Four Empirical Essays on Demographic Change and the Labor Market

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1 Introduction

The population trends of the 21st century are unprecedented. For the first time in human history, the elderly population will exceed the number of children, while the world population will grow from roughly 7 billion today to nearly 11 billion in 2100. In the more developed regions of the planet people aged 60 or older have already outnumbered the young under 20 years of age, and less developed regions will follow suit by the end of the century (United Nations, 2014, 2013a,b).

The ongoing demographic trends constitute a tremendous challenge for societies and economies that is attracting research in economics and other disciplines. This thesis adds to this research by investigating several economic research questions related to demographic developments empirically. The aim of this introductory chapter is to disclose the relevance of this thesis. I start with outlining the historical origins, the current and future characteristics and the economic implications of population aging to illustrate why research on the topic is urgently needed. I conclude with a presentation of the contributions over the existing economic literature and the policy implications of each chapter, whereby also outlining the structure of the thesis.

1.1 Population Aging

The origins of population aging go back far into the past. Around 1800, declining mortality in Europe marked the start of a demographic transition that spread to all parts of the world. The typical course of the transition was a decline in death rates which was after some time accompanied by decreasing birth rates. This combination led to initially increased and then decreased population growth and finally to the onset of population aging (Lee, 2003). The associated shifts in the population age structure have important economic implications (Harper, 2014). In particular, the threats from population aging include old-age poverty, economic growth slowdowns and intergenerational injustice. To conquer these threats, political action is inevitable and urgent. In the following, the problem of population aging is explained in more detail, starting from its historical origins and concluding with a presentation of potential solution strategies.

1.1.1 Mortality Decline

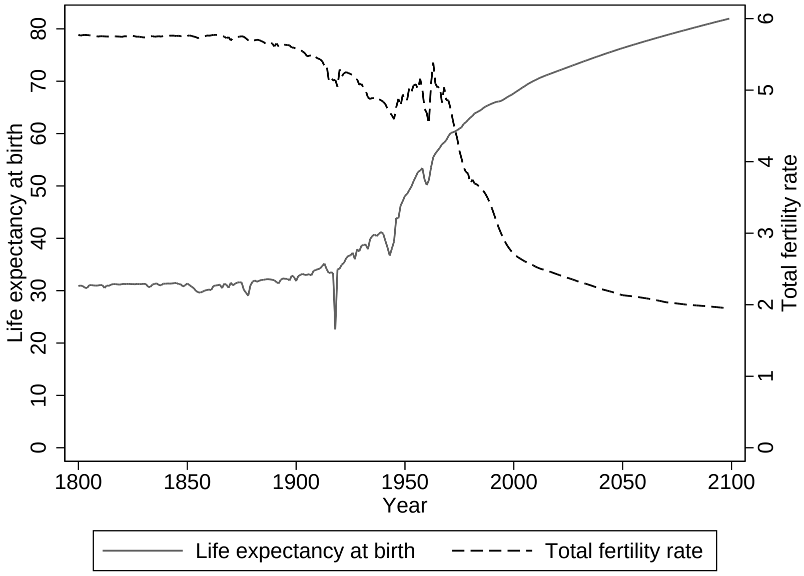
The world average of human life expectancy at birth was roughly 30 years, when around 1800 declining mortality in Europe marked the start of a revolutionary change. Within only three centuries, global life expectancy at birth is rising by 50 years to a level of at least 80 in 2100 (Lee, 2003). As Figure 1.1 indicates, the strongest gains were attained in the middle of the twentieth century. Nowadays, life expectancy at birth in developed countries amounts to 82 years for women and to about 76 years for men. In less developed countries, life expectancy equals 63 for women and around 60 years for men (WHO, 2014).

While today's high-income countries experienced the survival gains in parallel, mortality in the developing world started to fall only in the early twentieth century. However, poorer countries accelerated their survival rates sharply in the middle of the century, profiting from spreading knowledge of the medical experiences in richer countries. Life expectancy at birth in China, for example, increased from 41 in the early 1950s to 70 years in the late 1990s. The average gains in low-income countries stagnated when the HIV/AIDS epidemic spread and became the leading cause of death in sub-Saharan Africa. For the 35 most affected African countries, life expectancy at birth even decreased by on average 6.5 years in the late 1990s (Lee, 2003).

Future increases in life expectancy might be subject to biological limits regarding human longevity (Lee, 2003). However, experiences of countries with many very old people such as Japan suggest that these limits have not yet been touched. We therefore might experience a future increase not only in the numbers of centenarians but also of supercentenarians (Harper, 2014; Vaupel, 2010).

How could the extraordinary gains in life expectancy be achieved? Initially, medical advances, such as the development of the smallpox vaccine, allowed re-

Figure 1.1: Global Life Expectancy and Global Total Fertility 1800–2100



Own calculations based on data compiled by Gapminder (2012, 2014, 2015, combination of actual data, guesstimates and forecasts).

ductions in contagious and infectious diseases. Spreading medical knowledge, such as that of the germ theory of disease, induced an awareness of health threats and the utilities from personal hygiene and proper nutrition. Health care, nourishing food, insulated housing and appropriate clothing became more affordable with growing wealth levels in the course of the emerging Industrial Revolution. Improved nutrition early in life and throughout the life cycle allowed the development of stronger organ systems and improved the ability to resist disease. Wealth was also associated with higher educational attainments which in turn have been observed to correlate with health-preventing behaviors and to foster a more stress-free life in general. Technological advances also helped to reduce mortality. Examples are improvements in food storage, transportation and trade which allowed a regional smoothing of food shortages and therefore a reduction of famine mortality (Lee, 2003; Barker, 1990; Vaupel, 2010).

Throughout the nineteenth century, high-income countries realized substantial declines mostly in infant and child mortality (Lee, 2003). In the early twentieth century, still most of the gains in life expectancy were achieved early in the life cycle, with less than 20 percent being gained after age 65. However, the continued gains shifted from youth and working ages to later stages of the life cycle (Eggleston and Fuchs, 2012). Cardiovascular disease mortality and falling tobacco use were the main factors contributing to the rise in life expectancy at older ages in recent decades (Mathers et al., 2015). Nowadays, more than 75 percent of the additional years are attained after age 65, while this share is asymptotically approaching 100 percent (Eggleston and Fuchs, 2012).

1.1.2 Fertility Decline

Throughout the nineteenth century, the global total fertility rate amounted to at least six children per woman. Marital fertility started to decline in most parts of Europe between 1890 and 1920 (Lee, 2003). Birth rates are declining globally ever since, apart from few interruptions. After World War II, many high-income countries experienced baby booms (e.g. Germany and Sweden), leading to a temporary overall increase in birth rates, as Figure 1.1 shows. The strongest fertility reductions occurred subsequently between around 1970 and 2000. In 2012, the total fertility rate in high-income countries amounted to only 1.7 children per women, while in low-income countries it was on average still as high as 4.1 children per woman (WHO, 2014). Globally, total fertility is predicted to fall to a level as low or even below the replacement level of 2.1 children per woman by 2100.

While in high-income countries birth rates started to fall around 1900, less and least developed countries began their fertility transitions only around 1970. They reduced their birth rates, however, at a much higher speed than the more developed countries before them. India, for example, reduced its total fertility rate from 4.7 children per woman in 1980 to only 2.5 children in 2013. Over the same period, Vietnam's total fertility dropped from a level of even 5.0 to only 1.7 children per woman (World Bank, 2015c).

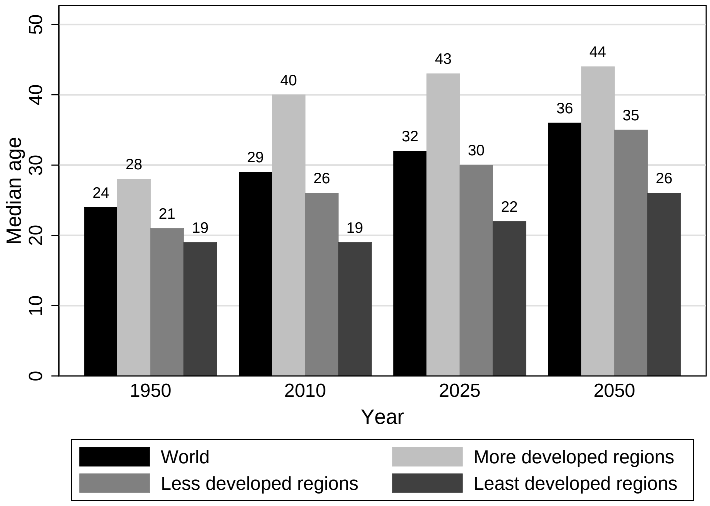
The exact reasons for the fertility decline are still not fully understood. However, the recent economic literature discusses six main hypotheses (Guinnane, 2011). First, the drop in mortality, that is always preceding the fertility decline, is a suspected cause. The argument is that declining infant and child mortality reduces the number of births necessary for a couple to achieve a desired number of surviving children (Canning, 2011; Guinnane, 2011). Second, the availability of contraception and abortion allowed couples who wanted smaller families to reduce their fertility (Van Bavel and Reher, 2013). Contraceptive technologies have likely accelerated the fertility decline, especially during the second half of the twentieth century when the birth control pill became available. However, ways of birth control, albeit less reliable, have existed at all times (Stevenson and Wolfers, 2007). Contraception may therefore not explain why couples wanted to reduce their fertility in the first place. Third, the decline in birth rates may have happened due to rising costs of children associated with rapid urbanization or by child-labor restrictions that reduced the expected family income. Fourth, changing economic conditions and expectations may have reduced fertility when in the course of the Industrial Revolution technological progress, increasing human capital and associated higher wages raised the opportunity costs of time investments into child rearing (Harper, 2014; Guinnane, 2011; Canning, 2011). Fifth, rising returns to child quality or decreasing education costs due to the creation of primary schools may have incentivized parents to make higher investments into a smaller number of children. Finally, with the establishment of social insurance and old-age support children lost their role as a form of old-age assurance (Guinnane, 2011).

1.1.3 Aging Populations

Declining mortality followed by falling fertility characterized the typical course of the demographic transition all over the world. Changes in fertility and mortality have consequences for population size and age structure (Preston and Stokes, 2012; Canning, 2011). In early stages of the transition, the mortality decline prior to fertility decline led to a substantial growth of young cohorts because it mostly translated into higher infant and child survival rates. In combination with the onset of fertility decline after some decades, this resulted in strong increases of the working-age population relative to the dependent population of young and elderly.

During the twentieth century the continued gains in life expectancy shifted

Figure 1.2: Median Age by Region, 1950–2050



Source: United Nations (2013a).

from youth and working ages to later stages of the life cycle, initiating a new kind of demographic transition with mortality gains concentrated among the elderly (Eggleston and Fuchs, 2012). In recent years, total fertility has fallen below the replacement level of 2.1 children per woman in many high-income countries. Consequently, successive cohorts are smaller and smaller and populations are shrinking as a whole. At the same time, large longevity gains are being realized. Rich countries therefore have rapidly rising numbers of old and oldest-old people, in absolute and in relative terms. As a result, a massive population aging is progressing in these countries that is associated with an ongoing shrinkage of the working-age population relative to the dependent population.

Population aging is mirrored by the median age, the threshold dividing the younger from the older half of the population. As illustrated in Figure 1.2, the global median age has risen from 24 to 29 between 1950 and 2010 and is expected to increase by another seven years until 2050. However, the aging process does not evolve uniformly across regions (Gerland et al., 2014). While the societies of the developed world have aged rapidly since 1950, the strong aging in the decades to come will be mostly driven by less and least developed countries (United Nations, 2013a). This reflects the late onset of the demographic transition in developing countries only a century ago, while in more developed nations the demographic variables may already approach their extremes.

Overall, population aging is more pronounced in the more developed regions of the world (Bloom et al., 2011). Germany, for example, is among the countries with the lowest reproductivity of all. In 2012, it had a total fertility rate of only 1.4 children per woman, while its life expectancy at birth was 78 years for males and 83 years for females. With a total fertility of 1.9 children per woman in 2012, Sweden exhibits slightly higher birth rates, but with a life expectancy at birth of 80 years for males and 84 years for females it is one of the leading countries in terms of longevity (WHO, 2014).

1.1.4 Labor Market Implications

How does population aging interact with labor markets? From an aggregate perspective, shifts in the relative supplies of people of different age have implications for economic growth and intergenerational justice. During the original demographic transition, the strong increase in the working-age population relative to the dependent population after the onset of fertility decline was typically associated with a boom in per-capita incomes and strong economic growth and therefore referred to as the "demographic dividend" (Bloom et al., 2003; Eggleston and Fuchs, 2012; Canning, 2011).[1] In contrast, the strong growth of the elderly population relative to the working-age population observed in recent decades is referred to as a "demographic deficit" (Harper, 2014) because it raises the the per-capita demographic burden on individuals in working age (Bengtsson and Scott, 2011), as measured by the dependency ratio.[2] Pay-as-you-go retirement and long-term care systems which redistribute financial measures from workers to retirees even worsen the demographic bur-

[1] It remains questionable though to which extent this growth was actually attributed to the demographic developments. First, the Industrial Revolution may have been a major driver of economic growth at the time (Canning, 2011). Second, for many developing countries experiencing demographic transitions today it is still unclear to which extent they will economically catch up.

[2] The dependency ratio is typically defined as the number of children under age 15 plus older persons aged 65 years or over (assumably reflecting the economically inactive population) divided by the number of persons aged 15 to 64 years (assumably reflecting the productive population; United Nations, 2013a).

den as they oblige workers to provide for the elderly while pushing down the labor force participation of the latter (Canning, 2011). Many countries rely on pay-as-you-go based social security systems.

Focusing on intergenerational justice, relatively large generations bear a low per-capita burden when they are in working age because their size depresses the dependency ratio. When they enter retirement, they raise the per-capita burden on a subsequent smaller generation because their size then raises the dependency ratio. This implies that large generations benefit at the cost of small generations. On the other hand, large generations may face higher competition levels throughout their life cycles which may suppress their educational, occupational and labor market outcomes and finally also retirement entitlements. Hence, population aging induces important concerns of intergenerational injustice (Harper, 2014), although it is not clear if there is a net gain or loss for larger cohorts relative to smaller cohorts.

What are the future economic implications from ongoing demographic change? In order to a keep pay-as-you-go system financially balanced, either the social security contributions paid by workers must rise, the tranfers to elderly must shrink, or both. Raising the contributions of workers is likely associated with productivity losses, because it decreases their net labor income and therefore likely demotivates them to be productive. In combination with the overall declining workforce this might seriously slow down economic growth (Eggleston and Fuchs, 2012; Harper, 2014). A decrease in the retirement and long-term care payments to the elderly or even a financial breakdown of the whole system may be associated with a growing threat of old-age poverty, but might also raise the labor force participation of elderly.

Demographic developments may affect labor markets also via behavioral changes at the individual level (Smeeding, 2014). Possible mechanisms are that fewer children may be associated with a greater labor market participation of women (Canning, 2011; Eggleston and Fuchs, 2012), that a higher life expectancy may raise the returns to education and therefore educational attainment, or that insecurity regarding future retirement payments may affect patterns of savings and consumption (Harper, 2014).

While behavioral changes at the individual level may stimulate or decelerate economic growth, overall shifts in the age structure imply inevitable threats from population aging, namely old-age poverty, economic growth slowdowns and intergenerational injustice, if no actions are taken (Eggleston and Fuchs, 2012). The following subsection surveys possible measures to conquer these threats.

1.1.5 Solution Strategies

How may the threats from population aging be conquered? In order to release the financial pressure on social security systems, obviously either the aggregate amount of contributions must be raised, the dependency of elderly on transfers has to be reduced, or both (Eggleston and Fuchs, 2012), while at the same time productivity losses and old-age poverty need to be avoided. In the following, potential policy parameters in achieving these goals are listed.

Increasing total contributions without raising the per-capita burden on workers may be reached by either increasing the number of contributors or by enhancing labor productivity. The number of contributors may be increased by fostering fertility or immigration, raising female labor market participation, reducing unemployment or lowering the job entry age (Börsch-Supan et al., 2014). Labor productivity may be enhanced by investments into education, health or technological research (Harper, 2014).

Altering fertility, education or health may affect the contribution sum only after several decades because successive cohorts enter the labor market only many years after birth, education improvements within the workforce may also take several years and health interventions might be most effective very early in the life cycle (Barker, 1990). In contrast, immigration may increase the working-age population immediately. However, the potential of immigrants to raise aggregate social security contributions depends on their productivity in the host countries' labor markets, which in turn relies on factors such as language barriers and the transferability of foreign human capital. Raising female participation, lowering unemployment or decreasing the labor market entry age would also be effective in the short-run, however, the latter measure reduces the time available for educational attainment, possibly negatively impacting productivity. Finally, technological progress may show immediate effect as well, but successful innovation is difficult to predict.

Reducing the dependency of elderly on retirement transfers while preventing old-age poverty may be reached by increasing elderly's labor force participation or by finding alternative sources of funding. The labor force participation of older people could be increased by extending their working careers. For this purpose, the legal retirement age could be raised or more flexible ways for gradual transitions into retirement may be found. In addition, investments in health could maintain older people's ability to execute job tasks (Harper, 2014), while further education may enable them for job changes. Alternative financial sources might be given by private pensions and savings.

Since alterations of health and private savings should be realizable only after several decades, they might affect the labor market participation of older people only in the long-run. Also, further training of elderly may show effect only after a couple of years. In contrast, raising the normal retirement age and allowing for more flexible exit transitions may be effective immediately.

Lowering elderly's need for long-term care payments may be reached by improving their health, by substituting formal by informal care or by increasing the efficiency of the long-term care system. Again, overall health improvements among the elderly may take their time. In contrast, raising informal relative to formal care provision may release long-term care costs immediately. However, informal care providers will not be available to the labor market and may even become dependent on financial support themselves for the time of care provision. Efficiency improvements in the long-term care system may therefore be a more promising way to reduce expenses in the short-run.

Policy reforms are generally difficult to enforce in democratic systems and their implementation is often time-consuming and costly. Since a release of the financial pressure on the workforce from the rising share of elderly is urgently necessary, good arguments and effective policy designs are needed to implement some combination of the described solution strategies. This requires sound knowledge, first, on the interactions between population aging and labor markets, and second, on the effectiveness of potential policy interventions. Empirical economic research can provide qualitative and quantitative evidence on both these aspects. The following section presents the contributions of this thesis to this research.

1.2 Contributions of the Thesis

This subsection gives an overview of the structure of the thesis and briefly summarizes content, literature contributions, policy implications and empirical findings of each chapter. The countries chosen for analysis are Germany and Sweden, both having rapidly aging populations, while their social security systems function, at least partly, on pay-as-you-go basis, qualifying them perfectly for analyses of demographic change and labor markets.

Chapter 2

The fertility changes of recent decades have created birth cohorts of very different size. As these cohorts grow older, they are constantly shifting the age structure of the workforce, whereby potentially affecting labor market outcomes. Chapter 2 of this thesis presents estimates of cohort size effects on wage, employment and work time. The analysis contributes to the literature by generating evidence on cohort size effects in Germany, which is an excellent case study for its large and rapidly aging population and still lacks comprehensive insights on the topic to date. Moreover, empirical evidence on the response pattern of different economic outcomes in the presence of restrictive labor market institutions is generated. Finally, to account for an expected effect heterogeneity by level of occupational specialization, a measure of actual task content is exploited rather than educational attainment which was used by former studies.

Analyzing cohort size effects on economic outcomes provides insights on the effects that population aging has on labor markets as well as on competition effects in general. While labor market effects of a changing age structure have important implications for the income distribution as well as the employment structure and the organization of work (Fertig et al., 2009), politicians may also view them as an inequitable redistribution of economic success between generations. The main findings of Chapter 2 are that a larger cohort size reduces the wages of workers with medium and high degrees of occupational specialization, while employment effects are detected for highly specialized males only. Work time rises in response to an increase in cohort size for medium specialized males and decreases for highly specialized males and females.

Chapter 3

During the past decades, Germany experienced several phases of strong immigration which were affecting size and age structure of the population. In contrast to other immigration countries, the literature has found very little evidence for an economic integration process of immigrants for the German case. Former studies, however, typically employe cross-section models, which suffer from the serious drawback that they do not account for cohort effects. The analysis presented in Chapter 3 provides a first application of a double cohort model to earnings assimilation processes. The model accounts for cohort effects and is therefore arguably superior to the conventional method.

Fostering immigration is considered a potential strategy to raise the working-age population and to hereby address the financial pressure on the social security systems induced by population aging. The analysis presented here delivers insights on the effectiveness of this strategy because it answers the question of whether an economic integration process of immigrants takes place. The empirical results of Chapter 3 do not provide evidence of an earnings assimilation process of immigrants, confirming the results of the existing literature and suggesting that the potential of immigration in addressing the threats from population aging may be limited.

Chapter 4

The analysis presented in Chapter 4 examines the labor market exit behavior of older manual and non-manual workers in Germany and quantifies the proportion of their retirement age difference that is explained by circumstances beyond individual influence. The analysis contributes to the retirement literature by providing a detailed empirical description of the labor force dynamics and retirement patterns of older manual and non-manual workers. Moreover, it adds to the growing literature on equality of opportunity which distinguishes legitimate inequalities, as arising from different individual effort levels, from illegitimate inequalities, as determined by circumstances beyond individual influence, and provides a first application of the framework to retirement timing (e.g., Roemer, 1993; Jusot et al., 2013).

Raising the normal retirement age to extend working lives is a potential strategy to change dependency ratios and to hereby relieve the growing financial pressure on social security systems associated with population aging. However, occupational differences in the capacity to extend working careers give reason for equity concerns, because retiring before reaching the normal retirement age often reduces benefit entitlements. Against this background, analyzing inequalities in the retirement dynamics of workers with different physical job demands contributes to detecting age limitations in the execution of particular tasks. Furthermore, evaluating the proportion of differences in retirement timing which are predetermined and thus beyond individual influence delivers valuable input to the normative discussion on the justness of a uniform increase of the normal retirement age. The results of Chapter 4 indicate that among individuals aged 55 to 65 non-manual workers have a 20% lower instantaneous risk of entering retirement than manual workers, while the latter are more likely to become unemployed during the transition process. Furthermore, it is estimated that circumstances explain at least one third of the observed differences in the retirement age between workers with different degrees of physical job demands.

Chapter 5

The final chapter of this thesis investigates the impacts of a reform of 1938 in Sweden which introduced universal access to free ante- and neonatal health care to the Swedish population on demographic, health and socioeconomic outcomes. By provoking sustainable health improvements, the reform may have been an important contributor to the ongoing extension of longevity. Moreover, by creating fertility incentives, it may have been a driver of Sweden's baby boom of the 1940s. The analysis presented here contributes to a growing strand of the literature suggesting that early-childhood conditions predetermine later-life health and socioeconomic outcomes, whereby establishing a social gradient in health which is widening in age (e.g., Barker, 1990; Currie and Stabile, 2003; Case et al., 2002). The study is among the first to measure the causal effects of a universal early-life health intervention on adult outcomes, contributing over existing studies that are often confounded because many policies target only disadvantaged subgroups.

