economic Statistics*, *40*(3), 1259–1267.

Picchio, M., & Ubaldi, M. (2023). Unemployment and Health: A Meta-Analysis. *Journal of Economic Surveys*, 1–36.

Pontén, J., Rönnholm, G., & Skiöld, P. (2017). *PPRI Pharma Profile 2017*. Dental; Pharmaceutical Benefits Agency (TLV).

Salm, M. (2011). The Effect of Pensions on Longevity: Evidence from Union Army Veterans. *The Economic Journal*, *121*(552), 595–619.

Snyder, S. E., & Evans, W. N. (2006). The Effect of Income on Mortality: Evidence from the Social Security Notch. *Review of Economics and Statistics*, *88*(3), 482–495.

Socialstyrelsen. (2022a). *DRG - Grundläggande begrepp och principer* [Published on December 1, 2022.]. Retrieved May 28, 2024, from https://www.socialstyrelsen.se/globalassets/sharepoint-dokument/dokument-webb/klassifikationer-och-koder/drg-grundlaggande-begrepp-och-principer.pdf

Socialstyrelsen. (2022b). *National Patient Register, Inpatient care (*Patientregister*), 1998–2014*. Retrieved February 12, 2022, from https://www.socialstyrelsen.se/statistik-och-data/register/alla-register/patientregistret/

Socialstyrelsen. (2022c). *National Patient Register, Outpatient care (*Patientregister*), 2001–2014*. Retrieved February 12, 2022, from https://www.socialstyrelsen.se/statistik-och-data/register/alla-register/patientregistret/

Socialstyrelsen. (2022d). *National Prescribed Drug Register (*Läkemedelsregistret*), 2005–2015*. Retrieved February 4, 2022, from https://www.socialstyrelsen.se/statistik-och-data/register/alla-register/lakemedelsregistret/

Socialstyrelsen. (2023). *Sekundär klassificering till diagnosrelaterade grupper (DRG)*. Retrieved April 17, 2024, from https://www.socialstyrelsen.se/statistik-och-data/klassifikationer-och-koder/drg/

Statistics Sweden. (2022). *Longitudinal Integrated Database for Health Insurance (LISA), 2000–2018*. Retrieved February 4, 2022, from https://www.scb.se/contentassets/f0bc88c852364b6ea5c1654a0cc90234/lisa-bakgrundsfakta-1990-2017.pdf

Statistics Sweden. (2023a). *Mikrodata för Registerbaserad arbetsmarknadsstatistik (RAMS)* [2023-08-02]. https://www.scb.se/vara-tjanster/bestall-data-och-statistik/bestalla-mikrodata/vilka-mikrodata-finns/individregister/registerbaserad-arbetsmarknadsstatistik-rams/

Statistics Sweden. (2023b). *Mikrodata för Registret över totalbefolkningen (RTB)* [2023-08-02]. https://www.scb.se/vara-tjanster/bestall-data-och-statistik/bestalla-mikrodata/vilka-mikrodata-finns/individregister/registret-over-totalbefolkningen-rtb/

Statistics Sweden. (2024a). *Genomsnittlig månadslön, lön i fasta priser och lönespridning efter utbildningsinriktning SUN 2000 och kön. År 1995 – 2019* [Last updated: 2021-01-13.]. Retrieved May 27, 2024, from https://www.statistikdatabasen.scb.se/pxweb/sv/ssd/START__AM__AM0110__AM0110D/TidsserieUtbniva/

Statistics Sweden. (2024b). *Konsumentprisindex (KPI), totalt, 1980=100. Månad 1980M01 - 2024M02. Sveriges officiella statistik.* Retrieved April 5, 2024, from https://www.statistikdatabasen.scb.se/pxweb/sv/ssd/START__PR__PR0101_PR0101A/KPItotM/

Sullivan, D., & Von Wachter, T. (2009). Job Displacement and Mortality: An Analysis using Administrative Data. *Quarterly Journal of Economics*, *124*(3), 1265–1306.

Sun, S., Huang, J., Hudson, D. L., & Sherraden, M. (2021). Cash Transfers and Health. *Annual Review of Public Health*, *42*, 363–380.

Svensk författningssamling. (1997). Lag om arbetslöshetsförsäkring [The Unemployment Insurance Act]. 1997:238.

Sveriges Kommuner och Regioner. (2020). *Nationella KPP-principer. Kostnad per patient. Version 4.* Linköpings Tryckeri AB.

Sveriges Kommuner och Regioner. (2023). *Kostnad per patient, KPP*. Retrieved April 17, 2024, from https://skr.se/skr/halsasjukvard/ekonomiavgifter/kostnadperpatientkpp.1076.html

Wanberg, C. R. (2012). The Individual Experience of Unemployment. *Annual Review of Psychology*, *63*(1), 369–396.

Wikström, J. (2023). *Liquidity Effects in Non-Linear Health Insurance Contracts* [Working paper, Stockholm University].

Wikström, J. (2024). *The Long-Run Effects of Stricter Eligibility Criteria in Short-Term Disability Insurance* [Working paper, Gothenburg University].

Appendices

Appendix 3.A Measuring the Number and Costs of Inpatient and Outpatient Care Visits

3.A.1 Total Costs of Inpatient and Outpatient Care Visits

We calculate the total costs of a person's inpatient and outpatient care visits in two steps. First, we determine the average per-day costs of inpatient and outpatient care visits for each Major Diagnostic Category (MDC). Second, we calculate the total costs of inpatient and outpatient care visits by multiplying the average per-day costs by the length of the visit for each visit and summing over all visits.

1. Determining average per-day costs of a visit. We calculate the average per-day costs of an inpatient or outpatient visit with a given MDC by using data on the total number of visits, average length of the visit, and average weights for all diagnosis-related groups (DRG) that belong to the MDC, and combining these with data on the cost per DRG point.

DRG codes are divided into codes only used in inpatient care and codes only used in outpatient care, but a given MDC code can contain both DRG codes used in inpatient care and outpatient care. Therefore, for each MDC we calculate average per-day costs separately for inpatient care and outpatient care visits.[33]

Denote the set of inpatient care DRG codes that belong to MDC m by $D(m,1)$, outpatient care DRG codes that belong to MDC m by $D(m,0)$, and fix a reference year t. We calculate the average per-day costs of an inpatient care visit with MDC code m as

$$c_{m,1} = \sum_{j \in D(m,1)} \underbrace{\left(\frac{N_j}{N_{m,1}}\right)}_{\substack{\text{DRG } j\text{'s share of all} \\ \text{inpatient visits with MDC } m}} \times \underbrace{\left(w_j \times \frac{c}{d_j}\right)}_{\substack{\text{Average per-day} \\ \text{costs of DRG } j}},$$

[33] DRG codes used in outpatient care are further divided into codes used in primary care and codes used in specialized outpatient care. Since our Patient Register data does not include primary care visits, we only consider DRG codes used in specialized outpatient care.

where N_j is the total number of inpatient care visits with DRG code j, $N_{m,1}$ is the total number of inpatient care visits with MDC code m, w_j is the weight for DRG j, d_j is the average duration (in days) of visits with DRG code j, and c is the cost per DRG point, all measured in the reference year. We define the average per-day cost $c_{m,0}$ of an outpatient care visit with MDC code m analogously, assuming that $d_j = 1$ for each DRG code $j \in D(m,0)$.

Appendix Table 3.3 shows the resulting average per-day costs of inpatient and outpatient care visits for all 29 MDC codes used in Sweden during our study period. For example, for MDC code 05 ("Diseases of the circulatory system"), the average per-day cost was 17,828 SEK for inpatient care visits and 5,181 SEK for outpatient care visits. For all MDC codes except for 0 ("Pre-MDC") and 25 ("HIV infection and HIV-related diseases"), we measure costs using 2020 as the reference year. MDC codes 0 and 25 were only used until 2011 and 2005, respectively, so we use these years as the reference years for these two codes.

Even though data on average costs, number of visits, and average visit lengths are published annually for each DRG code, we measure average costs using a fixed reference year for two reasons. First, we only have access to data on average costs of each DRG code for both inpatient and outpatient care from the year 2020 onwards. Second, using a fixed reference year ensures that the dynamic effects of unemployment insurance generosity on healthcare utilization in Section 3.5 reflect changes in the intensity and type of healthcare utilization, rather than changes over time in the costs of providing care in the healthcare system. The latter reason is analogous to e.g. the common practice of deflating measures of consumption expenditures using the consumer price index.

2. Determining total costs of all visits. Consider a healthcare visit j that appears in the Patient Register data. In the data, we observe whether the visit is an inpatient care visit ($I(j) = 1$) or outpatient care visit ($I(j) = 0$), the visit's MDC code $m(j)$, its admission date D_j^{start}, and for inpatient care visits its discharge date D_j^{end}. For outpatient visits, we assume admission and discharge dates coincide, that is, $D_j^{start} = D_j^{end}$.

Fix some interval of dates $D = [D^{min}, D^{max}]$ for $D^{min} < D^{max}$ (say, the first and last day of a calendar week). For a visit j that overlaps with period D (i.e.,

$[D^{min}, D^{max}] \cap [D_j^{start}, D_j^{end}] \neq \emptyset$), we calculate the total costs of visit j during the period D, C_j^D, by multiplying the per-day costs of visit j by the number days of visit j that fall within period D, that is

$$C_j^D = \left[1 + \min(D^{max}, D_j^{end}) - \max(D^{min}, D_j^{start})\right] \times c_{m(j),i},$$

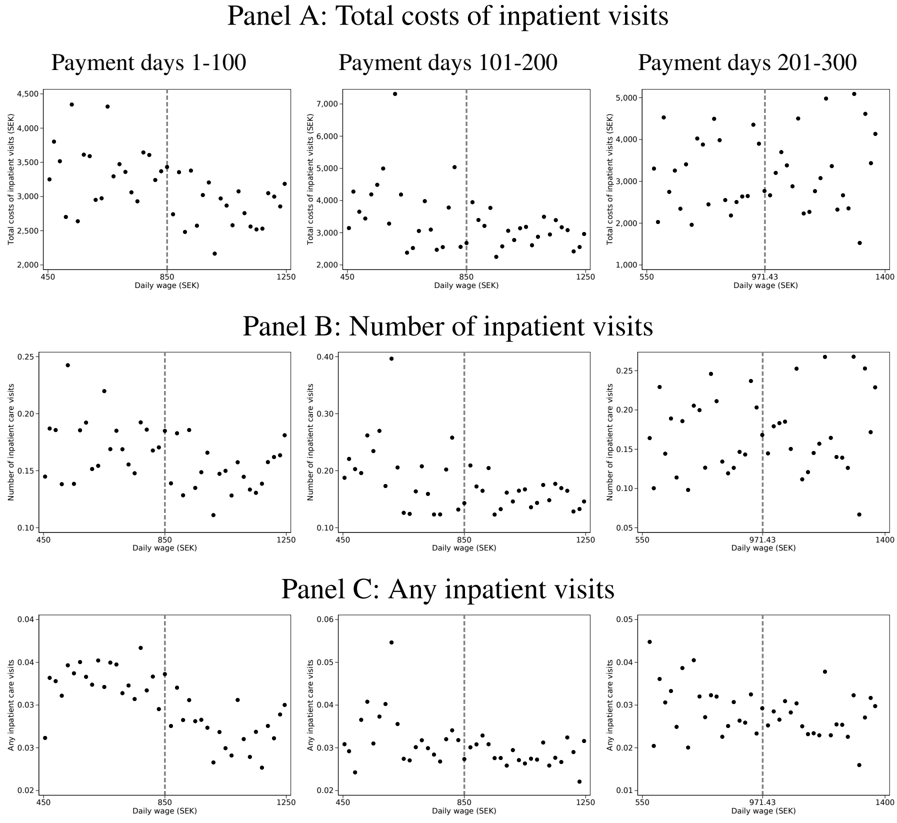
where $i = 1$ if visit j is an inpatient care visit and $i = 0$ if it is an outpatient care visit. Denote the set of visits that overlap with period D by J^D. We calculate the total costs of all visits during the period D as $C^D = \sum_{j \in J^D} C_j^D$.

For individuals in our analysis sample, we cannot assign costs for 0.6 percent of inpatient care visits and 1.1 percent of outpatient care visits. For the population aged 20–64 in Table 3.1, the corresponding shares are 0.6 and 3.2 percent, respectively. In virtually all cases the reason for this is that the MDC code for the visit is missing since we can assign costs for more than 99.99 percent of all visits with a non-missing MDC code. We assign zero costs for all visits for which we cannot assign costs, so our measure of total costs of healthcare utilization can be seen as a lower bound.

3.A.2 Number of Inpatient and Outpatient Care Visits

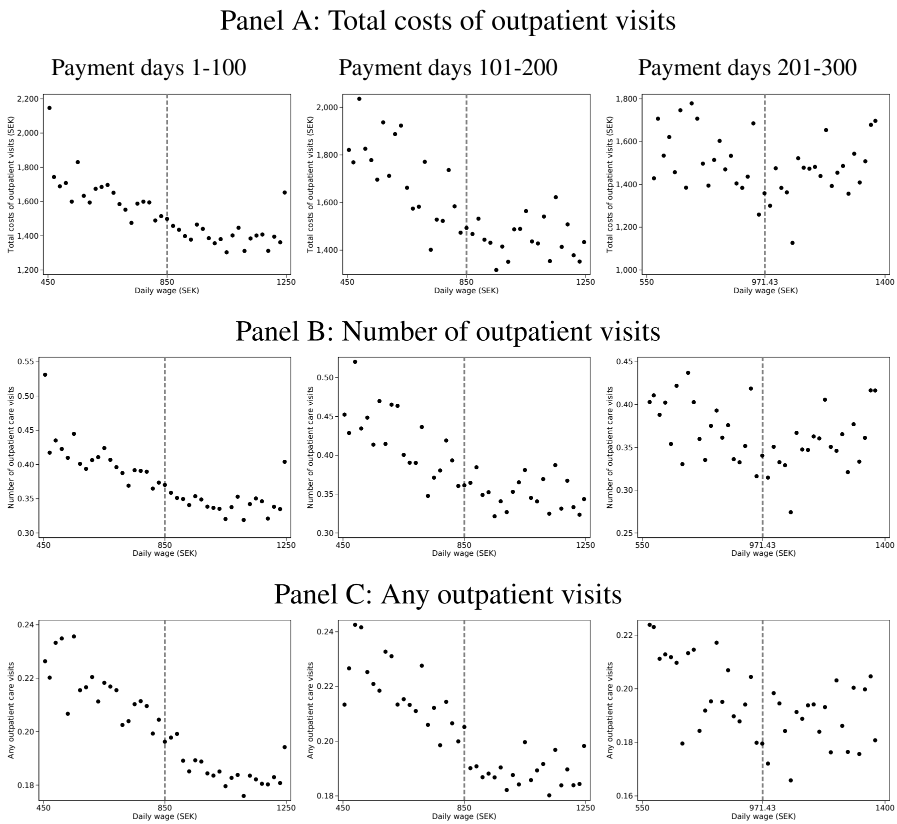
Fix some interval of dates $D = [D^{min}, D^{max}]$ for $D^{min} < D^{max}$ (say, the first and last day of a calendar week). Denote the set of visits that overlap with period D by J^D. We define the total number of in-/outpatient care visits during period D, N^D, as the number of visits with an admission date during period D, that is $N^D = \sum_{j \in J^D} 1\left\{D_j^{start} \in D\right\}$. We note that N^D also includes inpatient and outpatient care visits for which we cannot measure costs.

Appendix 3.B Supplementary Figures and Tables



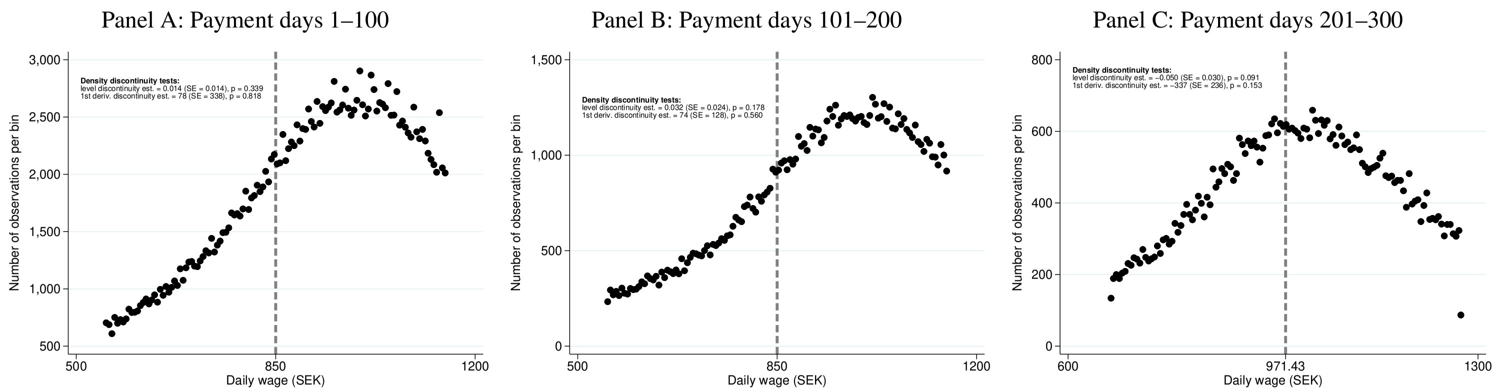
Appendix Figure 3.1: Inpatient Care Use Around Daily Wage Kinks

Notes. This figure shows binned scatterplots of inpatient care use as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). Outcomes are the total costs of inpatient care visits (Panel A), the total number of inpatient care visits (Panel B), and an indicator for having any inpatient care visits (Panel C). We show plots separately for payment days 1-100 (left column), payment days 101–200 (middle column), and payment days 201–300 (right column) of the unemployment spell. In each column, the unit of observation is an unemployment spell.



Appendix Figure 3.2: Outpatient Care Use Around Daily Wage Kinks

Notes. This figure shows binned scatterplots of outpatient care use as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). Outcomes are the total costs of outpatient care visits (Panel A), the total number of outpatient care visits (Panel B), and an indicator for having any outpatient care visits (Panel C). We show plots separately for payment days 1-100 (left column), payment days 101–200 (middle column), and payment days 201–300 (right column) of the unemployment spell. In each column, the unit of observation is an unemployment spell.

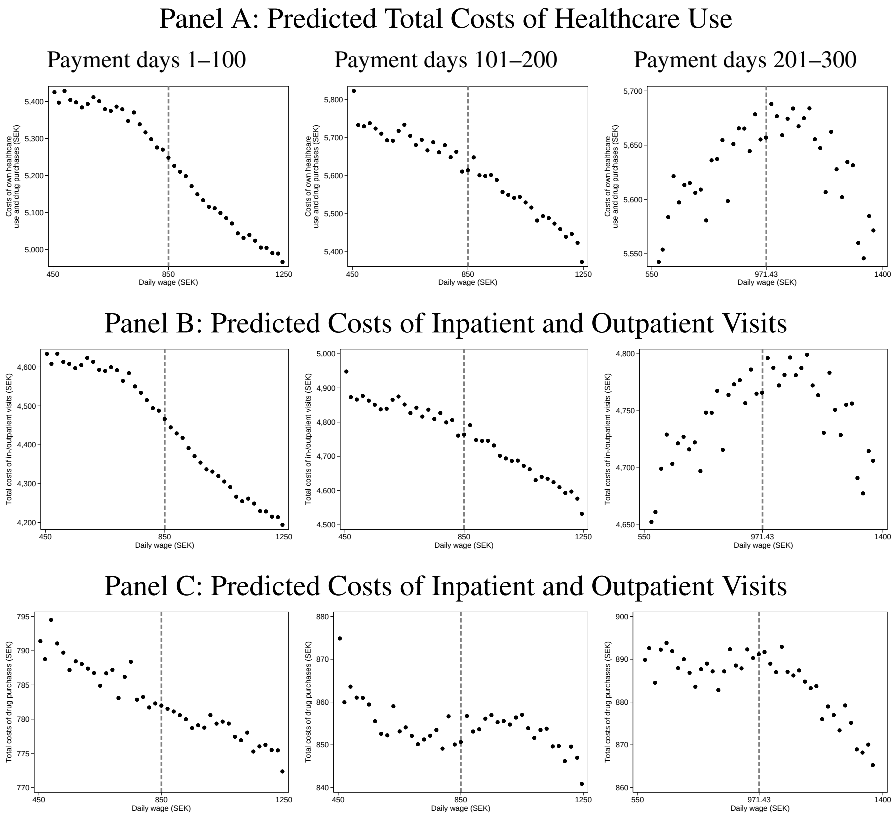


Appendix Figure 3.3: Testing for Discontinuities in the Density of the Daily Wage

Notes. This figure shows the density function of the daily wage, separately for the benefit payment days 1–100 (Panel A), payment days 101–200 (Panel B), and payment days 201–300 (Panel C) of the unemployment spell. The figure uses the analysis sample of unemployment spells with a starting date between March 5, 2007 and July 14, 2015 (see Section 3.3). The unit of observation in each panel is an unemployment spell. We use a bandwidth of 300 SEK around the kink point and construct 5 SEK bins of the running variable. Black dots show the number of observations in each bin. The top-left corner in each panel also reports point estimates and standard errors from two tests for discontinuities in the density of the running variable. The top estimate and standard error are from a McCrary (2008) test for a discontinuous jump in the logarithm of the density of the running variable at the cutoff. The bottom estimate is from a test for a discontinuity in the slope (first derivative) of the density of the running variable at the cutoff similar to Card et al. (2015) and Landais (2015). We implement the latter test by estimating

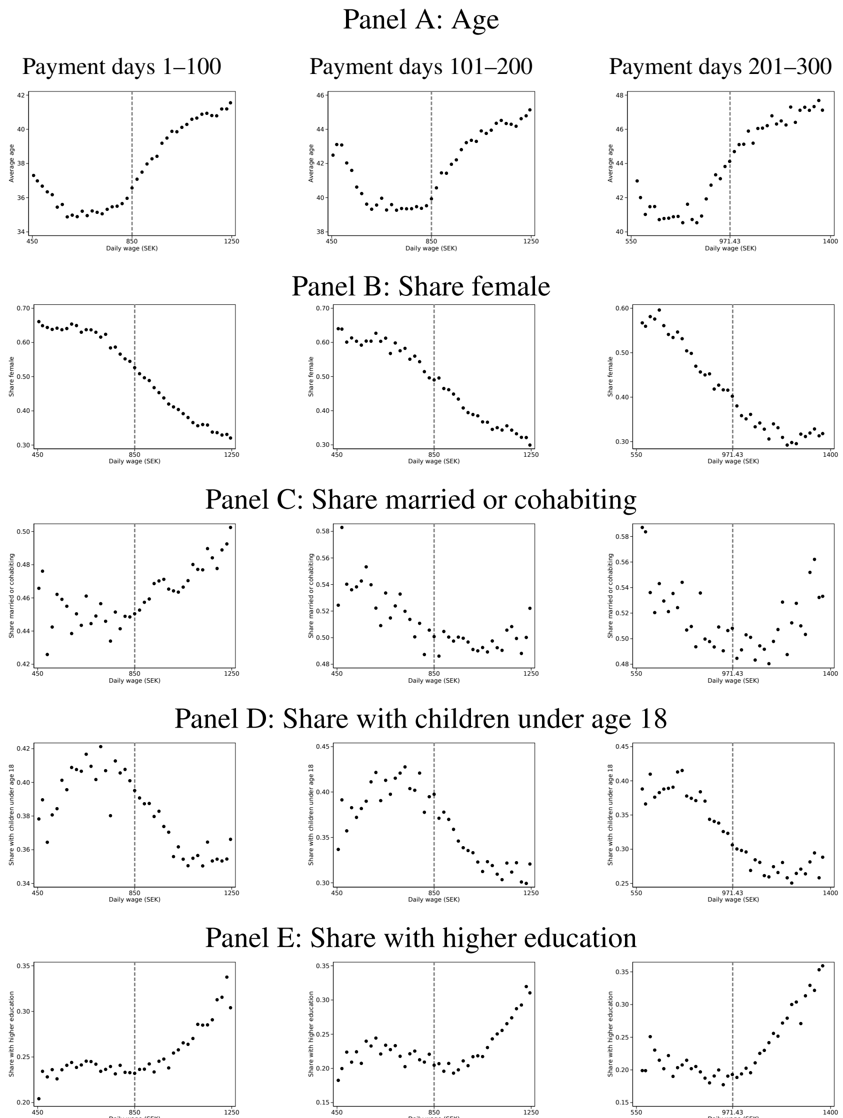
$$\text{obs}_b = \alpha_0 + \sum_{p=1}^{P} \left[\alpha_p \, w_b^p + \beta_p \, w_b^p \times (w_b \geq 0)\right] + \varepsilon_b,$$

where obs_b is the number of observations in bin $b \in \{1, \ldots, 120\}$, w_b is the mean of the running variable in bin b, $P = 5$ is polynomial order, and ε_b is an error term. The figure reports the OLS estimate of the parameter β_1 and its heteroskedasticity-robust standard error.



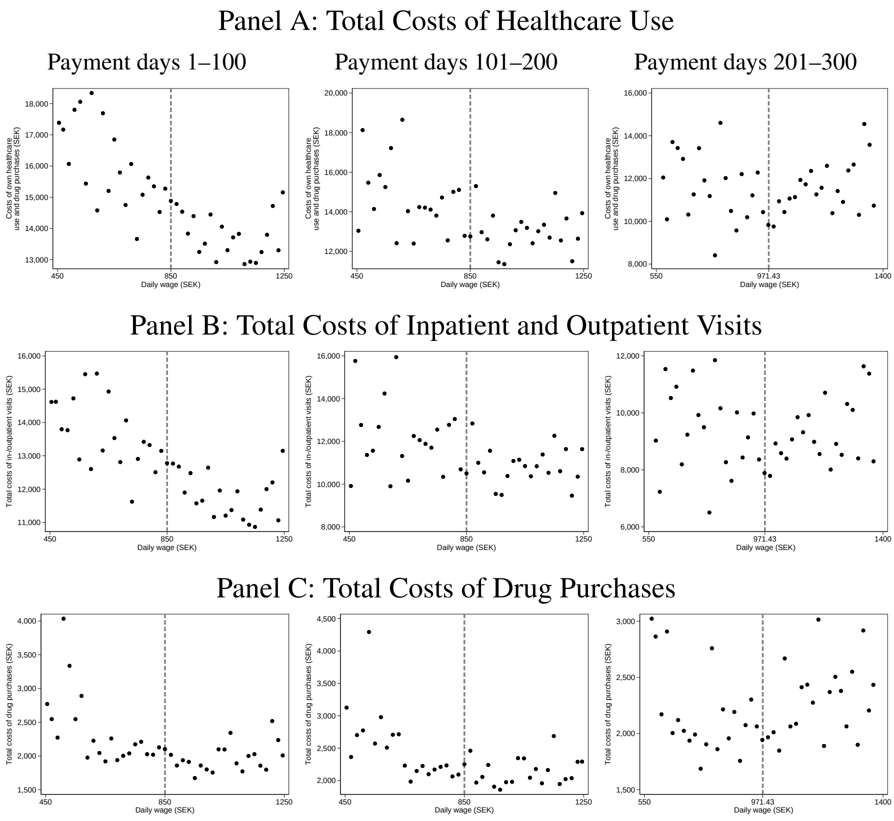
Appendix Figure 3.4: Predicted Healthcare Use Around Daily Wage Kinks

Notes. This figure shows binned scatterplots of predicted costs of healthcare use as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells starting between March 5, 2007 and July 14, 2015 (see Section 3.3). The outcomes are the predicted total costs of inpatient and outpatient care visits and drug purchases (Panel A), the predicted total costs of inpatient and outpatient care visits (Panel B), and the predicted total costs of drug purchases (Panel C). We show predicted outcomes separately for payment days 1-100 (left column), payment days 101–200 (middle column), and payment days 201–300 (right column) of the unemployment spell. In each column, the unit of observation is an unemployment spell. Predicted outcomes are fitted values obtained after regressing each outcome against indicators for being married or cohabiting, female, having higher education, and having children under age 18 at home, indicators for age, indicators for the region of residence, and indicators for the industry of the highest-paying employer (incl. missing industry as a separate category). Appendix Table 3.9 presents the estimation results from these regressions. We define a person as having higher education if s/he has completed at least one semester of post-secondary education. Control variables are measured in the calendar year before the start of the unemployment spell.