Evaluating early-life health interventions delivers insights on their effectiveness as measures to foster fertility in times of low birth rates. In addition, interventions of this kind may be a way to prevent health deteriorations up to older ages which might in turn contribute to maintain older workers physical ability to execute job tasks and to counteract the rising demand for long-term care associated with population aging. The implications from evaluating historical reforms may be of particular relevance for developing countries which often exhibit comparable health measures today as did richer countries in the past. The empirical results presented in Chapter 5 indicate negative effects on mortality which are increasing in age, positive effects on several long-run health outcomes and considerable improvements in income, but no effects on fertility.

2 Cohort Size Effects on Wages, Working Status, and Work Time

Abstract

This paper estimates the effects of cohort size on wages, employment and work time for workers in Germany. The empirical findings suggest that male workers with medium and high degrees of occupational specialization who were born at the peak of the baby boom earn at least 5.3% lower wages than comparable workers born during the subsequent baby bust. Highly specialized females born into large cohorts earn 2.5% lower wages than their counterparts from small cohorts. Employment effects are detected only for highly specialized males. The effects on work time are mixed and invariably larger when actual work time is considered rather than contractual work time. It is argued that the restrictive labor market institutions in place are key in shaping the response pattern across the different economic outcomes.

2.1 Introduction

Most regions of the planet experience rapidly rising numbers of elderly both in absolute and relative terms (United Nations, 2013a). Various studies investigate the consequences of this development for labor markets (e.g., Börsch-Supan, 2003, 2008; Feyrer, 2007, 2011) and social security systems (e.g., Börsch-Supan, 2000, 2005; Hirazawa et al., 2010). Several papers focus on shifts in the age structure of the workforce and estimate effects of relative cohort size on economic outcomes such as wages (e.g., Macunovich, 1999; Card and Lemieux, 2001; Araki et al., 2013) and employment (e.g., Korenman and Neumark, 2000; Macunovich, 2012). The rationale behind this approach emanates from the theoretical concepts of factor substitutability and competitive pricing: To the degree workers of different age are imperfect substitutes on the labor market, the number of rivals individuals face in job competition is reflected by the number of individuals of similar age. Because the degree of competition influences outcomes, cohort sizes are expected to have a negative impact on economic success.

This paper analyzes wage, employment and work time responses to shifts in the relative labor supplies of workers of different age. The empirical analysis is based on individual-level data and age-specific population numbers for Germany. The results are interpreted in the context of discussing the response pattern across different labor market outcomes and occupation types, first, because the effects of cohort sizes on different outcomes are likely dependent, and second, because Germany's restrictive labor market institutions may shape this pattern in a certain way by imposing rigidities upon some economic outcomes and some types of workers but not upon others.

The paper contributes to the literature in several respects. First, the German labor market is examined. The existing evidence on Germany lacks comprehensive insights on the topic to date because relatively few studies estimate cohort size effects on labor market outcomes using German data. While some papers provide evidence for employment effects (Zimmermann, 1991; Schmidt, 1993a; Jimeno and Rodriguez-Palenzuela, 2002; Fertig and Schmidt, 2004; Biagi and Lucifora, 2008; Garloff et al., 2013), only one study (to the best of my knowledge) examines cohort size effects on wages using cross-country data including Germany (Brunello, 2010). Although wages and employment may be rigid as a result of restrictive labor market institutions, only Garloff et al. (2013) investigate work time as an alternative response variable. This lack of evidence for the German case is surprising, firstly, because Germany has the largest labor force in the European Union (World Bank, 2015a), and secondly, because demographic change is particularly marked in Germany. In 2013, the German population happened to be the second oldest in the world in terms of median age (45.5), closely behind the Japanese population (45.9; United Nations, 2013b).

A second contribution of this study is the usage of a measure of actual job content rather than educational attainment to account for different degrees of worker substitutability. Plausibly, the substitutability of workers within and across birth cohorts determines how strong economic outcomes respond to cohort size changes. To account for the expected effect heterogeneity, former studies proxied worker substitutability by educational level. As will be argued below, the educational level is a poor indicator of worker substitutability for a range of occupations, such as emergency physicians or low-educated workers in experience-based positions. In the present study, worker substitutability is proxied by the physical demandingness of occupations measured on a ten-point scale, an indicator that is arguably superior to the educational level.

Finally, the present study generates empirical evidence on the interplay of the effects on different labor market outcomes in the presence of restrictive labor market institutions. For instance, the dismissal of a worker may be difficult under dismissal protection legislation, in which case an employer might instead lower the worker's wage. On the other hand, collective bargaining agreements may impede wage and work time adaptions, which incentivizes employers to adapt employment instead. Hence, rigidities in one outcome may be responsible for a relatively strong response in another outcome due to a rerouting of the effect. In contrast to previous studies, which mostly focus on a single economic outcome, the approach chosen here accounts for institution-induced ridigities in outcomes and the possibility of diverted effects across outcomes.

The empirical results reveal a robust response pattern across the different labor market outcomes. Large cohort sizes are found to depress economic success in terms of wages for male and female workers with medium and high degrees of occupational specialization. Specifically, a medium (highly) specialized man born at the peak of the baby boom earns on average 5.8% (5.3%) lower wages than a comparable man born during the subsequent baby bust. Highly specialized females born into large cohorts earn 2.5% lower wages than their counterparts from small cohorts. In addition, the employment probabilities of highly specialized males born at peak fertility are 0.3 percentage points lower than those of comparable workers born into later-born, small cohorts. For males with a medium degree of occupational specialization, weekly work time rises in response to an increase in cohort size, while it decreases for highly specialized males and females. The estimated effects on actual work times are stronger than those on contractual work times.

These results are broadly in line with the theoretical considerations. Germany's restrictive dismissal protection legislation covering all occupational specialization groups equally (while excepting workers in small establishments) lowers the probability of employment adaptations in general, which may explain why medium and highly specialized males and females respond in terms of wages and work times but only highly specialized males are slightly affected in terms of employment. The positive cohort size effect on work time measured for medium specialized males may result from an increased pressure to succeed on the individual worker when faced with intensified competition or from a worsening of working conditions enforced by employers as a reaction to increased labor supply. In contrast, the negative effects on work time for highly specialized males and females may indicate that in this group increased labor supply leads to a relief of the individual worker because a given amount of work is allocated among a larger number of workers.

The paper is organized as follows. Section 2.2 briefly illustrates the demographic trends in the German workforce and infers expectations regarding its effects on labor market outcomes. Section 2.3 details the empirical identification strategy. Section 2.4 describes the data. The empirical findings are presented in Section 2.5. Section 2.6 concludes.

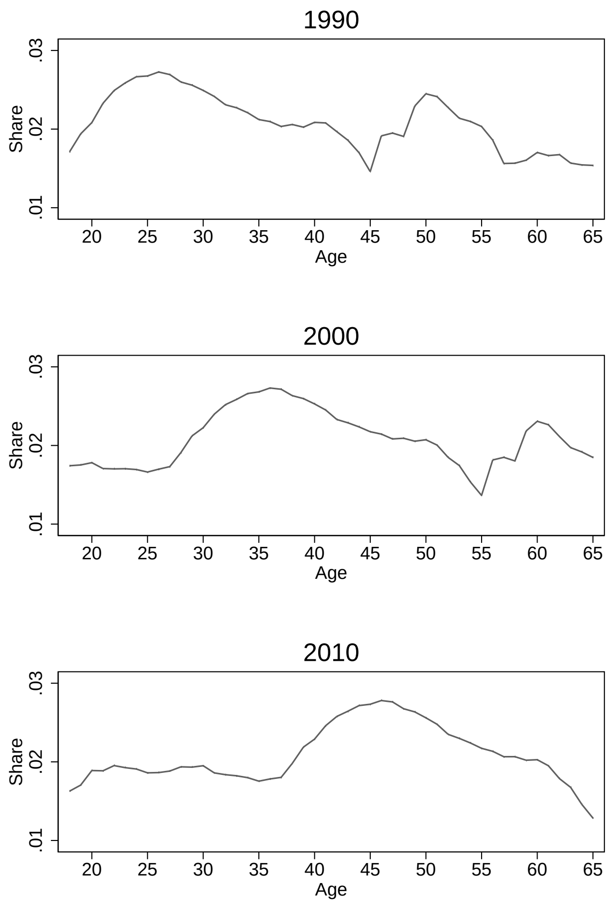
2.2 Workforce Aging and Competitive Labor Market Outcomes

Over the past century, the German population was subject to substantial demographic transformation resulting from a permanent decline in fertility and a steady growth of life expectancy. The changes, which entail a rapid aging of the population, are ongoing and will continue in the future. The German population share of 20 to 65 year-olds is predicted to decline from 61% in 2008 to 50% in 2060 (Federal Statistical Office, 2009), which indicates that by 2060 an average worker will have to provide for herself as well as for at least one non-working person. However, the labor force population is not only shrinking relatively to the rest, it also ages in itself. Figure 2.1 illustrates the relative labor force shares by age group over time and reveals considerable shifts in the age structure of the workforce with the labor supply of younger workers declining relatively to the supply of older workers. In particular, high shares of people in their 20s observed in 1990 shift to people in their 30s in 2000, and finally to people in their 40s in 2010.

How do wages, employment and working time respond to these shifts? The size of a cohort may influence individuals' labor market outcomes through various channels as it ages. School outcomes, educational track, occupational choice and further training may be subject to competition among individuals of similar age, all of which may affect later-life economic outcomes, before workers in the same occupational tracks compete for the same jobs on the labor market. In particular, the larger a cohort and the more restricted the educational capacities for popular professions, the more individuals will be forced into another than their desired occupation, which might decrease the quality of occupational matches and in turn affect later-life outcomes. At the same time, the quality within popular occupations should rise, since only the best matches prevail.[3] Regarding competition for jobs on the labor market, the size of a cohort may affect outcomes because an increase in labor supply

[3] Fertig et al. (2009), basing their study on SOEP data and actual population numbers, find that an increase in the relative cohort size of 1 percentage point is associated with a reduction in the probability of receiving a high schooling degree of at least 1.5 percentage points. In the present study, the sample utilized in the data description (Section 2.4) exhibits a correlation between the number of live births in the birth year and occupational prestige (magnitude-prestige-scale), which is negative and significant at 1%.

Figure 2.1: Labor Force Shares by Age over Time



Population by age as share of working-age population (18-65 year-olds) 1990, 2000 and 2010. Own calculations based on population numbers from the Federal Statistical Office.

will either lower equilibrium wages, increase unemployment, or both.

Both the direct competition effects and the indirect mechanisms via education and occupational sorting establish a theoretical impact of cohort size-related competition on economic outcomes and are therefore arguably part of the impact this study aims to measure. Besides competition, networking may play a role because a large cohort size may foster the formation and size of social networks. Networks may affect economic outcomes e.g. by enhancing information sharing during education or because jobs are found through friends rather than by undergoing regular application procedures. The literature suggests mixed network effects on economic success (e.g., Beaman, 2012; Anderberg and Andersson, 2007). Social networks may therefore enforce or counteract the negative cohort size effect on economic success induced by competition.

As for wages and employment, the expected cohort size effect on work time is ambiguous. High competition levels may incentivize workers to invest more effort by working more hours. In addition, it strengthens employers' bargaining positions, which may lead to an increase in working hours. On the other hand, in a situation with high labor supply employers may prefer to allocate a fix amount of work among more employees, possibly because a reduction in total work time per worker increases individual productivity per hour or a more diverse pool of skills is created and the potential for innovative ideas is enhanced. Furthermore, in large cohorts more workers may get actively involved in unions and hereby strengthen the bargaining power of unions which indicates a negative effect on work time.

The strength of the measured cohort size effect will not only depend on the competition-sensitivity of individual outcomes and variations in the propensity to formate networks but also on the potential of the utilized cohort size measure to reflect the number of relevant competitors. Age cohort sizes reflect the competitors well when workers are easily substitutable within but not between age cohorts. This would be the case in a situation where workers pass through different stages throughout their working careers (labor market entry, promotions) at which they compete for jobs mainly with individuals of similar experience levels (Welch, 1979). Employers on the German labor market tend to rely relatively strongly on signals in terms of vocational and diploma degrees which underlines the importance of career phases in the context of the present study (Bol and van de Werfhorst, 2011; Spence, 1973). In addition, changing job content within occupations over time may reduce the substitutability between age cohorts even further.[4]

Probably most occupations are characterized by career phases and changing job content (Biemann et al., 2012), which indicates that the size of a cohort

[4]Spitz-Oener (2006) shows that the complexity of skills within occupations rose since 1979 and that the changes in skill requirements have been most pronounced in rapidly computerizing occupations.

indeed reflects the number of relevant competitors. However, these factors are presumably of lesser relevance if the ability to execute job tasks is rather independent of age. This might be the case for a range of low-skilled occupations that can be carried out without experience-based knowledge.[5] Also, cohort size does not reflect competition levels well when workers are not even replaceable by individuals of similar age because their job duties require deep, position-specific knowledge. Autor and Dorn (2013) argue that the creative, problem-solving, and coordination tasks performed by highly educated workers such as professionals and managers can not as easily be automated as the routine, codifiable job tasks executed by many lower educated workers. In the most extreme, less specialized workers compete with individuals from all age groups, while highly specialized workers compete with nobody. If it is true that cohort sizes reflect the number of relevant competitors best for medium degrees of occupational specialization, the measured cohort size effect is likely to be strongest for this group.

The degree of substitutability between workers also depends on market flexibility. The German labor market is characterized by a rather restrictive employment protection legislation[6] as well as a system of unionized wage and work time bargaining.[7] Dismissal protection, on the one hand, lowers the probability of employment responses and suggests wages and work times to react instead. Collective bargaining agreements, on the other hand, cause rigidities in wages and work times promoting employment responses. Dismissal

[5] Low-skilled occupations often exhibit physically demanding job duties which indicates a negative relationship between age and the ability to execute job tasks because work-related health impairments likely accumulate over time and human physical capacity declines naturally with increasing age. Chapter 4 of this thesis discusses occupational differences in the labor market behavior of older workers in detail and concludes that among 55–65 year-olds non-manual workers have a 20% lower risk of exiting the labor market than manual workers. Although not examined in this thesis, occupational differences in employment probabilities arising from a limited ability to execute physically demanding tasks should be less prevalent among younger age groups. Therefore, since the present analysis focuses on individuals aged 26–55, the argument that high physical job demands reduce the substitutability of workers across age cohorts might be negligible.

[6] According to a comparison of 34 OECD countries regarding the OECD Indicators of Employment Protection, which measure the protection of permanent workers against dismissal, Germany is ranked 4th in terms of individual dismissal and shares rank 5 with Hungary and Switzerland in terms of collective dismissal. After summarizing the two indicators, Germany ranks 1st (OECD, 2013).

[7] The share of unionized employees amounted to 30% in 1985, to 27% in 1993 and declined to 20% in 2003 (Fitzenberger et al., 2011).

protection covers all dependent employees in establishments with at least a certain number of full-time equivalent employees equally.[8] In contrast, collective labor agreements are most likely more effective for less specialized workers, firstly, because they are more often organized in unions, and secondly, because universally binding collective agreements cover mostly physically demanding occupations.[9] Fitzenberger and Kohn (2005) show that strong unions are associated with a lower wage level as well as a compression of the lower part of the wage distribution. According to Franz and Pfeiffer (2006), German firms regard labor union contracts and implicit contracts as important reasons for the wage rigidity of the less skilled, while considering specific human capital and negative signals for new hires as responsible for wage stickiness of the highly skilled.

As soon as having chosen an occupation and entered the labor market, it is plausible that individuals compete for jobs mostly with individuals in the same occupational track, which indicates that the number of relevant competitors is not given by the overall size of an age cohort but rather by the number of individuals of similar age in the same occupational track, i.e. the occupation-cohort size. All above-mentioned arguments should therefore be thought of to apply within occupational tracks.

In summary, cohort size effects are expected to be strongest for medium specialization degrees because the size of the own age cohort should reflect the number of relevant competitors of this group best. Medium and highly specialized occupations can be expected to respond in terms of wages and work times rather than employment due to dismissal protection, while employment effects may be rerouted provoking accordingly higher responses in the other

[8] The exemption threshold from dismissal protection was lifted from five to ten full-time equivalent employees in 1996, which was returned to five employees in 1999. Bauer et al. (2007) do not find effects of these law changes on employment dynamics. In 2004, the threshold was lifted again to its current value of ten employees.

[9] The Federal Ministry of Labour and Social Affairs (2013) declared 506 collective agreements as universally binding (July 1, 2013), i.e. applicable even if neither the employee nor the employer belongs to the bargaining parties. Less than 1.5% of these agreements cover occupations in science and journalism. The remaining agreements refer to occupations in agriculture and forestry, cement and lime/ceramics, mining, metal and electrical trades, wood craft, leather and footwear, textile and clothing, food and beverage, construction, trade, transport, hotels and restaurants, cleansing and body care, and other public and private services (care sector, security services).

outcomes. In comparison, less specialized workers tend to have more rigid wages and work times due to collective agreements, which establishes a higher pressure on employment to respond. In such a situation, dismissal protection might be circumvented by employment adaptations via hiring or non-hiring at initial labor market entries in the first place. Finally, competition intensity should be better reflected by occupation-cohort sizes rather than by overall cohort sizes.

2.3 Estimation Strategy and Identification

To study the response of individual economic success to changes in the cohort size, individual economic outcomes are assumed to be a function of the population of similar age in the same occupational track, which is used as an approximation of an individual's number of competitors for job positions. Accordingly, the regression equation is formulated as follows:

$$Y = \beta_0 + \beta_1 \mathrm{OCS} + \mathbf{X}'\boldsymbol{\beta_2} + \varepsilon \,, \qquad (1)$$

where Y denotes the economic outcome, i.e. wage, employment status, or working hours, respectively. $\mathbf{X}$ represents a vector of covariates, such as education and labor market experience, which are likely to affect economic outcomes. ε is an error term for which $E(\varepsilon) = 0$ is assumed. OCS denotes the population number in an individual's occupation-age cell, the occupation-cohort size, which proxies the number of relevant competitors. Its coefficient β_1 reflects the effect of competition in the labor market on economic outcomes. This is the parameter of interest which this study aims to identify.

Equation (1) gives reason for an endogeneity concern. If workers migrate to locations with comparably favorable expected outcomes, economic outcomes influence the occupation-cohort size at different locations rather than the other way around. The mechanism at work is one of demand and supply: If, on the one hand, labor supply rises due to an increase in the occupation-cohort size, this depresses economic outcomes (which can be thought of as the price for labor) and raises labor demand. If, on the other hand, demand for labor rises (for whatever reasons), expected labor outcomes become more favorable, and in turn attract economic migration which increases the occupation-cohort size (reverse causality). Hence, OCS and Y are simultaneously determined by the movement of labor supply and demand into an equilibrium.

If internal migration is sizeable, this simultaneity is potentially confounding an OLS estimate of β_1. According to data from the Federal Statistical Office (2004, 2014a), a yearly average of 1.1 million moves across Germany's internal borders was observed between 1991 and 2012 with a spike of close to 1.2 million moves in 2001. These numbers are equivalent to a situation with about 1.4% of all German residents moving across federal state borders once a year, a number which may matter for the validity of an OLS estimate of β_1. Figure 2.A1 illustrates the total number of moves by year, while Figure 2.A2 reveals considerable state and time variation in the net influx rates. Apparently, some federal states are more attractive destinations than others and attractiveness, at least of some states, changes over time. These migration patterns are possibly determined by regional and temporal variation in expected economic outcomes.

To address the potential bias caused by internal migration empirically, Equation (1) is estimated employing 2SLS regressions.[10] Overall cohort size and educational capacities should be strong predictors of the number of individuals of similar age in the same occupation.[11] I instrument OCS with proxies of both measures, namely, the number live births in the birth year as well as the nationwide share of workers in the own occupation at age 15. The original number of children born in the birth year should be strongly correlated with overall age cohort size throughout the life cycle. Current overall occupation shares reflect past educational capacities, which I assume to be correlated with current educational capacities.[12] Both of these measures capture competition

[10] In the empirical analysis, regressions are estimated applying the Stata command `ivreg2` (Baum et al., 2010).

[11] Section 2.5.1 below presents correlations of indicators for overall cohort size and educational capacities with the realized degree of occupational specialization. The results suggest that educational capacities are more important for occupational sorting than overall cohort size.

[12] Besides, individual occupational choice is influenced by many further factors including personal talents, preferences regarding e.g. risk (Grazier and Sloane, 2008) and intended effort (Demiralp, 2011), individual knowledge of occupational task content (Bonin et al., 2007; Saniter and Siedler, 2014), social networks and peer groups (Bentolila et al., 2010; Drost, 2002) as well as parental occupation and wealth (Nicolaou and Shane, 2010; Mookherjee and Ray, 2010).

intensity in the occupational sorting process: Given fixed educational capacities, a larger cohort size intensifies competition. Given fixed cohort sizes, tightening educational capacities intensifies competition as well. I argue that both these variables are exogenous to individuals in the occupational sorting process but relevant to the final realization of occupation-cohort sizes. I assume that their influence via OCS is their only link to later-life economic outcomes. Both variables are determined long before later-life individual labor market outcomes are realized, eliminating the reverse causality concern.

Finally, the age-period-cohort identification problem needs to be addressed. In order to identify the effect of occupation-cohort size on economic outcomes β_1, age and time effects have to be controlled for to eliminate e.g. age-specific productivity differences and business cycle effects. Methods to effectively isolate cohort effects from age and period effects are a frequently debated topic in cohort analyses (e.g. Fukuda, 2006; Luo, 2013), as the three variables are connected in a perfectly multicollinear relationship (birth year + age = period). The strategy chosen here to address this issue is to avoid an overlap of the intervals of age, period, and cohort (following Kaushal and Kaestner, 2013). I define age fixed effects to comprise three consecutive age cohorts (26-28, 29-31, ... , 50-52, 53-55) and year fixed effects to vary by year. The measure for cohort size OCS is constructed to additionally vary by occupation and federal state.

As the effect of cohort size likely depends on the substitutability of workers within and between age cohorts (see Section 2.2), I estimate Equation (1) separately for three degrees of professional specialization. Furthermore, three different specifications are defined. In the baseline specification, vector $\mathbf{X}$ solely comprises age, year, occupation and region fixed effects. Occupation (region) fixed effects are included to isolate all constant occupation-specific (region-specific) differences in the outcome variables. Additional specifications are defined by gradually adding further control variables.

2.4 Data and Descriptive Analysis

2.4.1 Data Sources and Sample Restrictions

The empirical analysis of this paper uses data from the German Socio-Economic Panel (SOEP). The SOEP is a representative longitudinal study, which started in 1984. It annually collects household information from about 12,000 households since 2000 as well as individual data from all household members above age 15.[13]

The present analysis uses a range of socioeconomic characteristics provided by the SOEP. The utilized sample is restricted to individuals in prime labor force age, i.e. ages 26 to 55, to exclude age cohorts with large shares in education or already retired. The data is further restricted to West German residents observed between 1990 and 2012 to eliminate potential distortions due to different labor market situations in the Eastern states or before the reunification, respectively.[14] These restrictions leave the birth cohorts 1935 to 1986 for empirical investigation. Furthermore, civil servants, soldiers and self-employed persons are excluded as their wage and employment determinations should function differently from those of the salaried workforce in the private sector. In addition, individuals in vocational training, marginal irregular part-time employment or in a sheltered workshop are removed from the sample. Finally, observations with missing values on relevant variables are excluded.