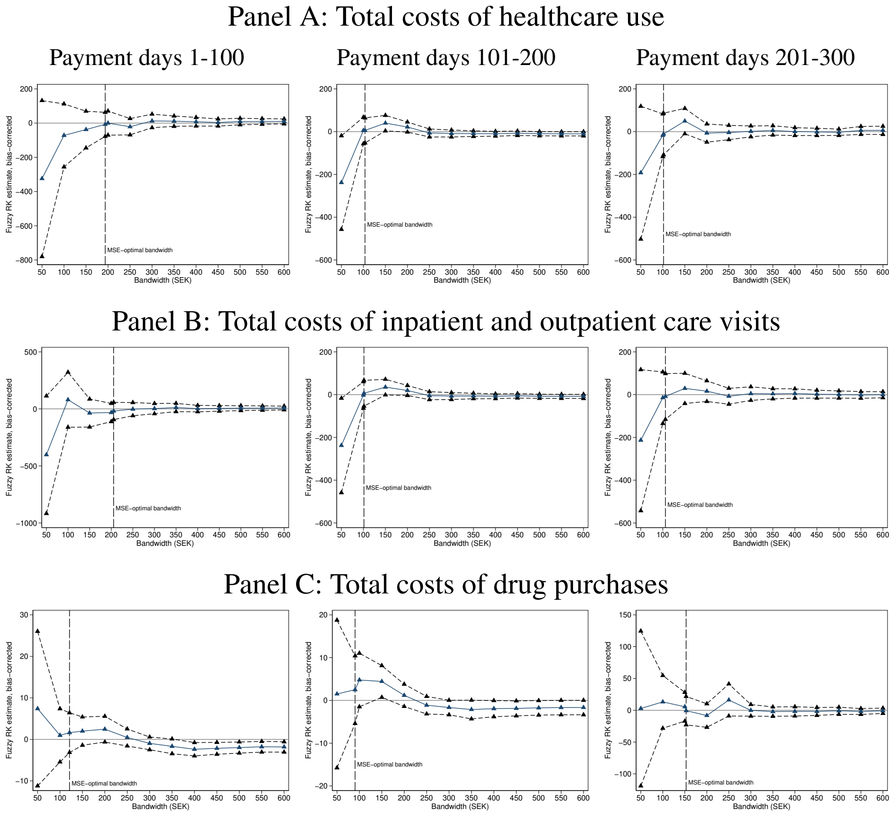
Appendix Figure 3.5: Pre-Determined Covariates Around Daily Wage Kinks

Notes. This figure shows binned scatterplots of selected pre-determined covariates as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells starting between March 5, 2007 and July 14, 2014 (see Section 3.3). We show plots separately for payment days 1-100 (left column), payment days 101–200 (middle column), and payment days 201–300 (right column) of the unemployment spell. In each column, the unit of observation is an unemployment spell. Each covariate is measured in the calendar year before the start of the unemployment spell. We define a person as having higher education if s/he has completed at least one semester of post-secondary education.



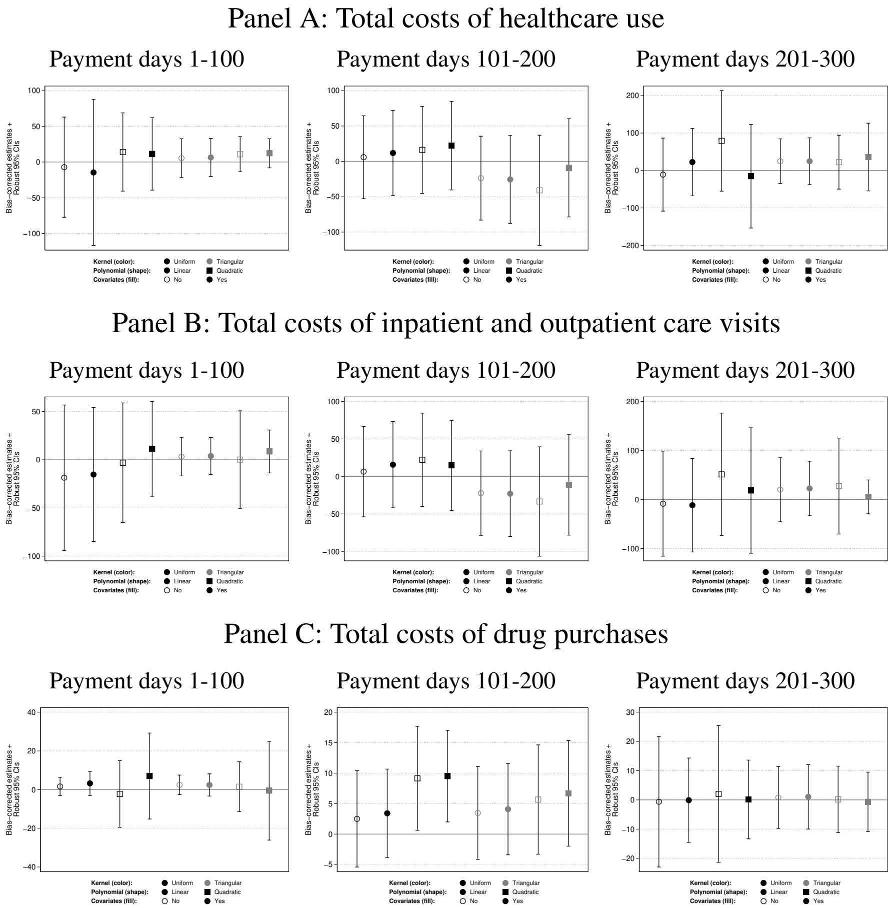
Appendix Figure 3.6: Pre-Unemployment Healthcare Use Around Daily Wage Kinks

Notes. This figure shows binned scatterplots of outcomes measured before the start of the unemployment spell as a function of the daily wage, using a bandwidth of 400 SEK and 20 SEK bins. The figure uses the analysis sample of unemployment spells starting between March 5, 2007 and July 14, 2014 (see Section 3.3). Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (Panel A), total costs of inpatient and outpatient care visits (Panel B), and total costs of drug purchases (Panel C). We show plots separately for payment days 1-100 (left column), payment days 101–200 (middle column), and payment days 201–300 (right column) of the unemployment spell. In each column, the unit of observation is an unemployment spell. For each outcome, we measure costs over the last twelve calendar months before the start of the unemployment spell and deflate costs using the overall CPI with 2020 as the reference year.



Appendix Figure 3.7: Effects on Healthcare Use for Varying Bandwidths

Notes. This figure presents coefficients of the effect of UI benefits on the costs of healthcare use for varying bandwidth choices along with their 95 percent pointwise confidence intervals. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). We report local linear estimates with a uniform kernel, quadratic bias correction, and robust pointwise 95 percent confidence intervals (Calonico et al., 2014a), without controlling for pre-determined covariates. Confidence intervals are based on standard errors clustered at the individual level. Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (Panel A), total costs of inpatient and outpatient care visits (Panel B), and total costs of drug purchases (Panel C). We show estimates separately for payment days 1–100 (left column), payment days 101–200 (middle column), and payment days 201–300 (right column) of the unemployment spell. The dashed vertical lines indicate the MSE-optimal bandwidth (Calonico et al., 2014b), which we use for our main estimates.



Appendix Figure 3.8: Effects on Healthcare Use for Alternative Specifications

Notes. This figure presents coefficients of the effect of UI benefits on the costs of healthcare use for alternative specifications along with their 95 percent pointwise confidence intervals. The figure uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). Specifications vary by (i) whether they use a uniform (black markers) or triangular (gray markers) kernel, (ii) whether they use a local linear (circle markers) or local quadratic (square markers) estimator, and (iii) whether they include (filled markers) or exclude (hollow markers) pre-determined covariates as controls. Each specification uses bias-corrected estimates, robust pointwise 95 percent confidence intervals, and MSE-optimal bandwidths following (Calonico et al., 2014a). Confidence intervals are based on standard errors clustered at the individual level. Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (Panel A), total costs of inpatient and outpatient care visits (Panel B), and total costs of drug purchases (Panel C). We show estimates separately for payment days 1–100 (left column), payment days 101–200 (middle column), and payment days 201–300 (right column) of the unemployment spell.

Appendix Table 3.1: Summary of the Income-Based Unemployment Insurance System

	Payment days 1–100	Payment days 101–200	Payment days 201–300
Replacement rate	80%	80%	70%
Maximum daily benefit amount	680 SEK	680 SEK	680 SEK
Kink point ($\frac{\text{max. benefit amount}}{\text{replacement rate}}$)	850 SEK	850 SEK	971.43 SEK

Notes. This table summarizes the income-based UI system during our study period covering unemployment spells with a start date between March 5, 2007 and July 14, 2014. The first row shows the replacement rate (as a percent of the pre-unemployment daily wage), the second row shows the maximum daily benefit amount, and the third row shows the kink point, i.e. the daily wage at which the individual reaches the maximum daily benefits. We show these separately for payment days 1–100 (column 2), payment days 101–200 (column 3), and payment days 201–300 (column 4) of the unemployment spell.

Appendix Table 3.2: Mapping Job Seeker and Deregistration Codes in the PES Data

(i) Job seeker codes

Code	Name	Employed	Unemployed	PES Program	Other registered	Other deregistered
0	Unknown	–	–	–	X	–
11	Openly unemployed	–	X	–	–	–
12	Unemployed, guidance service	–	X	–	–	–
13	Unemployed, waiting for decided action	–	X	–	–	–
14	Jobseeker with obstacles	–	–	–	X	–
15	Municipal effort	–	–	–	X	–
20	Establishment job	X	–	–	–	–
21	Part-time unemployed	X	–	–	–	–
22	Hourly employee	X	–	–	–	–
23	Professional fisherman	–	–	–	X	–
24	Protected work Samhall (temporary employees)	X	–	–	–	–
28	The establishment program, mapping	–	–	X	–	–
30	Introduction job	X	–	–	–	–
31	Temporary work	X	–	–	–	–
33	New start job	X	–	–	–	–
34	Outgoing EU/EEA job seeker	–	–	–	X	–
35	Change-seeking Samhall	X	–	–	–	–
38	Wage subsidy for development in employment	X	–	–	–	–
39	Wage subsidy for security in employment	X	–	–	–	–
40	Professional introduction	X	–	–	–	–
41	Change-seeking	X	–	–	–	–
42	Wage subsidy for employment	X	–	–	–	–
43	Publicly protected work	X	–	–	–	–
44	Graduate job	–	–	X	–	–
46	Support for starting a business	–	–	X	–	–
47	General employment support	X	–	–	–	–
48	Enhanced employment support (2-year enrollment)	X	–	–	–	–
49	Special employment support	X	–	–	–	–
50	Modern preparedness jobs	X	–	–	–	–
51	Extra services	X	–	–	–	–
52	Working life development	–	–	X	–	–
53	Temporary education	–	–	X	–	–
54	Work practice	–	–	X	–	–
55	Workplace introduction	–	–	X	–	–
57	Project work (unemployment benefit)	–	–	X	–	–
58	Wage subsidy for development in work at Samhall	X	–	–	–	–
60	Interpraktik	–	–	X	–	–
61	Youth practice	–	–	X	–	–
62	Academic internship	–	–	X	–	–
63	Youth introduction with education grant	–	–	X	–	–
64	Computer tech	–	–	X	–	–
65	Municipal youth program	–	–	X	–	–
66	Youth guarantee	–	–	X	–	–
68	The establishment program	–	–	X	–	–
69	Job guarantee for youth	–	–	X	–	–
70	Job and development guarantee	–	–	X	–	–
74	Mediation efforts	–	–	X	–	–
75	Project with labor market policy orientation	–	–	X	–	–
80	Preparatory measures	–	–	X	–	–
81	Labor market training	–	–	X	–	–
82	IT investment	–	–	X	–	–
83	Preparatory education	–	–	X	–	–
84	Deficiency training for employees	–	–	X	–	–
86	Validation	–	–	X	–	–
89	Off-year	–	–	–	X	–
91	Special category not included in statistics	–	–	–	X	–
95	Unemployed, revocation of decision	–	X	–	–	–
96	Unemployed, incorrect registration of decision	–	X	–	–	–
97	Unemployed, interruption/revocation of decision	–	X	–	–	–
98	Unemployed, completed decision period	–	X	–	–	–
99	Kalmarmodellen	–	–	–	X	–

(ii) Deregistration codes

Code	Name	Employed	Unemployed	PES Program	Other registered	Other deregistered
1	Got permanent employment	X	–	–	–	–
2	Got temporary employment	X	–	–	–	–
3	Got continued employment with the same employer	X	–	–	–	–
4	Got employment within Samhall	X	–	–	–	–
5	Contact terminated, other known cause	–	–	–	–	X
6	Contact terminated, unknown reason	–	–	–	–	X
7	Education other than labor market education	–	–	–	–	X

Notes. This table shows how we map the job seeker categories and deregistration codes in the Public Employment Service data (AF, 2024b, 2024c) to employment, unemployment, participation in labor market programs, others registered at PES, and those deregistered from the PES.

Appendix Table 3.3: Total Costs per Day of Care in 2020, Separately by Major Diagnostic Category (MDC)

		Total costs per day of care (SEK)	
MDC	Name	Inpatient care	Outpatient care
00	Pre-MDC	23,326	—
01	Diseases of the nervous system	16,858	5,355
02	Diseases of the eye and adnexa	22,678	3,315
03	Diseases of the ear, nose, mouth, and throat	22,250	4,411
04	Diseases of the respiratory system	12,947	5,769
05	Diseases of the circulatory system	17,828	5,181
06	Diseases of the digestive system	17,473	5,480
07	Diseases of the liver, biliary tract, and pancreas	17,176	6,816
08	Diseases of the musculoskeletal system and connective tissue	26,465	4,937
09	Diseases of the skin and subcutaneous tissue	17,549	3,653
10	Endocrine, nutritional and metabolic diseases	21,054	4,194
11	Diseases of the genitourinary system	15,693	5,227
12	Diseases of the male reproductive system	36,186	5,105
13	Diseases of the female reproductive system	35,067	4,088
14	Pregnancy, childbirth and the puerperium	19,245	2,774
15	Newborns and certain perinatal conditions	16,090	3,672
16	Blood diseases and immune disorders	13,076	5,763
17	Myeloproliferative diseases and unspecified tumors	15,553	6,384
18	Infectious and parasitic diseases including HIV	12,544	4,680
19	Mental disorders, behavioral disorders and alcohol- or drug-related disorders	19,751	3,628
21	Injuries, poisonings and toxic effects	21,182	4,359
22	Burns	21,118	4,186
23	Other and unspecified health problems	14,969	3,592
24	Multiple trauma excluding superficial injuries and wounds	20,627	5,956
25	HIV infection and HIV-related diseases	6,061	—
30	Diseases of the breast	77,630	8,881
40	MDC-wide problems in outpatient care	—	4,887
50	Provider-dependent groups in outpatient care	—	4,378
99	Unspecified or erroneous information	11,945	3,053

Notes. This table shows a list of the 29 Major Diagnostic Categories (MDC) used in Sweden during our study period. Note that MDC code 0 was used until 2011, while MDC code 25 was used until 2005. For each MDC, we also report the average per-day total care costs, separately for inpatient and outpatient care. For all MDC codes except for 0 and 25, we measure average costs in 2020. For MDC codes 0 and 25, we measure average costs in the last year the MDC code was used. Costs are deflated using the overall CPI with 2020 as the reference year. Appendix 3.A describes in detail how we calculate the average per-day costs.

Appendix Table 3.4: Effect of Unemployment Benefits on Having Any Healthcare Use

	Payment days 1–100				Payment days 101–200				Payment days 201–300			
	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction
First stage estimates												
Change in daily benefits per 1 SEK daily wage	-0.7738***	-0.7479***	-0.7532***	-0.7385***	-0.7319***	-0.6932***	-0.7469***	-0.6978***	-0.6401***	-0.6144***	-0.6511***	-0.6227***
	(0.00091)	(0.00430)	(0.00632)	(0.01401)	(0.00549)	(0.01159)	(0.00360)	(0.01074)	(0.00836)	(0.01222)	(0.00755)	(0.01163)
Fuzzy RK estimates												
Change in outcome per 100 SEK daily benefits	-0.0078*	-0.0059	0.0290	0.0498	-0.0243	-0.0172	-0.0075	0.0010	-0.0273	0.0182	0.0265	0.0338
	(0.00340)	(0.01481)	(0.01869)	(0.04211)	(0.01652)	(0.03553)	(0.01033)	(0.03150)	(0.04727)	(0.06801)	(0.03289)	(0.05273)
Implied elasticity												
% Change in outcome per 1% change in daily benefits	-0.0997*	-0.0755	0.3682	0.6322	-0.3029	-0.2144	-0.0938	0.0120	-0.3460	0.2303	0.3357	0.4276
	(0.04869)	(0.21275)	(0.21560)	(0.49638)	(0.21489)	(0.47488)	(0.12673)	(0.40845)	(0.64280)	(0.98803)	(0.46114)	(0.74095)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00	850.00	850.00	850.00	971.43	971.43	971.43	971.43
Covariates			✓	✓			✓	✓			✓	✓
Implied change (%)	-1.481	-1.122	5.469	9.391	-4.517	-3.197	-1.399	0.179	-5.113	3.404	4.961	6.319
Outcome mean around kink	0.53	0.53	0.53	0.53	0.54	0.54	0.54	0.54	0.53	0.53	0.53	0.53
Bandwidth	303.6	303.6	88.3	88.3	135.9	135.9	182.7	182.7	81.9	81.9	102.2	102.2
Number of observations	224,451	224,451	73,627	73,627	47,874	47,874	63,304	63,304	19,482	19,482	23,801	23,801

Notes. This table presents coefficients and standard errors of the effect of UI benefits on having any healthcare use. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification, a uniform kernel, and MSE-optimal bandwidths following Calonico et al. (2014b). We report conventional estimates (columns labeled "Local linear") and estimates with quadratic bias correction and robust standard errors (columns labeled "+ Bias correction"), with and without controlling for pre-determined covariates. Standard errors are clustered at the individual level. The outcome is an indicator for having any healthcare use, i.e. having total costs of healthcare use greater than zero. We show estimates separately for payment days 1–100 (columns 2-5), payment days 101–200 (columns 6-9), and payment days 201–300 (columns 10–13) of the unemployment spell. For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, and rows 5–6 show the implied elasticities. For elasticities, we obtain standard errors via a non-parametric bootstrap where we sample unemployment spells with replacement. Row 7 indicates the kink point, row 8 indicates whether covariates are included, and row 9 expresses the fuzzy RK estimate as a percent of the outcome mean around the kink. The last three rows show the outcome sample mean around the kink (using observations within 10 SEK of the kink), the MSE-optimal bandwidth, and the number of observations within the bandwidth.

Appendix Table 3.5: Effect of Unemployment Benefits on Inpatient and Outpatient Care Use

Panel A: Number of visits

	Payment days 1–100				Payment days 101–200				Payment days 201–300			
	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction
First stage estimates												
Change in daily benefits per 1 SEK daily wage	-0.7687***	-0.7305***	-0.7596***	-0.7339***	-0.7164***	-0.6842***	-0.7173***	-0.6863***	-0.6644***	-0.6346***	-0.6584***	-0.6150***
	(0.00135)	(0.02174)	(0.00278)	(0.01497)	(0.00973)	(0.01629)	(0.00953)	(0.01636)	(0.00284)	(0.00632)	(0.00345)	(0.01264)
Fuzzy RK estimates												
Change in outcome per 100 SEK daily benefits	0.0069	-0.2167	0.0320	-0.1263	-0.0588	-0.0666	-0.0292	-0.0155	0.0298	0.1350	0.0882	0.1571
	(0.02104)	(0.29028)	(0.03969)	(0.19585)	(0.12471)	(0.21020)	(0.11861)	(0.20900)	(0.06943)	(0.15215)	(0.07727)	(0.29046)
Implied elasticity												
% Change in outcome per 1% change in daily benefits	0.0837	-2.6221	0.3868	-1.5289	-0.7803	-0.8843	-0.3874	-0.2054	0.4041	1.8320	1.1961	2.1312
	(0.22754)	(3.59386)	(0.48979)	(2.40651)	(1.48582)	(2.52898)	(1.44514)	(2.51840)	(0.92039)	(2.01931)	(1.02882)	(3.57077)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00	850.00	850.00	850.00	971.43	971.43	971.43	971.43
Covariates			✓	✓			✓	✓			✓	✓
Implied change (%)	1.243	-38.951	5.745	-22.711	-11.636	-13.188	-5.777	-3.064	5.971	27.070	17.674	31.492
Outcome mean around kink	0.56	0.56	0.56	0.56	0.51	0.51	0.51	0.51	0.50	0.50	0.50	0.50
Bandwidth	235.2	235.2	145.5	145.5	93.6	93.6	95.9	95.9	184.2	184.2	161.8	161.8
Number of observations	183,602	183,602	118,833	118,833	33,305	33,305	34,110	34,110	39,019	39,019	35,171	35,171

Panel B: Any visits

	Payment days 1–100				Payment days 101–200				Payment days 201–300			
	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction
First stage estimates												
Change in daily benefits per 1 SEK daily wage	-0.7527***	-0.7401***	-0.7506***	-0.7371***	-0.7649***	-0.8669***	-0.7431***	-0.7192***	-0.6528***	-0.6268***	-0.6417***	-0.6208***
	(0.00797)	(0.01369)	(0.00802)	(0.01476)	(0.00198)	(0.00320)	(0.00390)	(0.00614)	(0.00355)	(0.00636)	(0.00682)	(0.01461)
Fuzzy RK estimates												
Change in outcome per 100 SEK daily benefits	0.0089	0.0071	0.0101	0.0050	-0.0084	-0.0132	0.0015	0.0108	-0.0194	-0.0381	-0.0203	-0.0414
	(0.01952)	(0.03398)	(0.01976)	(0.03679)	(0.00489)	(0.00695)	(0.00958)	(0.01524)	(0.01534)	(0.02664)	(0.02973)	(0.06294)
Implied elasticity												
% Change in outcome per 1% change in daily benefits	0.2879	0.2295	0.3261	0.1624	-0.2641	-0.4155	0.0461	0.3384	-0.7067	-1.3828	-0.7369	-1.5044
	(0.71447)	(1.25343)	(0.72334)	(1.36283)	(0.17425)	(0.23149)	(0.30893)	(0.50291)	(0.60864)	(1.05292)	(1.14889)	(2.39995)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00	850.00	850.00	850.00	971.43	971.43	971.43	971.43
Covariates			✓	✓			✓	✓			✓	✓
Implied change (%)	4.276	3.409	4.845	2.413	-3.938	-6.197	0.688	5.047	-10.443	-20.433	-10.889	-22.230
Outcome mean around kink	0.21	0.21	0.21	0.21	0.21	0.21	0.21	0.21	0.19	0.19	0.19	0.19
Bandwidth	76.3	76.3	75.6	75.6	280.7	280.7	171.4	171.4	152.2	152.2	95.2	95.2
Number of observations	63,716	63,716	63,180	63,180	91,821	91,821	59,678	59,678	33,506	33,506	22,307	22,307

Notes. This table presents coefficients and standard errors of the effect of UI benefits on the costs of healthcare use. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification, a uniform kernel, and MSE-optimal bandwidths following Calonico et al. (2014b). We report conventional estimates (columns labeled "Local linear") and estimates with quadratic bias correction and robust standard errors (columns labeled "+ Bias correction"), with and without controlling for pre-determined covariates. Standard errors are clustered at the individual level. Outcomes are the total number of inpatient and outpatient care visits (Panel A), and an indicator for having any inpatient or outpatient care visits (Panel B). We show estimates separately for payment days 1–100 (columns 2-5), payment days 101–200 (columns 6-9), and payment days 201–300 (columns 10–13) of the unemployment spell. For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, and rows 5–6 show the implied elasticities. For elasticities, we obtain standard errors via a non-parametric bootstrap where we sample unemployment spells with replacement. Row 7 indicates the kink point, row 8 indicates whether covariates are included, and row 9 expresses the fuzzy RK estimate as a percent of the outcome mean around the kink. The last three rows show the outcome sample mean around the kink (using observations within 10 SEK of the kink), the MSE-optimal bandwidth, and the number of observations within the bandwidth.

Appendix Table 3.6: Effect of Unemployment Benefits on Inpatient Care Use

Panel A: Total costs of visits

	Payment days 1–100				Payment days 101–200				Payment days 201–300			
	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction
First stage estimates												
Change in daily benefits per 1 SEK daily wage	-0.7652***	-0.7306***	-0.7722***	-0.7187***	-0.7193***	-0.6847***	-0.7191***	-0.6842***	-0.6471***	-0.6364***	-0.6421***	-0.6223***
	(0.00183)	(0.01756)	(0.00099)	(0.02542)	(0.00827)	(0.01467)	(0.00833)	(0.01472)	(0.00761)	(0.01635)	(0.00732)	(0.01577)
Fuzzy RK estimates												
Change in outcome per 1 SEK daily benefits	2.5350	-0.0607	2.1340	-5.4994	7.2282	3.3962	7.2185	4.2032	7.0795	-25.4391	6.6342	-19.6077
	(3.96103)	(33.36806)	(2.30443)	(48.01800)	(15.01681)	(26.83661)	(15.15271)	(26.97207)	(24.19403)	(59.08109)	(26.50438)	(55.03011)
Implied elasticity												
% Change in outcome per 1% change in daily benefits	0.4975	-0.0119	0.4188	-1.0792	1.7992	0.8454	1.7968	1.0462	1.8001	-6.4683	1.6869	-4.9856
	(0.77672)	(6.77395)	(0.46461)	(9.92688)	(3.32909)	(6.31668)	(3.34310)	(6.31672)	(5.98792)	(16.30413)	(6.57618)	(15.36626)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00	850.00	850.00	850.00	971.43	971.43	971.43	971.43
Covariates			✓	✓			✓	✓			✓	✓
Implied change (%)	0.074	-0.002	0.062	-0.160	0.268	0.126	0.268	0.156	0.266	-0.956	0.249	-0.737
Outcome mean around kink	3430	3430	3430	3430	2694	2694	2694	2694	2662	2662	2662	2662
Bandwidth	193.3	193.3	287.2	287.2	103.4	103.4	103.0	103.0	97.1	97.1	92.8	92.8
Number of observations	154,708	154,708	215,608	215,608	36,780	36,780	36,664	36,664	22,727	22,727	21,777	21,777

Panel B: Number of visits

	Payment days 1–100				Payment days 101–200				Payment days 201–300			
	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction
First stage estimates												
Change in daily benefits per 1 SEK daily wage	-0.7592***	-0.7335***	-0.7588***	-0.7373***	-0.7195***	-0.6834***	-0.7172***	-0.6829***	-0.6564***	-0.6087***	-0.6477***	-0.6352***
	(0.00334)	(0.01338)	(0.00339)	(0.01307)	(0.00822)	(0.01487)	(0.00907)	(0.01558)	(0.00365)	(0.01055)	(0.00756)	(0.01638)
Fuzzy RK estimates												
Change in outcome per 100 SEK daily benefits	0.0385	-0.0313	0.0422	-0.0449	0.0388	0.0316	0.0106	0.0269	0.1182	0.2960	0.0624	-0.0707
	(0.03522)	(0.13384)	(0.03579)	(0.13174)	(0.07869)	(0.14302)	(0.08776)	(0.15245)	(0.06504)	(0.21427)	(0.13082)	(0.30280)
Implied elasticity												
% Change in outcome per 1% change in daily benefits	1.4006	-1.1406	1.5379	-1.6331	1.8032	1.4698	0.4938	1.2507	4.9255*	12.3402	2.6020	-2.9465
	(1.15296)	(4.84284)	(1.16959)	(4.81915)	(3.27816)	(6.30534)	(4.00550)	(6.97562)	(2.50037)	(7.81192)	(5.41766)	(14.02544)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00	850.00	850.00	850.00	971.43	971.43	971.43	971.43
Covariates			✓	✓			✓	✓			✓	✓
Implied change (%)	20.806	-16.944	22.845	-24.259	26.892	21.919	7.363	18.652	72.781	182.345	38.449	-43.539
Outcome mean around kink	0.18	0.18	0.18	0.18	0.14	0.14	0.14	0.14	0.16	0.16	0.16	0.16
Bandwidth	129.6	129.6	128.7	128.7	103.8	103.8	98.3	98.3	158.4	158.4	97.2	97.2
Number of observations	106,696	106,696	105,942	105,942	36,906	36,906	34,980	34,980	34,548	34,548	22,753	22,753

Panel C: Any visits

	Payment days 1–100				Payment days 101–200				Payment days 201–300			
	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction
First stage estimates												
Change in daily benefits per 1 SEK daily wage	-0.7558***	-0.7363***	-0.7531***	-0.7392***	-0.7439***	-0.6849***	-0.7301***	-0.6833***	-0.6465***	-0.6218***	-0.6476***	-0.6342***
	(0.00402)	(0.01291)	(0.00609)	(0.01272)	(0.00381)	(0.01438)	(0.00564)	(0.01440)	(0.00776)	(0.01444)	(0.00678)	(0.01590)
Fuzzy RK estimates												
Change in outcome per 100 SEK daily benefits	-0.0004	-0.0105	-0.0073	-0.0179	0.0032	0.0012	0.0053	0.0071	-0.0127	-0.0331	-0.0099	-0.0288
	(0.00455)	(0.01414)	(0.00662)	(0.01383)	(0.00399)	(0.01431)	(0.00585)	(0.01495)	(0.01249)	(0.02357)	(0.01141)	(0.02842)
Implied elasticity												
% Change in outcome per 1% change in daily benefits	-0.0785	-2.0988	-1.4614	-3.5795	0.7759	0.2826	1.2888	1.7180	-3.0692	-8.0080	-2.4058	-6.9761
	(0.87052)	(2.66329)	(1.28831)	(2.60497)	(0.96173)	(3.71559)	(1.48892)	(3.91256)	(3.75017)	(7.45881)	(3.43065)	(8.22748)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00	850.00	850.00	850.00	971.43	971.43	971.43	971.43
Covariates			✓	✓			✓	✓			✓	✓
Implied change (%)	-1.167	-31.176	-21.708	-53.173	11.571	4.215	19.220	25.621	-45.352	-118.331	-35.550	-103.083
Outcome mean around kink	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03
Bandwidth	115.2	115.2	90.1	90.1	173.6	173.6	132.8	132.8	96.3	96.3	101.8	101.8
Number of observations	95,314	95,314	75,083	75,083	60,433	60,433	46,875	46,875	22,550	22,550	23,715	23,715

Notes. This table presents coefficients and standard errors of the effect of UI benefits on the costs of healthcare use. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification, a uniform kernel, and MSE-optimal bandwidths following Calonico et al. (2014b). We report conventional estimates (columns labeled "Local linear") and estimates with quadratic bias correction and robust standard errors (columns labeled "+ Bias correction"), with and without controlling for pre-determined covariates. Standard errors are clustered at the individual level. Outcomes are the total costs of inpatient care visits (Panel A), the total number of inpatient care visits (Panel B), and an indicator for having any inpatient care visits (Panel C). We show estimates separately for payment days 1–100 (columns 2-5), payment days 101–200 (columns 6-9), and payment days 201–300 (columns 10–13) of the unemployment spell. For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, and rows 5–6 show the implied elasticities. For elasticities, we obtain standard errors via a non-parametric bootstrap where we sample unemployment spells with replacement. Row 7 indicates the kink point, row 8 indicates whether covariates are included, and row 9 expresses the fuzzy RK estimate as a percent of the outcome mean around the kink. The last three rows show the outcome sample mean around the kink (using observations within 10 SEK of the kink), the MSE-optimal bandwidth, and the number of observations within the bandwidth.