Population and birth numbers by birth cohort and federal state as well as population numbers by occupation are provided by the German Federal Statistical Office. As the latter information is available for a limited number of years,[15] only data for the birth cohorts 1955 to 1976 are exploited in the regression analysis.

Taken together, three samples are being used in the empirical analysis. The descriptive statistics presented in the present section are based on 43131

[13]For documentations of the SOEP database, see Kroh (2011) or Haisken-DeNew and Frick (2005).

[14]Due to east-west heterogeneity, also an exploitation of the German reunification as an exogenous shock to occupation-cohort size might be unsuitable.

[15]In particular, population numbers by occupation are available only for the years 1970, 1973, 1976, 1978, 1980, 1982, 1984, 1985, 1989 and 1991. Numbers for the years in between are imputed based on the available numbers. The data are at KldB 2 digits level. KldB ("Klassifizierung der Berufe") denotes the German classification of occupations of the Federal Employment Agency.

records from 9266 individuals (Sample 1). The employment analysis uses 28985 records from 5803 individuals (Sample 2). The analysis of occupational sorting is also based on Sample 2 but uses time-constant variables only. The wage and work time analysis, which is restricted to employees, includes 28277 records from 5272 individuals (Sample 3).

2.4.2 Variable Definitions

2.4.2.1 Economic Outcomes

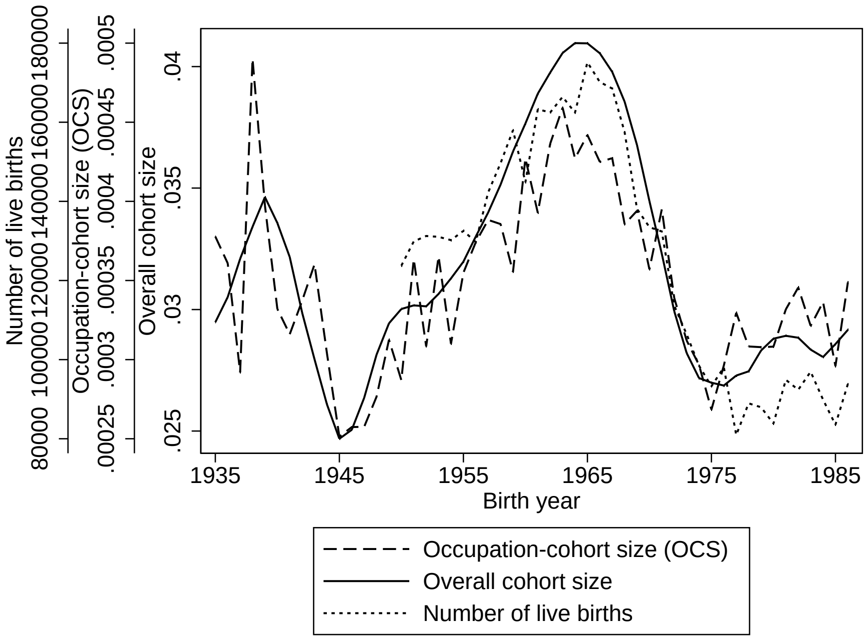
The SOEP questionnaire asks individuals for their current employment status and, in case they are employed, for their exact gross amount of labor income, their contractual working hours excluding overtime, as well as their actual working hours including overtime. From this information, the following four outcomes are generated and used as dependent variables in the regression analysis: (a) the natural logarithm of real hourly gross wages, (b) a binary variable indicating employment status, (c) the number of contractual working hours per week, and (d) the number of actual working hours per week.

2.4.2.2 Occupation-Cohort Size

As job competition in the labor market should take place mainly among individuals of similar age in the same occupational track, I attempt to measure competition intensity by the population numbers within occupation-age cells, i.e. the occupation-cohort size. Because these population numbers appear to be unavailable, I estimate them by weighting actual overall cohort sizes with occupation shares estimated from my sample using weights provided by the SOEP. The SOEP reports the detailed current occupation and the first occupation ever worked in according to the International Standard Classification of Occupations (ISCO-88; 4 digits level).

The relevant competitors of an individual are unlikely to be sharply concentrated within a single age cohort. Rather, they are probably spread to some extent over the surrounding cohorts (Welch, 1979). In addition, the most relevant competitors often reside geographically close. In order to account for all these aspects, occupation-cohort size is calculated as a weighted moving average of the sizes of the own and the surrounding age cohorts where cohort sizes are given by age-specific population numbers at federal state level. Finally, to

Figure 2.2: Cohort Size Measures by Birth Cohort



also account for changes in the size of the labor force over time, the measure is normalized by the number of individuals in core workforce age (26–55).

To formalize, occupation-cohort size (OCS) is calculated in each year (1990–2012) for each age cohort j as a weighted moving average of population numbers N by federal state s, multiplied by the estimated national share of workers ws in occupation o, and divided by the total size of the labor force at federal state level N_s:

$$\text{OCS}_{jso} = \frac{\left(\frac{1}{9}N_{(j-2)s} + \frac{2}{9}N_{(j-1)s} + \frac{3}{9}N_{js} + \frac{2}{9}N_{(j+1)s} + \frac{1}{9}N_{(j+2)s}\right) \times ws_o}{N_s} \quad (2)$$

Figure 2.2 illustrates occupation-cohort size (OCS), overall cohort size,[16] and the number of live births by birth year. All three measures rise during the baby boom of the 1950s and 1960s and decrease in the subsequent baby bust of the 1970s. Due to the sample restrictions, OCS is more noisy for relatively old and relatively young birth cohorts, because observation numbers drop at the tails of the distribution over birth years. Moreover, the representativeness

[16]Overall cohort size is calculated as OCS with ws_o equal to 1.

of OCS and overall cohort size for the original cohort size at birth deteriorates with decreasing birth year because the share of cohort members who already died increases.

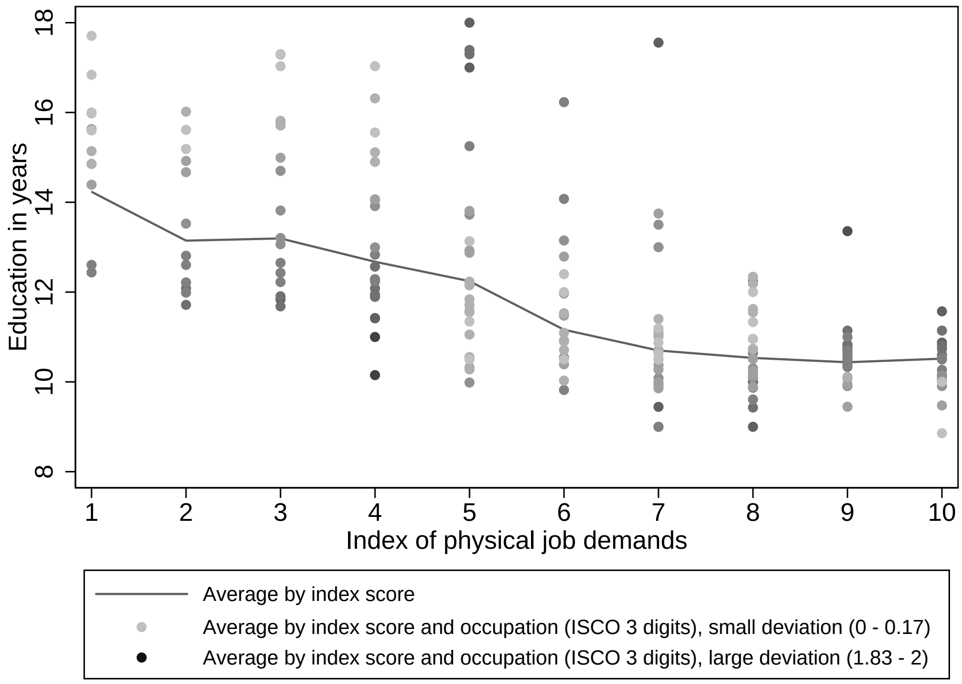
The values taken by OCS are rather small in size. For the sake of an easy interpretation of a one unit change in the regression analysis, I rescale OCS by dividing it by factor 0.00015, which roughly equals the average difference in OCS between birth cohorts born at the peak of the baby boom (around 1964) and cohorts born in the subsequent bust (around 1975), to obtain a rescaled occupation-cohort size measure (OCS*). As a consequence, a one unit change in OCS* refers to the average difference between baby boomers and baby busters.

2.4.2.3 Occupational Specialization

Economic theory implies that the effect of cohort size varies by degree of professional specialization (see Section 2.2). In order to account for this heterogeneity, previous studies divided their samples based on educational attainment, assuming occupational specialization to be increasing in education (e.g., Freeman, 1979; Brunello, 2010). Although education should be positively correlated with the degree of specialization, it is probably not a perfect measure of substitutability. For example, an emergency physician exhibits a high level of education but may be easily replaceable by any other physician of the same expertise since her daily job content consists of a series of practical routine tasks. In contrast, a less educated clerk who executes planning and organizing works may be indispensable in her position if she strongly relies on experience and position-specific knowledge in her daily work.

A more accurate measure of specialization would reflect the actual content of occupational duties. Rather than education, I utilize a 1-10 ordinal scale for physical job demands developed by Kroll (2011) to categorize my sample. The index was constructed based on a large-scale representative survey for Germany collected in 2006 which focused on workplace characteristics (i.e., job requirements, main tasks, working conditions and job demands). I hypothesize the relevance of experience and position-specific knowledge for the execution of everyday job tasks to be decreasing in physical job demands. To the extent physical job demands are superior in reflecting workers' substitutability to a

Figure 2.3: Education by Degree of Occupational Specialization



measure of educational attainment, the present study categorizes specialization groups more precisely than former studies did.

Figure 2.3 illustrates the relationship between the number of years in education and the index of physical job demands, based on Sample I. While the overall relationship may be negative, there is high variation at the occupational level.

To illustrate the extent to which a categorization based on the index of physical job demands differs from an assignment based on education, I define groups of occupational specialization using each of the two measures and compare the deviations in group assignment. In particular, using the index, I divide the sample into low (index values 8-10), medium (index values 4-7), and high specialization (index values 1-3). Using education, I define individuals with 7-10 years of education as less specialized, 10.5-13 years as medium specialized and 13.5-18 years as highly specialized. Only 47.1% of all observations in Sample 1 are assigned to the same specialization group by both measures. 3.3% of the observations even exhibit a deviation of two, i.e. they are assigned the lowest specialization degree by one measure and the highest degree by the other measure.[17] The deviations are also illustrated in Figure 2.3, where a darker dot color indicates a larger average deviation in group assignment.

2.4.3 Correlations

Table 2.1 reports correlations of occupation-cohort size as defined by Equation (2) with the economic outcome variables. As discussed in Section 2.2, both positive and negative relationships are plausible. An increase in occupation-cohort size intensifies competition for jobs, which may depress wages and employment probabilities. On the other hand, a larger occupation-cohort may be associated with higher social interaction which may foster knowledge sharing and networking with potentially positive wage and employment effects. According to Table 2.1, a negative correlation with wage and employment is found for medium and highly specialized males. In contrast, the coefficients for less specialized males are positive. For females, a negative relationship is measured only for wages of medium and highly specialized workers, while employment status is positively correlated with occupation-cohort size for less and medium specialized females. The remaining correlations with females' wages and employment status are insignificant.

Considering working hours, intensified competition may incentivize workers to invest more effort by working more hours, inducing a positive relationship with occupation-cohort size. On the other hand, the distribution of a fixed amount of work among a larger number of workers may be associated with potential productivity gains due to reduced individual work loads or enhanced innovation potential due to a more diverse pool of workers, which would suggest a negative relationship with occupation-cohort size. According to Table 2.1, all correlations are negative for females. For males, the picture is more mixed. While the coefficients are mostly negative for low and high specialization de-

[17] Examples with a deviation of two for low education and low physical job demands (ISCO-88 classification in parantheses): directors and chief executives (121), architects, engineers and related professionals (214), physical and enginieering science technicians (311), finance and sales associate professionals (341), secretaries and keyboard-operating clerks (411), client information clerks (422). Examples for high education and high physical job demands: travel attendants and related workers (511), building finishers and related trades workers (713), blacksmiths, tool-makers and related trades workers (722), machinery mechanics and fitters (723), wood treaters, cabinet-makers and related trades workers (742).

Table 2.1: Correlations with Occupation-Cohort Size

	Degree of Occupational Specialization		
	Low	Medium	High
Males:			
Real hourly gross wage	0.066***	-0.181***	-0.096***
Employed	0.030***	-0.038***	-0.014
Contractual work time	-0.107***	0.015	-0.009
Actual work time	-0.022**	0.053***	-0.043***
Females:			
Real hourly gross wage	-0.061	-0.089***	-0.146***
Employed	0.046*	0.023**	-0.003
Contractual work time	-0.398***	-0.166***	-0.137***
Actual work time	-0.403***	-0.164***	-0.154***

Correlation coefficients. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

grees, the correlation between actual work time and occupation-cohort size exhibits a positive sign for medium specialized workers.

2.5 Results

This section presents the estimation results for cohort size effects on occupational sorting, wage, employment and work time. To test the results for robustness, all regressions were also estimated releasing the sample restrictions described in Section 2.4 by including civil servants, soldiers and self-employed into the sample. As this changes the size of the estimated coefficients slightly, but not the quality of conclusions, these results are not reported here.[18]

2.5.1 Occupational Sorting

Table 2.2 shows the effects of the instruments, i.e. the number of live births in an individual's birth year and educational capacities as measured by the share of workers in the own occupation at age 15, on occupational sorting. The dependent variables are categorical variables reflecting the degree of occupational specialization, inferred from the index of physical job demands described

[18]Results are available upon request.

in Section 2.4.2.3 and differing only by categorical depth. The estimated effects of the number of live births indicate that belonging to a larger cohort decreases the probability for an occupation with a higher as compared to a lower specialization level. However, the estimated coefficients and odds ratios are insignificant for both genders. In contrast, educational capacities have significant positive effects in all regressions. In particular, considering the binary regressions, the OLS coefficient for males (females) indicates that a one percentage point increase in the occupation share at age 15 increases the probability for an occupation with a high specialization as compared to a low specialization by 6.2% (4.8%). Focusing on three categories of specialization (ordered logit I), a one percentage point increase in the occupation share raises the odds for males (females) to end up in a higher specialization category than the considered one or a lower category by factor 1.57 (1.41). When subdividing occupations into ten categories of specialization (ordered logit II), the odds ratios for males (females) decrease to 1.22 (1.18) but remain larger than one and highly significant.

Table 2.2: Occupational Sorting

	Males			Females		
	OLS	Ordered logit		OLS	Ordered logit	
		I	II		I	II
Live births in birth year / 10^5	-0.006	0.957	0.961	-0.007	0.964	0.981
	(0.010)	(0.042)	(0.038)	(0.011)	(0.050)	(0.041)
Occupation share at age 15 (%)	0.062***	1.569***	1.219***	0.048***	1.413***	1.183***
	(0.003)	(0.045)	(0.014)	(0.002)	(0.025)	(0.009)
Observations	3116	3116	3116	2687	2687	2687

Dependent variables (DV) measure degree of occupational specialization. OLS: DV has 2 categories; coefficients reported. Ordered logit I: DV has 3 categories; odds ratios reported. Ordered logit II: DV has 10 categories; odds ratios reported. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Assuming that a higher degree of occupational specialization is associated with higher economic success, the findings seem to indicate that restricted educational capacities force individuals into less prestigious occupations. Although the effects of the overall size of a cohort exhibit the expected signs, they are not significant. Instead, educational capacities seem to be the more important predictor of occupational choice.

2.5.2 Wage

Table 2.3 presents the estimates of occupation-cohort size effects on wages. In all regressions, the Kleibergen-Paap F statistic exceeds the Staiger and Stock (1997) rule-of-thumb threshold of 10, indicating that the models are identified (Baum et al., 2007; Dickson, 2013). Because two instruments are exploited in the regressions, overidentification tests are reported. The p-values of the Hansen J statistics exceed 0.05 in all regressions, indicating that the null hypothesis that the instruments are valid cannot be rejected. The test results do therefore not raise overidentification concerns.

As Table 2.3 shows, the coefficients of occupation-cohort size (OCS^*) are negative and significant for medium and high specialization degrees, indicating that a larger number of competitors on the job market depresses wages. In particular, considering the specification with additional covariates, males in medium (highly) specialized occupations who were born at the peak of the baby boom are estimated to earn 5.8% (5.3%) less than their counterparts born during the subsequent baby bust. The coefficients are insignificant for males in less specialized occupations.

The picture is similar for females, although the negative effect for medium specialized women is significant at the 10% level only. In particular, medium (highly) specialized women born at the peak of the baby boom are estimated to earn 6.2% (2.5%) lower wages than comparable women born during the baby bust.

These findings are largely in line with the theoretical expectations developed in Section 2.2. Less specialized workers are unlikely to respond in terms of wages, first, because they are probably more easily replaceable by workers of different age groups, and second, because their wages are more often subject to unionized wage-bargaining which causes rigidities in wages.[19] In contrast, considering medium specialized workers, the size of the own age cohort might reflect the number of relevant competitors well, because according

[19]Figure 2.A3 in the Appendix shows union membership shares by degree of specialization based on the SOEP data used here.

Table 2.3: Occupation-Cohort Size Effects on Wage

	Males								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Occupation-cohort size (OCS*)	0.020	-0.064***	-0.085***	0.012	-0.058***	-0.056***	0.009	-0.058***	-0.053***
	(0.013)	(0.020)	(0.015)	(0.012)	(0.019)	(0.013)	(0.013)	(0.019)	(0.013)
Education				0.021***	0.049***	0.052***	0.019***	0.046***	0.050***
				(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.007)
Labor market experience				0.006	0.027***	0.030***	0.005	0.023***	0.027***
				(0.010)	(0.006)	(0.010)	(0.010)	(0.006)	(0.010)
Labor market experience$^2/10^2$				0.000	-0.000*	-0.001**	0.000	-0.000	-0.000**
				(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Additional covariates							✓	✓	✓
Observations	6574	5193	6199	6574	5193	6199	6574	5193	6199
Kleibergen-Paap F statistic	25.98	30.50	17.20	26.62	32.00	16.42	27.07	32.29	16.38
Hansen J statistic	0.64	0.01	1.51	0.76	0.20	1.26	0.46	0.15	1.17
p-value	0.423	0.943	0.219	0.385	0.655	0.262	0.497	0.698	0.279
	Females								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Occupation-cohort size (OCS*)	0.039	-0.091**	-0.047***	0.038	-0.062*	-0.026***	0.032	-0.062*	-0.025***
	(0.034)	(0.036)	(0.011)	(0.033)	(0.035)	(0.009)	(0.032)	(0.035)	(0.009)
Education				0.017	0.040***	0.052***	0.021*	0.040***	0.052***
				(0.012)	(0.007)	(0.005)	(0.011)	(0.007)	(0.005)
Labor market experience				0.012*	0.012**	0.029***	0.010	0.011**	0.028***
				(0.007)	(0.005)	(0.006)	(0.008)	(0.005)	(0.007)
Labor market experience$^2/10^2$				-0.000	-0.000	-0.000**	-0.000	-0.000	-0.000**
				(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Additional covariates							✓	✓	✓
Observations	1086	3886	5339	1086	3886	5339	1086	3886	5339
Kleibergen-Paap F statistic	20.62	21.53	18.08	21.01	20.05	18.45	22.83	20.20	18.43
Hansen J statistic	1.22	0.00	0.36	1.15	0.12	0.14	1.21	0.13	0.16
p-value	0.269	0.955	0.551	0.284	0.733	0.708	0.271	0.718	0.687

2SLS. Dependent variable: logarithm of real hourly gross wage (*ln*). Age, year, occupation and state fixed effects are included in all regressions. Instruments: number of live births in birth year, occupation share in own occupation at age 15. Additional covariates: full-time employment, household size, immigration status. Robust standard errors in parentheses are clustered at occupation and birth year level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

to the career-phase model workers are easily substitutable within but not between age cohorts. Finally, for highly specialized occupations, the potential of the size of a cohort to reflect the number of relevant competitors may be lower, because these workers might not even be substitutable with other workers of similar age. This argumentation provides a plausible explanation for the finding of smaller cohort size effects for highly than for medium specialized workers (although the difference is small for males).

The corresponding first stage results are reported in Table 2.A2 in the Appendix. As expected, the number of live births in the birth year is positively associated with occupation-cohort size in all regressions and, except for medium specialized males and less specialized females, its effects are also significant. The effects of the occupation share at age 15 are even positive and significant in all regressions.

Reduced form results are given in Table 2.A5. Surprisingly, the effects of the number of live births in the birth year do not exhibit the expected negative sign but are insignificant instead. In contrast, the occupation share at age 15 has a negative effect for medium and high degrees of occupational specialization. In particular, a one percentage point increase in the occupation share at age 15 decreases the wage level by 3.1% (1.3%). Following the argumentation throughout this study, the mechanism behind these effects may be that a larger occupation share reflects larger educational capacities which in turn increase the occupation-cohort size. A larger occupation-cohort size is associated with intensified competition for jobs on the labor market and a depression of the wage level.

2.5.3 Employment

Table 2.4 presents the estimation results from the employment regressions. The findings are to be interpreted with caution because, as a consequence of restricting the sample to individuals in core labor force age, the shares of unemployed individuals are quite low in all subsamples.

As for the wage regressions, the Kleibergen-Paap F statistic exceeds a value of ten in all regressions. With the exception of medium specialized males, none of the overidentification tests is rejected. Hence, there are nearly no concerns

Table 2.4: Occupation-Cohort Size Effects on Employment

	Males								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Occupation-cohort size (OCS*)	0.000	0.003	-0.002**	-0.002	-0.000	-0.002**	-0.001	0.000	-0.003**
	(0.004)	(0.004)	(0.001)	(0.004)	(0.003)	(0.001)	(0.004)	(0.003)	(0.001)
Education				0.005**	0.005***	0.001**	0.006***	0.005***	0.001**
				(0.002)	(0.002)	(0.000)	(0.002)	(0.002)	(0.000)
Labor market experience				0.006	0.008***	0.000	0.007	0.008***	0.000
				(0.004)	(0.002)	(0.001)	(0.004)	(0.002)	(0.001)
Labor market experience$^2/10^2$				0.000	-0.000	0.000	0.000	-0.000	0.000
				(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Additional covariates							✓	✓	✓
Observations	6695	5260	6222	6695	5260	6222	6695	5260	6222
Kleibergen-Paap F statistic	26.35	31.14	17.26	26.98	32.63	16.49	27.49	32.74	16.54
Hansen J statistic	0.90	1.98	0.01	0.78	4.86	0.04	0.66	4.81	0.15
p-value	0.342	0.159	0.907	0.376	0.027	0.850	0.417	0.028	0.702
	Females								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Occupation-cohort size (OCS*)	0.008	0.001	-0.003	0.005	0.005	-0.002	0.004	0.004	-0.002
	(0.023)	(0.009)	(0.002)	(0.023)	(0.010)	(0.002)	(0.024)	(0.010)	(0.002)
Education				-0.002	0.005*	0.002*	-0.001	0.004	0.001
				(0.006)	(0.003)	(0.001)	(0.006)	(0.003)	(0.001)
Labor market experience				0.009***	0.002	0.003*	0.009***	0.001	0.001
				(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Labor market experience$^2/10^2$				-0.000*	0.000	0.000	-0.000*	0.000	0.000
				(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Additional covariates							✓	✓	✓
Observations	1143	4107	5558	1143	4107	5558	1143	4107	5558
Kleibergen-Paap F statistic	20.81	22.05	18.07	21.12	20.54	18.59	22.24	20.56	18.62
Hansen J statistic	0.20	0.26	0.06	0.23	0.69	0.60	0.21	0.83	1.03
p-value	0.653	0.613	0.802	0.633	0.408	0.440	0.648	0.362	0.309

2SLS. Dependent variable: binary indicator for employment status. Age, year, occupation and state fixed effects are included in all regressions. Instruments: number of live births in birth year, occupation share in own occupation at age 15. Additional covariates: household size, immigration status. Robust standard errors in parentheses are clustered at occupation and birth year level. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

of model overidentification. The second stage results for medium specialized males will not be interpreted in the following.

The estimation results presented in Table 2.4 suggest a significantly negative effect of occupation-cohort size on employment for highly specialized males. Specifically, males in occupations with high degrees of occupational specialization who were born at the peak of the baby boom are predicted to have a 0.3 percentage points lower employment probability than comparable males born during the subsequent baby bust. While the coefficient for less specialized males exhibits a negative sign too, the effect is not significant. Considering females, there is no evidence for significant employment effects at all.

These findings are partly in line with the expectations inferred in Section 2.2. Employment responses are unlikely in general due to Germany's restrictive dismissal protection legislation, which comprehensively applies to workers from all specialization groups equally. However, employment may adapt to variations in occupation-cohort size via hiring or non-hiring at initial labor market entry in the first place. As low-skilled workers are often additionally covered by universally binding collective agreements which prevent their wages and work times from responding, the pressure on employment to respond should be comparably higher for this group. Nevertheless, highly specialized males are the only group which is estimated to respond.

The first stage results reported in Table 2.A3 in the Appendix are very similar to the first stage results for the wage regressions presented in the previous section. Again, the number of live births in the birth year has a positive effect on occupation-cohort size in most of the regressions, while educational capacities have a positive impact in all regressions.

Table 2.A6 presents the reduced form regressions for employment. The estimated results reveal that the negative effect on employment measured in the second stage regression for highly specialized males is driven by the occupation share at age 15. Specifically, a one percentage point increase in the occupation share at age 15 decreases the employment probability for males in high specialization occupations by 0.1 percentage points. Additionally, the reduced form regressions suggest that the live births in the birth year have a positive employment effect for medium specialized males. In particular, increasing the

number of live births by 100,000 increases the probability for this group to be in employment by 0.7 percentage points. This may support the networking hypothesis described in Section 2.2 which suggests that a larger cohort may be associated with higher social interaction which may in turn have positive effects on education as well as on the probability to be employed. The remaining effects of the instruments on employment are insignificant.

2.5.4 Work Time

Tables 2.5 and 2.6 present the results from regressions of individual work time, whereby Table 2.5 refers to weekly work time agreed upon with employers and Table 2.6 refers to actual work time per week. As for wages and employment, the Kleibergen-Paap F statistics pass the rule-of-thumb threshold of ten for all reported regressions. With the exception of medium specialized females, the overidentification test is never rejected. In the regressions for medium specialized females, the p-values of the Hansen J statistics are slightly below the threshold of 0.05, leading to a rejection of the null hypothesis that the instruments are valid and inducing overidentification concerns. These regressions will not be interpreted in the following.

According to Table 2.5, males in medium specialized occupations exhibit higher contractual work times when they belong to a large occupation-cohort, possibly indicating that intensified competition either incentivizes them to invest more effort by working longer hours or demotivates employers to offer favorable working conditions in the form of short working hours. Specifically, males in medium specialized occupations born at the peak of the baby boom work 0.8 hours more than comparable males born during the bust. In contrast, the work times of highly specialized males decline as a result of larger occupation-cohort sizes, suggesting that as labor supply increases a fixed amount of work is distributed among a larger number of workers. In particular, when a highly specialized worker was born at fertility peak instead of its low, he works on average 0.2 weekly hours less. The work time effect for less specialized males is not statistically significant. For females, there is a negative effect on contractual working hours for less specialized workers, which is, while large in size, significant at the 10% level only. It suggests that less specialized

Table 2.5: Occupation-Cohort Size Effects on Contractual Work Time

	Males								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Occupation-cohort size (OCS*)	-0.186	0.958***	-0.180**	-0.185	0.831***	-0.192**	-0.186	0.829***	-0.190**
	(0.129)	(0.329)	(0.090)	(0.129)	(0.308)	(0.090)	(0.124)	(0.310)	(0.091)
Education				0.001	0.238**	0.101***	-0.002	0.230**	0.101***
				(0.055)	(0.100)	(0.030)	(0.057)	(0.098)	(0.031)
Labor market experience				0.031	0.708***	0.231***	0.028	0.712***	0.230***
				(0.058)	(0.125)	(0.064)	(0.059)	(0.124)	(0.064)
Labor market experience^2/10^2				-0.001	-0.013***	-0.004**	-0.001	-0.013***	-0.004**
				(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)
Additional covariates							✓	✓	✓
Observations	6574	5193	6199	6574	5193	6199	6574	5193	6199
Kleibergen-Paap F statistic	25.98	30.50	17.20	26.62	32.00	16.42	27.18	32.09	16.45
Hansen J statistic	0.92	0.04	0.01	0.91	0.68	0.00	0.67	0.70	0.01
p-value	0.336	0.847	0.904	0.340	0.411	0.946	0.413	0.403	0.938
	Females								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Occupation-cohort size (OCS*)	-0.532	-1.348	-0.370*	-1.165	-1.059	-0.190	-1.570*	-0.997	-0.152
	(1.040)	(0.863)	(0.225)	(0.837)	(0.833)	(0.168)	(0.861)	(0.815)	(0.153)
Education				-1.057***	0.007	0.462***	-1.147***	-0.078	0.322***
				(0.302)	(0.192)	(0.117)	(0.315)	(0.186)	(0.086)
Labor market experience				0.918***	1.144***	1.150***	0.688***	0.931***	0.875***
				(0.214)	(0.091)	(0.104)	(0.212)	(0.093)	(0.115)
Labor market experience^2/10^2				-0.015**	-0.017***	-0.016***	-0.010	-0.014***	-0.014***
				(0.007)	(0.003)	(0.004)	(0.007)	(0.003)	(0.004)
Additional covariates							✓	✓	✓
Observations	1086	3886	5339	1086	3886	5339	1086	3886	5339
Kleibergen-Paap F statistic	20.62	21.53	18.08	21.01	20.05	18.45	22.58	20.01	18.43
Hansen J statistic	1.29	7.17	3.52	1.41	7.62	0.85	0.84	3.95	0.94
p-value	0.256	0.007	0.061	0.235	0.006	0.357	0.360	0.047	0.333

2SLS. Dependent variable: contractual working hours per week. Age, year, occupation and state fixed effects are included in all regressions. Instruments: number of live births in birth year, occupation share in own occupation at age 15. Additional covariates: household size, immigration status. Robust standard errors in parentheses are clustered at occupation and birth year level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.6: Occupation-Cohort Size Effects on Actual Work Time

	Males								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Occupation-cohort size (OCS*)	-0.018	1.856**	-1.049***	-0.119	1.843**	-0.931***	-0.292	1.842**	-0.935***
	(0.315)	(0.759)	(0.218)	(0.322)	(0.720)	(0.219)	(0.306)	(0.724)	(0.219)
Education				0.306***	0.800***	0.413***	0.208*	0.730***	0.406***
				(0.112)	(0.190)	(0.086)	(0.112)	(0.185)	(0.086)
Labor market experience				0.139*	0.987***	0.380***	0.110	0.996***	0.380***
				(0.074)	(0.166)	(0.121)	(0.082)	(0.167)	(0.122)
Labor market experience$^2/10^2$				-0.005**	-0.017***	-0.004	-0.005**	-0.018***	-0.004
				(0.002)	(0.004)	(0.003)	(0.002)	(0.005)	(0.003)
Additional covariates							✓	✓	✓
Observations	6574	5193	6199	6574	5193	6199	6574	5193	6199
Kleibergen-Paap F statistic	25.98	30.50	17.20	26.62	32.00	16.42	27.18	32.09	16.45
Hansen J statistic	0.03	0.00	0.15	0.02	0.22	0.20	0.08	0.25	0.23
p-value	0.869	0.967	0.702	0.876	0.637	0.656	0.772	0.617	0.628
	Females								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Occupation-cohort size (OCS*)	-0.822	-1.748*	-0.807***	-1.413	-1.310	-0.492**	-1.695*	-1.279	-0.448**
	(1.156)	(0.946)	(0.313)	(0.997)	(0.946)	(0.238)	(1.010)	(0.942)	(0.215)
Education				-0.822**	0.199	0.789***	-0.973***	0.068	0.624***
				(0.344)	(0.214)	(0.147)	(0.354)	(0.209)	(0.115)
Labor market experience				1.099***	1.269***	1.283***	0.817***	1.015***	0.958***
				(0.255)	(0.108)	(0.118)	(0.246)	(0.112)	(0.127)
Labor market experience$^2/10^2$				-0.019**	-0.019***	-0.016***	-0.013*	-0.016***	-0.014***
				(0.008)	(0.004)	(0.004)	(0.008)	(0.004)	(0.004)
Additional covariates							✓	✓	✓
Observations	1086	3886	5339	1086	3886	5339	1086	3886	5339
Kleibergen-Paap F statistic	20.62	21.53	18.08	21.01	20.05	18.45	22.58	20.01	18.43
Hansen J statistic	3.29	7.08	4.22	3.32	7.38	1.83	1.99	4.09	2.04
p-value	0.070	0.008	0.040	0.069	0.007	0.176	0.159	0.043	0.153

2SLS. Dependent variable: actual working hours per week. Age, year, occupation and state fixed effects are included in all regressions. Instruments: number of live births in birth year, occupation share in own occupation at age 15. Additional covariates: household size, immigration status. Robust standard errors in parentheses are clustered at occupation and birth year level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

females born during the baby boom work 1.6 hours per week less compared to females born during the baby bust. The remaining coefficients for females are insignificant.

Considering actual work time as an outcome variable instead of contractual work time (Table 2.6) barely changes the quality of results. However, the coefficients increase considerably in size. Medium specialized males born at the peak of the baby boom are predicted to work 1.8 weekly hours more than comparable males born during the subsequent bust, while highly skilled males in large occupation-cohorts work 0.9 hours less. Also the negative effect for less specialized female workers increases slightly from 1.6 to 1.7. In addition, the coefficient for highly specialized women, which was insignificant for contractual work time, becomes significant at the 10% level. It suggests that highly specialized women born at fertility high work 0.4 weekly hours less than comparable women born at fertility low. The overall finding of stronger effects on actual work time than on contractual work time may reflect that actual work hours are generally more flexibly adaptable since they are not subject to contractual agreements.

The corresponding first stage regression results on effects of the instruments on occupation-cohort size (Appendix Table 2.A4) again barely differ from those for wage and employment discussed above. The reduced form regressions reported in Tables 2.A7 and 2.A8 reveal that the estimated second stage effects on contractual work time are driven by the occupation share in the own occupation at age 15. For example, a one percentage point increase in the occupation share at age 15 increases contractual weekly work time of medium specialized males by 0.4 hours. In contrast, Table 2.A8 shows that the negative second stage effect on actual work time for highly specialized females is driven by the number of live births in the birth year. While the second stage estimates for medium specialized females could not be interpreted due to rejection of the overidentification tests, the reduced form estimates reveal a negative work time effect of overall cohort size for this group. In particular, increasing the number of live births in the birth year by 100,000 reduces the weekly contractual work time of medium specialized females by 1.0 hour. The corresponding effect on actual work time is a decrease of 1.2 weekly hours.

2.6 Conclusion

In this paper, I present new evidence on cohort size effects on wages, employment and work time. The German labor market is considered as a case study because the existing evidence for this country lacks comprehensive insights on labor market effects based on recent data, although the German population ages particularly rapidly in international comparison. In contrast to previous studies, professional specialization levels are measured by an index reflecting actual job content rather than by educational attainment.

The empirical results indicate that belonging to a large cohort depresses economic success. In particular, male and female workers in medium and highly specialized occupations respond with wage reductions. Specifically, all other things equal, a medium (highly) specialized man born at the peak of the baby boom earns on average 5.8% (5.3%) lower wages than a comparable man born during the subsequent baby bust. Highly specialized females from the largest cohorts have 2.5% lower wages than their counterparts from small cohorts. Highly specialized male workers additionally adapt in terms of employment. Employment probabilities of highly specialized males born at peak fertility are estimated to be 0.3 percentage points lower than those of comparable workers belonging to later-born, smaller cohorts. Weekly work time is estimated to rise in response to an increase in cohort size for males with medium occupational specialization, which is reversed for highly specialized males. These findings are confirmed for both contractual and actual work times, while the effects on the latter outcome are stronger. In particular, medium (highly) specialized males born into boom cohorts work 0.8 (0.2) contractual working hours and 1.8 (0.9) actual working hours longer (shorter) than comparable males born into low-fertility cohorts. Highly specialized females work 0.4 weekly hours less when born into large instead of small cohorts.

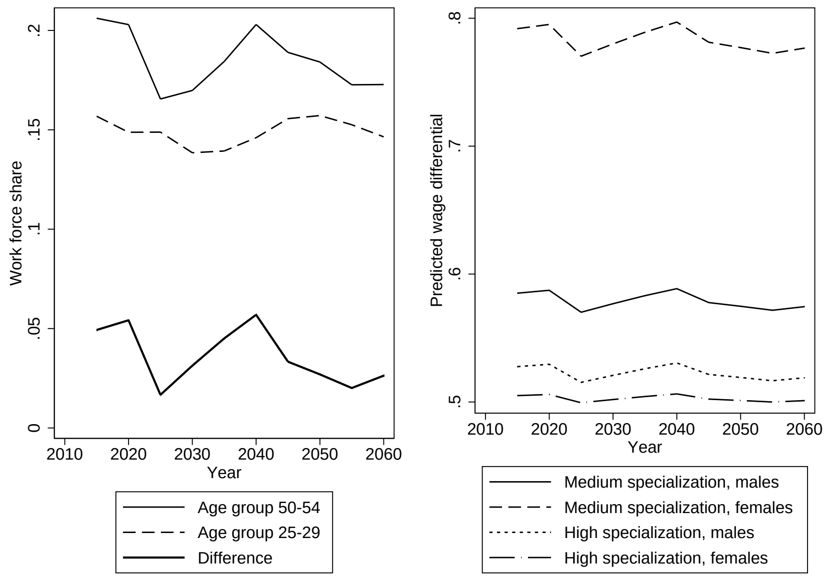
The estimated response pattern in the considered labor market outcomes is well in line with the expected role of Germany's restrictive labor market institutions. Institution-induced rigidities in one outcome might be responsible for a relatively strong response in another outcome due to a rerouting of the effect. In particular, the German dismissal protection legislation may hinder employment to adapt but instead foster wage and work time responses. However, further research is needed to supplement the findings presented in this article and to disclose the exact mechanisms at work.

How does demographic change affect labor market outcomes? A satisfactory answer to this question is clearly beyond the scope of this article as shifts in the age structure of the workforce remain a single mediator out of a range of potential mechanisms through which population aging might influence labor markets. However, the estimated effects happen to be robust with some of them being relatively large in size. Since population aging implies the supply of older workers to rise relatively to the supply of younger workers (at least for the initial period of demographic transition), the results of the present analysis suggest that younger workers benefit in terms of wages and employment probabilities at the cost of older workers.

Figure 2.4 presents projections of future relative cohort sizes and predicted wage differentials for the age groups 25 to 29 and 50 to 54. As the left panel shows, the supply of older workers exceeds the supply of younger workers already in 2015, which points at the shifts in the age structure that have taken place during the past decades. Furthermore, the projected workforce share of older workers remains larger throughout the whole period from 2015 until 2060. This indicates that when small young cohorts today age into small old cohorts tomorrow, tomorrow's young cohorts will be even smaller. Hence, the labor force as a whole will continue to decline since coming generations will be smaller and smaller. As the left panel of Figure 2.4 reveals, the relative supply of older and younger workers will not remain constant over the coming decades. In particular, while the difference in relative cohort size amounts to about 5 percentage points in 2015 and 2020, it drops to about 2 percentage points in 2025. Thereafter, it rises to 5 percentage points in 2040 again before declining to below 3 percentage points in 2060.

The right panel of Figure 2.4 presents predicted wage differentials for the two age groups over time, calculated by entering projected cohort sizes from the Federal Statistical Office (2015), estimation coefficients presented in Table 2.3 and variable group averages from Sample 3 into Equation (1). Medium specialized females are predicted to have the largest wage differential of all considered groups amounting to nearly 0.8, which indicates a very moderate wage growth throughout their working careers. Highly specialized females,

Figure 2.4: Predicted Young/Old Wage Differentials until 2060



Own calculations based on population projections from the c. Left panel: percentages aged 25–29 and 50–54 of population in core workforce age (25–54). Right panel: predicted young/old wage differentials (age groups 25–29 vs. 50–54) by gender and occupational specialization. Wages were predicted by entering projected cohort sizes, estimation coefficients presented in Table 2.3 and variable group averages from Sample 3 into Equation (1).

in contrast, exhibit the lowest differential close to 0.5, suggesting that they experience the strongest wage growth over time among the considered groups.

The comparison of the left and right panels of Figure 2.4 suggests a positive relationship between the difference in cohort size and the wage differentials between the two age groups. For example, the rise in the cohort size difference of about 3 percentage points from 2025 to 2040 is associated with a rise in the wage differential for medium specialized males (females) of about 1.7 (2.5) percentage points. Regarding highly specialized males (females), the increase amounts to 1.4 (0.6) percentage points.

These results suggest that increasing the workforce share of older relative to younger workers benefits younger workers in terms of wages at the cost of older workers. Especially in the past, at the onset of workforce aging when initially balanced workforce shares have shifted to become more and more unbalanced (compare Figure 2.1), younger workers may have profited at the disadvantage of older workers. This might be interpreted as an inequitable redistribution of economic success between generations (Harper, 2014).

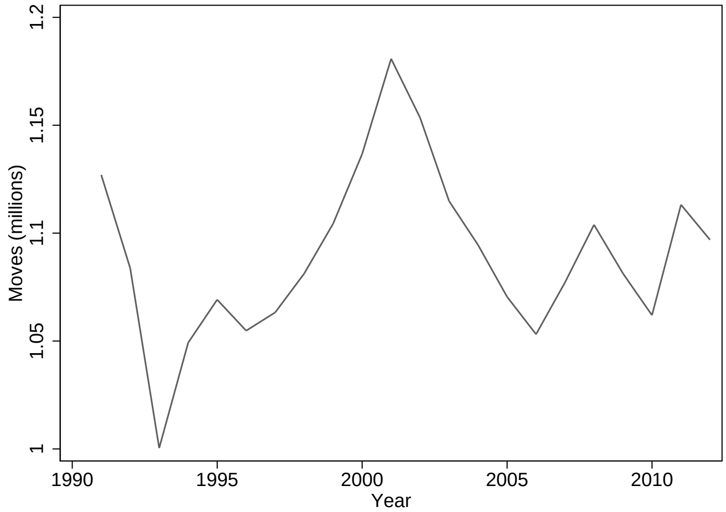
Appendix 2

Table 2.A1: Means and Standard Deviations

	Degree of occupational specialization					
	Low		Medium		High	
Variable	Mean	SD	Mean	SD	Mean	SD
Males, Sample 2:						
Age	37.88	7.04	38.82	6.97	38.44	6.70
Education	10.75	1.39	11.10	1.87	13.97	2.80
Labor market experience	16.44	7.78	16.52	7.96	13.87	7.88
Employed†	0.98	0.15	0.99	0.12	1.00	0.07
German native†	0.72	0.45	0.79	0.41	0.92	0.27
Household size	3.15	1.38	3.05	1.33	2.82	1.34
Observations	6695		5260		6222	
Males, Sample 3:						
Real hourly gross wage	15.02	4.45	15.14	5.78	21.10	7.69
Full-time employed†	0.99	0.08	0.96	0.19	0.98	0.14
Contractual work time	38.42	3.07	38.46	5.26	38.44	3.23
Actual work time	42.16	6.18	42.81	8.35	44.64	6.90
Observations	6574		5193		6199	
Females, Sample 2:						
Age	39.14	7.25	39.02	7.22	37.98	7.06
Education	10.10	1.54	11.35	2.17	12.95	2.58
Labor market experience	10.08	7.69	10.41	7.62	10.70	7.16
Employed†	0.94	0.23	0.94	0.24	0.96	0.21
German native†	0.56	0.50	0.77	0.42	0.91	0.29
Household size	3.12	1.28	2.89	1.26	2.61	1.15
Observations	1143		4107		5558	
Females, Sample3:						
Real hourly gross wage	10.54	5.14	11.96	5.68	15.76	7.05
Full-time employed†	0.54	0.50	0.53	0.50	0.60	0.49
Contractual work time	27.93	12.05	30.15	10.01	31.51	9.39
Actual work time	29.80	13.14	32.67	11.50	34.78	11.34
Observations	1086		3886		5339	

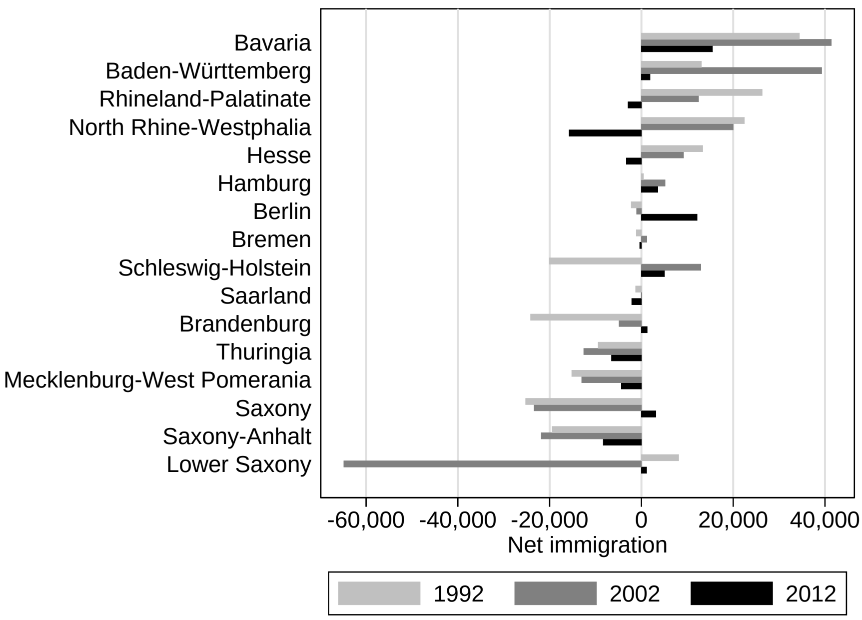
SD: standard deviation. † binary indicator variable. Weights provided by the SOEP are used.