Appendix Table 3.7: Effect of Unemployment Benefits on Outpatient Care Use

Panel A: Total costs of visits

	Payment days 1–100				Payment days 101–200				Payment days 201–300			
	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction
First stage estimates												
Change in daily benefits per 1 SEK daily wage	-0.7556***	-0.7354***	-0.7505***	-0.7358***	-0.7308***	-0.7060***	-0.7132***	-0.6891***	-0.6370***	-0.6209***	-0.6423***	-0.6187***
	(0.00507)	(0.01428)	(0.00654)	(0.01415)	(0.00576)	(0.00838)	(0.01048)	(0.01374)	(0.00937)	(0.01425)	(0.00862)	(0.01108)
Fuzzy RK estimates												
Change in outcome per 1 SEK daily benefits	-0.8846	-2.5843	-0.8455	-1.1015	1.5064	2.3518	-1.6021	0.4060	-7.3300	-6.9989	-3.4450	-2.9469
	(1.34878)	(3.83919)	(1.78363)	(3.86317)	(1.40612)	(2.07074)	(2.69070)	(3.53221)	(4.49030)	(6.63478)	(3.40341)	(4.45764)
Implied elasticity												
% Change in outcome per 1% change in daily benefits	-0.3961	-1.1571	-0.3786	-0.4932	0.6768	1.0566	-0.7197	0.1824	-3.6769	-3.5108	-1.7281	-1.4782
	(0.55387)	(1.73349)	(0.72503)	(1.70629)	(0.65448)	(0.97496)	(1.17045)	(1.56749)	(2.35968)	(3.37040)	(1.70181)	(2.39128)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00	850.00	850.00	850.00	971.43	971.43	971.43	971.43
Covariates			✓	✓			✓	✓			✓	✓
Implied change (%)	-0.059	-0.172	-0.056	-0.073	0.101	0.158	-0.107	0.027	-0.543	-0.519	-0.255	-0.218
Outcome mean around kink	1504	1504	1504	1504	1493	1493	1493	1493	1349	1349	1349	1349
Bandwidth	101.4	101.4	85.8	85.8	131.4	131.4	89.2	89.2	72.7	72.7	86.7	86.7
Number of observations	84,276	84,276	71,570	71,570	46,400	46,400	31,741	31,741	17,325	17,325	20,490	20,490

Panel B: Number of visits

	Payment days 1–100				Payment days 101–200				Payment days 201–300			
	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction
First stage estimates												
Change in daily benefits per 1 SEK daily wage	-0.7592***	-0.7341***	-0.7595***	-0.7309***	-0.7397***	-0.7257***	-0.7172***	-0.6961***	-0.6409***	-0.6322***	-0.6408***	-0.6233***
	(0.00318)	(0.01452)	(0.00283)	(0.01600)	(0.00429)	(0.00532)	(0.00957)	(0.01237)	(0.00829)	(0.01540)	(0.00901)	(0.01503)
Fuzzy RK estimates												
Change in outcome per 100 SEK daily benefits	-0.0102	-0.0493	0.0018	-0.0741	0.0098	0.0159	-0.0327	0.0104	-0.0571	0.0279	-0.0637	-0.0015
	(0.02302)	(0.09508)	(0.02035)	(0.10604)	(0.02698)	(0.03344)	(0.05801)	(0.07550)	(0.09699)	(0.18299)	(0.10174)	(0.17041)
Implied elasticity												
% Change in outcome per 1% change in daily benefits	-0.1853	-0.8934	0.0333	-1.3437	0.1811	0.2947	-0.6069	0.1931	-1.1494	0.5606	-1.2809	-0.0305
	(0.44595)	(1.70433)	(0.39960)	(1.89153)	(0.49229)	(0.60431)	(1.04803)	(1.35417)	(1.85858)	(3.16963)	(2.08068)	(3.13172)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00	850.00	850.00	850.00	971.43	971.43	971.43	971.43
Covariates			✓	✓			✓	✓			✓	✓
Implied change (%)	-2.753	-13.272	0.495	-19.960	2.701	4.395	-9.050	2.880	-16.984	8.284	-18.928	-0.450
Outcome mean around kink	0.37	0.37	0.37	0.37	0.36	0.36	0.36	0.36	0.34	0.34	0.34	0.34
Bandwidth	133.5	133.5	143.9	143.9	160.9	160.9	95.6	95.6	80.7	80.7	77.8	77.8
Number of observations	109,639	109,639	117,596	117,596	56,117	56,117	34,028	34,028	19,191	19,191	18,522	18,522

Panel C: Any visits

	Payment days 1–100				Payment days 101–200				Payment days 201–300			
	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction
First stage estimates												
Change in daily benefits per 1 SEK daily wage	-0.7592***	-0.7447***	-0.7569***	-0.7403***	-0.7553***	-0.7732***	-0.7475***	-0.7026***	-0.6465***	-0.6157***	-0.6423***	-0.6220***
	(0.00340)	(0.00802)	(0.00370)	(0.01116)	(0.00268)	(0.00382)	(0.00349)	(0.00910)	(0.00776)	(0.01325)	(0.00784)	(0.01520)
Fuzzy RK estimates												
Change in outcome per 100 SEK daily benefits	0.0049	0.0105	0.0058	-0.0179	-0.0053	-0.0074	0.0001	0.0116	-0.0028	-0.0019	-0.0392	-0.0517
	(0.00895)	(0.02113)	(0.00970)	(0.02849)	(0.00660)	(0.00895)	(0.00835)	(0.02122)	(0.02855)	(0.05004)	(0.03170)	(0.06279)
Implied elasticity												
% Change in outcome per 1% change in daily benefits	0.1673	0.3587	0.1977	-0.6139	-0.1738	-0.2413	0.0035	0.3782	-0.1066	-0.0702	-1.4843	-1.9566
	(0.35557)	(0.75303)	(0.38071)	(1.06624)	(0.23612)	(0.31430)	(0.25977)	(0.70563)	(1.07519)	(2.05451)	(1.21866)	(2.36954)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00	850.00	850.00	850.00	971.43	971.43	971.43	971.43
Covariates			✓	✓			✓	✓			✓	✓
Implied change (%)	2.485	5.329	2.936	-9.120	-2.591	-3.598	0.052	5.640	-1.575	-1.038	-21.933	-28.912
Outcome mean around kink	0.20	0.20	0.20	0.20	0.21	0.21	0.21	0.21	0.18	0.18	0.18	0.18
Bandwidth	128.5	128.5	121.7	121.7	221.3	221.3	186.6	186.6	96.3	96.3	90.1	90.1
Number of observations	105,855	105,855	100,474	100,474	75,346	75,346	64,607	64,607	22,550	22,550	21,209	21,209

Notes. This table presents coefficients and standard errors of the effect of UI benefits on the costs of healthcare use. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification, a uniform kernel, and MSE-optimal bandwidths following Calonico et al. (2014b). We report conventional estimates (columns labeled "Local linear") and estimates with quadratic bias correction and robust standard errors (columns labeled "+ Bias correction"), with and without controlling for pre-determined covariates. Standard errors are clustered at the individual level. Outcomes are the total costs of outpatient care visits (Panel A), the total number of outpatient care visits (Panel B), and an indicator for having any outpatient care visits (Panel C). We show estimates separately for payment days 1–100 (columns 2-5), payment days 101–200 (columns 6-9), and payment days 201–300 (columns 10–13) of the unemployment spell. For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, and rows 5–6 show the implied elasticities. For elasticities, we obtain standard errors via a non-parametric bootstrap where we sample unemployment spells with replacement. Row 7 indicates the kink point, row 8 indicates whether covariates are included, and row 9 expresses the fuzzy RK estimate as a percent of the outcome mean around the kink. The last three rows show the outcome sample mean around the kink (using observations within 10 SEK of the kink), the MSE-optimal bandwidth, and the number of observations within the bandwidth.

Appendix Table 3.8: Effect of Unemployment Benefits on Having Any Drug Purchases

	Payment days 1–100				Payment days 101–200				Payment days 201–300			
	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction	Local linear	+ Bias correction
First stage estimates												
Change in daily benefits per 1 SEK daily wage	-0.7568***	-0.7442***	-0.7588***	-0.7467***	-0.7396***	-0.6920***	-0.7187***	-0.6953***	-0.6482***	-0.6197***	-0.6424***	-0.6159***
	(0.00381)	(0.00762)	(0.00335)	(0.00657)	(0.00427)	(0.01778)	(0.00804)	(0.01131)	(0.00691)	(0.01076)	(0.00861)	(0.01213)
Fuzzy RK estimates												
Change in outcome per 100 SEK daily benefits	-0.0274*	-0.0223	-0.0080	-0.0016	-0.0261*	-0.0531	0.0088	0.0161	0.0373	0.0744	0.0183	0.0642
	(0.01249)	(0.02501)	(0.01070)	(0.02106)	(0.01293)	(0.05321)	(0.02357)	(0.03307)	(0.03261)	(0.05201)	(0.04212)	(0.06038)
Implied elasticity												
% Change in outcome per 1% change in daily benefits	-0.3862*	-0.3142	-0.1124	-0.0224	-0.3613	-0.7363	0.1215	0.2233	0.5016	1.0007	0.2467	0.8646
	(0.17962)	(0.33488)	(0.16101)	(0.29495)	(0.19316)	(0.77624)	(0.32753)	(0.47659)	(0.48889)	(0.82123)	(0.70135)	(1.04255)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00	850.00	850.00	850.00	971.43	971.43	971.43	971.43
Covariates			✓	✓			✓	✓			✓	✓
Implied change (%)	-5.737	-4.668	-1.670	-0.332	-5.387	-10.981	1.812	3.331	7.412	14.787	3.646	12.775
Outcome mean around kink	0.48	0.48	0.48	0.48	0.48	0.48	0.48	0.48	0.50	0.50	0.50	0.50
Bandwidth	119.6	119.6	129.4	129.4	161.3	161.3	105.0	105.0	105.7	105.7	86.8	86.8
Number of observations	98,760	98,760	106,530	106,530	56,230	56,230	37,375	37,375	24,528	24,528	20,501	20,501

Notes. This table presents coefficients and standard errors of the effect of UI benefits on having any healthcare use. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification, a uniform kernel, and MSE-optimal bandwidths following Calonico et al. (2014b). We report conventional estimates (columns labeled "Local linear") and estimates with quadratic bias correction and robust standard errors (columns labeled "+ Bias correction"), with and without controlling for pre-determined covariates. Standard errors are clustered at the individual level. The outcome is an indicator for having any drug purchases, i.e. having total costs of drug purchases greater than zero. We show estimates separately for payment days 1–100 (columns 2-5), payment days 101–200 (columns 6-9), and payment days 201–300 (columns 10–13) of the unemployment spell. For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, and rows 5–6 show the implied elasticities. For elasticities, we obtain standard errors via a non-parametric bootstrap where we sample unemployment spells with replacement. Row 7 indicates the kink point, row 8 indicates whether covariates are included, and row 9 expresses the fuzzy RK estimate as a percent of the outcome mean around the kink. The last three rows show the outcome sample mean around the kink (using observations within 10 SEK of the kink), the MSE-optimal bandwidth, and the number of observations within the bandwidth.

Appendix Table 3.9: Estimates from Regressions Used to Construct Covariate Indices

Panel A: Payment days 1–100

	Total healthcare use	Inpatient and outpatient care	Drug purchases
Constant	5008.57***	4179.69***	828.88***
	(109.65)	(106.21)	(21.54)
Any higher education	-59.17	-67.13	7.96
	(138.30)	(131.68)	(36.30)
Married or cohabiting	-499.81***	-452.80***	-47.01*
	(149.64)	(145.96)	(25.86)
Female	1294.57***	1236.10***	58.46
	(122.28)	(115.21)	(37.14)
Any children under age 18	-513.45***	-365.79**	-147.66***
	(157.53)	(152.91)	(30.80)
Region FEs	✓	✓	✓
Age FEs	✓	✓	✓
Industry FEs	✓	✓	✓
Observations	340,901	340,901	340,901

Panel B: Payment days 1–100

	Total healthcare use	Inpatient and outpatient care	Drug purchases
Constant	5793.66***	4862.71***	930.95***
	(186.20)	(180.38)	(38.25)
Any higher education	-554.59***	-469.92**	-84.67***
	(203.39)	(198.90)	(31.79)
Married or cohabiting	-720.60***	-673.49***	-47.11
	(221.11)	(216.35)	(29.63)
Female	1148.89***	1071.39***	77.50**
	(180.32)	(175.74)	(31.29)
Any children under age 18	-674.36***	-479.78**	-194.58***
	(235.31)	(229.78)	(32.96)
Region FEs	✓	✓	✓
Age FEs	✓	✓	✓
Industry FEs	✓	✓	✓
Observations	151,023	151,023	151,023

Panel C: Payment days 1–100

	Total healthcare use	Inpatient and outpatient care	Drug purchases
Constant	6427.30***	5480.00***	947.31***
	(300.39)	(284.34)	(64.33)
Any higher education	-315.55	-199.45	-116.10**
	(355.67)	(348.98)	(45.58)
Married or cohabiting	-1256.89***	-1205.70***	-51.19
	(337.13)	(331.48)	(41.52)
Female	94.62	-0.79	95.41*
	(285.56)	(272.29)	(57.00)
Any children under age 18	-355.40	-204.90	-150.50***
	(414.62)	(407.42)	(50.90)
Region FEs	✓	✓	✓
Age FEs	✓	✓	✓
Industry FEs	✓	✓	✓
Observations	74,981	74,981	74,981

Notes. This table presents estimated coefficients and their standard errors, clustered at the individual level, from regressions of health-related outcomes against a set of pre-determined covariates. All covariates are measured in the calendar year before the start of the unemployment spell. We construct covariate indices for each outcome as the fitted values from each regression and use these indices in Appendix Figure 3.4, separately for the subsamples of spells where the individual receives benefits during the first 100 payment days (Panel A), during payment days 101–200 (Panel B), and and during payment days 201–300 (Panel C). The rows "Region FEs" refers to indicators for the region (*kommun*) of residence. The rows "Industry FEs" refers to indicators for the industry of the highest-paying employer, also including a separate indicator for missing industry code. We define a person as having any higher education if s/he has completed at least one semester of post-secondary education.

Appendix Table 3.10: Effect of Unemployment Benefits on Predicted Healthcare Use

Panel A: Payment days 1–100

	Total healthcare use	Inpatient and outpatient care	Drug purchases
First stage estimates			
Change in daily benefits per 1 SEK daily wage	-0.7464***	-0.7465***	-0.7435***
	(0.00517)	(0.00784)	(0.00853)
Fuzzy RK estimates			
Change in outcome per 1 SEK daily benefits	-0.2557	0.0047	-0.0105
	(0.24777)	(0.32581)	(0.04717)
Kink point (SEK)	850.00	850.00	850.00
Implied change (%)	0.003	0.002	-0.002
Outcome mean around kink	5248.0	4466.0	782.0
Bandwidth	224.0	102.1	103.8
Number of observations	176,299	84,802	86,079

Panel B: Payment days 101–200

	Total healthcare use	Inpatient and outpatient care	Drug purchases
First stage estimates			
Change in daily benefits per 1 SEK daily wage	-0.6853***	-0.6838***	-0.6830***
	(0.01719)	(0.01641)	(0.01626)
Fuzzy RK estimates			
Change in outcome per 1 SEK daily benefits	-1.0889	-0.8669	-0.1204
	(0.80788)	(0.69124)	(0.10901)
Kink point (SEK)	850.00	850.00	850.00
Implied change (%)	-0.008	-0.008	-0.002
Outcome mean around kink	5614.3	4763.6	850.7
Bandwidth	91.5	94.4	207.9
Number of observations	32,542	33,569	71,254

Panel C: Payment days 201–300

	Total healthcare use	Inpatient and outpatient care	Drug purchases
First stage estimates			
Change in daily benefits per 1 SEK daily wage	-0.6426***	-0.6237***	-0.6328***
	(0.03208)	(0.01467)	(0.01620)
Fuzzy RK estimates			
Change in outcome per 1 SEK daily benefits	-2.7015	-0.3778	0.0552
	(2.52023)	(1.01641)	(0.18022)
Kink point (SEK)	971.43	971.43	971.43
Implied change (%)	0.012	0.003	0.010
Outcome mean around kink	5660.0	4768.4	891.5
Bandwidth	356.9	89.3	86.2
Number of observations	58,416	21,014	20,395

Notes. This table presents coefficients and standard errors of the effect of unemployment insurance benefits on predicted costs of healthcare use. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification with quadratic bias correction, a uniform kernel, and MSE-optimal bandwidths following Calonico et al. (2014b). Standard errors are clustered at the individual level. Outcomes are the predicted total costs of inpatient and outpatient care visits and drug purchases (column 2), predicted total costs of inpatient and outpatient care visits (column 3), and predicted total costs of drug purchases (column 4). We show estimates separately for payment days 1–100 (Panel A), payment days 101–200 (Panel B), and payment days 201–300 (Panel C) of the unemployment spell. For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, row 5 indicates the kink point, and row 6 expresses the fuzzy RK estimate as a percent of the outcome mean around the kink. The last three rows show the outcome sample mean around the kink (using observations within 10 SEK of the kink), the MSE-optimal bandwidth, and the number of observations within the bandwidth. The predicted outcomes are fitted values obtained after regressing each outcome against a set of pre-determined covariates. Appendix Table 3.9 presents the estimation results from these regressions.

Appendix Table 3.11: Effect of Unemployment Benefits on Pre-Determined Covariates

Panel A: Payment days 1–100

	Average age	Share female	Share with higher education	Share with partner	Share with children
First stage estimates					
Change in daily benefits per 1 SEK daily wage	-0.7772***	-0.7385***	-0.7461***	-0.7442***	-0.7454***
	(0.00184)	(0.01597)	(0.00632)	(0.00988)	(0.00700)
Fuzzy RK estimates					
Change in outcome per 1 SEK daily benefits	-0.0176***	0.0005	-0.0000	-0.0004	0.0003
	(0.00154)	(0.00049)	(0.00017)	(0.00030)	(0.00023)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00
Implied change (%)	-0.043	0.038	-0.044	-0.062	0.075
Outcome mean around kink	36.6	0.5	0.2	0.5	0.4
Bandwidth	419.8	77.9	100.6	83.4	118.9
Number of observations	275,461	65,166	83,647	69,694	98,281

Panel B: Payment days 101–200

	Average age	Share female	Share with higher education	Share with partner	Share with children
First stage estimates					
Change in daily benefits per 1 SEK daily wage	-0.6953***	-0.6843***	-0.6981***	-0.6848***	-0.6890***
	(0.01141)	(0.01497)	(0.01007)	(0.01439)	(0.01404)
Fuzzy RK estimates					
Change in outcome per 1 SEK daily benefits	-0.0251**	-0.0007	0.0002	-0.0005	-0.0000
	(0.00815)	(0.00044)	(0.00025)	(0.00044)	(0.00041)
Kink point (SEK)	850.00	850.00	850.00	850.00	850.00
Implied change (%)	-0.055	-0.066	-0.024	-0.053	0.057
Outcome mean around kink	39.9	0.5	0.2	0.5	0.4
Bandwidth	115.7	97.6	115.6	132.3	102.3
Number of observations	41,019	34,705	41,003	46,731	36,435

Panel C: Payment days 201–300

	Average age	Share female	Share with higher education	Share with partner	Share with children
First stage estimates					
Change in daily benefits per 1 SEK daily wage	-0.6411***	-0.6369***	-0.6183***	-0.6218***	-0.6051***
	(0.01739)	(0.01696)	(0.01354)	(0.01508)	(0.01509)
Fuzzy RK estimates					
Change in outcome per 1 SEK daily benefits	-0.0300	0.0015	0.0002	0.0009	-0.0003
	(0.02195)	(0.00100)	(0.00062)	(0.00078)	(0.00068)
Kink point (SEK)	971.43	971.43	971.43	971.43	971.43
Implied change (%)	0.049	0.099	-0.154	0.018	-0.119
Outcome mean around kink	44.1	0.4	0.2	0.5	0.3
Bandwidth	164.7	80.7	72.4	120.5	237.9
Number of observations	35,695	19,204	17,261	27,592	46,534

Notes. This table presents coefficients and standard errors of the effect of unemployment insurance benefits on selected pre-determined covariates, all measured in the calendar year before the start of the unemployment spell. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification with quadratic bias correction, a uniform kernel, and MSE-optimal bandwidths following Calonico et al. (2014b). Standard errors are clustered at the individual level. Outcomes are age (column 2) and indicators for being female (column 3), being married or cohabiting (column 4), having higher education (column 5), and having children under age 18 at home (column 6). We show estimates separately for payment days 1–100 (Panel A), payment days 101–200 (Panel B), and payment days 201–300 (Panel C) of the unemployment spell. For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, row 5 indicates the kink point, and row 6 expresses the fuzzy RK estimate as a percent of the outcome mean around the kink. The last three rows show the outcome sample mean around the kink (using observations within 10 SEK of the kink), the MSE-optimal bandwidth, and the number of observations within the bandwidth.

Appendix Table 3.12: Effect of Unemployment Benefits on Pre-Unemployment Healthcare Use

Panel A: Payment days 1–100

	Total healthcare use	Inpatient and outpatient care	Drug purchases
First stage estimates			
Change in daily benefits per 1 SEK daily wage	-0.7361***	-0.7366***	-0.7428***
	(0.01484)	(0.01492)	(0.00799)
Fuzzy RK estimates			
Change in outcome per 1 SEK daily benefits	-48.3462	-56.5970	5.7704
	(62.23088)	(60.31365)	(8.46304)
Kink point (SEK)	850.00	850.00	850.00
Implied change (%)	-0.031	-0.093	0.094
Outcome mean around kink	14923.1	12806.9	2116.2
Bandwidth	80.0	78.4	112.3
Number of observations	66,842	65,534	93,033

Panel B: Payment days 101–200

	Total healthcare use	Inpatient and outpatient care	Drug purchases
First stage estimates			
Change in daily benefits per 1 SEK daily wage	-0.6864***	-0.6815***	-0.6849***
	(0.01762)	(0.01629)	(0.01686)
Fuzzy RK estimates			
Change in outcome per 1 SEK daily benefits	-23.7540	-13.7811	10.7407
	(65.13779)	(57.59575)	(12.37813)
Kink point (SEK)	850.00	850.00	850.00
Implied change (%)	-0.307	-0.237	0.186
Outcome mean around kink	12763.8	10512.4	2251.4
Bandwidth	81.1	83.6	152.3
Number of observations	28,965	29,773	53,389

Panel C: Payment days 201–300

	Total healthcare use	Inpatient and outpatient care	Drug purchases
First stage estimates			
Change in daily benefits per 1 SEK daily wage	-0.6420***	-0.6320***	-0.6270***
	(0.01681)	(0.01590)	(0.01681)
Fuzzy RK estimates			
Change in outcome per 1 SEK daily benefits	-114.1242	-115.6981	-4.8924
	(87.89557)	(66.67012)	(21.21367)
Kink point (SEK)	971.43	971.43	971.43
Implied change (%)	-0.237	-0.431	-0.167
Outcome mean around kink	9976.2	8041.4	1934.8
Bandwidth	159.4	84.4	156.6
Number of observations	34,749	20,018	34,268

Notes. This table presents coefficients and standard errors of the effect of unemployment insurance benefits on healthcare use measured in the calendar year before the start of the unemployment spell. The table uses the analysis sample of unemployment spells with a start date between March 5, 2007 and July 14, 2014 (see Section 3.3). The unit of observation is an unemployment spell. Estimates are based on a local linear specification with quadratic bias correction, a uniform kernel, and MSE-optimal bandwidths following Calonico et al. (2014b). Standard errors are clustered at the individual level. Outcomes are the total costs of inpatient and outpatient care visits and drug purchases (column 2), total costs of inpatient and outpatient care visits (column 3), and total costs of drug purchases (column 4). We show estimates separately for payment days 1–100 (Panel A), payment days 101–200 (Panel B), and payment days 201–300 (Panel C) of the unemployment spell. For each column, rows 1–2 show the first stage estimates, rows 3–4 show the fuzzy RK estimates, row 5 indicates the kink point, and row 6 expresses the fuzzy RK estimate as a percent of the outcome mean around the kink. The last three rows show the outcome sample mean around the kink (using observations within 10 SEK of the kink), the MSE-optimal bandwidth, and the number of observations within the bandwidth.

Chapter 4

Family-Level Stress and Children's Educational Choice: Evidence from Parent Layoffs*

*This chapter is co-authored with Julia Tanndal. We thank David Seim, John Friedman, Brian Knight, Matthew Lindquist, José Montalbán Castilla, Emily Oster, Neil Thakral, and Jonas Vlachos as well as seminar participants at Brown University and Stockholm University for helpful comments. We thank Simon Kurt and Jan Eriksson at SCB and Arbetsförmedlingen for assistance with administrative data access. Funding has been received from the James M. and Cathleen D. Stone Wealth and Income Inequality Project.

4.1 Introduction

Educational attainment is highly correlated with parental income in a range of different settings.[1] This correlation may reflect a causal effect where poorer households are constrained from investing in education. Traditionally, such economic constraints are modeled as credit constraints that arise because credit markets are incomplete with respect to future earnings (e.g. Lochner and Monge-Naranjo, 2012). However, the evidence of credit constraints as a major impediment for poor families to invest in education is contested (Francesconi and Heckman, 2016; Lovenheim, 2011).

Family income can have a negative effect on children's skill formation even beyond credit constraints. [2] It is possible that economic insecurity affects parents' ability to make optimal long-term decisions or restrict parental time available for involvement in their child's education, both of which are crucial for educational outcomes.[3] Another factor highlighted in research on education and child psychology points to the causal effect of parental involvement in children's education on educational outcomes.[4]

In this paper, we study the effect of a parental income shock on children's educational outcomes beyond credit constraints, in the context of upper secondary school track choice in Sweden. As in most OECD countries, Swedish families choose whether their children should pursue a vocational or theoretical education at age 15.[5] This setting is particularly helpful because education is free, but parents' involvement in this complex choice is crucial. We esti-

[1]For US colleges, income has become increasingly predictive of enrollment for men in a way that cannot be explained by ability or college tuition (Lovenheim & Reynolds, 2011). Admission of middle-class students would be substantially higher if admissions were based on SAT scores alone (Chetty et al., 2020). When school access is based on proximity, the quality of school is priced into the housing market (Black, 1999). Even in the case of free school choice, the opportunity to choose tends to be more utilized by families with higher socio-economic status (Ambler, 1994; Skolverket, 2003). Belley and Lochner (2007) describe how the correlation between family income and college enrollment has increased over time.

[2]For example, Heckman and Mosso (2014) emphasize the empirical role of parenting in skill formation.

[3]For example, Sapolsky (2018) documents the increasing body of research in neurology showing how the increased level of stress associated with lower socio-economic status can lead to reduced function in the prefrontal cortex, associated with executive function and long-term planning.

[4]See Huat See and Gorard, 2015, for a review.

[5]The OECD average of the starting age of upper secondary school is 15.2, in Sweden it is 16. (OECD, 2019, Table X1.1b (2014))

mate the effect of parental economic stress during this choice on children's long-term educational outcomes. This allows us to capture any non-financial constraints on low-income parents' ability to provide optimal levels of education for their children.

We estimate the impact of family-level economic insecurity caused by a parent getting laid off. Layoffs are registered in our data when an employer needs to terminate the employment of five or more workers in a single region due to a long term reduction in labor demand. This decrease is driven by planned or unplanned shocks at the firm level such as plant closures, reduced demand for the firm's output, reorganisation of production, etc. Which employee is affected is predetermined by tenure or prior collective bargaining agreements. Therefore, we can rely on the exact timing of the layoff event to be exogenous to the characteristics of an individual's family, primarily the age of his children at the time. This allows us to use variation in the child's age and school progress at the time of the layoff event to estimate the heterogeneous impact of a parental layoff at different points in the child's life.

We find that children in families with a parental layoff are significantly less likely to finish high school than their peers. The effect is concentrated to children for whom the parental layoff coincides with the time when they transition from compulsory to upper secondary school (ages 15–16). The estimated likelihood of completing high school on time decreases from 73 to 58 percent for children whose parents are laid off 6–12 months before the school transition. For children who are already enrolled in high school at the time of the shock, graduation probability drops around 3 percentage points. We do not find any evidence that children leave high school to take gainful employment.

The effects appear to be driven by parents' time investment in their children's education. The effect of parental layoffs are larger when parental involvement in children's schooling is necessary, and the cost of such involvement is higher. The negative effects are larger when the layoff occurs before the time when children apply for high school, a time when parental support is crucial, than layoffs that happen within the same school semester but after the application deadline. Additionally, families with less information about the high school choice at the time of layoff are more adversely affected than families with more information. We use siblings as a proxy for the information level of the family at the time of the event. If the shock affects the school choice of the oldest child, the estimated effect on graduation is large, but for younger siblings, the estimated impact is not statistically different from zero. Since a younger sibling has the advantage of more family-level information about the choice before age 15, she will not be as adversely affected by a reduction of parental investment at the time.

Related literature. Our findings contribute to the literature on the scarring effects of layoffs as well as to understanding the role of parental income in children's education. A long-standing literature documents that involuntary job loss leads to large and persistent negative effects on the displaced workers' subsequent earnings, labor force participation and employment stability as well as adverse effects on mental health.[6]

There is some evidence of an effect of parental job loss on children in the short run. Rege et al. (2011) find a negative impact on children's GPA at age 16 when fathers loose their job due to plant closures. Children also appear to obtain some information from parents' unemployment, and are less likely to study in the same field as their parent if the layoff occurs during the child's teenage years (Huttunen & Riukula, 2019).

The effects of parental job loss on children's future earnings are less clear. Oreopoulos et al. (2008) find a statistically significant 9 percent negative effect on future earnings for children aged 10-14 at the time of parental job loss, while Mörk et al. (2020) and Hilger (2016) find no indication of negative future labor market outcomes for children who experienced a parental job loss. Mörk et al. (2020) find no effect on high school graduation rates, early unemployment or utilization of social assistance for parental job losses at child ages 6-18. Hilger (2016) finds no significant effect on college enrollment and only small effects on the education quality for parental layoffs at child ages 18-22. The different results may be driven by the different institutional settings or dif-

[6]For notable contributions on the effects of job displacement on earnings and unemployment, see e.g. Jacobson et al., 1993, Von Wachter et al., 2009, Couch and Placzek, 2010 and Stevens, 1997 for the U.S. and Eliason and Storrie, 2006 and Seim, 2019 for Sweden. Job loss also increases stress and mental distress, which affects health outcomes. Sullivan and Von Wachter, 2009 and Eliason and Storrie, 2009 find considerably higher mortality rates among displaced workers, while Black et al., 2015 and Mörk et al., 2020 show that job loss increases the risk of cardiovascular health problems and hospitalizations due to alcohol use and mental health problems. Brand, 2015 provides an overview of the sociological literature on the effects of job loss on the well-being of parents and children.

ferences in methodology. Oreopoulos et al. (2008) and Mörk et al. (2020) use firm closures to identify exogenous job losses, while Hilger (2016) uses layoff data. It could also be the case that effects of parental job loss on children are heterogeneous by age. We contribute to this literature by explicitly measuring heterogeneous treatment effects over the child's age and education, and testing the institutional mechanisms behind the differences across age. Similar to Hilger (2016), we use layoffs to identify job losses, as this method provides a natural control group, reducing the concern for selection into jobs based on family-level unobservables.

We also contribute to the literature on the role of parent income in children's education. Parent income matters for children's educational attainment (Lovenheim & Reynolds, 2011). Yet, Heckman and Mosso (2014) summarize the research on the role of family income in education by concluding that there is little evidence to support a pure cash transfer to poor families as a successful method to increase children's education. Lovenheim (2011) also finds little direct evidence that credit constraints are driving the increasing gap in educational attainment. Parenting does matter for children's outcomes, however (see Huat See and Gorard, 2015 for a review). Intervention studies have shown that increasing parent's information and involvement can have positive behavioral effects on their children (see for example Barrera-Osorio et al., 2020; Rogers and Feller, 2018; Spoth et al., 2008). We contribute to understanding what affects parental involvement in absence of active interventions. We also link this to economic insecurity, contributing to a fuller understanding of why family income matters, beyond credit constraints.

Outline. The remainder of the paper is structured as follows. Section 4.2 describes the Swedish school system and the administrative datasets we use in the analysis. Section 4.3 discusses our empirical strategy. In Section 4.4, we first discuss the effect of a layoff on parents, after which we look at the outcomes of their children. In Section 4.5, we test potential mechanisms and narrow the most sensitive time for children down to a few months before the high school choice. Section 4.6 concludes.

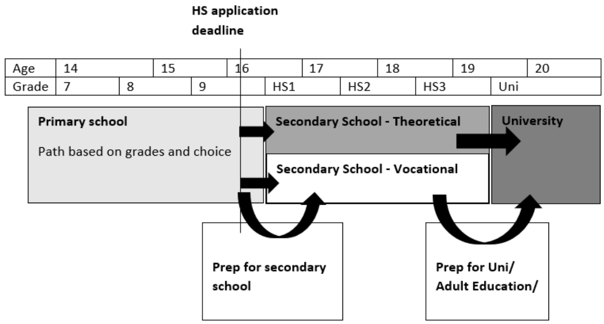


Figure 4.1: Schooling in Sweden

Notes. This figure shows the Swedish education system from grade 7 to university. The academic calendar runs from August to June, and class assignment is based on year of birth. This gives the overlapping of age (age 1 on January 1st, the year after birth year) and grade. In the second semester of grade 9, at age 16, students apply to the high school program of their choice.

4.2 Background

4.2.1 School Choice

Upper secondary school in Sweden, like most OECD countries, offers students age 16–18 a choice of specialization through a number of different vocational and theoretical degrees.[7] This section describes the details around the timeline and the choice market that prospective upper secondary students face.

Figure 4.1 illustrates the Swedish school system. There are 18 nationally standardized programs which can be either theoretical or vocational. A vocational program teaches occupation-specific skills as well as academic subjects, but may not ensure university-level qualifications. Around 30 percent vocational high school graduates attend at least one year of higher education, compared to 70 percent of graduates with a theoretical degree (see Table 4.1).

The choice environment varies by location and time. Since the school reform in 1992, the establishment of privately-run schools has changed the

[7] At least 40 percent of upper secondary school students attend vocational programs in Australia, Austria, Belgium, Brazil, Chile, the Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Italy, Japan, Korea, Latvia, Lithuania, Luxembourg, the Netherlands, Norway, Poland, Portugal, Russia, Slovakia, Slovenia, Spain, Sweden, Switzerland and Turkey (see OECD, 2019, Figure B3.3).

Table 4.1: High School Choice

Panel A: High school applications		
	Mean	(sd)
Nr Choices	3.343	(1.439)
Attend 1st choice	0.652	(0.476)
Admitted to first choice	0.897	(0.304)
Panel B: Track choice		
	Vocational	Theoretical
Share of HS students	0.54	0.42
Share in track who proceed to university	0.72	0.29

Notes. Summary statistics on high school applications are based on data from Statistics Sweden. Panel A uses data on high school applications and defines an admission as the students having passing grades at least as high as the minimum grade among students admitted to the program. Panel B shows the share of High school graduates who complete a Vocational or theoretical degree. In Panel B, individualized programs are classified as neither theoretical nor vocational. Individualized programs are most commonly set up for students who did not pass the minimum requirement to finish compulsory school on time, but can also be applied to other cases, for example, sports programs. The last row shows the share of students in each type of program who go on to get some university credits by age 30.

market. 53 percent of 15 year-olds in 2000 had access to most (at least 15 out of 18) nationally standardized programs, while 71 percent had the same access in 2010. The average number of schools per market has increased from 9 to 12.5. At the same time, the markets are fewer, which means that more students commute across municipalities to their high school. Furthermore, the share of potential students who live in a municipality with no school at all has grown from 33 percent to 42 percent (see Appendix Table 4.1 for details; see also Fjellman et al., 2019).

Students who wish to attend high school need to apply to programs in February, their last semester of compulsory school (the exact deadline varies by year and municipality, usually between February 1st and February 15th). Students usually apply to 2–4 programs, ranked by preference (Table 4.1). Most students who passed the minimum requirement for compulsory school completion are accepted to their first choice, but in the case of limited slots, allocation is based on grades. The preliminary admittance is based on grades in the Fall semester of grade 9 and final admittance is based on the final grades. National exams, which are important for determining the final grades, are administered in late April and early May during the final semester of grade 9, when students are 16 years old.[8]

A failure to pass necessary subjects in primary school will require the student to finish those subjects before entering high school. An individualized program is set up for the student, which is designed to on-board the student into a vocational degree of her choice, but may take more than the standard three years (Skolverket, 2020).

4.2.2 Data

This section describes the Swedish administrative data used in the analysis. We combine individual-level data on employment, earnings, and educational attainment with data on family links and a detailed registry of all layoff events.

The notification registry includes firm layoff notifications of five or more workers between 2005–2013. By law (Lag [1974:13]), all firms at risk of laying off more than five full-time employees in the region must notify the local unemployment agency office between 6 weeks to 6 months before the termination of employment. At the individual-level, we observe the termination date, which can be several months after the first notification at the firm-level. We treat the termination date as the relevant time of layoff, as this is the time when income drops and the employment is terminated.

Layoffs are driven by a reduction in labor demand. Stated reasons for notifications include plant closure, bankruptcy, reduction in production, or changes in the production process. Separations due to worker misbehavior are not included in this dataset. See Appendix 4.A.1 for a detailed description of the layoff and unemployment process. While employees facing layoffs are still negatively selected based on observables compared to the population at large, the restriction to firm-induced layoffs implies that the exact timing of the layoff is exogenous to employee characteristics. Appendix Figure 4.2 shows the variation over time in the total number of layoffs and the characteristics of the

[8] School classes are based on year of birth, and the academic year runs from August to June. In the rest of the paper we discuss child age in years since birth, abstracting from school grade, as the two definitions overlap.

affected employees. Pre-layoff income increases for layoffs after the Great Recession, while the share of layoffs in the manufacturing sector decreases.

We restrict our parent sample to individuals who experience a layoff or individuals who are at risk of layoff. We define an employee as having a non-zero risk of layoff if she is currently working at a firm which ever appears in the layoff data. This excludes most public sector jobs as well as a lot of jobs in sectors dominated by individuals with a university degree. For a comparison of the layoff sample to the general population, compare columns (1) and (5) in Table 4.2.

Our population of interest are children who may be affected by parental layoff. As the notification data covers layoffs in 2005–2013, we focus on families with children born between 1980 and 1992. This ensures that we can have a balanced panel of child outcomes for children aged 13–23.[9] The panel data follows individuals from age 18 (or 16 if they are employed) and onward. We know the biological parents of the children, and can also observe the household they live in when they first appear in the data. We can also observe whether both biological parents are in the same household in any given year. We observe the highest level of education by year: high school is registered after graduation, while college is registered as years of enrollment. High school graduation usually occurs at age 19, but we see students who finish at age 20 and 21 as well. Students enroll in university at ages 20–23. See Appendix Figure 4.3 for details on education levels by age. We also observe the field/major of education.

For parents, we observe a panel of income and employment from 1991–2015. Employer-employee match data indicates which individuals are currently working at firms in our layoff data, which makes it possible to identify individuals who are employed at a firm with layoff risk. We focus on parent income and industry of employment as matching characteristics, as well as demographic variables. Table 4.2 shows the characteristics of the parents in our sample, as well as in the population at large. These tables also illustrate the matching procedure, which we turn to next.

[9] See Appendix Figure 4.4 for the distribution of observations over the year $\times$ cohort $\times$ age distribution.

Table 4.2: Summary Statistics at Time of Matching

	(1) Layoff	(2) Control group	(3) Layoff matched	(4) Control matched	(5) Pop. >30yo
Female	0.265	0.265	0.207	0.208	0.464
	(0.442)	(0.441)	(0.405)	(0.406)	(0.499)
Age	40.69	41.49	40.62	40.66	39.29
	(5.225)	(5.267)	(4.645)	(4.645)	(7.325)
Labor income	274849.3	301971.3	297828.8	299090.6	239647.7
(2010 SEK)	(161831.4)	(210699.1)	(170528.3)	(209228.8)	(201762.2)
Income vintile	13.77	14.62	14.51	14.41	12.76
	(4.262)	(4.356)	(3.965)	(4.074)	(5.130)
Highest ed:	0.119	0.166	0.123	0.128	0.296
University	(0.324)	(0.372)	(0.329)	(0.334)	(0.457)
Highest ed:	0.538	0.502	0.542	0.542	0.408
High school	(0.499)	(0.500)	(0.498)	(0.498)	(0.492)
Married	0.557	0.546	0.569	0.584	0.347
	(0.497)	(0.498)	(0.495)	(0.493)	(0.476)
Unemployed	0.0779	0.0379	0.0607	0.0408	0.0845
	(0.268)	(0.191)	(0.239)	(0.198)	(0.278)
Tenure at job	57.81	71.90	60.63	72.68	49.95
(months)	(41.32)	(42.82)	(41.64)	(44.20)	(35.66)
Manufacturing	0.455	0.468	0.460	0.461	0.236
	(0.498)	(0.499)	(0.498)	(0.498)	(0.425)
Year	2001.5	2001.0	2001.6	2001.7	2001.5
	(2.352)	(2.694)	(2.235)	(2.226)	(2.332)
Any children?	1	1	1	1	0.893
					(0.309)
N	17172	680786	11323	48277	6469957

mean coefficients; sd in parentheses

Notes. Matching is based on child year of birth and gender (not shown) as well as parent age, gender, individual and household income and industry of last employer. Time-varying characteristics are all matched based on characteristics measured when the child is 12 years old. Note that layoff probability decreases by tenure on the job.

4.3 Empirical Strategy

To identify the effect of a parental layoff on children's educational outcomes, we rely on variation in the child's age at the time of layoff, as well as matching families who experience parental layoffs to families who do not, but where a parent is at risk of getting laid off. In this section, we first describe the restrictions on the control group and the matching procedure. Second, we discuss the assumption that the exact timing of parental layoff is exogenous of the child's year of birth. Third, we describe how this assumption is used to estimate the expected difference in potential outcomes between the treatment and control group, and we explain our estimation procedure.

In addition to families who experience a parental layoff, we use families in which one parent is at risk of layoff as a control group. We define all firms in the notification data as firms with a non-zero layoff risk, as they all have had at least one negative shock causing layoff over the 8 year period in which we observe layoffs. Any individual who is working for a firm that ever appears in the layoff data is considered to have a non-zero risk of layoff.

A parent in our control group will not be laid off in year t and will still be employed at the firm in year $t+1$. Her exposure to layoff risk can either come from a layoff at the firm in year t where she wasn't notified or from a layoff in a different year, before or after t. We do not require the control to be laid off in a future event, nor that she still is employed by the firm when it happens. So while we ensure all parents are exposed to risk, we will have a pure control group and not have to adjust our estimates for staggered event times.[10]

In addition to this restriction of our control group, we also perform a coarse exact matching on child and parent characteristics to ensure the treatment and control group are similar based on observables. Initially, we do an exact matching of the gender and birth year of the child, as well as the gender of the parent. For a given event year, we match parents who are laid off in this year to parents of the same age who are at risk of layoff by being connected to an at-risk firm the same year. For the families in the control group, this year is their potential, or placebo, layoff year.[11] We also match the families based on their income

[10] See Borusyak et al. (2024) for a discussion of a pure control group as a solution to the problem of staggered event studies, highlighted for example by Sun and Abraham (2021) and Goodman-Bacon (2021).

[11] We allow control families to enter with several different potential layoff years, as the exact

and industry of employment (2-digit level). We match all families on income and industry characteristics measured when the child is age 12. This ensures that the control group includes families with similar childhood experiences, regardless of when in the child's life the parental layoff occurs.

Table 4.2 presents the result of the matching. Columns 1 and 2 show the parents with a layoff and the parents at risk of layoff (columns "Layoff" and "Control group") without additional matching on child and parent characteristics. Columns 3 and 4 show the result of matching the control group to the layoff group. Column 5 shows the overall population over age 30 over the same time range. Restricting the control group to parents with a non-zero risk of layoff is the most important step to ensure a good match on observables. Adding the match on child and parent characteristics increases the precision of the similarity between the groups on the set of matched characteristics and on parental education level and marriage rates. Even after matching, there are some observable differences between the treatment and control group, for example in the tenure at the current job. Tenure is a good example of a variable that we know drives the likelihood of layoff, due to the mandated last-in first-out principle. There are likely to be other factors, related to layoff probability but unobservable to us, that are not balanced over the treatment and control group by this matching procedure. Therefore, to adjust for selection on observables we also rely on variation in the exact timing of parental layoffs. This is where we turn next.

As layoffs are induced by negative shocks to firms, we assume that the exact timing of the layoff is exogenous to the age of the child at the time of layoff.[12] Even if there are unobserved family characteristics that predict both the future educational attainment of the children and the likelihood of layoff for the parent, the exact arrival time of the layoff shock needs to be unrelated to the birth year of the child. Figure 4.2 does not reject this assumption, as it shows that conditional on event year and parent age, we find no significant correlation between layoff probability and child age in the sample of parents at risk of layoff.

If potential outcomes differ systematically between the treatment and control group (selection into layoff) then we can use the random variation in the

matching procedure will reduce the weight of the observation accordingly.

[12]See the discussion of the definition of a layoff in our data in Section 4.2.2.

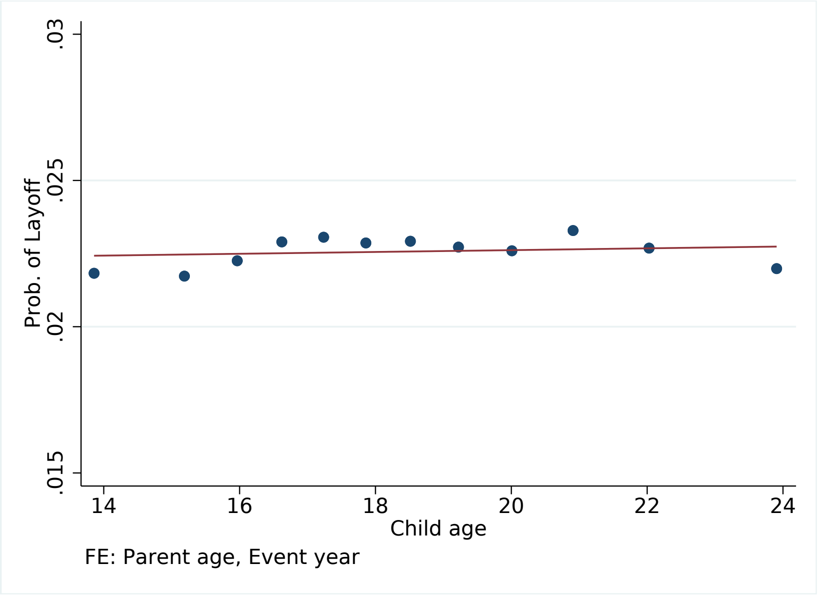


Figure 4.2: Likelihood of Parental Layoff

Notes. The figure shows a binned scatterplot of the likelihood of layoff in the sample of parents connected to firms in the Notification registry. Conditional on parent age and year of layoff, we cannot reject the null hypothesis that there is no linear relation between child age and parent layoff probability. The 95 percent confidence interval on the linear coefficient is (-.00005, .00011) with a p-value of 0.428.

age of the child to estimate this difference. Let $Y_i(Z)$ be the outcome for an individual i after receiving the treatment Z, where $Z \in \{0, z\}$ can be either no parental layoff, $Z = 0$, or a parental layoff in the year the individual is z years old, $z \in [14, 25]$. Let S_1 be the set of treated individuals and S_0 be the set of non-treated individuals. Under the assumption that selection into layoff is independent of the age of the child at the time of layoff, then the expected outcome for any $Z = z$ are the same. For example, it is as-good-as-random whether the child experiences the parental layoff at age 18 or age 20, that is, $E(Y_i(Z)|z = 18, i \in S_1) = E(Y_i(Z)|z = 20, i \in S_1)$.

Consider the main outcome of interest in our analysis, on-time high school graduation. This is a one-time event, which either realizes or not at age 19. If a child is treated at age 20, this cannot have any effect on her outcome, as it was already realized in the previous year. Therefore, the potential outcome for treatments at age 20 or above will be equal to the potential outcome of not being treated at all, that is, $Y_{i,19}(z \geq 20) = Y_{i,19}(Z = 0)$. This allows us to obtain an estimate of the counterfactual effect of not being treated for the control group.

$$
\begin{aligned}
&E(Y_{i,19}(z \geq 20)|i \in S_1) = E(Y_{i,19}(Z = 0)|i \in S_1) \\
&E(Y_{i,19}(Z = 20)|i \in S_1) - E(Y_{i,19}(Z = 0)|i \in S_0, \text{age} = 20) = \\
&E(Y_{i,19}(Z = 0)|i \in S_1) - E(Y_{i,19}(Z = 0)|i \in S_0)
\end{aligned}
$$

Any difference between the treatment and control group observed at age 20 is therefore entirely driven by differences in potential outcomes. Also note that if we assume that the timing of layoff would be random also for individuals in our control group, the difference in potential outcomes between treatment and control does not vary by age. Hence, we can use the estimated difference in outcomes at age 20 as an estimate of the average difference in potential outcomes for all age groups. This difference $(Y_{i,19}(Z = 20)|i \in S_1) - E(Y_{i,19}(Z = 0)|i \in S_0, \text{age} = 20)$ is observable, and we use the estimated value of this difference to normalize the estimation of treatment effects.

Equation 4.1 shows the regression specification we use to estimate the effect of parental layoffs on children's educational outcomes. Note that, even though we have panel data on families, the regression is cross-sectional at the child-level. The outcome of interest, y_{i,a_O}, is observed once for each child i at age a_O. The variable of interest is the layoff status of the parent at the child's age and semester in school (a_E). In order to capture the different schooling contexts for children at the time of parental layoff, we define age as the age in semesters, from January – June when the child is 13 ($a_E = 1$) to July – December when the child is 25 ($a_E = 24$). As we are interested in the heterogeneous effect of parental layoffs over the child's schooling, we will estimate a set of coefficients δ_{a_E} to describe the average effect of parental layoffs in each age-semester a_E.

$$
y_{i,a_O} = \sum_{a_E=1}^{24} \delta_{a_E} \mathbf{1}_{[\text{Layoff}_{a_E,i}=1]} + \tau_c + \gamma_{t_E} + \beta X_i + \varepsilon_i \tag{4.1}
$$

A child in the control group will have no positive values of the dummies $\text{Layoff}_{a_E,i}$. The control group is crucial to capture the unequal distribution of time, age and cohort (Appendix Figure 4.4 illustrates this issue) by cohort and event year fixed effects, τ_c, γ_{t_E}. Control variables X_i are captured by the matching procedure described above and are either time invariant, measured at age 12, or at the year of (potential) layoff. The results are also robust to including separate event year fixed effects for the treated, to alleviate any concern about differential selection into treatment over time.

Any of the estimates δ_{a_E} for $age_E > a_O$ is a valid estimate of the average difference in potential outcomes between treatment and control group. To mimic the presentation of a conventional event study, we normalize the effect using the youngest of the post-outcome group. However, we include all estimates for children above the outcome age up to age 25 in our figures, to allow the reader to assess the validity of the assumption that any of the older children can be used to estimate the bias due to selection on unobservables. These estimates are highlighted by the shaded area in Figures 4.4 to 4.7.

4.4 Results

This section describes how children's educational outcomes are affected by parental layoff. We begin by establishing that layoffs lead to increased economic insecurity within the family through an increase in unemployment risk and earnings variability, along with a decrease in earnings in the short-to-medium run. We then move on to the impact on child high school graduation rates. We find that children who are transitioning from compulsory school to high school are adversely affected by parental layoff, with lower graduation rates from high school and no increased uptick in employment. Finally, we study mechanisms and find that the layoffs that coincide with the high school choice have the most negative impact, and that the effects are higher for families with higher information costs associated with the high school choice.

4.4.1 Parental Layoffs and Earnings

The initial step, or first stage, in the intergenerational transmission of a layoff shock is to understand how parents are affected. In this section, we document the short-term and medium-term effects of layoff on parents' earnings and unemployment. We show that insecurity about future earnings increases, and we interpret a higher level of uncertainty as an important effect of the layoff.

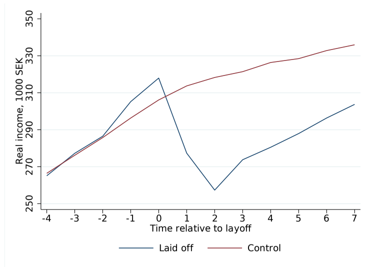
Labor earnings for parents who get laid off initially drop by 20 percent relative to the control group, and the gap remains economically significant even 7 years after the layoff, as shown in Figure 4.3a. This is in line with previous findings on displacements in Sweden (e.g. Eliason & Storrie, 2006; Mörk et al., 2020; Seim, 2019). As explained in Section 4.2.2, we define a layoff as a termination of employment induced by the employer due to a reduction in labor demand at the firm. Layoffs do not necessarily lead to unemployment, as there may be sufficient time between the time of notification and termination for the employee to find a new job. Figure 4.3b shows that only 50 percent of laid off parents register as unemployed in the following year.

Conditional on unemployment, annual labor income falls by almost one third in the year after layoff, see Figure 4.3c. Unemployment insurance (UI) covers up to 80 percent of pre-unemployment income, which is reflected in the effects on disposable income shown in Figure 4.3d. Unemployment insurance coverage is not automatic, and any severance pay affects the start of UI benefits.[13]

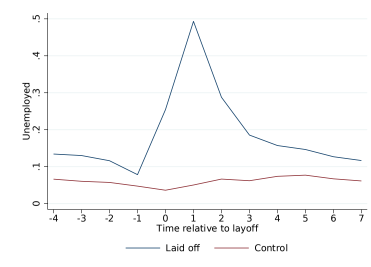
In addition to a decrease in expected disposable income due to unemployment or a worse match with the next employer, the variability of earnings increases at the time of layoff. Two years after the employment shock, disposable income is lower than pre-layoff for 75 percent of the sample, and the spread between the 25th and 75th percentiles of labor income in the layoff sample increases by 50 percent 7 years after layoff relative to the pre-layoff range (see Appendix Figure 4.5).

We will refer to this combination of a spike in probability of unemployment, a moderate drop in expected future earnings in both the short and the long run, and an increase in variability of earnings as *economic insecurity*. Getting fired is a major factor of increased stress (Holmes & Rahe, 1967), and even in the Swedish context where the unemployment insurance is relatively generous, there is evidence that job loss causes an increase in mental distress and harmful behaviors, in the worst case leading to premature death (Eliason

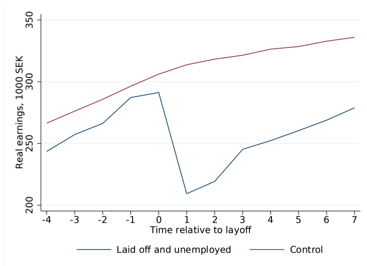
[13] See Appendix 4.A.1 for a detailed discussion of the layoff procedure, UI benefits, and severance pay.