Figure 2.A1: Internal Migration over Time



Moves within Germany across federal state borders. Graph based on data from the Federal Statistical Office (2004, 2014a).

Figure 2.A2: Net Migration Influx by State



Moves within Germany across federal state borders. Graph based on data from the Federal Statistical Office (2004, 2014a).

Figure 2.A3: Union Membership by Degree of Specialization

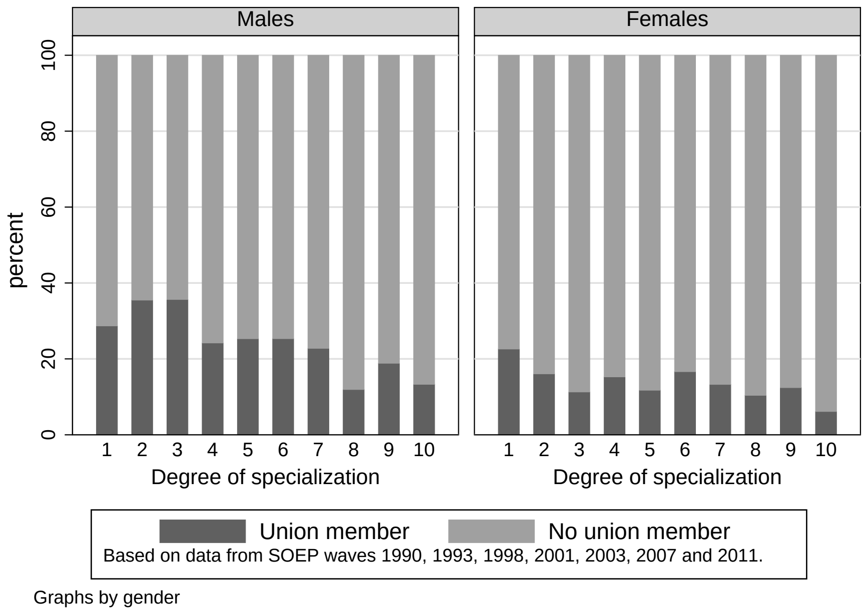


Table 2.A2: First Stage, Wage

	Low	Medium	High	Low	Medium	High	Low	Medium	High
Males									
	Degree of Occupational Specialization								
Live births in birth year / 10^5	0.120***	0.036	0.204***	0.121***	0.026	0.201***	0.123***	0.030	0.199***
	(0.034)	(0.063)	(0.071)	(0.034)	(0.063)	(0.071)	(0.033)	(0.062)	(0.071)
Occupation share at age 15 (%)	0.532***	0.521***	0.237***	0.529***	0.523***	0.233***	0.525***	0.524***	0.231***
	(0.031)	(0.061)	(0.024)	(0.031)	(0.061)	(0.024)	(0.030)	(0.061)	(0.024)
Education				0.015	-0.041**	-0.020	0.011	-0.031	-0.017
				(0.016)	(0.020)	(0.020)	(0.016)	(0.020)	(0.020)
Labor market experience				-0.014	0.024	-0.005	-0.015	0.043**	0.002
				(0.012)	(0.019)	(0.027)	(0.012)	(0.018)	(0.028)
Labor market experience$^2/10^2$				0.000	-0.001**	0.000	0.000	-0.002***	0.000
				(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Additional covariates							✓	✓	✓
Observations	6574	5193	6199	6574	5193	6199	6574	5193	6199
Females									
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Live births in birth year / 10^5	0.036	0.317***	0.374***	0.035	0.278**	0.350***	0.042	0.271**	0.344***
	(0.072)	(0.110)	(0.100)	(0.073)	(0.109)	(0.098)	(0.074)	(0.109)	(0.101)
Occupation share at age 15 (%)	0.765***	0.274***	0.269***	0.756***	0.254***	0.259***	0.755***	0.256***	0.259***
	(0.100)	(0.068)	(0.017)	(0.097)	(0.066)	(0.018)	(0.095)	(0.066)	(0.018)
Education				-0.047	-0.129***	-0.095***	-0.050	-0.126***	-0.085***
				(0.031)	(0.026)	(0.024)	(0.032)	(0.026)	(0.024)
Labor market experience				-0.009	0.016	-0.005	0.002	0.026*	0.013
				(0.019)	(0.013)	(0.026)	(0.019)	(0.015)	(0.026)
Labor market experience$^2/10^2$				0.000	-0.001*	-0.001	-0.000	-0.001*	-0.001
				(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)
Additional covariates							✓	✓	✓
Observations	1086	3886	5339	1086	3886	5339	1086	3886	5339

OLS. Dependent variable: occupation-cohort size (OCS*). Age, year, occupation and state fixed effects are included in all regressions. Additional covariates: full-time employment, household size, immigration status. Robust standard errors in parentheses are clustered at occupation and birth year level. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.A3: First Stage, Employment

	Males								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Live births in birth year / 10^5	0.117***	0.040	0.201***	0.117***	0.032	0.198***	0.120***	0.033	0.198***
	(0.033)	(0.062)	(0.071)	(0.032)	(0.062)	(0.071)	(0.032)	(0.062)	(0.071)
Occupation share at age 15 (%)	0.533***	0.521***	0.238***	0.531***	0.522***	0.233***	0.527***	0.521***	0.232***
	(0.030)	(0.060)	(0.024)	(0.030)	(0.060)	(0.024)	(0.029)	(0.060)	(0.024)
Education				0.012	-0.037*	-0.020	0.008	-0.033	-0.020
				(0.015)	(0.020)	(0.020)	(0.015)	(0.020)	(0.020)
Labor market experience				-0.012	0.028	-0.007	-0.013	0.031*	-0.003
				(0.011)	(0.018)	(0.027)	(0.011)	(0.018)	(0.028)
Labor market experience^2/10^2				0.000	-0.002***	0.000	0.000	-0.002***	0.000
				(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Additional covariates							✓	✓	✓
Observations	6695	5260	6222	6695	5260	6222	6695	5260	6222

	Females								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Live births in birth year / 10^5	0.043	0.322***	0.354***	0.041	0.287***	0.330***	0.047	0.283***	0.328***
	(0.069)	(0.106)	(0.097)	(0.069)	(0.105)	(0.095)	(0.070)	(0.105)	(0.095)
Occupation share at age 15 (%)	0.735***	0.276***	0.269***	0.731***	0.255***	0.259***	0.733***	0.256***	0.259***
	(0.095)	(0.067)	(0.016)	(0.093)	(0.065)	(0.017)	(0.093)	(0.065)	(0.017)
Education				-0.034	-0.126***	-0.094***	-0.032	-0.124***	-0.088***
				(0.031)	(0.025)	(0.023)	(0.030)	(0.025)	(0.023)
Labor market experience				-0.010	0.016	-0.006	-0.006	0.020	0.006
				(0.018)	(0.012)	(0.025)	(0.018)	(0.014)	(0.026)
Labor market experience^2/10^2				0.000	-0.001*	-0.001	-0.000	-0.001*	-0.001
				(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)
Additional covariates							✓	✓	✓
Observations	1143	4107	5558	1143	4107	5558	1143	4107	5558

OLS. Dependent variable: occupation-cohort size (OCS*). Age, year, occupation and state fixed effects are included in all regressions. Additional covariates: household size, immigration status. Robust standard errors in parentheses are clustered at occupation and birth year level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.A4: First Stage, Work Time

	Males								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Live births in birth year / 10^5	0.120***	0.036	0.204***	0.121***	0.026	0.201***	0.123***	0.028	0.202***
	(0.034)	(0.063)	(0.071)	(0.034)	(0.063)	(0.071)	(0.033)	(0.063)	(0.071)
Occupation share at age 15 (%)	0.532***	0.521***	0.237***	0.529***	0.523***	0.233***	0.525***	0.521***	0.231***
	(0.031)	(0.061)	(0.024)	(0.031)	(0.061)	(0.024)	(0.030)	(0.060)	(0.024)
Education				0.015	-0.041**	-0.020	0.011	-0.036*	-0.019
				(0.016)	(0.020)	(0.020)	(0.016)	(0.021)	(0.020)
Labor market experience				-0.014	0.024	-0.005	-0.015	0.028	-0.001
				(0.012)	(0.019)	(0.027)	(0.012)	(0.019)	(0.028)
Labor market experience$^2/10^2$				0.000	-0.001**	0.000	0.000	-0.002***	0.000
				(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Additional covariates							✓	✓	✓
Observations	6574	5193	6199	6574	5193	6199	6574	5193	6199

	Females								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Live births in birth year / 10^5	0.036	0.317***	0.374***	0.035	0.278**	0.350***	0.041	0.274**	0.347***
	(0.072)	(0.110)	(0.100)	(0.073)	(0.109)	(0.098)	(0.074)	(0.109)	(0.098)
Occupation share at age 15 (%)	0.765***	0.274***	0.269***	0.756***	0.254***	0.259***	0.764***	0.255***	0.259***
	(0.100)	(0.068)	(0.017)	(0.097)	(0.066)	(0.018)	(0.096)	(0.066)	(0.018)
Education				-0.047	-0.129***	-0.095***	-0.045	-0.127***	-0.088***
				(0.031)	(0.026)	(0.024)	(0.031)	(0.026)	(0.024)
Labor market experience				-0.009	0.016	-0.005	-0.004	0.020	0.007
				(0.019)	(0.013)	(0.026)	(0.019)	(0.014)	(0.027)
Labor market experience$^2/10^2$				0.000	-0.001*	-0.001	-0.000	-0.001*	-0.001
				(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)
Additional covariates							✓	✓	✓
Observations	1086	3886	5339	1086	3886	5339	1086	3886	5339

OLS. Dependent variable: occupation-cohort size (OCS*). Age, year, occupation and state fixed effects are included in all regressions. Additional covariates: household size, immigration status. Robust standard errors in parentheses are clustered at occupation and birth year level. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.A5: Reduced Form, Wage

	Males								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Live births in birth year / 10^5	0.014	-0.004	0.007	0.014	0.006	0.011	0.010	0.005	0.011
	(0.015)	(0.017)	(0.018)	(0.014)	(0.017)	(0.018)	(0.014)	(0.017)	(0.018)
Occupation share at age 15 (%)	0.010	-0.033***	-0.021***	0.006	-0.031***	-0.014***	0.004	-0.031***	-0.013***
	(0.007)	(0.012)	(0.003)	(0.006)	(0.011)	(0.003)	(0.007)	(0.011)	(0.003)
Education				0.021***	0.051***	0.053***	0.019***	0.048***	0.051***
				(0.005)	(0.006)	(0.007)	(0.005)	(0.006)	(0.007)
Labor market experience				0.006	0.026***	0.030***	0.005	0.020***	0.027**
				(0.010)	(0.006)	(0.010)	(0.010)	(0.006)	(0.010)
Labor market experience^2/10^2				0.000	-0.000	-0.001**	0.000	-0.000	-0.001**
				(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Additional covariates							✓	✓	✓
Observations	6574	5193	6199	6574	5193	6199	6574	5193	6199
	Females								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Live births in birth year / 10^5	-0.044	-0.030	-0.030	-0.043	-0.012	-0.015	-0.043	-0.011	-0.016
	(0.036)	(0.023)	(0.022)	(0.037)	(0.021)	(0.018)	(0.036)	(0.021)	(0.018)
Occupation share at age 15 (%)	0.030	-0.025**	-0.012***	0.029	-0.017*	-0.006***	0.025	-0.017*	-0.006***
	(0.027)	(0.011)	(0.003)	(0.026)	(0.010)	(0.002)	(0.025)	(0.010)	(0.002)
Education				0.015	0.048***	0.055***	0.019*	0.048***	0.054***
				(0.011)	(0.006)	(0.005)	(0.011)	(0.006)	(0.005)
Labor market experience				0.012*	0.011**	0.029***	0.010	0.009*	0.028***
				(0.007)	(0.005)	(0.006)	(0.008)	(0.005)	(0.007)
Labor market experience^2/10^2				-0.000	-0.000	-0.000**	-0.000	-0.000	-0.000**
				(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Additional covariates							✓	✓	✓
Observations	1086	3886	5339	1086	3886	5339	1086	3886	5339

OLS. Dependent variable: logarithm of real hourly gross wage (*ln*). Age, year, occupation and state fixed effects are included in all regressions. Additional covariates: full-time employment, household size, immigration status. Robust standard errors in parentheses are clustered at occupation and birth year level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.A6: Reduced Form, Employment

	Males								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Live births in birth year / 10^5	0.004	0.004	-0.001	0.003	0.007**	-0.001	0.003	0.007**	-0.001
	(0.004)	(0.003)	(0.001)	(0.004)	(0.003)	(0.001)	(0.004)	(0.003)	(0.001)
Occupation share at age 15 (%)	-0.000	0.001	-0.001**	-0.001	-0.000	-0.001**	-0.001	-0.000	-0.001**
	(0.002)	(0.002)	(0.000)	(0.002)	(0.002)	(0.000)	(0.002)	(0.002)	(0.000)
Education				0.005**	0.005***	0.001**	0.006***	0.005***	0.001**
				(0.002)	(0.002)	(0.000)	(0.002)	(0.002)	(0.000)
Labor market experience				0.006	0.008***	0.000	0.007	0.008***	0.000
				(0.004)	(0.002)	(0.001)	(0.004)	(0.002)	(0.001)
Labor market experience$^2/10^2$				0.000	-0.000	0.000	0.000	-0.000	0.000
				(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Additional covariates							✓	✓	✓
Observations	6695	5260	6222	6695	5260	6222	6695	5260	6222
	Females								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Live births in birth year / 10^5	0.006	0.003	-0.000	0.006	0.006	0.002	0.006	0.007	0.003
	(0.013)	(0.008)	(0.004)	(0.013)	(0.008)	(0.004)	(0.013)	(0.008)	(0.003)
Occupation share at age 15 (%)	0.006	-0.001	-0.001	0.004	-0.000	-0.001	0.003	-0.000	-0.001
	(0.017)	(0.003)	(0.000)	(0.017)	(0.003)	(0.001)	(0.018)	(0.003)	(0.001)
Education				-0.002	0.004*	0.002**	-0.002	0.004	0.001
				(0.006)	(0.002)	(0.001)	(0.006)	(0.002)	(0.001)
Labor market experience				0.009***	0.002	0.003	0.009***	0.001	0.001
				(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Labor market experience$^2/10^2$				-0.000*	0.000	0.000	-0.000*	0.000	0.000
				(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Additional covariates							✓	✓	✓
Observations	1143	4107	5558	1143	4107	5558	1143	4107	5558

OLS. Dependent variable: binary indicator for employment status. Age, year, occupation and state fixed effects are included in all regressions. Additional covariates: household size, immigration status. Robust standard errors in parentheses are clustered at occupation and birth year level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.A7: Reduced Form, Contractual Work Time

	Males								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Live births in birth year / 10^5	0.120	0.084	-0.024	0.119	0.220	-0.031	0.099	0.225	-0.029
	(0.144)	(0.245)	(0.106)	(0.144)	(0.230)	(0.111)	(0.146)	(0.230)	(0.111)
Occupation share at age 15 (%)	-0.109	0.498***	-0.043*	-0.108	0.429***	-0.045**	-0.107	0.426***	-0.044**
	(0.071)	(0.162)	(0.022)	(0.071)	(0.155)	(0.021)	(0.068)	(0.156)	(0.022)
Education				-0.002	0.206**	0.104***	-0.004	0.202**	0.105***
				(0.055)	(0.094)	(0.031)	(0.057)	(0.094)	(0.031)
Labor market experience				0.032	0.730***	0.232***	0.030	0.738***	0.231***
				(0.057)	(0.123)	(0.065)	(0.059)	(0.122)	(0.064)
Labor market experience$^2/10^2$				-0.001	-0.015***	-0.004**	-0.001	-0.015***	-0.004**
				(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)
Additional covariates							✓	✓	✓
Observations	6574	5193	6199	6574	5193	6199	6574	5193	6199
	Females								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Live births in birth year / 10^5	1.150	-1.779***	-0.832**	1.122	-1.315***	-0.415	0.833	-0.999**	-0.348
	(1.122)	(0.516)	(0.406)	(1.071)	(0.390)	(0.402)	(1.064)	(0.387)	(0.322)
Occupation share at age 15 (%)	-0.405	0.003	-0.081	-0.879	-0.004	-0.040	-1.209*	-0.069	-0.032
	(0.837)	(0.288)	(0.060)	(0.665)	(0.231)	(0.043)	(0.706)	(0.228)	(0.040)
Education				-1.002***	0.136	0.480***	-1.071***	0.044	0.336***
				(0.289)	(0.128)	(0.115)	(0.297)	(0.125)	(0.086)
Labor market experience				0.925***	1.132***	1.153***	0.696***	0.919***	0.876***
				(0.222)	(0.090)	(0.104)	(0.222)	(0.092)	(0.115)
Labor market experience$^2/10^2$				-0.015**	-0.016***	-0.016***	-0.010	-0.013***	-0.014***
				(0.007)	(0.003)	(0.004)	(0.007)	(0.003)	(0.004)
Additional covariates							✓	✓	✓
Observations	1086	3886	5339	1086	3886	5339	1086	3886	5339

OLS. Dependent variable: contractual working hours per week.. Age, year, occupation and state fixed effects are included in all regressions. Additional covariates: household size, immigration status. Robust standard errors in parentheses are clustered at occupation and birth year level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2.A8: Reduced Form, Actual Work Time

	Males								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Live births in birth year / 10^5	-0.059	0.048	-0.336	-0.069	0.252	-0.333	-0.135	0.267	-0.348
	(0.330)	(0.468)	(0.312)	(0.336)	(0.438)	(0.323)	(0.326)	(0.435)	(0.324)
Occupation share at age 15 (%)	-0.006	0.967***	-0.245***	-0.059	0.958***	-0.212***	-0.146	0.954***	-0.211***
	(0.179)	(0.346)	(0.048)	(0.182)	(0.329)	(0.050)	(0.174)	(0.330)	(0.050)
Education				0.304***	0.727***	0.433***	0.204*	0.665***	0.424***
				(0.112)	(0.168)	(0.086)	(0.112)	(0.164)	(0.086)
Labor market experience				0.141*	1.034***	0.384***	0.115	1.050***	0.381***
				(0.074)	(0.159)	(0.119)	(0.080)	(0.159)	(0.120)
Labor market experience^2/10^2				-0.005**	-0.020***	-0.004	-0.005**	-0.021***	-0.004
				(0.002)	(0.004)	(0.003)	(0.002)	(0.004)	(0.003)
Additional covariates							√	√	√
Observations	6574	5193	6199	6574	5193	6199	6574	5193	6199
	Females								
	Degree of Occupational Specialization								
	Low	Medium	High	Low	Medium	High	Low	Medium	High
Live births in birth year / 10^5	1.647	-2.118***	-1.392**	1.613	-1.578***	-0.862	1.238	-1.240***	-0.781*
	(1.007)	(0.617)	(0.589)	(0.986)	(0.471)	(0.542)	(1.001)	(0.462)	(0.461)
Occupation share at age 15 (%)	-0.626	-0.049	-0.188**	-1.066	-0.017	-0.109*	-1.309	-0.099	-0.100*
	(0.937)	(0.314)	(0.078)	(0.793)	(0.266)	(0.058)	(0.824)	(0.266)	(0.052)
Education				-0.755**	0.358**	0.836***	-0.889***	0.224*	0.664***
				(0.318)	(0.139)	(0.148)	(0.325)	(0.135)	(0.116)
Labor market experience				1.107***	1.253***	1.289***	0.826***	0.999***	0.958***
				(0.268)	(0.107)	(0.117)	(0.262)	(0.111)	(0.125)
Labor market experience^2/10^2				-0.019**	-0.019***	-0.016***	-0.013	-0.015***	-0.014***
				(0.008)	(0.004)	(0.004)	(0.008)	(0.004)	(0.004)
Additional covariates							√	√	√
Observations	1086	3886	5339	1086	3886	5339	1086	3886	5339

OLS. Dependent variable: actual working hours per week. Age, year, occupation and state fixed effects are included in all regressions. Additional covariates: household size, immigration status. Robust standard errors in parentheses are clustered at occupation and birth year level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

3 Estimating Earnings Assimilation of Immigrants to Germany: Evidence from a Double Cohort Model

Abstract

Following the seminal work of Chiswick (1978), many studies have examined the extent to which earnings of immigrants vary over the settlement process. While these studies usually find that the initial earnings gap between native and immigrant workers in traditional immigration countries disappears as the duration of residence in the host country increases, empirical evidence mostly suggests that immigrants to Germany experience persistent earnings disadvantages and, if at all, only a moderate earnings assimilation process for some immigrant groups. However, due to variations in the economic performance of different immigration cohorts, estimates derived from cross-sectional models may be biased (Borjas, 1985). Against this background, this paper employs a double cohort model to revisit the existing evidence on earnings assimilation processes of immigrants to Germany. In line with this literature, no evidence for a robust assimilation process for immigrants is found, even after accounting for potential cohort effects.

3.1 Introduction

Given the increasing number of immigrants worldwide, the social and economic integration of immigrants into the societies of their host countries is of particular importance. The economic literature on the integration of immigrants focuses especially on exploring the convergence of immigrant earnings to the earnings of (comparable) natives. Following the seminal work of Chiswick (1978), a broad literature measures the economic performance of immigrants by estimating cross-section earnings regressions. In general, these studies interpret the coefficient of the variable "years since migration" as earnings assimilation pattern, starting from an initial earnings differential between immigrants and natives.

In this context, cohort effects and selection processes are of special interest in the empirical discussion. Borjas (1985) argues, that cross-section estimates might be biased, when basic differences between immigration cohorts exist or the composition of immigration cohorts has changed over time (e.g. due to systematic return migration). In this case, the parameter estimate of the variable "years since migration" does not solely measure the assimilation effect, but might also reflect differences in trajectory paths between immigration cohorts. If, for example, earlier immigration cohorts follow a flatter assimilation path than more recent cohorts, the assimilation effect might be underestimated in a cross-section regression. Myers and Lee (1996) and Myers et al. (1998) state, that the same argument holds when trajectory paths vary by birth cohort, since these differences are carried by the age or labor market experience variable, respectively, which is normally included as a regressor as well. The coefficient of this variable, which is meant to measure the trajectory path of the reference group, is then potentially distorted. In spite of this fundamental critique, existing studies on earnings assimilation nearly exclusively focus on the estimation of cross-section models,[20] while cohort effects are rarely taken into account.

In the decades after World War II, Germany experienced an intensive immigration history. In the 1960s and 1970s the government recruited a large number of guestworkers mainly from Southern Europe. Although these work-

[20] For a literature overview, see Bauer et al. (2005).

ers were expected to return to their home countries after some years, many of them decided for a permanent residence in Germany. After the oil crisis started in 1973, migration inflows were due to family reunions, the immigration of "Aussiedler" (ethnic Germans residing in East European countries), refugees and asylum-seekers. In 1992, Germany experienced a historical peak with 1.5 million new immigrants (Bauer et al., 2005). Since the mid 1990s the foreign population equals about 9% of the total population (Federal Statistical Office, 2010). Given this impressive immigration history, Germany provides an eminent case study for analyses of the economic and social integration of immigrants.

While for other traditional immigration countries like the U.S., Canada and Australia a clear earnings assimilation process is empirically confirmed by cross-sectional studies, empirical evidence mostly suggests that immigrants to Germany experience persistent earnings disadvantages and, if at all, only a moderate earnings assimilation process for some immigrant groups. Given the rather pessimistic picture drawn by the existing empirical evidence for Germany and the fundamental critique in the literature regarding cross-section regressions, the question arises, whether the existing literature underestimates the economic performance of immigrants to Germany.

The current paper reexamines the question of earnings assimilation of immigrants to Germany under exploration of the relevance of cohort effects for the validity of cross-sectional estimates. The empirical analysis is based on data from the German Socio-Economic Panel (SOEP) for the time period 1990 to 2012 and is restricted to men residing in West Germany or Berlin. Both a traditional cross-section design and a double cohort model, which controls for potential distortions due to cohort effects, are estimated in order to allow for direct comparisons of the model predictions regarding the economic performance of immigrants over the settlement process. The estimation results of the cross-section regressions confirm the frequent finding of no assimilation process for immigrants to Germany. Likewise, cohort model estimates from several specifications deliver either insignificant or, for some immigrant groups, even slightly negative duration effects. Hence, no evidence for an earnings assimilation process for immigrants to Germany is found, even after accounting for potential cohort effects.

The paper contributes to the empirical migration literature by providing a first application of a double cohort model to earnings assimilation processes. This model circumvents the identification problem of age, cohort and period effects in a more convincing way than traditional cross-section models. Further, empirical evidence is provided, which confirms the frequent finding of no universal assimilation process for immigrants to Germany, even after accounting for potential cohort effects.

The paper proceeds as follows. Referring to the respective literature, Section 3.2 briefly surveys the age-period-cohort identification problem, different models of assimilation as well as the existing empirical evidence for Germany. Section 3.3 describes the utilized data and the descriptive statistics. In Section 3.4 the empirical strategy is outlined. Section 3.5 reports and discusses the empirical results, and Section 3.6 concludes.

3.2 Literature

3.2.1 The Classic Age-Period-Cohort Identification Problem

The problem of separating age, period and cohort effects is well discussed in the literature on cohort analysis (e.g. Heckman and Robb, 1985; Mason and Fienberg, 1985). Applying the problem to the context of earnings determinants, all three temporal dimensions might have separate effects on earnings. First, earnings are determined by age, since they typically grow positively at decreasing rates over individuals' life cycles. Second, earnings levels depend on period-specific economic conditions like the business cycle. Third, trajectory paths might be birth cohort-specific, that is, the speed of earnings growth might vary by cohort structure, size or cohort-specific economic optimism. All three variables can therefore be considered as eligible for inclusion as covariates in earnings regressions. An identification problem arises, however, due to perfect multicollinearity:

$$P = BC + A ,$$

where P denotes period, BC denotes birth year, and A denotes age.

When focusing on immigrants, two additional temporal earnings determinants become obvious. Typically, immigrants earn significantly lower wages immediately after their immigration than comparable natives. This may be explained by imperfectly transferable human capital between countries (Chiswick, 1978; Friedberg, 2000). Basilio et al. (2014) empirically confirm the hypothesis of imperfect transferability of human capital between countries for the case of Germany. If immigrants gain host country-specific human capital over time (like language skills and information on labor market conditions), an additional earnings growth process is implemented by the event of immigration, which is not related to age but to the duration of stay in the host country (Myers and Lee, 1996). Again, trajectory paths might vary between immigration cohorts because of different cohort sizes and structures, or because the composition of immigration cohorts has changed over time (e.g. due to systematic return migration, Borjas, 1985). In this context, an identification problem arises from the following relation:

$$P \;=\; MC \;+\; D\,,$$

where P again denotes period, MC denotes year of immigration, and D denotes the duration of stay in the host country.

As a consequence of the perfect multicollinearity, effect identification for all temporal variables by including them simultaneously as regressors in a cross-section regression is impossible. The omission of variables, however, leads to biased effect estimates. As Bell and Jones (2013) show, there is no solution to the age-period-cohort identification problem which does not rely on any kind of assumptions. The following section discusses different strategies taken in the earnings assimilation literature and their implicit assumptions.

3.2.2 Relating Models of Earnings Assimilation

Studies on the economic and social integration of immigrants constitute an important strand of the economic literature. In this context, empirical studies on earnings assimilation processes focus on comparisons between natives and immigrants regarding their speed of earnings growth. Theoretically, due to imperfect transferability of human capital between countries, immigrants have lower opportunity cost of investments in (host country-specific) education than comparable natives (Regets and Duleep, 1999). Therefore, immigrants are expected to have higher earnings growth rates than natives, such that their initial earnings disadvantage is expected to narrow over time. The question of interest in empirical analyses of earnings assimilation processes is whether such an adaptation process indeed takes place. Based on the assumption that natives and immigrants follow the same aging trajectory path over time, existing empirical studies deduce an earnings assimilation process, when the earnings growth path related to duration of stay (which is followed by immigrants but not by natives) is estimated to exhibit positive growth rates (e.g. Chiswick, 1978; Borjas, 1985).

In his seminal paper on the earnings assimilation of immigrants to the U.S. Chiswick (1978) undertakes the first empirical attempt to measure the effect of duration of stay on earnings. He estimates a cross-section earnings regression including, besides other socioeconomic characteristics, labor market experience (as calculated from age) and years since migration as independent variables. Considering natives as reference group, he interprets the coefficient of years since migration as earnings assimilation path. The coefficients of experience and years since migration reflect earnings differences between individuals with different age and duration of stay, respectively. But as pointed out above, earnings differences between individuals at a specific point in time might be due to both age differences and birth cohort differences. Hence, the coefficient of labor market experience captures both aging effects and birth cohort effects. Likewise, the coefficient of years since migration reflects duration effects and immigration cohort effects. Hence, while Chiswick's approach assumes natives and immigrants to follow the same aging trajectory path, another strong implicit assumption behind it is the absence of any cohort-specific earnings differences.

In Borjas' (1985) famous reply study he criticizes the potential bias in a cross-section comparison of immigrants from different immigration cohorts. Exploiting data for natives and immigrants from two periods, he decomposes the cross-section effect of years since migration into two parts, the first measuring earnings differences of immigrants from the same immigration cohort over time, the second measuring differences of immigrants from different cohorts but with identical durations of stay. Borjas interprets the first part, denoted as earnings growth within a cohort, as earnings assimilation effect, while the second part, denoted as earnings growth between cohorts, is interpreted to capture immigration cohort-specific differences. Borjas' method controls for immigration cohorts but not for birth cohort-specific earnings differences, since, as Chiswick, he includes labor market experience (as measured from age) as a cross-sectional variable into his regression. While Borjas recognizes the necessity of controlling for immigration cohort differences, his approach makes the implicit assumption of a non-existence of birth cohort-specific earnings differences. If birth cohort-specific earnings differences are present, the estimated aging trajectory path of natives, who serve as reference group, might be biased (Myers and Lee, 1998).

Hence, both of the described approaches rely on strong assumptions that may be unrealistic. A superior strategy to estimate the duration effect of interest would be one that makes more reasonable assumptions and recognizes the presence of both birth and immigration cohort-specific earnings differences. Myers and Lee (1996) and Myers et al. (1998) provide such a strategy which controls for age, duration, period, birth cohort and immigration cohort simultaneously. As Borjas, they exploit data from several periods.

The implicit assumptions made by this model are the following: Period effects apply to all individuals equally. Members of the same birth cohort have the same birth cohort effect and follow the same aging path. Natives and immigrants from the same birth cohort have identical birth cohort and aging effects. Members of the same immigration cohort have the same immigration cohort effect and follow the same duration path. Finally, the model allows for wage effects that are specific to immigration cohorts nested within birth cohorts.

Applying these assumptions, the model identifies changes over time applying to all individuals equally as period effects. To account for potential cohort differences, aging and duration effects are allowed to vary by birth and immigration cohort, respectively. Aging effects are identified as changes over time applying to natives from a specific birth cohort, and duration effects are estimated for each immigration cohort as the difference in changes between natives and immigrants from the same birth cohort. Technically, the method isolates dynamic effects from constant cohort effects by regressing on both cohort dummy variables and interaction terms between cohort and period.[21]

3.2.3 Empirical Evidence for Germany

Although the presence of the age-period-cohort identification problem in cross-section regressions has long been recognized, a wide range of studies on earnings assimilation patterns adopts the estimation strategy of Chiswick (1978). While for other traditional immigration countries like the U.S., Canada or Australia an earnings assimilation process is empirically confirmed by cross-sectional regressions, studies for immigrants to Germany, which are based on data mainly from the SOEP, deliver ambiguous results (Bauer et al., 2005).

Only few studies find evidence confirming an assimilation process. Based on the first wave of the SOEP, Schmidt (1993b) estimates an initial earnings disadvantage for guestworkers of 12% relative to comparable Germans. On average 17 years after immigration guestworkers reach income equality with Germans. Constant (1998) finds an initial earnings disadvantage for female guestworkers, using the first 10 waves of the SOEP. After 10 years they overtake the earnings of comparable German women. Basing their study on the first 14 SOEP waves, Constant (2005) conclude that immigrants reach income equality with Germans after 23 years.

In contrast to these results, Pischke (1992) measures, based on the first six waves of the SOEP, an initial earnings differential between 20% and 25%, which does not significantly decline over time. He finds evidence for an assimilation process only for immigrants from guestworker countries, who immigrated after 1976. Dustmann (1993) estimates different specifications of a cross-section regression on the basis of the first wave of the SOEP and finds a persistent earnings disadvantage of 13% to 19% for guestworkers relative to comparable Germans. After control for potential distortions due to systematic selection into the labor market and the return migration decision, Licht and Steiner (1994) also find, based on the first six waves of the SOEP, a large initial earnings disadvantage for immigrants, which is not narrowing over time. However, for immigrants with relatively short durations of stay they find similar earnings levels and higher earnings growth rates as for Germans. Schmidt (1997) as well

[21]Myers and Lee (1996) apply the model to residential overcrowding, while Myers et al. (1998) explore homeownership attainment.

finds a persistent earnings disadvantage of 20% for guestworkers compared to Germans. He concludes, that this earnings differential is caused by long-run differences in education. Based on the first 10 waves of the SOEP, Constant (1998) finds a significant and persistent earnings disadvantage for male guestworkers compared to Germans. However, she also finds an initial but short-lived earnings advantage for immigrants. Fertig and Schurer (2007) investigate assimilation patterns for different immigrant groups regarding earnings as well as unemployment probability. They find evidence for an earnings assimilation process only for ethnic Germans and the youngest immigrant group immigrated between 1969 and 2002. The results of Zibrowius (2012) suggest that although immigrants in Germany experience wage growth, their earnings profiles are mostly flatter than those of Germans and a persistent earnings differential remains. Taking a slightly different perspective, Gathmann and Keller (2014) detect wage returns to citizenship for female immigrants to Germany, while there are no returns for men and traditional guestworkers.

Summarized, the majority of studies arrives at rather pessimistic conclusions, mostly predicting persistent earnings disadvantages for immigrants, while an earnings assimilation process can be confirmed, if at all, only for specific immigrant groups. Given these pessimistic results, the question arises whether unconsidered cohort effects might have caused an underestimation of the economic performance of immigrants to Germany in existing cross-sectional studies of earnings assimilation patterns.

3.3 Data and Descriptive Statistics

3.3.1 Data

The empirical analysis is based on data from the German Socio-Economic Panel (SOEP). The SOEP is a representative longitudinal study for Germany collecting information on native and foreign households. All household members above 15 years of age are questioned individually in face-to-face interviews. In addition, household-related questionnaires are answered by household heads (Kroh, 2011; Haisken-DeNew and Frick, 2005). The yearly repeated survey started in 1984 with about 6,000 interviewed households and samples about 12,000 households per year since 2000 (Goebel et al., 2008).

The empirical analysis of this paper is based on data from the waves 1990 to 2012. To focus on a population with a high share of full-time employed, the sample is restricted to male individuals aged from 18 to 65 years who are employed and no apprentices. Immigrants are defined as foreign-born individuals who immigrated to Germany since 1948. Since the population share of immigrants is relatively small in East Germany (Federal Statistical Office, 2010), only individuals residing in West Germany or East or West Berlin are included. Foreign-born ethnic Germans who received German citizenship after immigration are excluded from the sample because it is unclear whether they should be assigned as natives or as immigrants.

3.3.2 Descriptive Statistics

Table 3.1 reports average labor earnings by birth and immigration cohort.[22] As expected, the mean wages of immigrants are lower than that of natives in most categories, implying earnings disadvantages for immigrants compared to natives. The overall earnings increase from 1990-96 to 2004-12 is higher for immigrants than for natives, suggesting that an assimilation process over the considered time period may potentially take place. However, dividing the sample into birth cohorts confirms this picture only for individuals born before 1955. Within the two younger birth cohorts, immigrants experience a lower wage growth than natives. A comparison by immigration cohort reveals that wages tend to be higher and to increase stronger the earlier is the period of immigration.

Comparing immigration cohorts by birth cohort shows that the wage increase for immigrants before 1974 is mainly driven by the strong increase for the youngest birth cohort born after 1965 of 3.69€. These individuals have immigrated during childhood, meaning that their human capital was mostly attained within Germany, which might explain their comparably high economic success. However, also immigrants from this birth cohort who immigrated later in their life cycles experienced a wage growth of more than 2€. Considering the

[22]Inconsistencies between the means result from the weighting of the observations with weights provided by the SOEP. (For example, the absolute increase from 1990-96 to 2004-12 is larger for natives from all birth cohorts separately than it is for the whole group of natives.)

Table 3.1: Real Labor Earnings by Birth and Immigration Cohort

	All				*Born before 1955*				*Born 1955-1965*				*Born after 1965*			
	1990-96	1997-04	2004-12	Δ	1990-96	1997-04	2004-12	Δ	1990-96	1997-04	2004-12	Δ	1990-96	1997-04	2004-12	Δ
Natives	17.03	18.07	17.56	0.53	18.52	19.90	19.95	1.43	16.28	18.65	18.55	2.27	13.30	15.33	16.06	2.77
	(7.29)	(7.50)	(7.78)		(7.86)	(7.71)	(8.79)		(5.64)	(7.16)	(7.42)		(6.99)	(6.89)	(7.35)	
	[12556]	[21077]	[21122]		[5937]	[6479]	[4084]		[4362]	[7594]	[7423]		[2257]	[7004]	[9615]	
Immigrants	15.15	15.68	16.01	0.86	15.64	16.93	18.01	2.37	15.16	15.79	16.31	1.15	12.37	13.80	14.62	2.25
	(4.95)	(5.79)	(7.70)		(5.14)	(6.67)	(10.40)		(4.59)	(5.42)	(7.07)		(3.50)	(4.12)	(6.13)	
	[4957]	[4438]	[2402]		[2892]	[1588]	[501]		[1343]	[1498]	[902]		[722]	[1352]	[999]	
Immigrants before 1974	15.67	17.27	17.87	2.20	15.52	17.08	17.23	1.71	16.95	18.58	18.59	1.64	13.24	14.37	16.93	3.69
	(4.87)	(6.04)	(8.42)		(4.89)	(5.83)	(7.68)		(4.76)	(6.65)	(9.40)		(2.98)	(4.42)	(5.11)	
	[3388]	[1826]	[656]		[2571]	[1131]	[273]		[592]	[477]	[269]		[225]	[218]	[114]	
Immigrants 1974-1989	14.37	15.17	16.30	1.93	15.80	16.93	21.91	6.11	14.31	15.33	15.86	1.55	12.30	14.03	14.68	2.38
	(4.27)	(5.12)	(7.76)		(4.84)	(7.24)	(14.23)		(3.75)	(4.51)	(5.32)		(3.50)	(3.97)	(5.05)	
	[1366]	[1714]	[1085]		[280]	[290]	[143]		[689]	[745]	[450]		[397]	[679]	[492]	
Immigrants after 1989	13.68	13.99	13.95	0.27	18.67	16.19	12.63	-6.04	12.61	13.25	13.75	1.14	11.76	13.39	14.29	2.53
	(7.37)	(5.76)	(6.36)		(11.35)	(9.16)	(4.36)		(5.61)	(3.78)	(4.99)		(3.80)	(4.21)	(7.15)	
	[203]	[898]	[661]		[41]	[167]	[85]		[62]	[276]	[183]		[100]	[455]	[393]	

Means of real hourly gross earnings in €. Standard deviations in parentheses. Number of observations in brackets. Weights provided by the SOEP are used. Δ: absolute change from 1990-96 to 2004-12.

oldest birth cohort born before 1955, there is a strong heterogeneity in wage growth. In particular, within this birth cohort, immigrants between 1974 and 1989 experience an increase of over 6€, while the wages of immigrants after 1989 decrease by more than 6€. However, the observation numbers for these groups are relatively low. In summary, the reported variation in earnings levels and in changes over time by birth and immigration cohort indicate that earnings levels and earnings growth paths might differ remarkably between birth and immigration cohorts, underlining the necessity to control for both in the empirical analysis.

3.4 Empirical Strategy

In order to explore the relevance of cohort effects for the validity of cross-sectional earnings assimilation estimates, the empirical analysis focuses on a comparison of the results of a cross-section regression model after Chiswick (1978) and a double cohort regression model after Myers and Lee (1996) and Myers et al. (1998), respectively. Only the latter model allows an estimation of assimilation effects undistorted by cohort effects. The current paper provides a first application of this estimation strategy to earnings assimilation processes.

To appropriately apply the double cohort regression model, observations from several points in time are needed, which cover a sufficiently long time span for an earnings assimilation process to potentially take place. The present analysis exploits all years from 1990 to 2012. To account for the possibility that assimilation patterns differ by country of origin, the regressions are run separately for immigrants from OECD countries, which are relatively highly industrialized, and other countries of origin. Moreover, to exclude the possibility that substantial variation by educational level between immigration cohorts may distort the results (most immigrants before 1974 were relatively low educated guestworkers), the regressions are also run separately for individuals with less than 11 years of education and individuals with at least 11 years of education, such that only the latter group includes individuals who received at least an upper secondary degree or technical school degree.

3.4.1 Cross-Section Regression Model

Chiswick (1978) extended the Human Capital Earnings Function (Mincer, 1974) to application on datasets containing immigrants. The following variant of this extended specification is estimated:

$$\begin{aligned} ln\,Y &= \alpha_0 + \sum_i \alpha_{1i}\, P_i + \alpha_2\, exp + \alpha_3\, exp^2 \\ &\quad + mig\Big[\alpha_4 \; + \alpha_5\, ysm + \alpha_6\, ysm^2\Big] + \alpha_7\, educ + \varepsilon\,, \end{aligned} \tag{3}$$

where Y is gross hourly earnings in nominal terms, P_i are year dummy variables, which equal one for observations from the particular year i, exp is years of labor market experience in full-time employment and ysm is years since migration. mig is a dummy variable, which equals one if an individual immigrated to Germany since 1948, zero otherwise. $educ$ is education in years and ε is a random error with expectation value zero.

Following Chiswicks interpretation, which derives from the human capital theory, $\hat{\alpha_4}$ measures the initial earnings differential between natives and immigrants after immigration, which is under the assumption of imperfectly transferable human capital between countries expected to be negative. The coefficients of labor market experience are interpreted to capture the concave aging trajectory path of natives, who serve as reference group. The coefficients of years since migration should capture all deviations of immigrants from the natives' trajectory path and are therefore interpreted to measure the earnings assimilation process. However, as pointed out above, both the coefficients of years since migration and experience might also carry cohort differences, such that they might not reflect the pure effects of duration of stay and aging, respectively.

3.4.2 Double Cohort Regression Model

Adopting the estimation strategy of Myers and Lee (1996) and Myers et al. (1998), the following regression equation is estimated:

$$
\begin{aligned}
ln\,Y \;=\;& \beta_0 + \sum_i \beta_{1i}\,P_i + \sum_{j=2,3}\Big[\beta_{2j}\,BC_j \,+\, \beta_{3j}\,(BC_j\cdot T)\Big] \\
& + mig\,\Bigg\{\sum_{k=1,2,3}\Big[\beta_{4k}MC_k \,+\, \beta_{5k}\,(MC_k\cdot T)\Big] \\
& + \sum_{j=2,3}\sum_{k=1,2,3}\Big[\beta_{6jk}\,(BC_j\cdot MC_k) + \beta_{7jk}\,(BC_j\cdot MC_k\cdot T)\Big]\Bigg\} \\
& + \beta_8\,educ \,+\, \varepsilon, \qquad\qquad (4)
\end{aligned}
$$

where Y is again gross hourly earnings in nominal terms and P_i are year dummy variables, which equal one for observations from the particular year i, zero otherwise. BC_j are dummy variables for different birth cohorts, which equal one for observations of individuals born during the corresponding time period, zero otherwise (BC_1: born before 1955 [serves as reference group]; BC_2: born between 1955 and 1965; BC_3: born after 1965). The birth cohorts have been chosen such that the medium-aged birth cohort roughly comprises the baby boomers. MC_k are dummy variables for different immigration cohorts, which equal one for observations of immigrants during the particular time period, zero otherwise (MC_1: immigrant before 1974; MC_2: immigrant between 1974 and 1989; MC_3: immigrant after 1989; natives serve as reference group). The earliest birth cohort includes the guestworkers, who were recruited by the German government until the beginning of the oil crises in 1974. The most recent immigration cohort comprises immigrants who entered the country after the German reunification in 1989. T gives the observation year with 1990 set to zero. mig is a dummy variable, which equals one if an individual immigrated to Germany since 1948, zero otherwise. $educ$ is education in years, the terms in parentheses are interaction terms and ε is an error term with expectation value zero.

The coefficients of P_i measure year-specific effects, which occur to all observations equally (e.g. because of changes in macroeconomic conditions). The coefficients of BC_j measure the initial average earnings level of the particular birth cohort compared to BC_1. This differential partly results from the different initial age levels of the birth cohorts, but also captures, for example, differences in the cohort structure between BC_j and BC_1. The interaction terms between birth cohort and period ($BC_j \cdot T$) represent the cohort-specific linear time trends in earnings, such that $\hat{\beta}_{3j}$ can be interpreted as the aging effect of birth cohort BC_j compared to BC_1. The coefficients of MC_k quantify the initial earnings differential between the particular immigration cohort and natives, which is not explained by birth cohort-specific earnings differences. Besides earnings differences due to different initial durations of stay, these coefficients also capture immigration cohort-specific differences. The coefficients $\hat{\beta}_{5k}$ measure the average earnings change of the particular immigration cohort compared to natives, net of birth cohort-specific changes, such that these coefficients provide estimates for the duration effects of interest. The interaction term between birth and immigration cohort ($BC_j \cdot MC_k$) controls for the case that specific birth cohorts within immigration cohorts have effects on earnings, which appear neither for the whole birth cohort nor the whole immigration cohort (age-at-arrival effect). The highest interaction term, finally, ($BC_j \cdot MC_k \cdot T$) represents dynamic effects specific to birth cohorts nested within immigration cohorts and therefore captures duration effects, which do not appear for a whole immigration cohort, but only for a specific birth cohort within an immigration cohort. In contrast to the assimilation effects derived from a cross-section regression model like Equation (3), the estimated duration effects derived from Equation (4) are not potentially distorted by birth or immigration cohort effects.