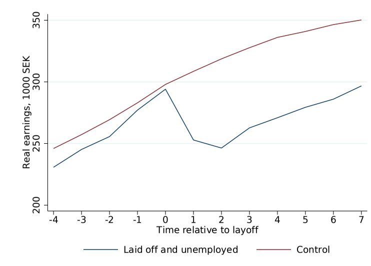
(a) Labor Income



(b) Unemployment



(c) Labor Income – Conditional on Unemployment



(d) Disposable Income – Conditional on Unemployment

Figure 4.3: Effect of Layoff on Parent's Outcomes

Notes. Time is normalized to 0 in the year of termination of employment for the layoff group, and a placebo layoff year for the control group. In Panels (a) and (b), the layoff group is defined as individuals who appear in the layoff data. In Panels (c) and (d), the layoff group only includes individuals who experience unemployment following layoff. The control group is weighted according to the procedure described in Section 4.3. Disposable income includes unemployment benefits and labor income. Both labor and disposable income are measured annually in 1000s of 2010 SEK. Note that the unemployment probability in Panel (b) is higher for the layoff group even before the layoff. Mechanically, this will happen as layoffs are determined by a law (Lagen om anställningsskydd, LAS) with a strong last-in first-out component. See Appendix Section 4.A.1 for further details.

& Storrie, 2009).

4.4.2 Parental Layoffs and Children's Educational Outcomes

We now turn to our main results – the impact of the increase in economic insecurity on children's educational attainment. We find large effects for children who are about to transition to high school, and we find some evidence that the impact is larger for families where the economic insecurity from a layoff is more severe.

The impact of parental layoffs on children's high school graduation rates are concentrated to layoffs that occur in the last three semesters of compulsory school, right before the transition to high school. Figure 4.4 shows the estimated age-semester effect on the graduation rate of children with a laid-off parent relative to children in the control group in the same age-semester. Recall that all estimates shaded in gray are estimates for children who are older at the time of treatment than at the time of outcome. These coefficients serve as an additional control group, showing that there is no significant difference between the estimate at age 20 (used for normalization) and any of the other ages where we expect the treatment effect to be zero. We denote the time of parental layoffs in the Spring semester (Jan-June) when the child is 13 as 13.0 and a parental layoff in the Fall semester (July-Dec) as 13.5 for each age 13.0-25.5. In Figure 4.4, the dotted line at age 16 represents the end of compulsory school and start of high school. The dotted line at age 19 represents the high school graduation age for students who graduate on time.

Parental layoffs prior to child high school enrollment have a significant negative effect on children's eventual graduation rates. In Figure 4.4, we see a clear dip in the graduation rates of students who experience parent layoff in age 15-16, right around the completion of compulsory school and transition to high school. The impact on high school graduation rates for students in the semester prior to compulsory school graduation is large, decreasing the expected on-time graduation rate from 73 percent for students with no family shock to 58 percent for students who experienced a parental layoff at this time in their school life (age 15.5). Students already enrolled in high school at the time of parental layoffs (ages 16.5 to 19) are not significantly less likely to complete high school than their peers. Table 4.3 and Appendix Table 4.3 show

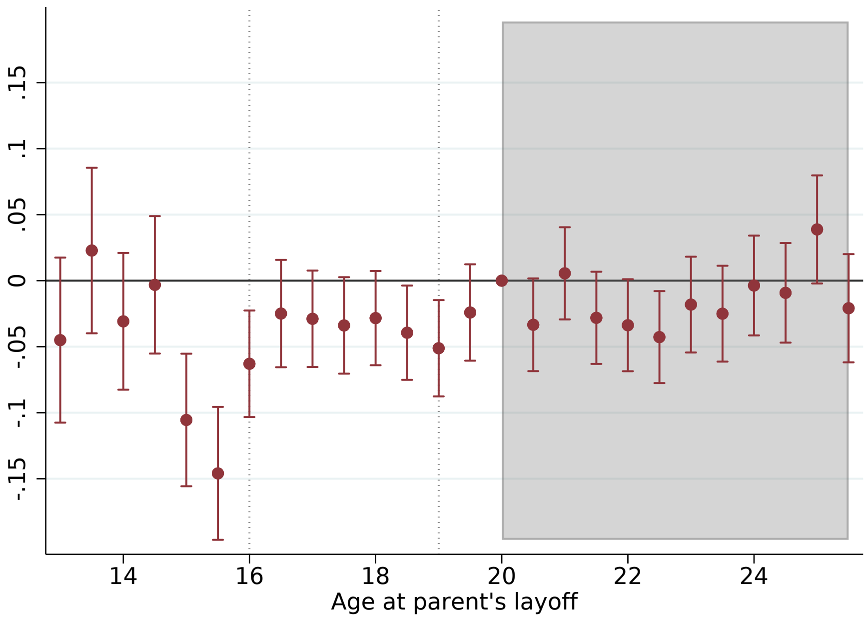


Figure 4.4: Effect of Parental Layoffs: High School Completion by Age 19

Notes. This figure shows the estimated impact of parental layoffs on children's on-time high school graduation rate separately by child age at the time of the layoff. The dotted line at age 19 represents the time when students are expected to graduate from high school. The dotted line at age 16 represents the time when students transition from compulsory school to high school. The parameter estimates correspond to the coefficients $\delta_{a_E,s}$ from Equation 4.1 for child ages at parent layoff of $a_E \in [13,25]$, normalized to 0 at $a_E = 20, s = 0$. The regressions include cohort and event year fixed effects and are run on the matched sample. The outcome, high school graduation by age 19, is defined as a dummy equal to 1 if the child's highest education 19 years after birth is a completed 2- or 3-year high school degree or higher, and 0 otherwise. 84 percent of all students who will graduate high school have graduated by age 19. Estimates are reported in Appendix Table 4.2 and F-tests are reported in Appendix Table 4.3. Table 4.3 column 1 shows the joint estimate of the effects of a pre-high school and high school layoff, respectively.

Table 4.3: Effect of Parental Layoffs: Gender Subsets

	(1) Baseline	(2) Men	(3) Women	(4) Fathers	(5) Mothers
Layoff	0.00668	-0.00387	0.0205***	0.00247	0.0226*
	(0.00542)	(0.00724)	(0.00784)	(0.00615)	(0.0116)
Layoffs in Ages:					
14.5-16	-0.0602***	-0.0400*	-0.0857***	-0.0605***	-0.0585**
	(0.0152)	(0.0207)	(0.0215)	(0.018)	(0.0286)
16.5-19	-0.0162*	-0.018	-0.0148	-0.0200*	-0.00649
	(0.00905)	(0.0122)	(0.0129)	(0.0104)	(0.0186)
Constant	0.637***	0.599***	0.686***	0.645***	0.609***
	(0.00427)	(0.00574)	(0.00611)	(0.00483)	(0.00903)
Ob	223,119	126,664	96,455	135,247	87,872
R-squared	0.011	0.016	0.018	0.012	0.03

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Cohort and year of layoff fixed effects included in all specifications
Children younger than 14.5 dropped

Notes. Column 1 shows the baseline regression (corresponding to Figure 4.4) but with age brackets based on high school. Ages 14 and younger are dropped from the estimation. Columns 2 to 5 run this specification for different subsets of the population based on the gender of the child (columns 2 and 3) and parent (columns 4 and 5).

the joint significance test for students of high-school age.

To drop out of school without a completed high school degree is associated with substantially lower lifetime earnings than a high school graduate. In our sample, earnings are up to 48 percent lower for dropouts compared to high school graduates in the first years out of high school, see Appendix Table 4.4. The earnings of high school dropouts in the population follow a parallel income path to high school graduates at 60 to 75 percent of earnings up until at least age 30.[14] As children's earnings may be impacted by local labor market shocks that are correlated with parental layoffs, we are hesitant to argue that our empirical framework would be valid to study the impact on intergenerational income correlation directly. Nevertheless, we do observe a negative

[14]See Appendix Section 4.A.2 for a detailed description of earnings by education level.

Table 4.4: Parent Heterogeneity – F-tests

	(1) Single vs dual earner HHs	(2) Rel. income pre-layoff	(3) UI days post-layoff	(4) Real income post-layoff	(5) Rel. income post-layoff
F, 15.0	0.65	0.18	2.97	0.08	0.05
	(0.42)	(0.674)	(0.085)	(0.776)	(0.823)
F, 15.5	0.61	0.16	5.98	1.05	0.00
	(0.433)	(0.686)	(0.014)	(0.305)	(0.945)
F, 16.0	1.23	1.52	3.18	1.15	0.12
	(0.268)	(0.217)	(0.074)	(0.283)	(0.732)

P-values in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. Each estimation represents the F-test of equality at each semester-age for children with different family characteristics. Column (1) tests the equality of coefficients between single and dual earner households. Column (2) tests the equality of coefficients between households with above and below median pre-layoff relative earnings. Column (3) tests the equality of coefficients between households where the parent spent above or below median number of days in UI following layoff. Column (4) tests the equality of coefficients between households with above-median or below-median real income in the two years following the layoff. Column (5) performs the same test, but using the same relative income specification as in column (2). In columns 1 and 2, both the treatment and the control group are split by the characteristics. In columns 3-5, we split only the families affected by layoff, as these are post-layoff outcomes. The median income is therefore relative to other individuals who were laid off.

– albeit imprecisely estimated – effect of parental layoffs on child earnings in their early 20s precisely for children of age 15.5 at the time of layoff (see Appendix Figure 4.5b).

We do not find any evidence that children drop out of school to find employment to support their family financially. In addition to lower expected earnings later in life, children who do not graduate high school are not earning more than their peers at ages 16–19.[15] Figure 4.5a shows the estimated effect on children's cumulative earnings in their late teens by parental layoff. We find no significant impact, but the point estimates are noisy due to the low share of teenagers with observable income in our sample. In the population,

[15] Note that we define high school dropouts by the absence of high school graduation. Therefore, students who are still attempting to complete their degree but fail to ever pass the minimum number of classes required will be considered dropouts. This will not allow for time to seek outside employment.

those who will not get a high school degree by age 19 earn less from age 16 onward than those who will (see Appendix Figure 4.6). Employment during high school can be facilitated through vocational program internships, increasing the teenage earnings of students in vocational relative to theoretical high school tracks.[16]

Children who do not graduate from high school on time at age 19 may still be able to graduate later, in which case the economic impact would not be as severe. Our estimates, however, are robust to looking at graduations by age 21, thus allowing for two years of grade repetition or complementary education to finish primary school. Appendix Figure 4.8 shows graduation rates by age 21, when 98 percent of all individuals who will ever complete high school have graduated. Even here, we find a drop in graduation rates of 9 percentage points for students in the last two semesters of compulsory school. Even a delay in graduation age (i.e. someone who has finished by age 21 but not by age 19) signals lower educational achievement than on-schedule graduation. Students who delay graduation tend to have lower earnings during their high school years, earning 20 percent less than on-time graduates during high school, and 14 percent less at age 22 (the first year after graduation).[17]

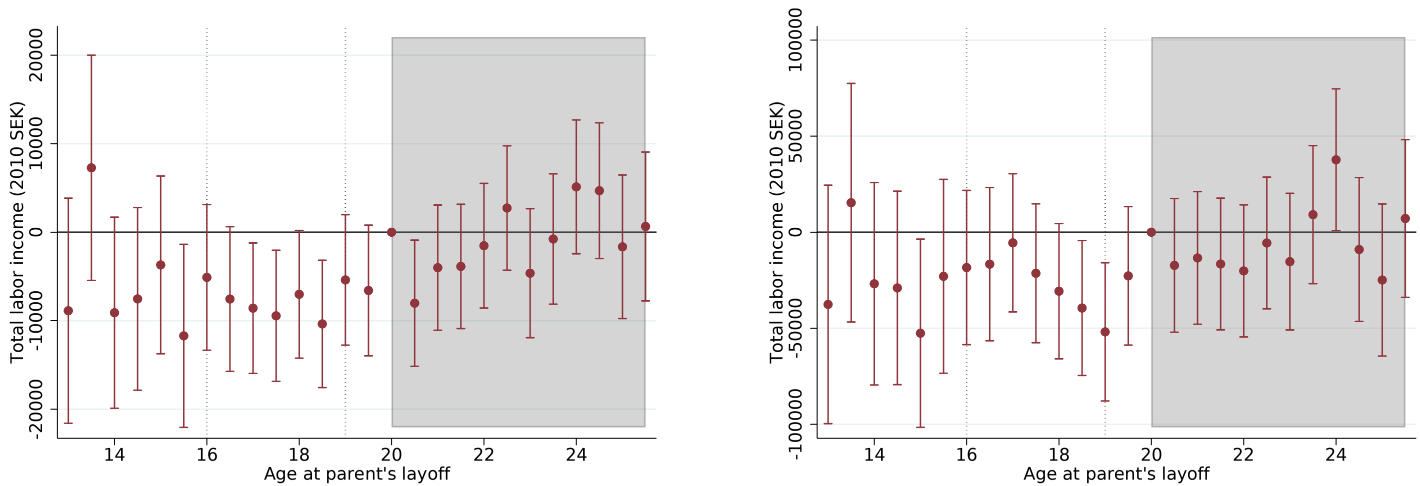
Parental layoffs do not affect the graduation rate from academically more demanding degrees. Figure 4.6a shows that graduation rates for scientific degree programs are unaffected by parental layoffs for all ages. These degree programs are usually attended by highly motivated students.[18] Graduation rates from vocational degree programs are depressed for students who are about to start their high school, and to a smaller extent for students already enrolled at the time of parental layoffs (see Figure 4.6b). This is in line with the literature on parental involvement which focuses on at-risk or low-achieving students (Huat See & Gorard, 2015).

To summarize, we find that the increase in economic insecurity caused by parental layoffs can have large negative effects on children's likelihood to complete high school, if the shock occurs when children are transitioning from compulsory school to high school. We find that the impact doesn't vary by the

[16] See Appendix Section 4.A.2 for more details on children's earnings.

[17] Calculated from Table 4.4.

[18] Note that we do not observe grade enrollment in real time, but only after graduation. Hence, we do not know which program dropouts enrolled in, if any.

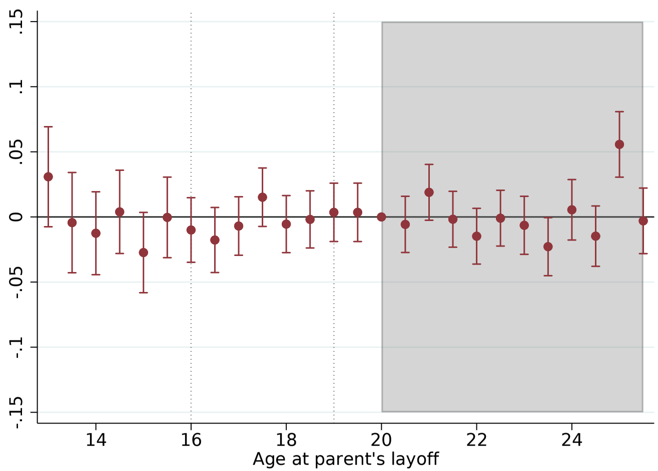


(a) Cumulative Earnings, Age 16-19

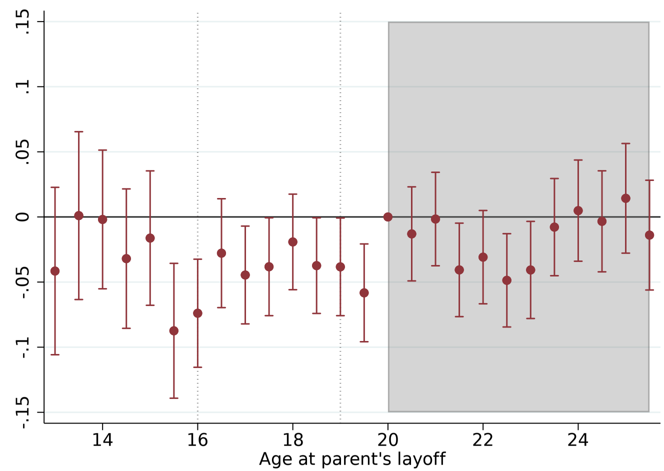
(b) Cumulative Earnings, Age 20-23

Figure 4.5: Effect of Parental Layoffs: Child Earnings

Notes. Cumulative earnings are the sum of real labor earnings (2010 SEK) during high school age (Panel a) and the first 4 years after the age of timely high school graduation (Panel b). The dotted line at age 19 represents the time when students are expected to graduate from high school. The dotted line at age 16 represents the time when students transition from compulsory school to high school. The parameter estimates correspond to the coefficients $\delta_{a_E,s}$ from Equation 4.1 for child ages at parent layoff of $a_E \in [13, 25]$, normalized to 0 at $a_E = 20, s = 0$. The regressions include cohort and event year fixed effects and are run on the matched sample.



(a) Scientific High School Program



(b) Vocational High School Track

Figure 4.6: Graduation Rates by Education Field

Notes. Panel (a) shows the likelihood of graduation from a scientific program (*Naturvetenskapliga programmet*) by age 19. Panel (b) shows the likelihood of graduation from any vocational program (*Yrkesprogram*) by age 19. The parameter estimates correspond to the coefficients $\delta_{a_E,s}$ from Equation 4.1 where the outcome is graduation by age 19 from a scientific or vocational program, respectively. The regressions include cohort and event year fixed effects and are run on the matched sample. The dotted line at age 19 represents the time when students are expected to graduate from high school if they are graduating on time. The dotted line at age 16 represents the time when students transition from compulsory school to high school.

size of the income shock, but that academically strong students are insulated from the shock as they do not appear to change their choice of track to less prestigious degrees or fail to graduate. We now turn to examining the drivers of the sizable effect for this particular age group.

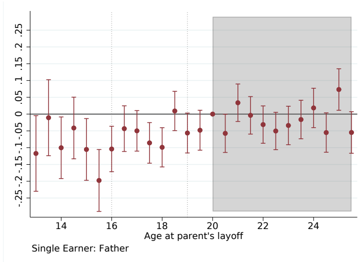
4.5 Mechanisms

4.5.1 Parent Income

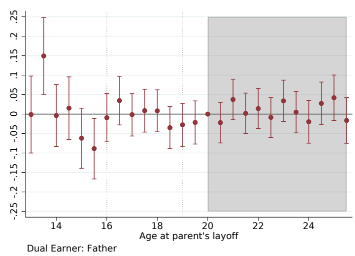
The size of the income shock following parental layoffs does not significantly alter the impact on children's high school dropout rates. The realization of the economic uncertainty caused by the layoff will occur after the layoff date, and the duration of the shock varies. Hence, our current setup allows us to capture the effect of the uncertainty, not the effect of a material decrease in living standards.

We do not find significant evidence of larger effects on households with more pre-layoff economic insecurity. Figure 4.7 shows the split between households with better or worse economic circumstances at the time of layoff. In panels 4.7a and 4.7b, we see the estimated effects for children with a single-earner father getting laid off relative to a father layoff in a two-earner household. The effects appear to be larger for the single-earner family, but are not statistically different from each other. Similarly, Panels 4.7c and 4.7d show families with above-median or below-median pre-layoff earnings. Here, the trends are more similar and not statistically different. We also show the split based on the realized outcome of the number of days in unemployment in panels 4.7e and 4.7e. This outcome occurs after the intervention of interest, the layoff date, and we are therefore careful in interpreting the results. We believe the actual days in unemployment can be informative about the perceived severity of the layoff shock at the termination date, but we acknowledge that this is a bad control. F-tests of the difference between coefficients to the right and left in Figure 4.7 can be found in Table 4.4.

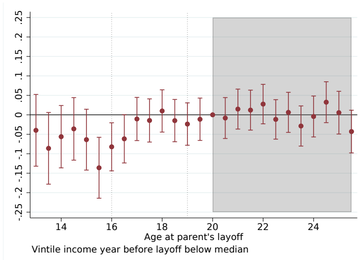
We conclude that better economic conditions prior to the layoff shock do not insulate children from experiencing the economic uncertainty caused by a parental layoff.



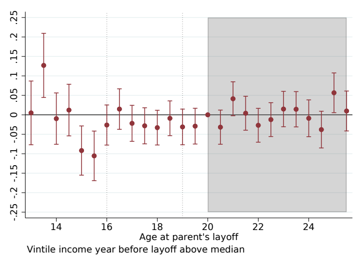
(a) Father is Single Earner in Household



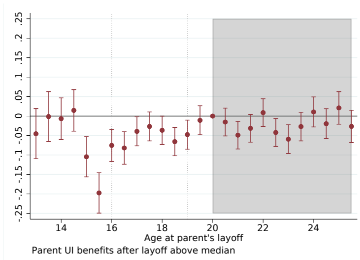
(b) Dual Earner Household



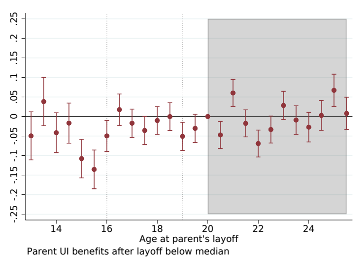
(c) Below Median Pre-Layoff Earnings



(d) Above Median Pre-Layoff Earnings



(e) Above Median Days in Unemployment



(f) Below Median Days in Unemployment

Figure 4.7: Heterogeneity by Parent Characteristics

Notes. Panel (a) shows the likelihood of on-time high school graduation for parental layoffs when the parent was the single earner in the household. Panel (b) shows the effect of a parental layoff when a second working parent is present. The control groups are also restricted to be in single/dual earner households, in addition to standard matching. Note that we restrict the households to fathers in this case. Our baseline results are robust to this specification, see Table 4.3. Panel (c) and (d) shows the estimated coefficients in restricted samples where the pre-layoff parental income is below or above the median income in the layoff sample. The control group is also split at the same income rate. Here, both fathers and mothers are included. Panel (e) and (f) show the estimated coefficients for families with below-median or above-median days in unemployment following a layoff. The entire control group is included for both estimates, as the split is relevant only for the families where a layoff actually occurs. Tests of equality of coefficients are presented in Table 4.4.

4.5.2 Transition to High School

There are two potential reasons why the transition between compulsory school and high school can be a vulnerable time in the student's education path.

First, consider the effect of enrollment on high school completion rates. Schools are incentivized to help struggling students in their programs to completion, but not to take on new students who do not appear to meet their academic standards. A shock to academic performance prior to high school enrollment may lead to students not getting into their preferred school, or require students to retake classes prior to enrollment. If a student was already enrolled in the high school, when the shock happened, she might get additional support.

Second, consider the high school choice. As described in Section 4.2.1, the choice of high school requires students and their parents to know the expected labor market return to at least 15 different education tracks as well as to individual schools. This is a family-level decision with a high degree of parental involvement (Skolverket, 2003, Chapter 5). Not having sufficient information and parental support can lead to a suboptimal school choice which decreases the child's likelihood of graduation. As high school is not mandated by law, teenagers with a higher discount factor may even take this opportunity to not apply to any schools at all.

We first differentiate between these two channels by considering the exact timing of the parental layoffs relative to the child's school calendar. We focus on two important dates in the last semester of compulsory school: the high school application deadline in early February and national exams in the second week of May.[19] February and May deadlines correspond to the school choice and academic performance shock channels, respectively. We detect no change in graduation rates for layoffs around the time of national exams, see Appendix Figure 4.9 for the raw data on families with parental layoffs only. However, we do find a need to investigate the drop around the high school application deadline further.

Figure 4.8 illustrates the effect of a parental layoff right before the February deadline. We show a nonparametric estimate of the change in outcome by child age at the time of layoff, allowing for an interruption in the time series in

[19]The exact test dates and application dates vary by school year. We use February 1st as the application deadline, as the historical application dates have varied between February 1st and February 16th.

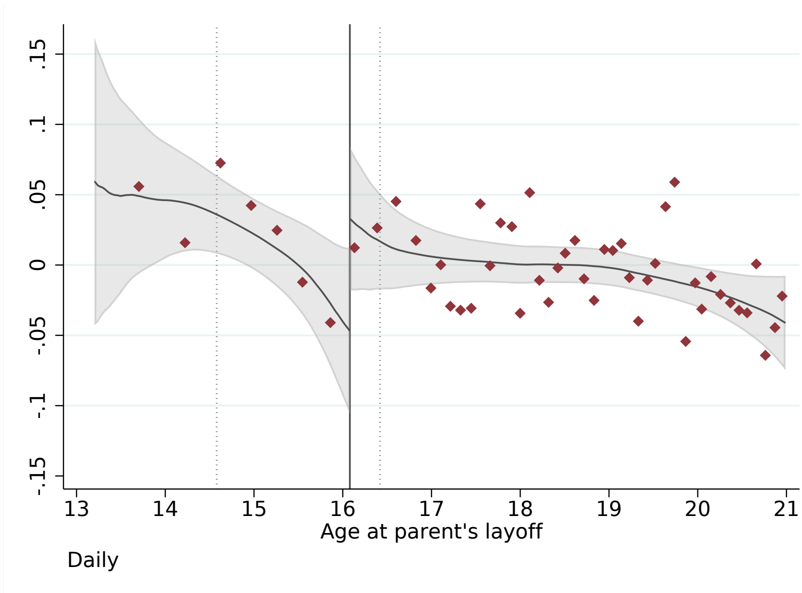


Figure 4.8: High School Graduation Rate by Parent's Separation Date

Notes. This figure shows the differential impact of parent layoffs on on-time high school graduation around the time of high school applications. The outcome is defined as the high school completion rate by age 19 relative to the matched control group. The estimated effect at the cutoff is robust to a wide selection of bandwidths, see Appendix Figure 4.10. See Figure 4.9 for placebo tests. The solid line represents the high school graduation deadline on February 1st, the Spring semester when children are 15. The dotted line furthest to the left represents the last final exam date, in the second week of May. For completeness, we also show the line at the start of grade 8, in the Fall semester at age 14, when children first start to receive grades.

February. Layoffs right before the school choice deadline in February lead to significantly lower high school graduation probabilities for children than layoffs with termination deadlines after February, as shown in Figure 4.8. The nonparametric specification and the selection of the optimal bandwidth follow the approach in Calonico et al. (2014), treating the time series interruption as a cutoff of a regression discontinuity (RD) design.

We find no significant effect of parental layoffs around the time of the national exams. This contrasts with Rege et al. (2011) who find a negative effect on final grades at age 16 in a similar setting in Norway. As we do not observe grades directly, we cannot reject any effects on grades following layoffs around May in the last semester of compulsory school, but we do not

Table 4.5: Interrupted Time Series Estimates of Application Deadline

VARIABLES	(1) Graduation	(2) Graduation	(3) Graduation	(4) Graduation | Parental unemp
RD Estimate	0.10504**	0.09568*	0.09566*	0.235***
	(0.0431)	(0.05172)	(0.05171)	(0.0829)
Effective Obs Pre	703	473	473	289
Effective Obs Post	1218	663	663	375
Bandwidth (MSE)	1.20	0.747	0.747	0.815
Cohort-year FE	No	Yes	Yes	No
Control weights	No	No	Yes	No

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. We follow Calonico et al. (2019) to estimate the optimal bandwidth and robust confidence interval for the point estimate at the time series interruption as if it was an regression discontinuity design with controls. We use cohort-year dummies as controls. In column 3, we include the control group to be able to allow for re-weighting, but this only has a marginal impact on the estimate. Column 4 is specified as in column 1, but only for parents who experience unemployment after layoffs.

find any medium run effects of shocks close to the final exams. To the extent that compulsory school grades are affected, we do not observe any direct effect on high school graduation from grades, separate from the effect on high school application behavior.

The point estimate at the February cutoff is robust to a wide selection of bandwidths (see Appendix Figure 4.10) and the inclusion of controls (see Table 4.5). Appendix Figure 4.10 shows the robustness of the point estimates with respect to the bandwidth of the nonparametric estimate. The standard errors are chosen based on the discussion of optimal bandwidths for regression discontinuity designs (Calonico et al., 2014; Imbens and Kalyanaraman, 2012). The point estimate at the cutoff is also robust to including or excluding controls and event year fixed effects, see Table 4.5.

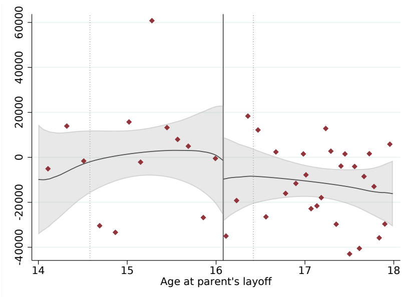
Figure 4.9 shows the smoothness around the application deadline cutoff in density, pre-determined variables, and parent characteristics. There is no change in density around the February cutoff, but it should be noted that higher ages in calendar years can include more cohorts, and we therefore see an increase in observations for older age groups (Panel 4.9e). Pre-determined variables such as parental pre-layoff income (Panel 4.9a) and the birth year of the child (Panel 4.9c) are not different on either side of the cutoff. The characteristics around parental layoffs also appear to be similar around the deadline, with an equal number of days in unemployment (Panel 4.9d) and similar earnings in the two years after the layoff (Panel 4.9b).