3.5 Empirical Results

Table 3.2 reports cross-sectional estimates of Equation (3) for both natives and immigrants from OECD countries as well as natives and immigrants from other countries. The coefficients of labor market experience have the expected signs in both regressions, indicating that the individuals follow a concave aging trajectory path over time.

For immigrants from OECD countries the coefficient of the immigrant

Table 3.2: Cross-Sectional Earnings Regressions

	OECD		Other	
	Coef.	SE	Coef.	SE
Labor market experience	0.029***	0.001	0.030***	0.001
Labor market experience$^2/10^2$	-0.046***	0.002	-0.048***	0.002
Immigrant	-0.101***	0.034	-0.248***	0.032
Years since immigration	0.004	0.003	0.008*	0.004
Years since immigration$^2/10^2$	-0.005	0.006	-0.006	0.008
Education	0.070***	0.001	0.070***	0.001
Full-time employed	0.119***	0.021	0.120***	0.022
Married	0.081***	0.007	0.080***	0.007
Constant	1.131***	0.042	1.093***	0.042
R^2	0.37		0.38	
Observations	64340		60186	

OLS. Year, county and industry fixed effects are included in all regressions. Robust standard errors (SE) are clustered at the household level. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

dummy variable exhibits a negative and significant sign, suggesting an initial earnings disadvantage for these immigrants compared to Germans of about 10%. For immigrants from countries not participating in the OECD, this differential amounts to even 25%. Immigrants from non-OECD countries may have a larger initial earnings disadvantage because human capital may be more easily transferable within the OECD than across OECD borders. At the same time, the coefficients of years since migration are either insignificant or, for immigrants from non-OECD countries, significant only at the 10% level. This result suggests that the dynamic growth path of immigrants does not significantly deviate from the native trajectory path. Hence, the estimated initial earnings disadvantage may be persistent over time, so that there is no evidence for an earnings assimilation process. This confirms the results of most existing cross-sectional studies for Germany.

Table 3.3 reports cross-sectional estimates separately for individuals with low and high education levels. As before, the coefficients of the immigrant dummy are significantly negative in all regressions. A higher education level as well as originating from a country other than an OECD country seem to

Table 3.3: Cross-Sectional Earnings Regressions, Effect Heterogeneity

	Less than 11 years of education				At least 11 years of education			
	OECD		Other		OECD		Other	
	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Labor market experience	0.021***	0.001	0.022***	0.001	0.034***	0.001	0.034***	0.001
Labor market experience$^2/10^2$	-0.032***	0.003	-0.034***	0.003	-0.052***	0.003	-0.054***	0.003
Immigrant	-0.103**	0.041	-0.171***	0.044	-0.121**	0.052	-0.313***	0.038
Years since immigration	0.006*	0.004	0.008*	0.005	-0.003	0.005	0.006	0.004
Years since immigration$^2/10^2$	-0.010	0.008	-0.014	0.011	0.013	0.011	0.004	0.009
Education	0.042***	0.005	0.045***	0.006	0.070***	0.002	0.069***	0.002
Full-time employed	0.212***	0.044	0.225***	0.045	0.077***	0.025	0.078***	0.025
Married	0.077***	0.009	0.082***	0.010	0.086***	0.009	0.082***	0.009
Constant	1.358***	0.072	1.308***	0.081	1.128***	0.065	1.094***	0.061
R^2	0.21		0.22		0.38		0.38	
Observations	25749		22211		38591		37975	

OLS. Year, county and industry fixed effects are included in all regressions. Robust standard errors (SE) are clustered at the household level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

increase the initial earnings disadvantage compared to German natives. Again, the estimated coefficients of years since migration are either insignificant or significant at the 10% level only. Hence, the results by education confirm the results reported in Table 3.2.

Table 3.4 shows results from cohort regressions of Equation (4) by country of origin. Both regressions predict an earnings disadvantage of about 12.4% for the medium-aged birth cohort and of about 22.4% for the youngest birth cohort both compared to the oldest birth cohort, reflecting that earnings are increasing in age. The estimated aging effects suggest that over time the earnings of the medium-aged birth cohort grow at a significantly higher rate than the earnings of the oldest birth cohort.

The average earnings differential between natives and immigrants from OECD countries is insignificant for the immigration cohort after 1989, while it is negative and significant for immigrants between 1974 and 1989 and immigrants before 1974. However, positive age-at-arrival effects of different size are measured for these immigration cohorts, mostly offsetting the negative overall effects. Hence, there may not be an earnings disadvantage for immigrants from OECD countries after all. Also, the duration effects are insignificant for all immigration cohorts and may be even negative for some nested cohorts.

For immigrants from countries other than OECD countries there is a significantly negative earnings differential between natives and all three immigration cohorts. In particular, immigrants before 1974 earn 5.5% less, immigrants between 1974 and 1989 earn 13.5% less and immigrants after 1989 earn even 24.1% less than comparable Germans. While for the earliest immigration cohort, the positive age-at-arrival effects may offset the overall earnings disadvantage, this is not the case for immigrants between 1974 and 1989 and immigrants after 1989. As the duration effects suggest, a narrowing of these differentials does not take place at all. On the contrary, there is a negative effect for the most recently immigrated group that is significant at 5%, suggesting that the earnings disadvantage may be even growing. However, within this immigration cohort the duration effect of nested cohorts is significantly positive for individuals born after 1965, offsetting the widening of the overall disadvantage for younger immigrants. Overall, a convergence of immigrant wages to the wages of natives is not predicted.

Table 3.4: Double Cohort Earnings Regressions

	OECD		Other	
	Coef.	SE	Coef.	SE
Birth cohort				
Born before 1955 (reference group)				
Born 1955-1965 (BC_2)	-0.124***	0.013	-0.125***	0.013
Born after 1965 (BC_3)	-0.225***	0.015	-0.224***	0.015
Aging effect				
Born before 1955 (reference group)				
Born 1955-1965 ($BC_2 \cdot T$)	0.004***	0.001	0.004***	0.001
Born after 1965 ($BC_3 \cdot T$)	-0.001	0.001	-0.001	0.001
Immigration cohort				
Natives (reference group)				
Immigrant before 1974 (MC_1)	-0.044***	0.017	-0.055**	0.021
Immigrant 1974-1989 (MC_2)	-0.180***	0.044	-0.135**	0.062
Immigrant after 1989 (MC_3)	-0.157	0.227	-0.241***	0.090
Duration effect				
Natives (reference group)				
Immigrant before 1974 ($MC_1 \cdot T$)	-0.001	0.003	-0.006*	0.003
Immigrant 1974-1989 ($MC_2 \cdot T$)	0.007	0.006	-0.001	0.006
Immigrant after 1989 ($MC_3 \cdot T$)	-0.003	0.015	-0.015**	0.007
Age-at-arrival effect				
Born before 1955; natives (reference groups)				
Immigrants before 1974:				
Born 1955-1965 ($BC_2 \cdot MC_1$)	0.129***	0.027	0.124*	0.064
Born after 1965 ($BC_3 \cdot MC_1$)	0.124***	0.039	0.127***	0.042
Immigrants 1974-1989:				
Born 1955-1965 ($BC_2 \cdot MC_2$)	0.129**	0.051	0.002	0.083
Born after 1965 ($BC_3 \cdot MC_2$)	0.234***	0.051	0.090	0.082
Immigrants after 1989:				
Born 1955-1965 ($BC_2 \cdot MC_3$)	-0.036	0.237	-0.088	0.104
Born after 1965 ($BC_3 \cdot MC_3$)	0.068	0.235	0.046	0.101
Duration effect of nested cohorts				
Born before 1955; natives (reference groups)				
Immigrants before 1974:				
Born 1955-1965 ($BC_2 \cdot MC_1 \cdot T$)	-0.006*	0.004	-0.004	0.007
Born after 1965 ($BC_3 \cdot MC_1 \cdot T$)	-0.003	0.004	0.003	0.006
Immigrants 1974-1989:				
Born 1955-1965 ($BC_2 \cdot MC_2 \cdot T$)	-0.013**	0.006	-0.005	0.007
Born after 1965 ($BC_3 \cdot MC_2 \cdot T$)	-0.011*	0.006	0.001	0.007
Immigrants after 1989:				
Born 1955-1965 ($BC_2 \cdot MC_3 \cdot T$)	0.006	0.016	0.012	0.008
Born after 1965 ($BC_3 \cdot MC_3 \cdot T$)	0.007	0.016	0.023***	0.008
Control variables				
Education in years	0.062***	0.001	0.063***	0.001
Full-time employed	0.210***	0.021	0.209***	0.021
Married	0.137***	0.007	0.138***	0.007
Constant	1.451***	0.042	1.425***	0.042
R^2	0.34		0.35	
Observations	64340		60186	

OLS. Year, county and industry fixed effects are included in all regressions. Robust standard errors (SE) are clustered at the household level. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3.5 reports cohort regressions of Equation (4) by country of origin and educational group. Focusing on the estimated birth cohort effects, the medium-aged birth cohort and especially the youngest birth cohort have on average significantly lower earnings than the oldest birth cohort, again reflecting that wages are increasing in age. The differentials are more pronounced for higher education levels, suggesting a higher wage inequality in this group. While the corresponding aging effects are positive in the regressions for higher education levels, indicating wage growth over time, they are insignificant for lower educated individuals born between 1955 and 1965 and even negative for those born after 1965 in the lower education group, suggesting an average earnings decline of 0.4% per year.

Focusing on immigration cohort effects for immigrants from OECD countries with a lower education level, there is a negative wage differential between natives and immigrants between 1974 and 1989 as well as immigrants after 1989. In particular, the former are estimated to earn 9.2% less, while the latter are measured to earn even 43.3% less than comparable natives. The corresponding duration effects and the duration effects of nested cohorts, which are either insignificant or even slightly negative, do not indicate a narrowing of these differentials over time.

For lower educated immigrants from countries not participating in the OECD, no significant differentials compared to natives are measured. Additionally, all age-at-arrival effects are insignificant. Hence, although the wages of immigrants before 1974 decline significantly over time and those of immigrants after 1989 from the youngest birth cohort rise, there are no overall earnings disadvantages compared to natives.

Considering higher educated individuals, there are significant overall earnings differentials for all immigration cohorts except for immigrants after 1989 from OECD countries. However, the positive age-at-arrival effects for immigrants before 1974 and younger immigrants between 1974 and 1989 potentially offset the differentials for these groups. As for lower educated individuals, the overall duration effects in combination with the duration effects of nested cohorts do not point at any earnings assimilation process taking place. The positive duration effect for immigrants before 1974 may be compensated by the negative duration effects of nested cohorts for this immigration cohort.

Table 3.5: Double Cohort Earnings Regressions, Effect Heterogeneity

	Less than 11 years of education				At least 11 years of education			
	OECD		Other		OECD		Other	
	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
Birth cohort								
Born before 1955 (reference group								
Born 1955-1965 (BC_2)	-0.051***	0.017	-0.046***	0.017	-0.193***	0.018	-0.196***	0.018
Born after 1965 (BC_3)	-0.134***	0.020	-0.121***	0.021	-0.341***	0.021	-0.347***	0.021
Aging effect								
Born before 1955 (reference group)								
Born 1955-1965 ($BC_2 \cdot T$)	0.001	0.002	0.001	0.002	0.007***	0.001	0.007***	0.001
Born after 1965 ($BC_3 \cdot T$)	-0.004**	0.002	-0.004***	0.002	0.004***	0.001	0.004***	0.002
Immigration cohort								
Natives (reference group)								
Immigrant before 1974 (MC_1)	0.017	0.022	0.011	0.031	-0.263***	0.031	-0.168***	0.031
Immigrant 1974-1989 (MC_2)	-0.092**	0.044	-0.065	0.125	-0.289***	0.068	-0.206***	0.067
Immigrant after 1989 (MC_3)	-0.434***	0.147	-0.075	0.150	-0.100	0.320	-0.409***	0.082
Duration effect								
Natives (reference group)								
Immigrant before 1974 ($MC_1 \cdot T$)	-0.005*	0.003	-0.009***	0.003	0.019***	0.005	0.004	0.006
Immigrant 1974-1989 ($MC_2 \cdot T$)	-0.003	0.005	-0.004	0.009	0.015*	0.008	0.001	0.006
Immigrant after 1989 ($MC_3 \cdot T$)	0.016	0.015	-0.018	0.012	-0.005	0.023	-0.009	0.007
Age-at-arrival effect								
Born before 1955; natives (reference groups)								
Immigrants before 1974:								
Born 1955-1965 ($BC_2 \cdot MC_1$)	0.025	0.032	0.001	0.078	0.318***	0.053	0.288***	0.071
Born after 1965 ($BC_3 \cdot MC_1$)	0.003	0.045	-0.009	0.047	0.280***	0.065	0.235**	0.101
Immigrants 1974-1989:								
Born 1955-1965 ($BC_2 \cdot MC_2$)	0.046	0.052	0.040	0.137	0.125	0.077	0.002	0.096
Born after 1965 ($BC_3 \cdot MC_2$)	0.083*	0.049	-0.125	0.147	0.256**	0.103	0.244**	0.100
Immigrants after 1989:								
Born 1955-1965 ($BC_2 \cdot MC_3$)	0.240	0.169	-0.255	0.168	-0.082	0.334	0.058	0.107
Born after 1965 ($BC_3 \cdot MC_3$)	0.286*	0.164	-0.126	0.156	0.063	0.337	0.177	0.111
Duration effect of nested cohorts								
Born before 1955; natives (reference groups)								
Immigrants before 1974:								
Born 1955-1965 ($BC_2 \cdot MC_1 \cdot T$)	0.005	0.004	-0.001	0.008	-0.030***	0.006	-0.009	0.009
Born after 1965 ($BC_3 \cdot MC_1 \cdot T$)	0.008	0.005	0.009	0.008	-0.020***	0.008	-0.005	0.009
Immigrants 1974-1989:								
Born 1955-1965 ($BC_2 \cdot MC_2 \cdot T$)	-0.000	0.005	-0.005	0.012	-0.016*	0.009	-0.005	0.008
Born after 1965 ($BC_3 \cdot MC_2 \cdot T$)	0.006	0.005	0.017	0.012	-0.020*	0.011	-0.008	0.009
Immigrants after 1989:								
Born 1955-1965 ($BC_2 \cdot MC_3 \cdot T$)	-0.008	0.016	0.019	0.014	0.002	0.025	0.005	0.009
Born after 1965 ($BC_3 \cdot MC_3 \cdot T$)	-0.005	0.016	0.032**	0.013	-0.001	0.025	0.013	0.008
Control variables								
Education in years	0.043***	0.005	0.051***	0.006	0.058***	0.002	0.058***	0.002
Full-time employed	0.256***	0.044	0.262***	0.046	0.192***	0.023	0.190***	0.024
Married	0.121***	0.010	0.130***	0.010	0.147***	0.009	0.145***	0.009
Constant	1.525***	0.075	1.439***	0.085	1.578***	0.065	1.549***	0.061
R^2	0.19		0.20		0.34		0.35	
Observations	25749		22211		38591		37975	

OLS. Year, county and industry fixed effects are included in all regressions. Robust standard errors (SE) are clustered at the household level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The remaining duration effects are mostly insignificant.

In summary, there are considerable earnings differences between immigrants from OECD countries and immigrants with other countries of origin, while the differences to Germans seem negligible for some immigrant groups from OECD countries. Dividing the samples in lower and higher educated individuals still reveals no convergence in earnings. Neither the cross-sectional estimates nor the cohort model predictions yield evidence for a robust earnings assimilation process.[23] This confirms the pessimistic findings of most existing studies on earnings assimilation processes of immigrants to Germany. Hence, although the double cohort estimates suggest remarkable cohort differences, these seem in general not to qualitatively distort predictions derived from earlier cross-section studies on earnings assimilation processes for the case of Germany.

3.6 Conclusion

This paper estimates earnings assimilation effects for immigrants to Germany under exploration of the relevance of cohort effects for the validity of cross-sectional estimates. In the empirical analysis, which is based on data for male immigrants to Germany, a traditional cross-section regression model is estimated, which does not control for birth or immigration cohort effects and therefore yields potentially biased results. Consistent with the majority of existing empirical studies, this model predicts a huge initial earnings disadvantage for immigrants from countries not participating in the OECD compared to Germans, which remains persistent over time.

In order to measure earnings assimilation effects under consideration of potential birth or immigration cohort effects, a double cohort model, which circumvents the identification problem of age, cohort and period effects in a more convincing way than traditional cross-section models do, is estimated by both country of origin and educational level. The paper provides the first application of a double cohort model to earnings assimilation processes. The

[23]To check for the robustness of the results, all regressions were also estimated including interaction terms between the immigrant dummy and all control variables, except for the variables of the basic model in Equation (4) but education. This as well did not yield significant duration effects.

estimation results suggest that birth cohorts nested within immigration cohorts affect earnings remarkably differently. However, in spite of controlling for these differences, no evidence for a universal earnings assimilation process can be found. This confirms the frequent finding of no appreciable earnings assimilation process for immigrants to Germany. Hence, the results of this paper do not indicate a qualitative distortion of cross-sectional estimates of earnings assimilation processes for the case of Germany. In contrast, the result of no significant earnings assimilation process appears to be robust for the case of Germany.

4 Inequality of Opportunity in Retirement Age: The Role of Physical Job Demands

Abstract

We quantify differences in the retirement age between manual and non-manual workers and evaluate these differences in the context of the literature on equality of opportunity. The focus is on the question how individual background during childhood transmits through physical demands of occupations on retirement ages. Individual retrospective data from the German Socio-Economic Panel are used to analyze labor force dynamics over the years 1984 to 2011. Discrete time duration models suggest that retirement ages differ substantially between manual and non-manual workers. To elaborate how such differences are explained by individual background characteristics on the one hand and effort and luck on the other hand, we make use of tests for stochastic dominance and a Blinder-Oaxaca decomposition. The result is that individual background characteristics explain a share of about one third of inequality in retirement ages as transmitted through physical demands of occupations.

4.1 Introduction

In 2007, the German Federal Parliament approved a law to gradually increase the normal retirement age from 65 to 67. The decision was accompanied by a public debate on the justness of a legal retirement age fixed at a high level towards workers being exposed to different levels of physical job demands. In particular, high physical demands of job duties may force a worker to retire early because work-related health impairments accumulate over time and human physical capacity declines naturally with increasing age. This may disadvantage respective occupations since retirement previous to the normal retirement age reduces benefit entitlements for two reasons. First, fewer years of work imply fewer years of contribution and thus lower pension claims, since their overall amount depends on the duration of preceding contributions. Second, early retirement is subject to actuarial adjustments, which additionally reduces pension claims by 3.6 per cent for each year by which an old age pension is claimed early.[24] In addition, occupations with high physical job demands tend to be low-wage professions, which restricts the potential for private pension provision.

The purpose of this paper is to quantify differences in the retirement age between manual and non-manual workers and to evaluate these differences with respect to equality of opportunity. The focus is on the question how individual background during childhood transmits through physical demands of occupations on retirement ages. Our study contributes to the existing literature in several respects. First, we provide a precise empirical description of labor force dynamics of older manual and non-manual workers with a particular focus on retirement patterns. Second, we contribute to the literature on equality of opportunity (EOP hereafter) as prominently discussed by Roemer (1993, 1998), by distinguishing between individual background beyond individual influence on the one hand and effort and luck on the other hand. This framework is useful to structure thoughts in a debate, where early retirement of manual workers is frequently declared as "unfair" because this usually implies a reduction in social security wealth. To the best of our knowledge, this

[24] Actuarial adjustments have been introduced in the German public pension system between 1997 and 2004.

is the first study on EOP in retirement age. Finally, we use data for Germany which provides an eminent case study for analyses of an aging population. Demographic change will continue to impose a fair amount of pressure on the pay-as-you-go pension system. The baby boom cohorts born between 1955 and 1970 move towards retirement as of the year 2015, once they successively become eligible for old age pensions. At the same time, younger birth cohorts are much smaller (Federal Statistical Office, 2014b). Severe population aging will either induce raising contributions, alleviate benefit entitlements or both.

Our analysis departs from a description of labor force dynamics of older workers beginning at age 40 to elaborate differences in the retirement age for manual workers compared to non-manual workers. In a first step, discrete time duration models are used to estimate the hazard rate for transitions out of full-time employment, part-time employment or unemployment into retirement. To distinguish manual from non-manual occupations, we make use of a well-defined measure for the degree of physical demands on respective jobs (Kroll, 2011). In a second step, the question of EOP in retirement is elaborated. We begin with a non-parametric test for stochastic dominance at first order, which is applied to the EOP framework as in Lefranc et al. (2009, 2008); Trannoy et al. (2010). This approach compares the cumulative distribution of the outcome retirement age, conditional on specific individual background characteristics or "circumstances" in the terminology of Roemer (1998). We then proceed to a decomposition as established by Blinder (1973) and Oaxaca (1973). This technique allows us to infer on how much of the difference in retirement ages between manual and non-manual workers is due to circumstances. Finding an answer to this question is crucial when evaluating policy designs that may involve benefit reductions for early retirees.

Our results indicate that the estimated hazard profile of non-manual workers is about 20% lower compared to individuals with physically demanding occupations for the age group 55 to 65. Moreover, non-parametric tests for stochastic dominance at first order indicate that the distribution of retirement age differs significantly between individuals across circumstances. Most importantly, the Blinder-Oaxaca decomposition suggests that circumstances explain at least one third of the observed differences in the retirement age between workers of different degrees of physical job demands. This finding is important because it indicates that a considerable part of differences in retirement age is predetermined and thus not subject to individual choice.

The remainder of this paper is structured as follows. Section 4.2 reviews previous research on EOP, discusses the ambiguity in the evaluation of early retirement as a "good" or a "bad" and provides an overview on the employment behavior of older workers. Section 4.3 describes the data and sample construction. Section 4.4 provides the empirical analysis, quantifies differences in retirement age between manual and non-manual workers and attributes these differences to circumstances and effort/luck in a corresponding decomposition. Section 4.5 concludes.