We find that the estimated difference at the cutoff is larger for parents with a binding termination date. As the time between the first individual notification and actual termination can be several months, the termination date is not necessarily binding for all parents. If the parent has already managed to secure a new job prior to her termination date, she might actually have more time to help her child with their education than in the absence of a layoff. Appendix Figure 4.11 shows the result for parents who transition from the employer who laid them off to registered unemployment. For this sample, we know the termination date is binding, as they have not found new employment before termination. For families with their first unemployment date the day before the high school application deadline, the estimated effect implies a drop of 23 percentage points in the probability of completing high school on time (see Table 4.5).[20]

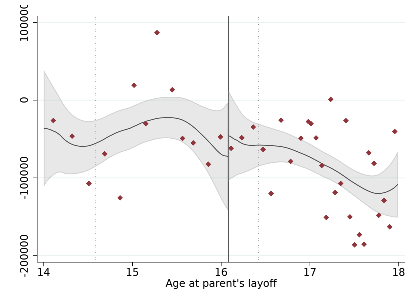
The results from Figure 4.8 are consistent with a model of limited parental time with differential returns to parental involvement depending on the school activity. After the layoff is announced, the parent will need to devote more of her leisure time to job search, which decreases the time available to engage with her children's schooling. Increased stress due to economic insecurity may also have a negative impact on the quality of parental involvement, holding time constraints fixed.

Huat See and Gorard (2015) identify two mechanisms through which parental involvement affects children's educational outcomes: improved learning and aligned expectations. Involved parents improve learning by functioning as an additional teacher, or they can reinforce the school's message about the expectations about student behavior and the importance of going to school. Spoth et

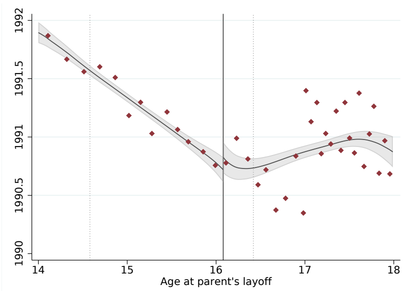
[20] The interpretation of this result should be done with caution, as we are conditioning on an event that occurs after the treatment.



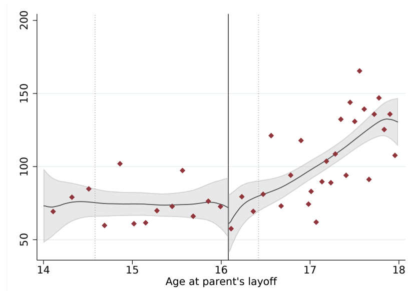
(a) Parent Pre-layoff Earnings



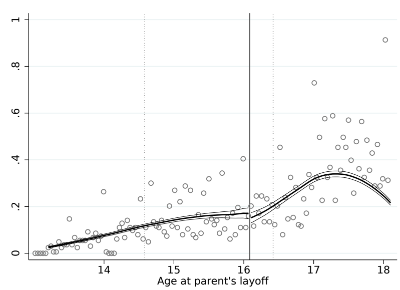
(b) Parent Post-layoff Earnings



(c) Child Cohort



(d) Parent days of Unemployment



(e) Density

Figure 4.9: Robustness: Density and Covariates around Application Deadline

Notes. The non-parametric fit is estimated by a local linear estimator (polynomial of degree 1) with an MSE-optimal bandwidth following Calonico et al. (2014). The cutoff is set to be February 1st, age 16, which represents the high school application deadline studied in Figure 4.8.

al. (2008) find that increasing parental competency and communication skills has a positive effect on educational outcomes of children in the same age group as in this case (the intervention takes place at age 14 and outcomes are observed at age 18).

In our case, parental involvement in the high school choice process may be important to communicate the importance of going on to high school (which is voluntary), as well as expectations about what kind of track choice would be acceptable. Parents may be better suited than teachers to help students understand their individual returns to different schooling options. In order to credibly be involved, parents need to be informed about the current state of the high school market.

Informational costs related to the high school choice are high (Skolverket, 2003). In the average high school market, there were 10 different schools to choose from in 2005, with the number rising to 16 different schools by 2013 (see Appendix Table 4.1). Schools advertise themselves to prospective students and their families primarily through visiting days (evenings) outside of regular office hours, which means that unemployed parents have no time advantage.

We proxy for the family's knowledge about the high school choice market using the sibling order of the child whose high school choice is impacted by parental layoff. Given the nature of knowledge, the cost of gathering information is going to be highest the first time a choice is made. Repeat choices may require some updating, but we assume that families do not unlearn over time (similar to Chetty et al., 2013).[21] Therefore, the cost of parental involvement in the high school choice of the oldest sibling is going to be higher than the cost for younger siblings.

Figure 4.10 shows the estimated effects for the oldest (or only) child in the family (Panel a) and younger siblings (Panel b). For older siblings, we estimate a clear negative effect of parental layoffs prior to high school choice. For younger siblings, the estimates are more noisy, but the point estimate at age 15 is only a third of that for older siblings, and not statistically different

[21] In Chetty et al. (2013), the authors argue that if knowledge about the specifics of the tax code is present in a location in one year, people will still remember the following year, so information can only grow. In this paper, we argue that once the family has gathered information about the high school choice for their first child, the family will still remember once it is time to make the same choice for the next child.

from zero. Table 4.6 tests the equality of the point estimates shown in Figures 4.10a and 4.10b. For each semester-age 15, 15.5 and 16, we test if the effect of parent layoff on the oldest sibling is equal to the effect on a younger sibling in the same situation. The effects are not statistically different for on-time completion (by age 19), but if we allow for delayed students to also complete their degrees (by age 21), we do find a significantly lower completion rate for oldest siblings who experience parental layoff, compared to younger siblings.

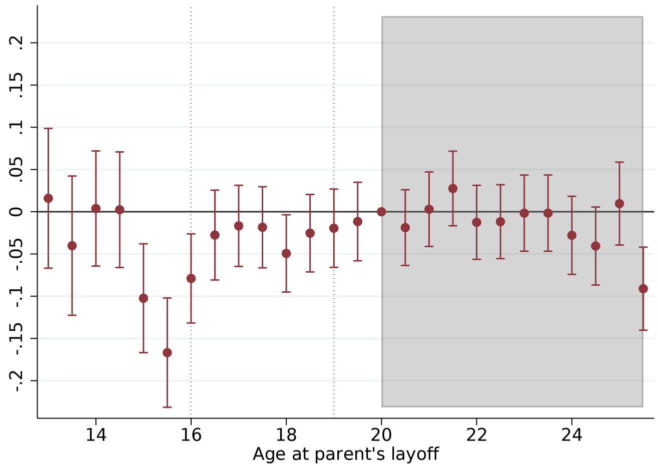
We find that the large drop in graduation rates caused by parental layoffs is most severe around the time of high school choice, a complex and individual choice where parental involvement is crucial. Evidence from the exact timing of the layoff favors the school choice over grades as the driver of the result, and we also find evidence that families with more prior information about the school choice are not as adversely impacted by the layoff.

4.6 Conclusion

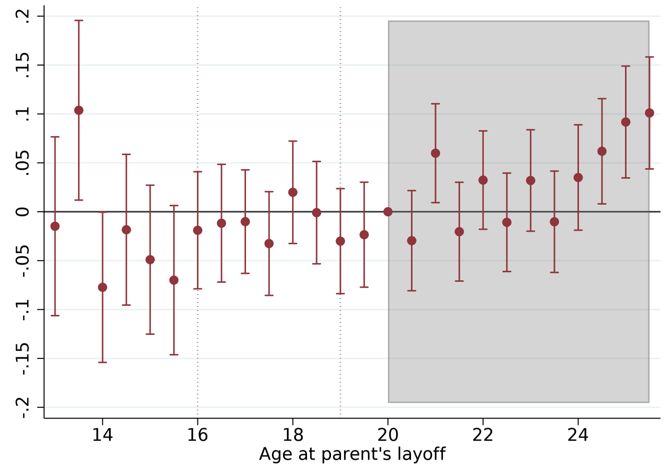
We have explored a particular channel through which economic insecurity can affect children's educational outcomes. We show that children in Sweden are relatively insulated against economic shocks that affect their parents, except for when they are about to transition between compulsory school and high school. The high school choice is a complex decision which requires a lot of parental involvement. From the education literature, we know that that parental involvement (Barrera-Osorio et al., 2020; Huat See and Gorard, 2015; Rogers and Feller, 2018) and parental competency during adolescence (Spoth et al., 2008) have a causal effect on educational outcomes.

We use parental layoffs as a source of variation in the level of economic insecurity faced by parents. Data on individual layoffs initiated by the firm have several advantages relative to identifying job loss from plant closure. First, we ensure that the timing of the parental shock is independent of the child's year of birth, leading to exogenous variation in the age of the child at time of layoff. Second, we can identify parents at risk of layoff as a basis for the control group. Third, we know the exact layoff date, and can use variation in time relative to specific deadlines within the school year to identify the effect of a shock close to exams and application deadlines.

Parental layoffs which happen right before the child's high school appli-



(a) Oldest Sibling in Family



(b) Younger Siblings

Figure 4.10: Information Effects: Heterogeneity by Sibling Order

Notes. Panel (a) shows the likelihood of on-time high school graduation for the sample of children who are the oldest sibling or only child in their family. Panel (b) shows the effect on graduation for younger siblings only. The control group is also restricted to only include children of the right sibling order. Sibling order is used as a proxy for information about the high school choice, as younger siblings have the benefit of a family-level recent experience with applying for high school. Tests of equality of coefficients are presented in Table 4.6.

Table 4.6: Heterogeneity: Sibling Order

VARIABLES	(1) HS graduate, age 19	(2) HS graduate, age 21
Older sibling	0.0478***	0.0402***
	(0.00615)	(0.00554)
Layoff	-0.0243	-0.0199
	(0.0259)	(0.0233)
Oldest: Treated age 15.0	-0.0924**	-0.0852**
	(0.0439)	(0.0396)
Younger: Treated age 15.0	-0.0282	0.0479
	(0.0503)	(0.0453)
Oldest: Treated age 15.5	-0.160***	-0.153***
	(0.0469)	(0.0422)
Younger: Treated age 15.5	-0.0522	-0.0139
	(0.0529)	(0.0475)
Oldest: Treated age 16.0	-0.0609*	-0.0268
	(0.0362)	(0.0325)
Younger: Treated age 16.0	0.000923	-0.0172
	(0.0391)	(0.0352)
Constant	0.606***	0.723***
	(0.00138)	(0.00124)
Observations	569,075	574,331
R-squared	0.015	0.017
F Test: (Old = Young) age 15.0	0.930	4.900
Prob>F, age 15.0	0.336	0.0269
F Test: (Old = Young) age 15.5	2.320	4.760
Prob>F, 15.5	0.128	0.0291
F Test: (Old = Young) age 16.0	1.350	0.0400
Prob>F, 16.0	0.246	0.842

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes. This table tests the equality of the point estimates shown in Figures 4.10a and 4.10b. For each semester-age 15, 15.5 and 16, we test if the effect of parent layoff on the oldest sibling is equal to the effect on a younger sibling in the same situation. The hypothesis is that when the oldest sibling is about to apply for high school (age 15.5) the family has less information about the process and is more vulnerable to a shock. The regression specification adds a control for sibling order (binary: Oldest or younger) as well as interactions between this control and age of layoff for treated families. Note that we do not include family fixed effects. If there are two siblings in the same family, we will observe the family as treated twice, once at the age of the oldest children in the layoff year, once at the age of the younger child in the same year.

cation deadline have the largest impact on high school graduation rates. The impact is precise enough to exclude the effect of parental layoffs on grades as the driver of the effect on graduation rates. We also find that families who are likely to have less information about the high school choice are more sensitive to a parental layoff at the time of high school applications than families with more experience of the application process. This highlights the parent's role in communicating and aligning home and school expectations over the parent's role as an additional teacher.

Using layoffs as a natural experiment restricts our understanding to a scenario when the economic insecurity of the family sharply increases over night. We do not find evidence that the actual drop in material standards is what is affecting children's educational outcomes. At the time of layoff, the future economic outcome of the family is unknown, and we find that the economic insecurity affects children about to transition to high school negatively, regardless of what the future outcome of earnings is.

It is well established, however, that lower income families experience a higher level of economic stress and insecurity than more affluent families.[22] Our results highlight the link between economic insecurity, parental involvement and educational outcomes. This link can be an important explanation for the correlation between parental income and educational outcomes. As discussed by Heckman and Mosso (2014) and others, the causal link between parent income and child education is not very well understood. A model which reduces the income-education channel to credit constraints risks severely underestimating the causal effect of income on education in settings where credit constraints are low.

[22] See for example Sapolsky (2018) for neurological evidence.

References

Ambler, J. S. (1994). Who Benefits from Educational Choice? Some Evidence from Europe. *Journal of Policy Analysis and Management*, *13*(3), 454–476.

Barrera-Osorio, F., Gonzalez, K., Lagos, F., & Deming, D. J. (2020). Providing Performance Information in Education: An Experimental Evaluation in Colombia. *Journal of Public Economics*, *186*, 104185.

Belley, P., & Lochner, L. (2007). The Changing Role of Family Income and Ability in Determining Educational Achievement. *Journal of Human Capital*, *1*(1), 37–89.

Black, S. E. (1999). Do Better Schools Matter? Parental Valuation of Elementary Education. *Quarterly Journal of Economics*, *114*(2), 577–599.

Black, S. E., Devereux, P. J., & Salvanes, K. G. (2015). Losing Heart? The Effect of Job Displacement on Health. *ILR Review*, *68*(4), 833–861.

Borusyak, K., Jaravel, X., & Spiess, J. (2024). Revisiting Event-Study Designs: Robust and Efficient Estimation. *Review of Economic Studies*, *forthcoming*.

Brand, J. E. (2015). The Far-Reaching Impact of Job Loss and Unemployment. *Annual Review of Sociology*, *41*, 359–375.

Calonico, S., Cattaneo, M. D., Farrell, M. H., & Titiunik, R. (2019). Regression Discontinuity Designs using Covariates. *Review of Economics and Statistics*, *101*(3), 442–451.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014). Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica*, *82*(6), 2295–2326.

Chetty, R., Friedman, J. N., & Saez, E. (2013). Using Differences in Knowledge across Neighborhoods to Uncover the Impacts of the EITC on Earnings. *American Economic Review*, *103*(7), 2683–2721.

Chetty, R., Friedman, J. N., Saez, E., Turner, N., & Yagan, D. (2020). Income Segregation and Intergenerational Mobility Across Colleges in the United States. *Quarterly Journal of Economics*, *135*(3), 1567–1633.

Couch, K. A., & Placzek, D. W. (2010). Earnings Losses of Displaced Workers Revisited. *American Economic Review*, *100*(1), 572–89.

Eliason, M., & Storrie, D. (2006). Lasting or Latent Scars? Swedish Evidence on the Long-Term Effects of Job Displacement. *Journal of Labor Economics*, *24*(4), 831–856.

Eliason, M., & Storrie, D. (2009). Does Job Loss Shorten Life? *Journal of Human Resources*, *44*(2), 277–302.

Fjellman, A.-M., Hansen, K. Y., & Beach, D. (2019). School Choice and Implications for Equity: The New Political Geography of the Swedish Upper Secondary School Market. *Educational Review*, *71*(4), 518–539.

Francesconi, M., & Heckman, J. J. (2016). Child Development and Parental Investment: Introduction. *The Economic Journal*, *126*(596), F1–F27.

Goodman-Bacon, A. (2021). Difference-in-Differences with Variation in Treatment Timing. *Journal of Econometrics*, *225*(2), 254–277.

Heckman, J. J., & Mosso, S. (2014). The Economics of Human Development and Social Mobility. *Annual Review of Economics*, *6*(1), 689–733.

Hilger, N. G. (2016). Parental Job Loss and Children's Long-Term Outcomes: Evidence from 7 Million Fathers' Layoffs. *American Economic Journal: Applied Economics*, *8*(3), 247–83.

Holmes, T. H., & Rahe, R. H. (1967). The Social Readjustment Rating Scale. *Journal of Psychosomatic Research*, *11*(2), 213–218.

Huat See, B., & Gorard, S. (2015). The Role of Parents in Young People's Education – A Critical Review of the Causal Evidence. *Oxford Review of Education*, *41*(3), 346–366.

Huttunen, K., & Riukula, K. (2019). *Parental Job Loss and Children's Careers* (tech. rep.). Institute of Labor Economics (IZA).

Imbens, G., & Kalyanaraman, K. (2012). Optimal Bandwidth Choice for the Regression Discontinuity Estimator. *Review of Economic Studies*, *79*(3), 933–959.

Jacobson, L. S., LaLonde, R. J., & Sullivan, D. G. (1993). Earnings Losses of Displaced Workers. *American Economic Review*, 685–709.

Lochner, L., & Monge-Naranjo, A. (2012). Credit Constraints in Education. *Annual Review of Economics*, *4*(1), 225–256.

Lovenheim, M. F. (2011). The Effect of Liquid Housing Wealth on College Enrollment. *Journal of Labor Economics*, *29*(4), 741–771.

Lovenheim, M. F., & Reynolds, C. L. (2011). Changes in Postsecondary Choices by Ability and Income: Evidence from the National Longitudinal Surveys of Youth. *Journal of Human Capital*, *5*(1), 70–109.

Mörk, E., Sjögren, A., & Svaleryd, H. (2020). Consequences of Parental Job Loss on the Family Environment and on Human Capital Formation – Evidence from Workplace Closures. *Labour Economics*, *67*, 101911.

OECD. (2019). *Education at a Glance 2019*.

Oreopoulos, P., Page, M., & Stevens, A. H. (2008). The Intergenerational Effects of Worker Displacement. *Journal of Labor Economics*, *26*(3), 455–483.

Rege, M., Telle, K., & Votruba, M. (2011). Parental Job Loss and Children's School Performance. *Review of Economic Studies*, *78*(4), 1462–1489.

Rogers, T., & Feller, A. (2018). Reducing Student Absences at Scale by Targeting Parents' Misbeliefs. *Nature Human Behaviour*, *2*.

Sapolsky, R. M. (2018). The Health-Wealth Gap. *Scientific American*, *319*(5), 62–67.

Seim, D. (2019). On the Incidence and Effects of Job Displacement: Evidence from Sweden. *Labour Economics*, *57*, 131–145.

Skolverket. (2003). *Valfrihet och dess effekter inom skolområdet* (Report No. 230).

Skolverket. (2020). *Att välja för framtiden – Skolans stöd genom vägledning och lärande*.

Spoth, R., Randall, G. K., & Shin, C. (2008). Increasing School Success Through Partnership-Based Family Competency Training: Experimental Study of Long-Term Outcomes. *School Psychology Quarterly*, *23*(1), 70–89.

Stevens, A. H. (1997). Persistent Effects of Job Displacement: The Importance of Multiple Job Losses. *Journal of Labor Economics*, *15*(1, Part 1), 165–188.

Sullivan, D., & Von Wachter, T. (2009). Job Displacement and Mortality: An Analysis using Administrative Data. *Quarterly Journal of Economics*, *124*(3), 1265–1306.

Sun, L., & Abraham, S. (2021). Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects. *Journal of Econometrics*, *225*(2), 175–199.

Von Wachter, T., Song, J., & Manchester, J. (2009). *Long-term Earnings Losses due to Mass Layoffs during the 1982 Recession: An Analysis using US*

Administrative Data from 1974 to 2004 [Unpublished paper, Columbia University].

Appendices

Appendix 4.A Background

4.A.1 Layoffs

The register data on layoffs in this paper comes from firms reporting notifications to the public unemployment authority (Arbetsförmedlingen). According to Swedish law (1974:13), firms that plan to lay off five or more full-time employees are required to report this to the local UI office. The minimum notification period varies by the size of the layoff. The reason for layoff needs to be specified in the report, ensuring that layoffs are initiated because of a reduction in labor demand at the firm. Initially, the firm has to report the number of individuals getting laid off. After negotiations with the unions are finished, they also need to report which employees are affected.

Who gets laid off is determined by last-in first-out laws, but can in practice be rounded by negotiations over severance pay. An employee is categorized as laid off if she is registered as subject to layoff and we observe her termination date, but we do not require her to actually register as unemployed. Hence, there may be selection into layoff, as mediated by the laws and union negotiations, but we do not condition treatment status on being unable to find a new job after the initial shock.

To receive UI benefits, the laid-off worker needs to have been a member of a UI fund (A-kassa) for at least a year prior to their termination date, and have run out of severance pay. (Any severance payment sum is calculated in wage equivalents based on the last monthly wage prior to layoff.) Hence, we do not expect all layoffs to appear at the unemployment office. In the data, around 50 percent of everyone experiencing layoff is registered at the unemployment office the year after layoff, see Figure 4.3b. If eligible, UI benefits are 80 percent of earnings up to a ceiling. The ceiling is determined by law, which was held fixed in nominal terms over the period 2002–2014. In 2005, the ceiling was at roughly median earnings.

The average drop in real earnings following a layoff is not very large, but has a permanent effect on earnings. Appendix Figure 4.1 shows the evolution of mean earnings over time relative to layoff for the treatment and control group, excluding (Figure 4.1a) and including (Figure 4.1b) UI benefits. Unfortunately, we cannot separately observe severance pay from labor earnings, and there appears to be severance payments in the year after layoff as well as in the layoff year. Earnings drop by 15-20 percent 2 years after layoff relative to pre-layoff earnings. 7 years after the shock (in a balanced panel), earnings are still 10 percent lower than in the control group.

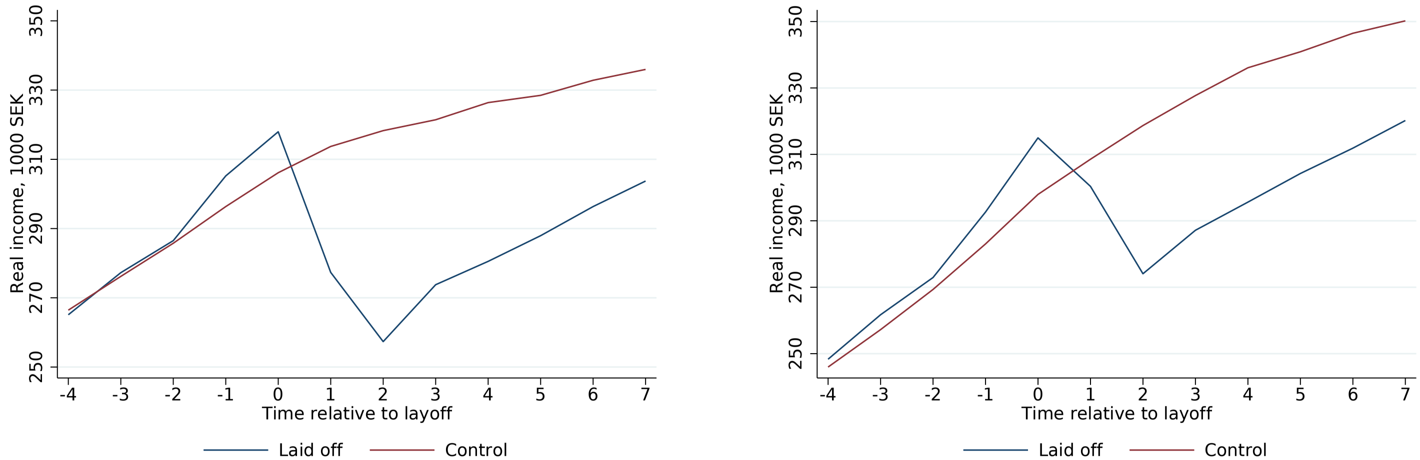
Characteristics of the laid-off population vary by year of layoff. This is to be expected as in recession laid-off individuals are going to be less adversely selected than laid-off individuals in years when the business cycle is more favorable. Figure 4.2 shows the pre-layoff earnings, household income, and the share of layoffs in manufacturing over the time period.

4.A.2 Earnings by Education Level

Individuals with less than a high school degree in our data earn significantly less than graduates. Figures 4.6 (a)–(h) show the income paths for selected cohorts by highest degree in 2015 (the last year of observation). The sample is restricted to native-born individuals with a known level of education. The earnings path for high school dropouts is parallel to the path for high school graduates for all cohorts, both for mean and median earnings, until at least age 30. Dropouts earn on average less than 70 percent of a high school graduate's earnings up to the age of 30.

Appendix Figure 4.7 shows the earnings by education for the same cohorts, focused on earnings age 16-20. Dropouts earn less than in-school peers, even during the school years. Official unemployment surveys show that youth unemployment (individuals aged 15 to 24) in Sweden has been 15-25 percent since 2001. Among 15-19 year-olds in the labor force, the unemployment rate is 22-37 percent for the same time period. This relation is driven by selection or negative opportunity costs of going to high school. A vocational school may facilitate connections to employers in their vocation, offering higher-paid job opportunities than to those who have left school. We do not observe the time of dropout, therefore it is possible that some share in the dropout group are students who attempt to graduate but fail to meet minimum requirements. Note that informal jobs will not appear in this administrative dataset.

Appendix 4.B Supplementary Figures and Tables

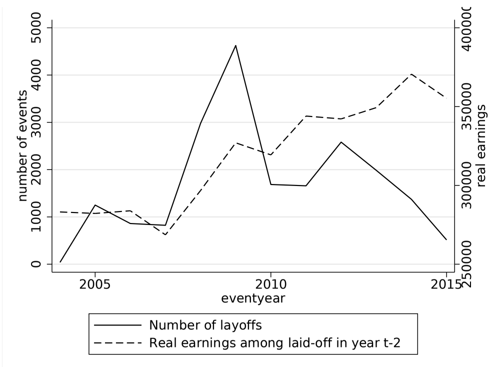


(a) Labor income

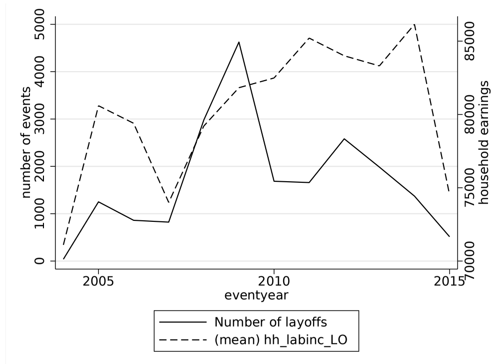
(b) Disposable income

Appendix Figure 4.1: Effects of Layoffs on Parent's Outcomes

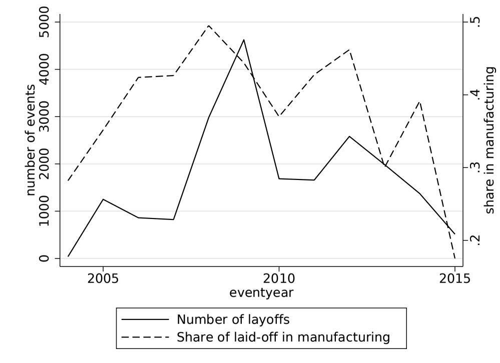
Notes. In panels (a) and (b), the layoff group is defined as individuals who appear in the layoff data with a termination date in event year 0. The control group is weighted according to the procedure described in Section 4.3. Disposable income includes unemployment benefits and labor income. Both labor and disposable income are measured annually in 1000s of 2010 SEK. See Appendix Section 4.A.1 for further details.



(a) Pre-layoff earnings by layoff year



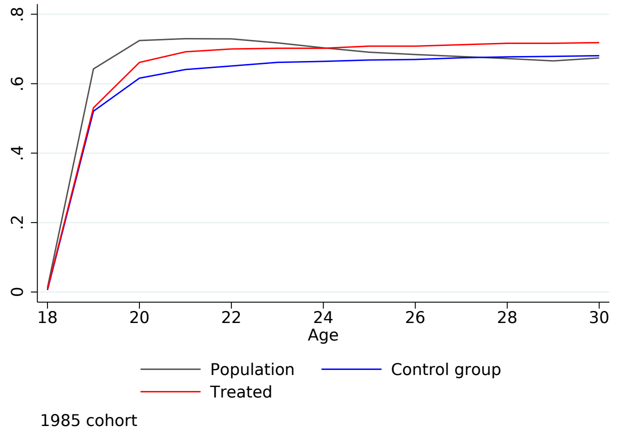
(b) Pre-layoff household earnings by layoff year



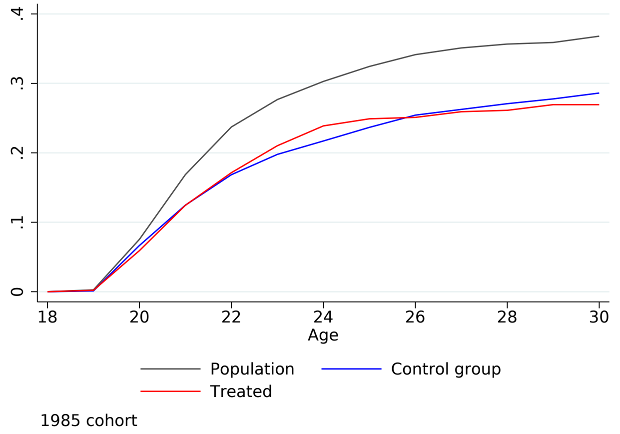
(c) Pre-layoff employment in manufacturing sector by layoff year

Appendix Figure 4.2: Characteristics of Laid-Off Employees by Year

Notes. The solid line represents the total number of layoffs per year in our dataset. This is constant across all panels. The dashed line represents the characteristics of the set of laid-off workers in the given year. Earnings are defined as all labor earnings in event year $t-2$ relative to layoff. Manufacturing is defined as a dummy equal to one if the parent's main employer in event year $t-2$ is defined as a manufacturing firm according to the 2-digit industry (SNI) code.



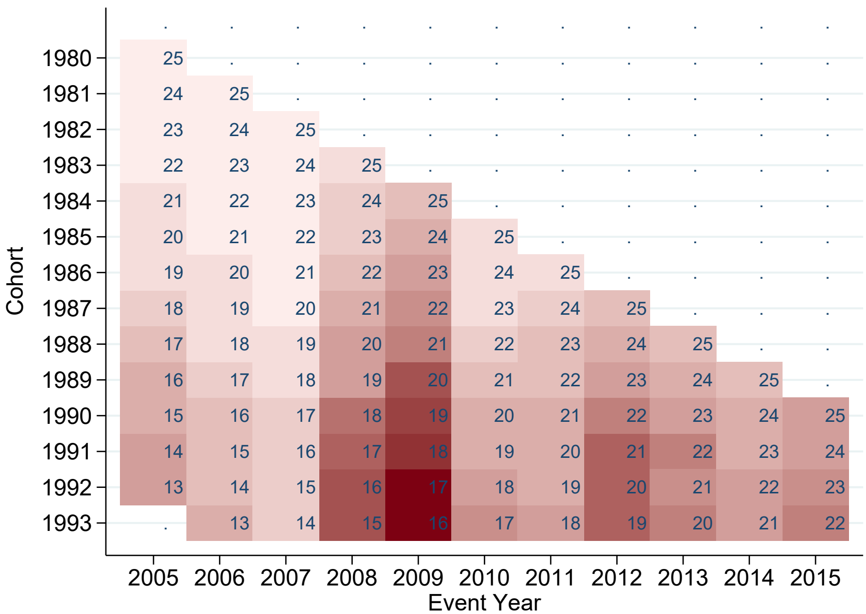
(a) Share of population with at least a high school degree by age



(b) Share of population with at least one year of University by age

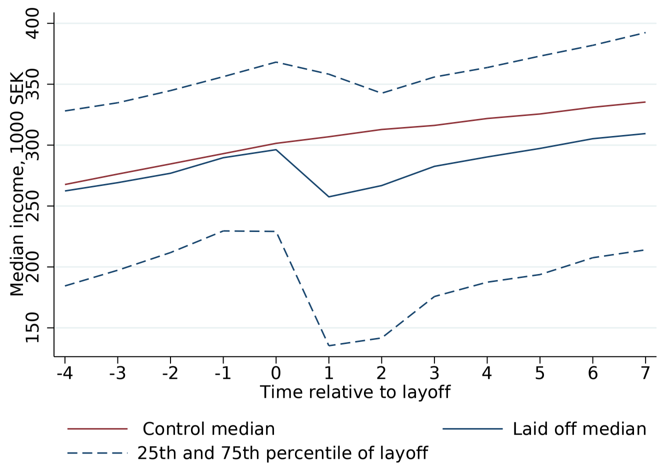
Appendix Figure 4.3: Highest Education by Age

Notes. Note: This figure shows the share of the total population who have reached the education level (high school in Panel a and at least one year of university in Panel b) by age.

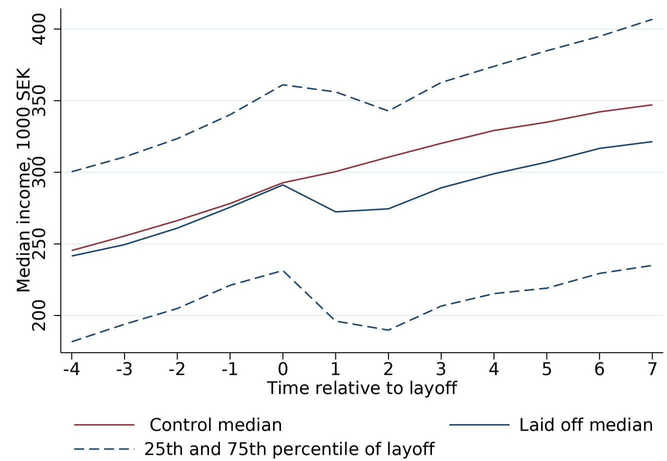


Appendix Figure 4.4: Age of Treated Children in the Baseline Sample by Event Year and Cohort

Notes. Mapping of the distribution of treated children by year of parent lay-off, child cohort and age. The number of observations in the lightest area is 35 and the darkest red area is 618. Each shade represents an interval of 40 observations.



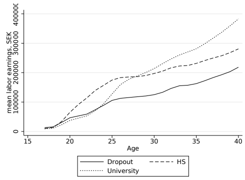
(a) Labor income, range



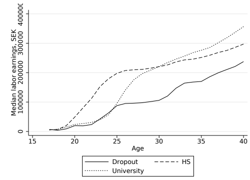
(b) Disposable income, range

Appendix Figure 4.5: Range of Parent Outcomes Around Layoff

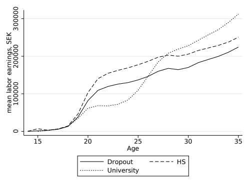
Notes. The treatment group is defined as individuals who appear in the layoff data with a termination date normalized to year 0. The control group is weighted according to the procedure described in Section 4.3. Disposable income includes unemployment benefits and labor income. Income is measured annually in 1000s of 2010 SEK. Percentiles are estimated from the annual distribution.



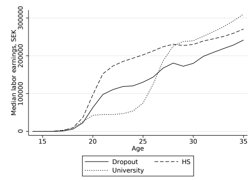
(a) Mean earnings, 1975 cohort



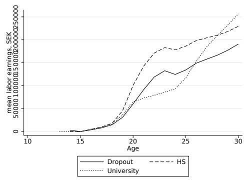
(b) Median earnings, 1975 cohort



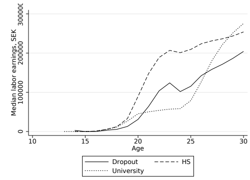
(c) Mean earnings, 1980 cohort



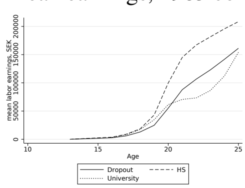
(d) Median earnings, 1980 cohort



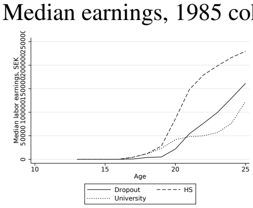
(e) Mean earnings, 1985 cohort



(f) Median earnings, 1985 cohort

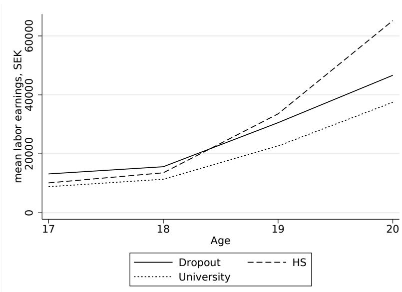


(g) Mean earnings, 1990 cohort

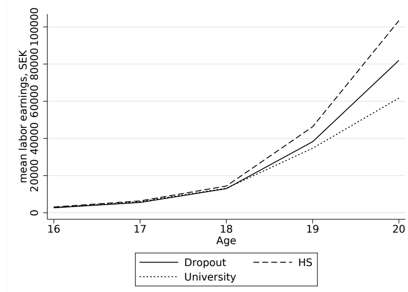


(h) Median earnings, 1990 cohort

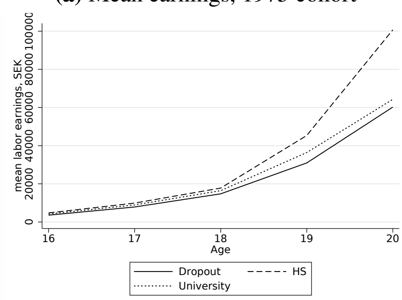
Appendix Figure 4.6: Income Paths by Education Level – Selected Cohorts



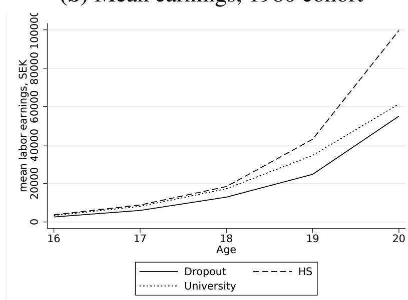
(a) Mean earnings, 1975 cohort



(b) Mean earnings, 1980 cohort

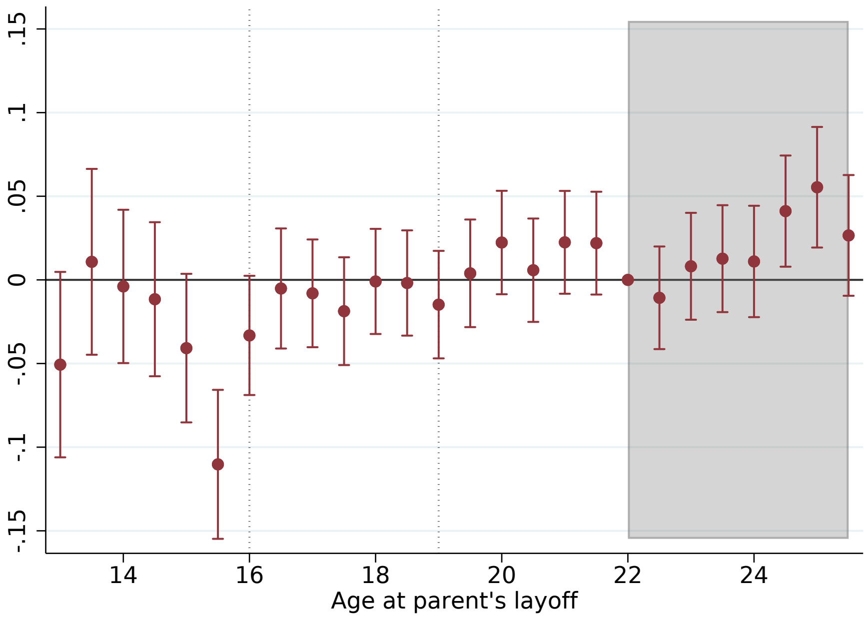


(c) Mean earnings, 1985 cohort



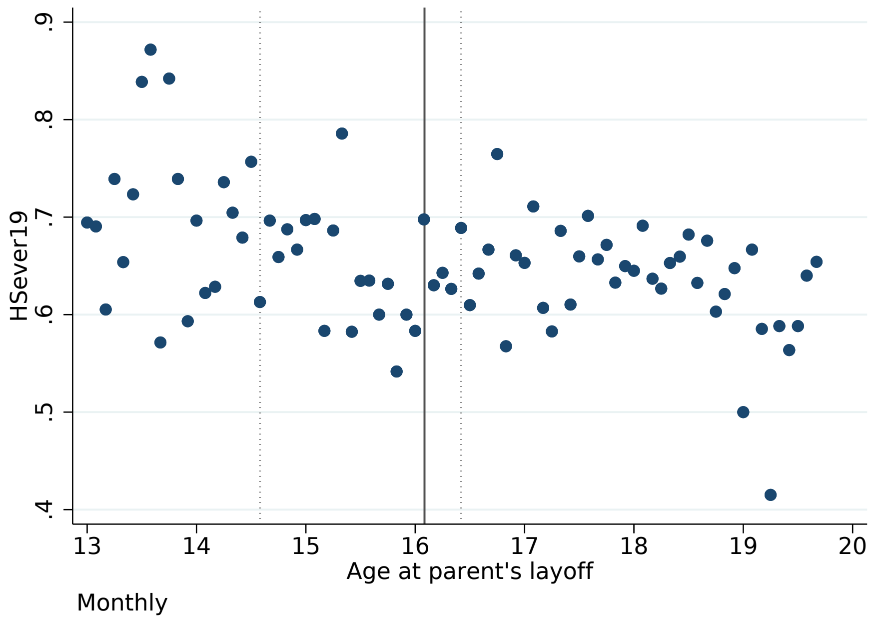
(d) Mean earnings, 1990 cohort

Appendix Figure 4.7: Income Paths by Education Level at Ages 16-20 – Selected Cohorts



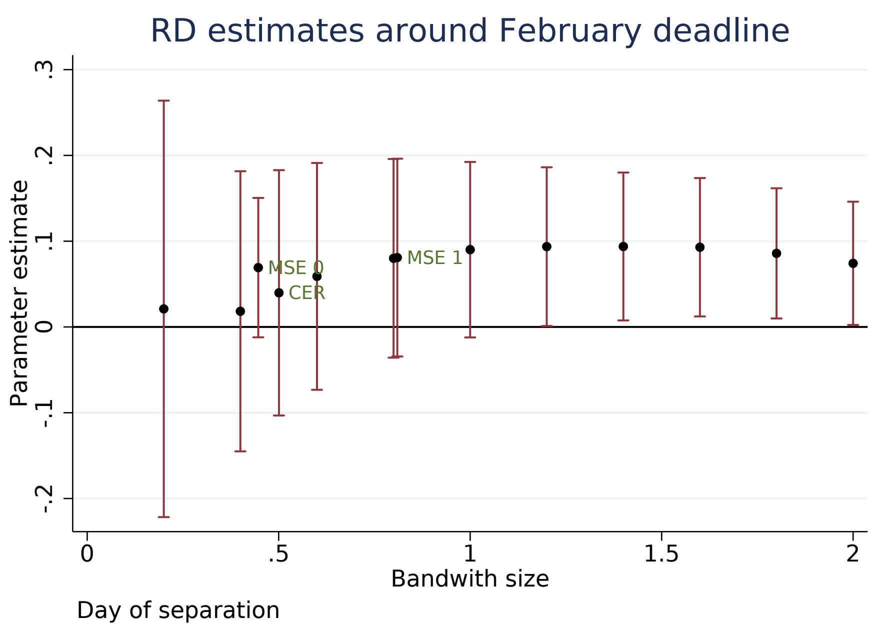
Appendix Figure 4.8: High School Completion by Age 21

Notes. This figure shows the estimated impact of parental layoffs on children's high school graduation rate by age 21 separately by child age at the time of parent separation. The dotted line at age 19 represents the time when students are expected to graduate from high school if they are graduating on time. The dotted line at age 16 represents the time when students transition from compulsory school to high school. The parameter estimates correspond to the coefficients $\delta_{a_E,s}$ from Equation 4.1 for child ages at parent layoff of $a_E \in [13,25]$, normalized to 0 at $a_E = 22, s = 0$. The regressions include cohort and event year fixed effects and are run on the matched sample. The outcome, high school graduation by age 21, is defined as a dummy equal to 1 if the child's highest education 21 years after birth is a completed 2- or 3-year high school degree or higher, and 0 otherwise. 98 percent of all high school graduates have graduated by age 21.



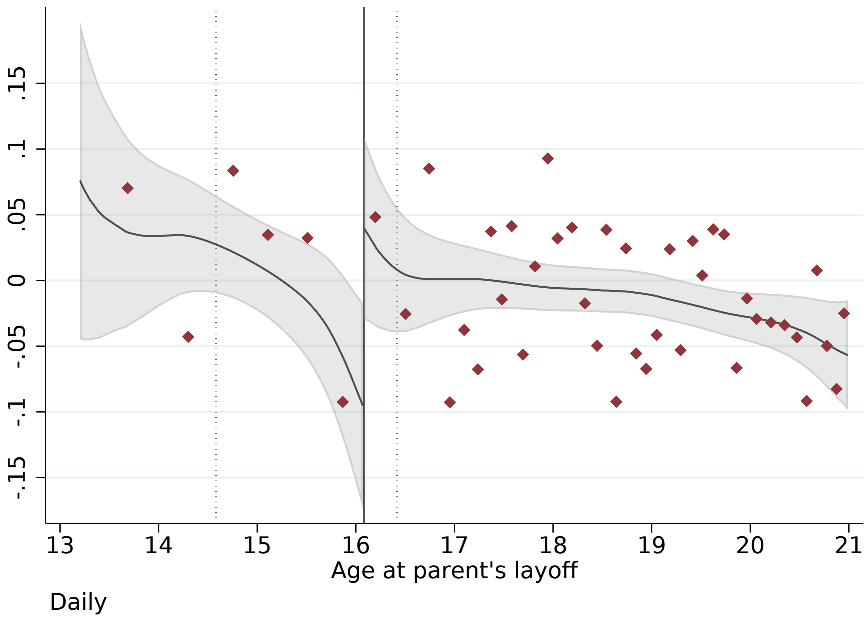
Appendix Figure 4.9: Graduation Rates for Children with Parental Layoffs Only

Notes. Raw data: Binned scatterplot of the high school completion and month of separation. No controls, only data from families with a parental layoff. Each scatter is a calendar month-age bin.



Appendix Figure 4.10: Robustness of ITS Estimates to Varying Bandwidths

Notes. The MSE optimal bandwidth follows Imbens and Kalyanaraman (2012) and the CER optimal bandwidth follows Calonico et al. (2014), in line with the literature on regression discontinuity designs.



Appendix Figure 4.11: High School Graduation Rate by First Day of Parent Unemployment

Notes. This figure shows the differential impact of parent layoffs around the time of high school applications for parents who experience unemployment subsequent to their termination date. The outcome is defined as the high school completion rate relative to the matched control group. The bandwidth is chosen to be MSE optimal at the interruption time using the method of Calonico et al. (2014), following the standard approach for regression discontinuity designs. The solid line represents the high school graduation deadline on February 1st, the Spring semester when children are 15. The dotted line furthest to the left represents the last final exam date, in the second week of May. For completeness, we also show the line at the start of grade 8, in the Fall semester at age 14, when children first start to receive grades.

Appendix Table 4.1: High School Choice Environment

Year	School Market level			Municipality level	
	N Schools	**N Programs**	**Programs $\geq$15**	**N Schools**	**No school**
2000 mean	8.96	14.51	0.53	1.04	0.33
sd	(14.84)	(2.26)	(0.50)	(1.08)	(0.47)
2005 mean	10.41	15.28	0.64	0.89	0.36
sd	(14.73)	(2.83)	(0.48)	(0.86)	(0.48)
2010 mean	12.46	15.65	0.71	0.84	0.42
sd	(16.32)	(2.59)	(0.46)	(0.90)	(0.49)
2013 mean	15.61	15.75	0.69	1.55	0.38
sd	(19.02)	(2.76)	(0.46)	(1.80)	(0.48)

Notes. The average number of schools and programs a student making a high school choice will face in her school market* or municipality.

* School markets are defined based on 2010 commuting patterns defined by Skolverket (2011).

Source: Skolverket.

Appendix Table 4.2: Regression Coefficients Used in Figure 4.4

	(1)
VARIABLES	HSever19
Treatment age 15.0	-0.112***
	(0.0333)
Treatment age 15.5	-0.152***
	(0.0349)
Treatment age 16.0	-0.0627**
	(0.0263)
Treatment age 16.5	-0.0313
	(0.0291)
Treatment age 17.0	-0.0270
	(0.0247)
Treatment age 17.5	-0.0319
	(0.0249)
Treatment age 18.0	-0.0207
	(0.0240)
Treatment age 18.5	-0.0415*
	(0.0244)
Treatment age 19.0	-0.0513**
	(0.0246)
1.layoff event	0.0221
	(0.0169)
Constant	0.638***
	(0.00103)
Observations	445,642
R^2	0.015

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes. This table displays the coefficients plotted in Figure 4.4 and are the basis for the F-tests in Table 4.3.

Appendix Table 4.3: The Effect of a Layoff in High School Ages: Joint Significance Test

	F test specification	Prob>F
F: 19 = 0	4.360	0.0368
F: 19-18.5 =0	2.500	0.0819
F: 19-18 =0	1.740	0.156
F: 19-17.5 =0	1.320	0.261
F: 19-17 =0	1.740	0.383
F: 19-16.5 =0	0.880	0.507

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes. This table tests if there is a significant effect of experiencing a parental layoff during high school based on the coefficients in Figure 4.4 (reported in Appendix Table 4.2). Starting with the effect of a layoff in the spring semester of age 19, each row adds the effect of a layoff at a younger age.

Appendix Table 4.4: Earnings Path for Dropouts and High School Graduates in the Baseline Sample

	Dropout at 21 (1)	HS graduate at 21 (2)	Dropout at 19 (3)	HS graduate at 19 (4)
Age 16	2527.7	3644.1	2561.7	3879.6
	(6664.9)	(6562.1)	(6494.0)	(6657.0)
Age 17	5626.9	7741.7	5578.8	8281.1
	(14417.4)	(11566.8)	(13613.7)	(11552.0)
Age 18	12578.8	16022.7	11996.2	17319.2
	(27422.0)	(20662.1)	(25119.1)	(21031.8)
Age 19	29117.4	50860.2	26559.7	58116.4
	(50167.1)	(49222.0)	(45475.6)	(50105.7)
Age 20	52173.2	109137.4	51019.1	123550.5
	(76473.8)	(91116.9)	(71388.0)	(91765.8)
Age 21	70909.5	140402.5	78690.0	150370.0
	(92493.2)	(104072.7)	(93259.3)	(104196.2)
Age 22	87670.8	162874.6	99762.1	170618.2
	(103576.9)	(112757.7)	(106138.8)	(112820.2)
Age 23	103704.6	178837.4	116881.7	185664.2
	(113974.9)	(119975.4)	(117006.6)	(119425.7)
N	75461	83393	102975	55879

mean coefficients; sd in parentheses

Notes. Real income by age in baseline sample with weights. Individuals with any university enrollment at age 23 or younger are excluded from the sample. Columns 1 and 2 define individuals as high school graduates or dropouts based on their highest level of education at age 21. Columns 3 and 4 use the highest level of education at age 19.

Appendix References

Skolverket. (2011). *Skolmarknadens geografi: Om gymnasieelevers pendling på lokala och regionala skolmarknader*.

Sammanfattning

Denna avhandling innehåller fyra fristående uppsatser inom arbetsmarknadsekonomi, offentlig ekonomi och hälsoekonomi. Kapitel 1 undersöker om avkastning på färdigheter på arbetsmarknaden kan förklara kohorttrender i uppmätta kognitiva färdigheter bland svenska män. Kapitel 2 studerar användningen av nya läkemedel i Sverige, med fokus på skillnader mellan sjukhus och socioekonomiska grupper. Kapitel 3 undersöker hur generositeten i arbetslöshetsförsäkringen påverkar arbetslösas användning av hälso- och sjukvård. Kapitel 4 studerar hur föräldrars arbetsförlust påverkar barns utbildningsval. Icke-tekniska sammanfattningar av varje kapitel följer.

Kapitel 1 – Kan marknadsincitament hjälpa till att förklara trender i kognitiva färdigheter? En omfattande litteratur, som började med Flynn (1984, 1987) och har granskats av bland andra Schaie m. fl. (2005) och Pietschnig och Voracek (2015), har dokumenterat en betydande och långvarig ökning i mått på kognitiv förmåga över födelsekohorter i industrialiserade länder. Denna FFlynn-effekt"har fått stor uppmärksamhet och har givits flera förklaringar inom kognitionsvetenskap. Vanligtvis betonar dessa föreslagna förklaringar faktorer som förbättrad hälsa och näring (t.ex., Pietschnig & Voracek, 2015; Rindermann m. fl., 2017). I nationelekonomisk teori kan sådana faktorer ses som att de ökar *tillgången* på färdigheter.

Vissa forskare har dock föreslagit att förändrad *efterfrågan* också kan forma kohorttrender i kognitiv förmåga (t.ex., Dickens & Flynn, 2001). Till och med James R. Flynn (2018, s. 79) själv noterade att *"[n]är samhället ber oss att öka vår användning av någon färdighet över tid, svarar hjärnan på detta"*, men det är oklart hur viktiga dessa efterfrågefaktorer är i praktiken.

Det första kapitlet, **Labor Market Returns and the Evolution of Cogni-**

tive Skills: Theory and Evidence, samförfattat med Santiago Hermo, David Seim och Jesse M. Shapiro, tar sig an denna fråga genom att undersöka huruvida *marknadsincitament*, mätt genom avkastning på färdigheter på arbetsmarknaden, kan hjälpa till att förklara kohorttrender i kognitiva färdigheter.

Vår analys baseras på administrativa data med poäng från standardiserade tester av kognitiva färdigheter, genomförda vid mönstring, matchade med registerdata över inkomster och socioekonomisk bakgrund för nästan hela populationen av svenska män födda 1962–1975. De tester som användes för att mäta kognitiva färdigheter förblev identiska under vår studieperiod, vilket gör det möjligt för oss att mäta trender i kognitiva färdigheter över födelsekohorter.

Vi börjar med att utveckla en ny ekonomisk modell för investering i multidimensionella färdigheter. I modellen beror en individs färdigheter på en exogen begåvning (som fångar upp tillgångsfaktorer, såsom hälsa) och investeringar i färdigheter som görs innan inträdet på arbetsmarknaden (av föräldrar, barn och skolor). Investeringar i färdigheter beror i sin tur på den livslånga avkastningen på dessa färdigheter.

Vi tillämpar modellen på de administrativa data och fokuserar på två dimensioner av kognitiv förmåga: logiskt tänkande och ordförrådskunskap. Den förstnämnda är ett typiskt mått på *flytande* intelligens (Carroll, 1993), för vilken kognitionsvetare har dokumenterat särskilt uttalade ökningar över tid. Den sistnämnda är ett typiskt mått på *kristalliserad* intelligens (Carroll, 1993), där observerade ökningar vanligtvis har varit mindre uttalade. Vi estimerar modellen under antagandet att andra determinanter än arbetsmarknadsavkastning inte har gynnat en färdighetsdimension oproportionerligt mycket över den andra.

Vår analys ger tre huvudsakliga resultat. För det första, i linje med tidigare forskning (Castex & Dechter, 2014; Edin m. fl., 2022; Markussen & Røed, 2020), finner vi att den livslånga avkastningen på arbetsmarknaden för båda typerna av kognitiva färdigheter minskade över födelsekohorter. Däremot minskade avkastningen på ordförrådskunskap *relativt* till logiskt tänkande med 46 procent. Samtidigt förbättrades prestationen på testet för logiskt tänkande med 4,4 percentilenheter, medan prestationen på ordförrådskunskapstestet minskade med 2,9 percentilenheter, båda mätta i förhållande till testpoängsfördelningen för män födda 1967.

Enligt vår modell ledde ökningen av arbetsmarknadsavkastningen för logiskt tänkande relativt till ordförrådskunskap till en ökning av investeringar i logiskt tänkande på bekostnad av ordförrådskunskap. Men hur mycket av trenderna i testresultaten kan förklaras av förändringar i arbetsmarknadsavkastningen? När vi tillämpar modellen på data, visar vårt andra huvudreslutat att förändrade arbetsmarknadsavkastningar kan förklara 37 procent av ökningen i logiskt tänkande, medan resten förklaras av andra faktorer. Vidare kan förändrade arbetsmarknadsavkastningar helt förklara minskningen i ordförrådskunskap, eftersom vi uppskattar att dessa färdigheter skulle ha förbättrats om arbetsmarknadsavkastningarna hade förblivit oförändrade på 1962 års nivå.

Vårt tredje huvudresultat visar att föräldrar och skolor, två centrala aktörer i barns färdighetsinvesteringar, har lagt ökad vikt vid att utveckla resonemangsförmåga relativt till faktakunskap. Med hjälp av en egen enkätundersökning visar vi att föräldrar till senare kohorter anser att resonemangsförmåga är viktigare för deras barn än faktakunskap. Vidare genomför vi en textanalys som visar att de svenska läroplanerna för grundskolan över tid har skiftat fokus mot att utveckla resonemangsförmåga relativt till faktakunskap, ett resultat som är konsekvent med en omfattande pedagogisk litteratur. Vi ser dessa resultat som förenliga med vår förklaring av trenderna i testresultaten för logiskt tänkande och ordförrådskunskap.

Våra resultat tyder på att det är användbart att införliva marknadsincitament och ekonomiska verktyg i studiet av bestämningsfaktorerna för kohorttrender i kognitiva färdigheter. Vår analys ger upphov till många intressanta frågor: Är skolorna de huvudsakliga drivkrafterna bakom ökningen av tillgången på resonemangsförmåga? Varför belönar arbetsmarknaden i allt högre grad logiskt tänkande framför faktakunskap? Kan förändringar i arbetsmarknadsavkastning också påverka trender i *icke-kognitiva* färdigheter?

Kapitel 2 – Hur införs nya läkemedel i olika socioekonomiska grupper?
När ny teknik introduceras är det vanligt att det finns stor variation i hur den tas i bruk. Stora skillnader i spridningen av teknik har dokumenterats både inom och mellan länder och i sammanhang som jordbruk, tillverkning, transport och medicin (se t.ex., Comin & Mestieri, 2014; Miraldo m. fl., 2019; Skinner & Staiger, 2007, 2015).

Inom hälso- och sjukvården är det viktigt att förstå i vilken utsträckning användningen av innovationer, såsom nya läkemedel, varierar mellan sjukhus och patientgrupper eftersom långsam införande kan vara kostsamt när nya behandlingar är avsevärt bättre än befintliga. Skillnader i införande mellan socioekonomiska grupper kan också bidra till hälsoskillnader (t.ex., Chetty m. fl., 2016; Finkelstein m. fl., 2021; Mackenbach, 2012; Zhang m. fl., 2010).

Det andra kapitlet, **Adoption of Medical Innovations Across Hospitals and Socioeconomic Groups: Evidence from Sweden**, samförfattat med Fabian Sinn, undersöker införandet av nya läkemedel med hjälp av administrativa data från Sverige. Vår analys kombinerar individdata från register över sluten- och öppenvårdsbesök och läkemedelsinköp med registerdata över socioekonomisk bakgrund och arbetsmarknadshistorik.

För att mäta införandet av ett nytt läkemedel approximera vi först dess målgrupp genom att koppla dess indikationer till diagnos- och åtgärdskoder i registerdata. Därefter mäter vi införandet genom att matcha datum för vårdbesök med receptdatum för köpta läkemedel. För att mäta införandet på sjukhusnivå spårar vi andelen patienter som besökt ett visst sjukhus och som köper läkemedlet efter utskrivning.

Genom att fokusera på 58 nya läkemedel för 47 hälsotillstånd (som hjärt-kärlsjukdomar, lungsjukdomar och diabetes) dokumenterar vi betydande skillnader i införandegrad mellan sjukhus och socioekonomiska grupper. Till exempel, vid slutet av vår studieperiod var införandegraden för sjukhus i 90:e percentilen ungefär tre gånger så hög som för sjukhus i 10:e percentilen. Liknande mönster gäller när vi tittar på specifika grupper, såsom patienter med hjärtinfarkt, förmaksflimmer eller kroniskt obstruktiv lungsjukdom (KOL).

Dessutom finner vi en positiv korrelation mellan en patients inkomstposition (mätt före sjukhusvistelse) och införandegraden av nya läkemedel för olika hälsotillstånd, från hjärt-kärlsjukdomar till lungsjukdomar och ADHD. Sammanlagt för alla våra nya läkemedel finner vi att en övergång från den lägsta till den högsta inkomstpercentilen ökar sannolikheten för att köpa ett nytt läkemedel med cirka 0,1 procentenheter, eller 10 procent i förhållande till den genomsnittliga införandegraden.

För att bedöma de potentiella konsekvenserna av skillnaderna i införandemönster använder vi ett nytt blodförtunnande läkemedel som fallstudie, vilket fick brett genomslag under vår studieperiod. Genom en enkel beräkning finner vi att om införandegraden hade harmoniserats mellan de högsta och lägsta inkomstdecilen, kunde överlevnadsgraden efter 12 månader för förstgångspatienter med hjärtinfarkt potentiellt ha ökat med 1,2 procent. Det är värt att notera att vi för detta läkemedel inte fann att bättre sjukhusledning eller läkemedlets inkludering i regionala riktlinjer var associerat med snabbare införande, vilket tyder på att andra faktorer kan ligga bakom skillnaderna i införandegrad.

Kapitel 3 – Påverkar arbetslöshetsförsäkring användningen av sjukvård?
En omfattande litteratur inom ekonomi, sociologi, folkhälsa, psykologi och andra samhällsvetenskaper visar att arbetslöshet och jobbförlust är stressande händelser som påverkar den mentala och fysiska hälsan negativt (t.ex., Brand, 2015; Dooley m. fl., 1996; Jahoda, 1982; Picchio & Ubaldi, 2023; Wanberg, 2012).

Förutom att skapa oro för de som förlorar sina jobb, kan dessa hälsoeffekter av arbetslöshet också bli kostsamma för samhället om de arbetslösa ökar sin användning av sjukvård, t.ex. på grund av långvarig stress. Dessa kostnader kan potentiellt vara stora eftersom individer vanligtvis betalar en liten andel av den totala kostnaden för den vård de får. Exempelvis stod hushåll för endast 6 procent av kostnaderna för slutenvård, 18 procent av kostnaderna för öppenvård och 25 procent av kostnaderna för receptbelagda läkemedel år 2016 i OECD-länder (Organisation for Economic Co-operation and Development, 2019).

Det tredje kapitlet, **Unemployment Insurance Generosity and Health: Evidence from Sweden**, samförfattat med Arash Nekoei och David Seim, undersöker om arbetslöshetsförsäkringen påverkar mottagarnas användning av sjukvård. Om tillgång till mer generösa arbetslöshetsförmåner hjälper till att mildra de negativa hälsoeffekterna av arbetslöshet, bör detta beaktas när man fastställer den optimala nivån för arbetslöshetsförmåner. Att studera effekterna på sjukvårdsanvändning bidrar också till att belysa om de negativa hälsoeffekterna av arbetslöshet främst beror på inkomstbortfallet efter jobbförlust eller om andra faktorer, såsom social stigma eller förlust av sociala kontakter och identitet (som betonats av t.ex., Jahoda, 1982), är viktigare.

Vår analys använder individdata från administrativa register över cirka 340,000 arbetslöshetsperioder, som vi kopplar till detaljerad registerdata över slutenvårds- och öppenvårdsbesök samt läkemedelsinköp. Vårt kostnadsmått syftar till att fånga de fullständiga kostnaderna för sjukvårdsanvändning, inklusive egenkostnader men också kostnader som täcks av läkemedelsförsäkringen för läkemedelsinköp samt resurskostnader för patientens vård, såsom personal- och administrativa kostnader för slutenvårds- och öppenvårdsbesök.

För att uppskatta den kausala effekten av arbetslöshetsersättning på sjukvårdsanvändning använder vi en så kallad *regression kink design*. Denna metod utnyttjar att arbetslöshetsersättningen har ett tak – en vanlig egenskap i arbetslöshetsförsäkringssystem världen över. Detta skapar ett knyck i förhållandet mellan arbetslöshetsersättningen och inkomsterna före arbetslösheten vid den punkt där individen når ersättningstaket. Så länge individer med inkomster strax under och över detta knyck är liknande i andra avseenden som påverkar sjukvårdsanvändning, kan vi tillskriva eventuella knyckar i förhållandet mellan sjukvårdsanvändning och inkomster före arbetslösheten en kausal effekt av arbetslöshetsersättningen på sjukvårdsanvändning.

Vi finner att generositeten i arbetslöshetsförsäkringen inte påverkar sjukvårdsanvändningen hos personer med arbetslöshetsersättning runt knyckpunkten. Till exempel kan vi för de första 20 veckorna av arbetslöshetsperioden utesluta förändringar i de totala sjukvårdskostnaderna större än 9 procent per en procent ökning i dagliga arbetslöshetsersättningar. Våra resultat är likartade för både män och kvinnor, yngre och äldre individer samt korttids- och långtidsmottagare av förmåner.

Resultaten från detta kapitel indikerar att, åtminstone i en svensk kontext, små justeringar av nivån på arbetslöshetsförmåner inte skulle ha någon större påverkan på sjukvårdsanvändningen bland de arbetslösa. En intressant öppen fråga är om samma gäller för andra socialförsäkringsprogram, såsom sjukförsäkring, där mottagarna generellt sett har sämre hälsa än de arbetslösa. Till exempel visar ny forskning från USA att tillgång till mer generös sjukförsäkring till och med kan minska dödligheten bland mottagarna av förmåner (Gelber m. fl., 2023).

Kapitel 4 – Påverkar föräldrars jobbförlust barnens utbildningsval? Utbildningsnivå är starkt korrelerad med föräldrarnas inkomst i många sammanhang. Till exempel ökade korrelationen mellan familjeinkomst och universitetsinskrivning mellan 1980-talet och 2000-talet i USA (Belley & Lochner, 2007), och familjer med högre socioekonomisk status utnyttjar möjligheten till fritt skolval oftare i Sverige och andra europeiska länder (Ambler, 1994; Skolverket, 2003). Även om denna korrelation kan spegla kreditbegränsningar, kan den också spegla andra faktorer, såsom ekonomisk osäkerhet som begränsar den tid föräldrar har för att engagera sig i sina barns utbildning.

Det fjärde kapitlet, **Family-Level Stress and Children's Educational Choice: Evidence from Parent Layoffs**, samförfattat med Julia Tanndal, undersöker hur en förälders uppsägning påverkar barnens utbildningsresultat, med fokus på valet av gymnasieprogram i Sverige. Eftersom det inte finns några terminsavgifter, men valet av skola och program kan vara komplext för familjer, hjälper denna kontext till att skilja mellan effekterna av finansiella och icke-finansiella begränsningar på barnens utbildningsresultat i låginkomstfamiljer.

Vi använder svenska registerdata om händelser där en arbetsgivare planerar att säga upp fem eller fler anställda på samma arbetsplats på grund av en långsiktig minskning av arbetskraftsbehovet. Vi kopplar information om föräldrar som är anställda på företag med minst en sådan händelse till information om föräldrarnas barns utbildningsnivå. Centralt för vår analys är att tidpunkten för uppsägningarna sannolikt inte är relaterad till egenskaperna hos de drabbade föräldrarna eller hur gamla deras barn är vid uppsägningstillfället. Vi använder därför variationen i barnets ålder vid tidpunkten för uppsägningen för att uppskatta hur uppsägningar som inträffar vid olika tidpunkter i barnets liv påverkar utbildningsnivån.

Vi finner att barn i familjer med en förälder som blivit uppsagd är mindre benägna att slutföra gymnasiet jämfört med sina jämnåriga, särskilt när uppsägningen sammanfaller med övergången från grundskola till gymnasiet (åldrarna 15–16 år). Sannolikheten att slutföra gymnasiet i tid minskar med 15 procentenheter (från 73 till 58 procent) för barn vars föräldrar sägs upp 6–12 månader före skolövergången. Däremot minskar sannolikheten för examen endast med cirka 3 procentenheter för barn som redan är inskrivna på gymnasiet vid tidpunkten för förälderns uppsägning.

Två resultat tyder på att våra resultat drivs av att uppsägningar minskar den tid föräldrar har för att investera i sina barns utbildning. För det första ser vi att effekterna på gymnasieavslut är större när uppsägningen sker före tidpunkten för gymnasievalet, en period då föräldrarnas stöd är viktigt. För det andra är effekterna av föräldrars uppsägningar större när de drabbar det äldsta barnet. Däremot kan vi inte utesluta att uppsägningar inte påverkar sannolikheten att slutföra gymnasiet bland yngre syskon. Detta sista resultat är förenligt med att föräldraledigheter är mer skadliga i familjer med mindre information om skolsystemet, eftersom yngre syskon borde ha tillgång till mer kunskap om skolval innan gymnasievalet blir relevant för dem.

Sammanfattningsvis belyser våra resultat att tidpunkten för föräldrars jobbförlust och hur den samverkar med kritiska övergångsperioder i utbildningssystemet är viktiga för att avgöra hur skadliga uppsägningar är för barns utbildningsresultat.

Referenser

Ambler, J. S. (1994). Who Benefits from Educational Choice? Some Evidence from Europe. *Journal of Policy Analysis and Management*, *13*(3), 454–476.

Belley, P., & Lochner, L. (2007). The Changing Role of Family Income and Ability in Determining Educational Achievement. *Journal of Human Capital*, *1*(1), 37–89.

Brand, J. E. (2015). The Far-Reaching Impact of Job Loss and Unemployment. *Annual Review of Sociology*, *41*, 359–375.

Carroll, J. B. (1993). *Human Cognitive Abilities*. Cambridge University Press.

Castex, G., & Dechter, E. K. (2014). The Changing Roles of Education and Ability in Wage Determination. *Journal of Labor Economics*, *32*, 685–710.

Chetty, R., Stepner, M., Abraham, S., Lin, S., Scuderi, B., Turner, N., Bergeron, A., & Cutler, D. (2016). The Association Between Income and Life Expectancy in the United States, 2001-2014. *Journal of the American Medical Association*, *315*(16), 1750–1766.

Comin, D., & Mestieri, M. (2014). Technology Diffusion: Measurement, Causes, and Consequences. I P. Aghion & S. N. Durlauf (Red.), *Handbook of Economic Growth* (s. 565–622, Vol. 2). Elsevier.

Dickens, W. T., & Flynn, J. R. (2001). Heritability Estimates Versus Large Environmental Effects: The IQ Paradox Resolved. *Psychological Review*, *108*, 346–369.

Dooley, D., Fielding, J., & Levi, L. (1996). Health and Unemployment. *Annual Review of Public Health*, *17*, 449–465.

Edin, P.-A., Fredriksson, P., Nybom, M., & Öckert, B. (2022). The Rising Return to Non-cognitive Skill. *American Economic Journal: Applied Economics*, *14*(2), 78–100.

Finkelstein, A., Gentzkow, M., & Williams, H. (2021). Place-Based Drivers of Mortality: Evidence from Migration. *American Economic Review*, *111*(8), 2697–2735.

Flynn, J. R. (1984). The Mean IQ of Americans: Massive Gains 1932 to 1978. *Psychological Bulletin*, *95*(1), 29.

Flynn, J. R. (1987). Massive IQ Gains in 14 Nations: What IQ Tests Really Measure. *Psychological Bulletin*, *101*(2), 171.

Flynn, J. R. (2018). Reflections About Intelligence Over 40 Years. *Intelligence*, *70*, 73–83.

Gelber, A., Moore, T., Pei, Z., & Strand, A. (2023). Disability Insurance Income Saves Lives. *Journal of Political Economy*, *131*(11), 3156–3185.

Jahoda, M. (1982). *Employment and Unemployment: A Social-Psychological Analysis*. Cambridge University Press.

Mackenbach, J. P. (2012). The Persistence of Health Inequalities in Modern Welfare States: The Explanation of a Paradox. *Social Science & Medicine*, *75*(4), 761–769.

Markussen, S., & Røed, K. (2020). Economic Mobility Under Pressure. *Journal of the European Economic Association*, *18*, 1844–1885.

Miraldo, M., Hauck, K., Vernet, A., & Wheelock, A. (2019). Variations in the Adoption of Healthcare Innovation: A Literature Review. *Oxford Research Encyclopedia of Economics and Finance*.

Organisation for Economic Co-operation and Development. (2019). *Out-of-Pocket Spending: Access to Care and Financial Protection*. Hämtad 27 maj 2024, från https://www.oecd.org/health/health-systems/OECD-Focus-on-Out-of-Pocket-Spending-April-2019.pdf

Picchio, M., & Ubaldi, M. (2023). Unemployment and Health: A Meta-Analysis. *Journal of Economic Surveys*, 1–36.

Pietschnig, J., & Voracek, M. (2015). One Century of Global IQ Gains: A Formal Meta-analysis of the Flynn Effect (1909–2013). *Perspectives on Psychological Science*, *10*, 282–306.

Rindermann, H., Becker, D., & Coyle, T. R. (2017). Survey of Expert Opinion on Intelligence: The FLynn Effect and the Future of Intelligence. *Personality and Individual Differences*, *106*, 242–247.

Schaie, K. W., Willis, S. L., & Pennak, S. (2005). An Historical Framework for Cohort Differences in Intelligence. *Research in Human Development*, *2*, 43–67.

Skinner, J., & Staiger, D. (2007). Technology Adoption from Hybrid Corn to Beta-Blockers. I E. R. Berndt & C. R. Hulten (Red.), *Hard-to-Measure Goods and Services: Essays in Honor of Zvi Griliches* (s. 545–570). University of Chicago Press.

Skinner, J., & Staiger, D. (2015). Technology Diffusion and Productivity Growth in Health Care. *Review of Economics and Statistics*, *97*(5), 951–964.

Skolverket. (2003). *Valfrihet och dess effekter inom skolområdet* (Report Nr 230).

Wanberg, C. R. (2012). The Individual Experience of Unemployment. *Annual Review of Psychology*, *63*(1), 369–396.

Zhang, Y., Baicker, K., & Newhouse, J. P. (2010). Geographic Variation in the Quality of Prescribing. *New England Journal of Medicine*, *363*(21), 1985–1988.

Dissertations in Economics (1975–2024)

1975	Claes-Henric Siven	A study in the theory of inflation and unemployment
1976	Bo Axell	Prices under imperfect information: a theory of search market equilibrium
1976	Thomas Franzén Kerstin Lövgren Irma Rosenberg	Skatters och offentliga utgifters effekter på inkomstfördelningen: en teoretisk och empirisk studie
1977	Hans-Olof Hagén	Grafisk industri i omvandling: en produktionsteknisk studie
1977	Peter Svedberg	Foreign investment and trade policies in an international economy with transnational corporations
1978	Nils Bruzelius	The value of travel time: theory and measurement
1979	Alfred Kanis	Demand for factors of production: an interrelated model of Swedish mining & manufacturing industry
1979	Aleksander Markowski	A formal versus an unformal forecasting model: an investigation of the forecasting procedure of the Swedish National institute of economic research
1980	Lorenzo Brown	The technical representation of returns to scale on cost and production functions
1981	Dick Kling	Spridning av ny teknik: en analys av anpassningsprocesser i svensk industri
1981	Richard Murray	Kommunernas roll i den offentliga sektorn
1981	Åsa Sohlman	Education, labour market and human capital models: Swedish experiences and theoretical analyses
1981	Edward E. Palmer	Determination of personal consumption: theoretical foundations and empirical evidence from Sweden
1983	Mats Bohman	Effektivitetsproblem inom vatten- och avloppsområdet
1983	Hillman Soeria-Atmadja	Product guarantees and liability rules
1984	Dag Lindskog	Foreign disturbances and domestic reactions: Study of wage and exchange rate policy reactions in Denmark, Finland, Iceland and Sweden 1973-81
1985	Nils Gottfries	Essays on price determination and expectations
1986	Farouk Kobba	Foreign investment export promotion and economic development

1986	Hans Wijkander	Indirect corrections and disequilibrium pricing: studies in second-best policies
1986	Göran Östblom	Structural change in the Swedish economy: empirical and methodological studies of changes in input-output structures
1987	Ronny Norén	Comparative advantages revealed: Experiments with a quadratic programming model of Sweden
1988	Emil Ems	Economics of public information systems
1988	Sten Kjellman	International trade in steam-coal
1988	Christopher Sardelis	Local employment policy: a study on the properties of intermunicipal rivalry
1990	Hans Lind	Tanken bakom tänkta ekonomier: om forskningsstrategi i modern nationalekonomi
1991	Kari Lantto	Optimal deterrents to malingering: the role of incentives, attitudes and information costs in social insurance, especially sickness benefit and welfare
1991	Kjell Jansson	Efficient prices and quality in public transport
1991	Tekaligne Godana	The behavior and performance of public enterprises: a theoretical and empirical analysis
1992	Claes Berg	Optimal investment and option values under risk-aversion with empirical evidence from Swedish manufacturing
1992	No-Ho	Optimal tax mixes
1992	Mats Kinnwall	Target zones: theory and evidence
1993	Kurt Lundgren	Aspects of learning by doing and firm behavior
1993	Ann-Mari Sätre	The effect of the Soviet shortage economy on the environment and the use of natural resources
1995	Jonas Björnerstedt	Essays in evolutionary game theory
1996:1	Ariane Lambert-Mogiliansky	Essays on corruption
1996:2	Joakim Sonnegård	Essays on experimental economics: bargaining, auctions, reservation-price elicitation and political stock markets
1997:1	Jan-Eric Nilsson	Efficiency Implications of Innovations in Administering Transport Infrastructure
1998:1	Xiang Li	Essays on the political economy of central bank policy
1998:2	Pehr-Johan Norbäck	Multinational Firms, Technology and Location
1999:1	Gabriel C Oxenstierna	Market power in the Swedish banking oligopoly
1999:2	Kerstin Hallsten	Essays on the effects of monetary policy
1999:3	Tomas Forsfält	Timing options and taxation: essays on the economics of firm creation and tax evasion
2000:1	Björn Carlén	Studies in climate change policy: theory and experiments
2001:1	Sven-Olof Fridolfsson	Essays on endogenous merger theory
2001:2	Helen Jakobsson	Essays on concentration, border effects and R&D cooperation
2002:1	Matthew J. Lindquist	Essays on the dynamics of wage inequality
2002:2	Jesper Roine	The political economics of not paying taxes
2002:3	Anne D. Boschini	Three essays on the economics of institutions
2002:4	Helena Svaleryd	Essays in finance, trade and politics
2002:5	Mikael Priks	Corruption, rent seeking and efficient governance
2002:6	Anne-Sophie Crépin	Tackling the economics of ecosystems

2003:1	Yoshihiko Fukushima	Essays on employment policies
2003:2	Lena Nekby	Empirical studies on health insurance, employment of immigrants and the gender wage gap
2003:3	Nils Bohlin	Essays on urban wages, location and retail trade
2003:4	Dan Nyberg	Essays on exchange rate risk and uncertainty
2003:5	Tobias Lindqvist	Essays on mergers and financial markets
2003:6	Adam Jacobsson	War, drugs, and media: arenas of conflict
2004:1	Tobias Nilsson	Essays on Voting and Government Inefficiency
2004:2	John Ekberg	Essays in Empirical Labor Economics
2004:3	Petroulas Pavlos	International Capital Flows: Effects, Defects and Possibilities
2005:1	Carlos Razo Perez	Mergers, Congestion and Collusion: Essay on Merger Policy
2005:2	Jan Pettersson	Three Empirical Studies on Development: Democracy, the Resource Curse and Aid
2005:3	Anna Nilsson	Indirect effects of unemployment and low earnings: Crime and children's school performance
2005:4	Bo Larsson	Essays on Banking and Portfolio Choice
2005:5	Carl Wilkens	Auri sacra fames: Interest Rates – Prediction, Jumps and the Market Price of Risk
2006:1	Magnus Wiberg	Essays on the Political Economy of Protection and Industrial Location
2007:1	Alberto J. Naranjo	Drugonomics: Industrial Organization of Illegal Drug Markets
2007:2	Maria Jakobsson	Empirical Studies on Merger Policy and Collusive Behaviour
2007:3	Camilo von Greiff	Income Redistribution, Educational Choice and Growth
2007:4	Sara Åhlén	Firms, Employment and Distance: Essays on the Swedish Regional Economy
2008:1	Anders Åkerman	Essays on International Trade, Productivity and Firm Heterogeneity
2009:1	Li-Ju Chen	Essays on Female Policymakers and Policy Outcomes
2009:2	Johan Kiessling	Essays on technology adoption and political reform in developing countries
2009:3	Paolo Zagaglia	The Macroeconomics of the Term Structure of Interest Rates
2009:4	Lars M. Johansson	Studies of the relationship between aid and trade and the fiscal implications of emigration and HIV/AIDS intervention
2009:5	Tobias Heinrich	Essays on Growth Econometrics and Endogenous Information
2009:6	Marie Gartell	Educational Choice and Labor Market Outcomes: Essays in Empirical Labor Economics
2010:1	Shon Ferguson	Essays on Trade, Technology and the Organization of Firms
2010:2	Eva Skult	Studies on Saving under Uncertainty
2010:3	Hans Lindblad	Essays on Unemployment and Real Exchange Rates
2011:1	Gûlay Özcan	Essays on Labor Market Disparities and Discrimination
2011:2	Jaewon Kim	Trade, Unemployment and Real Exchange Rates
2011:3	Magnus Rödin	Gender, Ethnicity and Labor Market Disparities
2012:1	Karolina Holmberg	Empirical Essays in Macroeconomics and Finance
2012:2	Martin Olssson	Essays on Unemployment Protection, Private Equity and Spousal Behavior
2012:3	Nicholas Sheard	Regional Economics, Trade and Transport Infrastructure
2012:4	Gustav Engström	Essays on Economics Modelling of Climate Change
2012:5	Lisa Laun	Studies on Social Insurance, Income Taxation and Labor Supply

2013:1	Christian Odendahl	Parties, Majorities, Incumbencies
2013:2	Maria Cheung	Education, Gender and Media
2013:3	Emma von Essen	Understanding Unequal Outcomes
2013:4	Mathias Ekström	Cues, Conformity, and Choice Architecture
2013:5	Eric Sjöberg	Essays on Environmental Regulation, Management and Conflict
2013:6	Pedro Soares Brinca	Essays in Quantitative Macroeconomics
2013:7	Marc Sanctuary	Essays on trade and environment
2014:1	Linnea Wickström Östervall	Essays on Antibiotics Use: Nudges, Preferences & Welfare Benefits
2014:2	Wei Xiao	Migration, Crime and Search in Spatial Markets
2015:1	Johan Egebark	Taxes, Nudges, and Conformity: Essays in Labor and Behavioral Economics
2015:2	Manja Gärtner	Prosocial Behavior and Redistributive Preferences
2015:3	Sara Fogelberg Lövgren	Markets, Interventions and Externalities: Four Essays in Applied Economics
2016:1	Theodoros Rapanos	Essays on the Economics of Networks Under Incomplete Information
2017:1	Kiflu Gedefe Molla	Essays in International trade, exchange rates and prices
2017:2	Jürg Fausch	Essays on Financial Markets and the Macroeconomy
2017:3	Anders Österling	Housing Markets and Mortgage Finance
2017:4	Mengyi Cao	Labor, Trade and Finance: Essays in Applied Economics
2018:1	Evangelia Pateli	Essays on International Trade: Theory and Evidence on the Determinants and Implications of Firms' Import Behaviour
2018:2	Wei Si	Empirical Essays in Labor and Development Economics
2018:3	Jakob Almerud	Public Policy, Household Finance and the Macroeconomy
2018:4	Dany Kessel	School Choice, School Performance and School Segregation: Institutions and Design
2018:5	Daniel Knutsson	Public Health Programmes, Healthcare and Child Health
2018:6	Joakim Jansson	We are (not) anonymous: Essays on anonymity, discrimination and online hate
2018:7	Laurence Malafry	Inequality and Macroeconomic Policy: Essays on Climate, Immigration and Fiscal Intervention
2019:1	Elisabet Olme	Essays on Educational Choices and Integration
2019:2	Vanessa Sternbeck Fryxell	Essays on Interbank Markets
2019:3	Mathias Pronin	Essays in Macroeconomics and Political Economy
2020:1	Niklas Blomqvist	Essays on Labor Economics: The Role of Government in Labor Supply Choices
2020:2	Daniel Almén	Societal Impacts of Modern Conscription: Human Capital, Social Capital and Criminal Behaviour
2020:3	Louise Lorentzon	Empirical Essays on Public Policies: Social Insurances, Safety Nets, and Health Care
2020:4	Tamara Thornquist	Essays in economics: The impact of changes on the labor market induced by structural change, the adoption of a new computer-based technology and economic slowdowns on family formation, family fertility outcomes and new careers
2021:1	Nanna Fukushima	Essays on the Economics of the 1956 Clean Air Act
2021:2	Roza Khoban	Globalization and Development: The Impact of International Trade on Political and Social Institutions

2022:1	Ulrika Ahrsjö	Essays on Economic Disadvantage: Criminal Justice, Gender and Social Mobility
2022:2	Erik Lindgren	One coin - One vote: the rural political power shift that pushed Sweden towards industrialization
2023:1	Jon Olofsson	An Economic Backbone of Development: Essays in Financial and Political Economy
2023:2	Yangzhou Yuan	Policy, Institutions and Misallocation
2023:3	Jens Wikström	Essays in Health Economics and the Economics of Social Security
2023:4	Anna Linderoth	Essays on Men's Preferences and Gender Inequality in the Labor Market
2024:1	David Jackson	Enforcing Social Norms: How Economics Shapes Reputation and Social Punishment
2024:2	Nicklas Nordfors	Essays on Development and the Environment
2024:3	Charlotte Lucke	Essays on Labour Supply and Inequality
2024:4	Miika Päällysaho	Essays in Labor, Public, and Health Economics

This thesis consists of four self-contained essays on topics in labor, public, and health economics.

Labor Market Returns and the Evolution of Cognitive Skills: Theory and Evidence argues that changes in labor market returns can help explain trends in measured cognitive skills among Swedish men.

Adoption of Medical Innovations Across Hospitals and Socioeconomic Groups: Evidence from Sweden studies the adoption of novel medicines in Sweden, with a focus on differences between hospitals and socioeconomic groups.

Unemployment Insurance Generosity and Health: Evidence from Sweden studies how the generosity of unemployment insurance affects the healthcare use of benefit recipients.

Family-Level Stress and Children's Educational Choice: Evidence from Parent Layoffs argues that the timing of parental job loss is important for determining how harmful the effects of parental job loss are for children's education.

Miika Päällysaho
holds a BSocSc and an MSocSc in Economics from the University of Helsinki.
He is an applied microeconomist with interests in labor, public, and health economics.

ISBN 978-91-8014-911-2
ISSN 1404-3491

Stockholm University

Department of Economics