4.2 Equality of Opportunity and Retirement

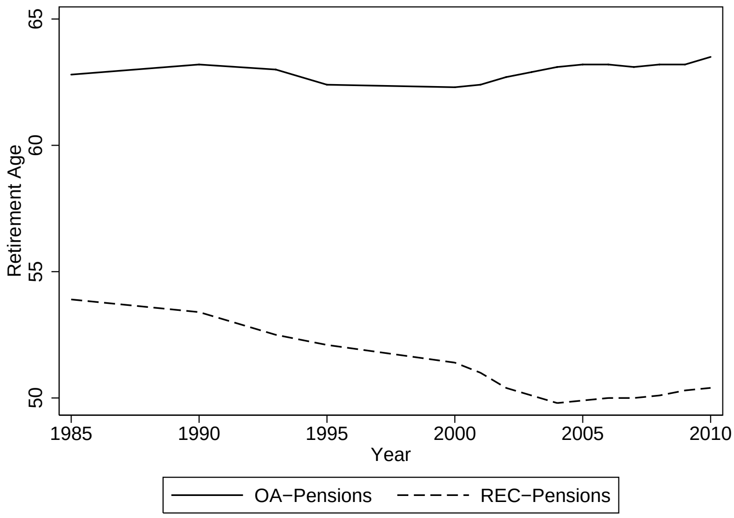
Modern egalitarian views such as expressed in Rawls (1971); Cohen (1989); Fleurbaey (1995a,b) postulate that, instead of equality in outcomes, distributive justice only requires equality of opportunity in achieving those outcomes. The recent economic literature usually follows the terminology as introduced by Roemer (1998), according to which individual outcomes are generated by two fundamental determinants: "circumstances" and "effort", which are defined to be orthogonal. While circumstances reflect background characteristics for which an individual cannot be held responsible, differences in outcome due to effort are considered a legitimate source of inequality. Consequently, given equal circumstances, all remaining differences in outcomes are subject to personal responsibility. Lefranc et al. (2009) state that no consensus has been reached so far on how opportunities are precisely defined. They provide an extension of the EOP framework by introducing luck as an additional determinant of individual outcome. Lefranc et al. (2009) conclude that luck is a legitimate source of inequality in outcomes, as long as it is not correlated to circumstances and is thus "even-handed".

A large body of recent economic literature on EOP has emerged, with numerous applications to income distributions (e.g. Devooght, 2008; Lefranc et al., 2008, 2009; Aaberge et al., 2011) and health (e.g. Jusot et al., 2013; Fleurbaey and Schokkaert, 2009; Trannoy et al., 2010). In the present paper, we provide the first application of the EOP framework to inequality in retirement age. Specifically, we investigate the extent to which circumstances are mediated through physical demands of occupations to the age of retirement. This question is often raised in the public debate, where early retirement of manual workers is frequently considered to be "unfair" because this usually implies a reduction in social security wealth.

Whether early retirement is a "good" or a "bad" is not unambiguous and deserves some further discussion. In previous applications of EOP, a natural ordering of the outcome of interest is straightforward, i.e. "more income is better than less income" or "good health is better than poor health". Our research question differs from former applications in a sense that such an ordering is not as straightforward for the retirement age; it is difficult to say whether early retirement is good or bad. We take this puzzle as a motivation to briefly outline the view which is taken in this paper. Traditionally, retirement decisions are viewed to be determined by a combination of individual preferences for consumption and leisure and incentives set by the social security system (see e.g. Weiss, 1972; Sheshinski, 1978 for early contributions). Clear consequences of early retirement are an increase in leisure time (less work time) and a reduction in consumption (due to reduced income from labor and/or social security), and thus retirement is an issue of labor supply (Hurd, 1990). Beyond individual preferences for leisure and consumption, retirement may interact with subsequent phenomena that either support or prevent an individual to live longer. Retirement may relieve individuals from work-related stress with a positive impact on the remaining years to live, but empirical evidence suggests that a cognitive decline sets in after retirement (see e.g. Rohwedder and Willis, 2010; Bonsang et al., 2012). Moreover, according to Hernaes et al. (2013) no causal link between retirement age and mortality can be established. Instead, recent work by Giesecke and Schnabel (2014) indicates a strong selection into specific retirement ages, where the type of individual to retire at a certain age differs substantially by characteristics that are strongly correlated to mortality, such as health related behaviors and wealth. Aside from labor supply, employer behavior may be responsible for the early termination of employment contracts if demand is weak and layoffs are necessary (Hutchens, 1999). In this context, demand-sided factors may induce early retirement even if employees wish to retire later (Hakola and Uusitalo, 2005).

The evaluation of early retirement is obviously an intricate task because a complex mix of "goods" and "bads" needs to be taken into account. For example, an individual with a strong preference for consumption may want to work longer and retire later but may be forced to behave differently due to plant closure or health issues. In the context of this paper, our interest in early retirement refers to the case where individuals retire early for reasons that are correlated to physical demands of occupations. Specifically, we calculate the difference in mean retirement age between manual and non-manual workers to assess the proportion of this difference which is due to individual background characteristics. Therefore, our view on early retirement focuses on its adverse effects, because it reduces retirement benefits and thus social security wealth in a situation where postponed retirement would avoid a decline in retirement income but is either difficult or impossible to realize.

Figure 4.1: Development of Average Retirement Age in Germany



Source: German Federal Pension Insurance (2013). Retirement ages previous to 1993 are for former West Germany only, while all subsequent values are reported for reunified Germany.

Recent retirement patterns for Germany suggest that the average retirement age increases. However, Figure 4.1 indicates that this trend is not quite the same for old age pensions (OA-pensions) and reduced earnings capacity pensions (REC-pensions).[25]

Apparently, there is a large difference in the average retirement age between OA-pensions and REC-pensions. Most notably, while the average retirement age for OA-pensions exhibits an upward trend at least after the introduction of actuarial adjustments in the late 1990s, the retirement age for REC-pensions declined. These differences are substantial, which needs some explanation. First, while REC-pensions do not necessarily coincide with physically demanding occupations, eligibility for REC-pensions is usually due to poor health, which itself is expected to be positively correlated to physically demanding occupations. Thus, manual workers should be largely over-represented in the group of individuals that receive REC-pensions. Second, selection into REC-pensions may have changed in the course of a large decrease in the total number of entries into REC-pensions (German Federal Pension Insurance, 2013), leaving a sample of the "worst cases" who retire earlier on average. Consequently, the diverging pattern of average retirement ages for OA-pensions and REC-pensions in Figure 4.1 has important implications for the present study as it provides a first hint of differences in the retirement age between manual and non-manual workers. Subsequently, these differences will be analyzed in further detail.

4.3 Data and Sample Construction

The empirical analysis of this paper is based on data from the German Socio-Economic Panel (SOEP) for the waves 1984 to 2012. The SOEP is a representative panel study for Germany which annually interviews households and its individual members since 1984. The survey started with about 6,000 interviewed households per year and comprises about 12,000 households per year since 2000 (see Haisken-DeNew and Frick, 2005). As the focus of this study is on transitions into retirement, we use individual retrospective calendar data on employment spells as provided by the SOEP.[26]

[25]In contrast to old age pensions which are available after age 60 depending on the eligibility type, reduced earnings capacity pensions are available at any age before 60, once the corresponding medical indication has been assigned.

[26]Taking into account retrospective calendar records where individuals are asked to report their labor force participation from the previous year, we effectively draw on information until 2011.

In order to analyze labor force transitions of older workers we restrict our sample to individuals aged 40 and older. For those individuals, who meet this restriction in the observation period from 1984 to 2011, we construct spells with respect to four defined states of labor force participation. Specifically, we distinguish spells of (i) full-time employment, (ii) part-time employment, (iii) unemployment and (iv) retirement. Our primary sample provides 13,304 total transitions from 17,594 individual spells as reported in Tables 4.A1 and 4.A2 in the Appendix.[27] Central to our analysis is the number of total transitions into retirement from all other states, which amounts to 3,036.

4.4 Empirical Analysis

4.4.1 Retirement across Physical Job Demands: Evidence from Duration Models

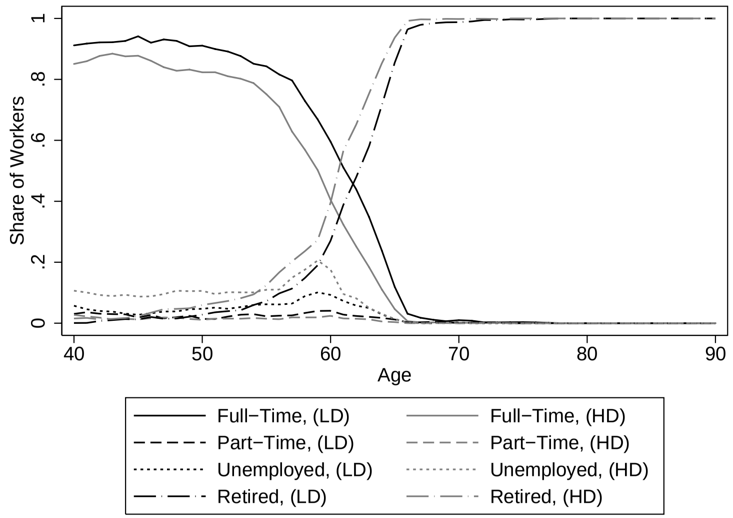
To classify individuals by the physical demands of their reported occupation, we make use of the International Standard Classification of Occupations from 1988 (ISCO 88). This classification serves to categorize physical demands on a 1-10 ordinal scale for physical job demands as constructed by Kroll (2011).[28] Using this index, we categorize individuals into a group of low physical job demands (index values 1-5, abbreviated as LD and also referred to as manual workers), and a group of high physical job demands (index values 6-10, abbreviated as HD and also referred to as non-manual workers). Figure 4.2 distinguishes between low physical demands (LD) and high physical demands (HD) in occupations and shows how shares of individuals in defined labor force states evolve over age separately for the two sexes.

According to Figure 4.2, manual workers both exit full-time employment and enter retirement at lower ages compared to non-manual workers. Exits from full-time employment increase substantially after age 55. Moreover, retirement predominantly takes place between age 55 and 65, which is strongly

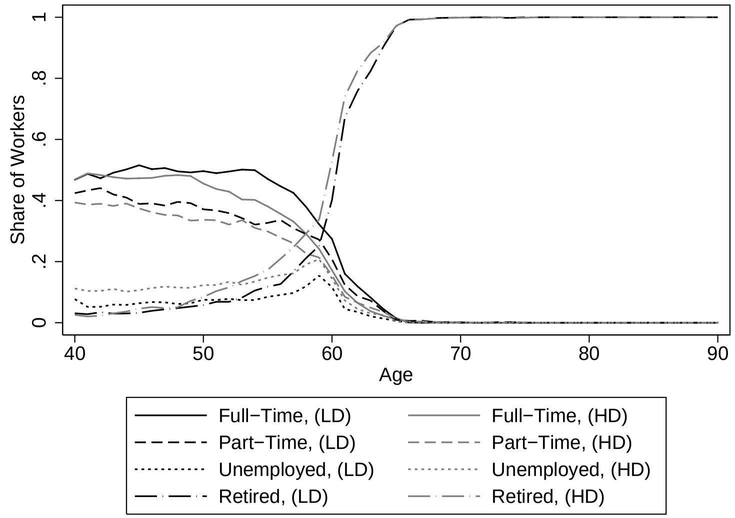
[27] Note that depending on the state of departure and the state of destination for specific transitions, the number of observations varies considerably in subsequent empirical investigations and is thus lower compared to the primary sample.

[28] The scale was developed using data from a large-scale representative survey for Germany from 2006, which collected information on workplace characteristics such as job requirements, main tasks, working conditions and job demands.

Figure 4.2: Distribution of Labor Force States across Age by Occupational Types



(a) Male



(b) Female

High and low (physical) demands are abbreviated by HD and LD respectively.

driven by social security legislation. Aside from social security legislation, retirement patterns as displayed in Figure 4.2 capture a rather broad situation where potentially unobserved factors such as mutual agreements or social norms do play a role; such aspects are taken into account in the subsequent duration model. Finally, the figure indicates that full-time employment is more prevalent among male individuals while female individuals work more frequently in part-time employment. As we are primarily interested in labor force dynamics that document transitions into retirement, the observed patterns in Figure 4.2 justify that we restrict our analysis to individuals aged 55 to 65 (equivalent to 11 years or 132 months) in subsequent regressions, while capturing all relevant transition dynamics.

Further descriptive evidence for systematic differences in retirement across physical demands is attained from discrete time duration models.

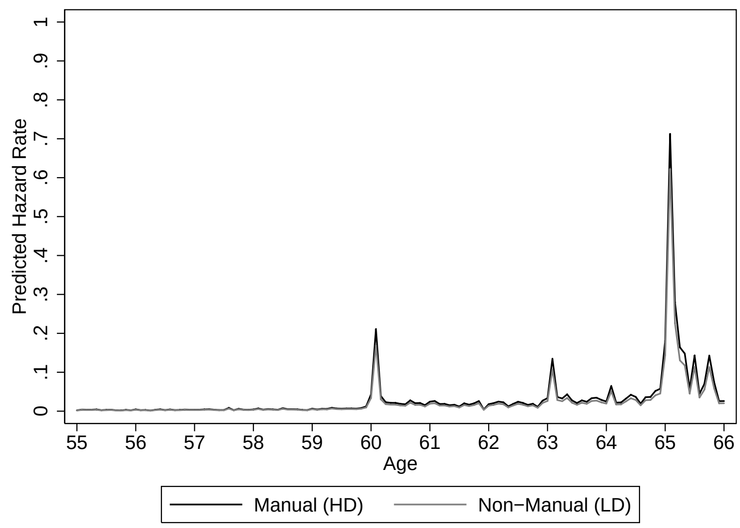
Table 4.1: Discrete Time Duration Models: Differences Estimation for Transitions into Retirement by Physical Demands.

	Compl. Log-Log		Multinomial Logit	
	coeff.	s.e.	coeff.	s.e.
Binary Case				
Manual	.247	(.041)	–	–
MNL (Coefficient w.r.t. Outcome):				
Manual (Part-Time)	–	–	–.417	(.098)
Manual (Unemployment)	–	–	.765	(.075)
Manual (Retirement)	–	–	.277	(.063)
+ Duration-Dummies	Yes		Yes	
Predicted Transition Rate (%)	3.00		3.19	
Obs.(Person-Month-Obs.)	5146(210770)		3630(134960)	

Reported values are estimated coefficients. For the multinomial logit, coefficients are estimated with respect to full-time employment as base category. Precise definition of labor force states for all ages requires more information and thus the multinomial logit has more missings, i.e. fewer observations.

Table 4.1 reports the results for two discrete time duration models, i.e. a binary complementary log-log model assuming discrete time proportional hazards and a multinomial logit assuming discrete time proportional odds. The binary model describes transitions into retirement from any of the other labor force states, while the multinomial logit model allows for transitions into all different states, i.e. full-time employment, part-time employment, unemployment and retirement. Duration time enters both models in its most flexible form, where 132 dummies capture variation for each month (i.e. age) and thus no functional form assumption on the baseline hazard is imposed. The variable “Manual” discriminates between occupations with high physical demands (Manual = 1) and low physical demands (Manual = 0). The estimated coefficient on “Manual” is positive for the binary proportional hazards model, which indicates that manual workers have a larger hazard to enter retirement on average. In the multinomial logit model, the coefficient for “Manual” is as well positive and of similar magnitude for transitions into retirement (i.e. the respective outcome). This result is important to underline what we have found in the binary case, because the multinomial model allows for transitions into other states and is thus more general.

Figure 4.3: Predicted Hazard Profile for Retirement Entries by Physical Demands



High and low (physical) demands are abbreviated by HD and LD, respectively.

Figure 4.3 reports hazard profiles for exits into retirement as predicted from the complementary log-log model. Hazard profiles in Figure 4.3 show that retirement entries accumulate around age 60, 63 and 65, as suggested by the respective peaks in predicted hazard rates. This pattern is a perfect projection of the German social security legislation, where eligibility for specific types of old-age pensions are achieved at these specific ages.[29] Similar patterns for Germany with spikes at age 60, 63 and 65 have been recognized in previous studies, such as Börsch-Supan and Schnabel (1998); Börsch-Supan (2000). Moreover, the hazard profile for non-manual workers (average hazard to enter retirement is 2.5%) is systematically lower compared to manual workers (average hazard to enter retirement is 3.2%) for the observed ages. Thus, the hazard profile of non-manual workers is about 20% lower compared to individuals with physically demanding occupations.

4.4.2 Retirement and Individual Background: Evidence from Non-Parametric Tests

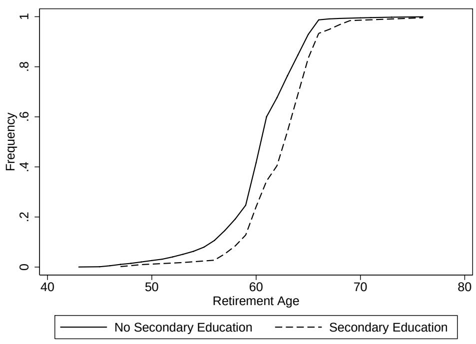
A simple test of equality of opportunity is to check whether the retirement age distributions differ between individuals with different circumstances. If so, this points at differences in the timing of retirement which are beyond individual responsibility. Assume two retirement age distributions A and B and their cumulative distribution functions (CDF) $F_A(r)$ and $F_B(r)$. Then, A dominates B at first order if and only if $F_A(r) \leq F_B(r)$ for any retirement age $r_j = \{r_1, r_2, ..., r_k\}$. We apply the first order stochastic dominance concept using three different categories of circumstance variables to divide our sample, namely, personal characteristics, socio-economic background, and urbanisation of area of residence during childhood. We test for equality of distributions conducting Kolmogorov-Smirnov tests of equality of distributions.

4.4.2.1 Dominance According to Socio-Economic Background

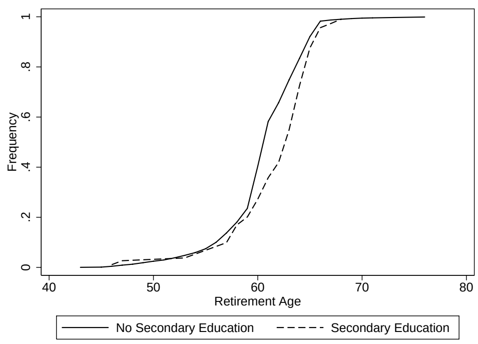
We use parental educational attainment as a proxy for socio-economic background. Figure 4.4 plots the CDFs of retirement age for individuals whose fathers achieved a secondary education degree (i.e. German “Abitur” or “Fa-

[29]Note that eligibility refers to both early and normal retirement ages. The normal retirement age for a regular old-age pension was 65 until December 2011, where our observation period ends. Besides regular old-age pensions, other types are available such as pensions for unemployed, women, long-term insured and severely disabled individuals. These types of old-age pensions are typically available “early”, i.e. at age 60 or 63.

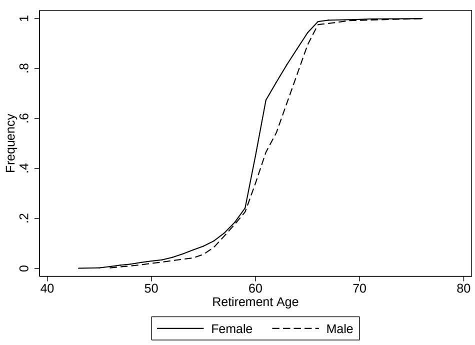
Figure 4.4: Cumulative Distribution Function of Retirement Age by Circumstances



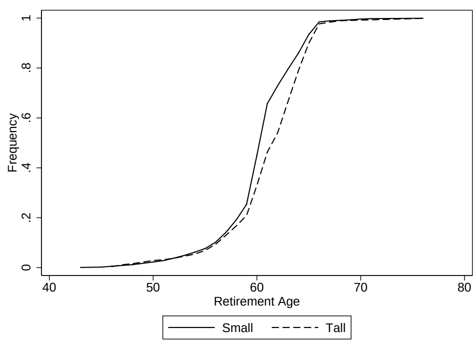
(a) Father's Schooling



(b) Mother's Schooling



(c) Sex



(d) Body Height

(e) Urbanisation

Graphs are smoothed by averaging cumulative distributions at the level of age in years.

choberstufe") and individuals whose fathers did not achieve such a degree (panel (a)). With an exception for the tails, the CDF of individuals born to highly educated fathers lies below the CDF of individuals born to less educated fathers across the complete distribution of retirement age. The largest difference between highly and less educated fathers is 0.286, the largest difference between less and highly educated fathers is zero. The distributions are tested to be significantly different from each other. Hence, the CDF of individuals born to highly educated fathers first order dominates the CDF of individuals born to less educated fathers.

Figure 4.4 illustrates the analogous CDFs with respect to maternal educational attainment (panel (b)). The overall picture is similar to the one based on paternal education. Again, the CDFs are significantly different with the CDF of individuals born to mothers with a secondary degree lying below the CDF of individuals born to mothers without such a degree. Both the findings based on paternal and maternal education indicate that equality of opportunity is violated.

4.4.2.2 Dominance According to Personal Characteristics

We consider sex and body height as personal characteristics, which are attributes that individuals clearly cannot be held responsible for. Figure 4.4 illustrates CDFs of age at retirement by specific circumstances, where sex is displayed in panel (c). While the overall shapes of the distributions appear to be quite similar, the lines diverge between ages 60 and 65, implying that females enter retirement earlier than males. Table 4.2 reports all results of according stochastic dominance tests for equality of distribution by specific circumstances. The largest difference between the distribution functions is 0.215. The maximum difference between females and males is negative (-0.004), which indicates that the CDF of males lies below the CDF of females at any possible retirement age. The small p-value for the combined test indicates that the distributions of males and females are significantly different. Hence, the CDF of males first order dominates the CDF of females. However, differences in retirement age by sex cannot generally be considered as illegitimate inequalities since males and females are subject to different retirement rules and social norms and are therefore of limited comparability.

In contrast, retirement rules are independent of body height. Height is a proxy for childhood health as well as height-related differences in self-perception and the perception by others which may both provoke differences in career

Table 4.2: Tests for Stochastic Dominance at First Order: Differences in Distribution of Retirement Age by Circumstances

Test	Maximum Difference	p-value	Corrected p-value
By Father's Schooling			
No Secondary Education	0.286	0.000	
Secondary Education	0.000	1.000	
Combined	0.286	0.000	0.000
By Mother's Schooling			
No Secondary Education	0.273	0.000	
Secondary Education	-0.024	0.000	
Combined	0.273	0.000	0.000
By Sex			
Female	0.215	0.000	
Male	-0.004	0.185	
Combined	0.215	0.000	0.000
By Body Height			
Small	0.209	0.000	
Tall	-0.009	0.001	
Combined	0.209	0.000	0.000
By Urbanisation			
Rural	0.078	0.000	
Urban	-0.016	0.000	
Combined	0.078	0.000	0.000

chances. In a comparison of CDFs by body height, first order stochastic dominance would clearly imply the presence of inequality of opportunity in the timing of retirement. Figure 4.4 (panel d) illustrates the distribution functions for individuals who differ by body height, where the sample mean of body height has served as a threshold to assign individuals to the two groups. Again, the distributions appear very similar with a divergence between ages 60 and 65; the maximum difference amounts to 0.209. The largest difference between small and tall individuals is negative (-0.009), which implies that the CDF of tall individuals is below the CDF of small individuals at any retirement age. According to the combined test, the CDFs are significantly different from each other. The conditions for first order stochastic dominance are fulfilled, suggesting the presence of inequality of opportunity in favour of tall people.

4.4.2.3 Dominance According to Urbanization in Childhood

In contrast to personal characteristics and socio-economic background, visible differences in retirement age are not as distinctive when individuals who grew up in areas of different degrees of urbanization are compared. Figure 4.4 suggests that the CDFs about coincide at most retirement ages (panel (e)). There is a slight divergence between ages 60 and 63. The stochastic dominance test reveals that at any retirement age the CDF of individuals who grew up in urban areas lies below the CDF of individuals who grew up in rural areas. The largest difference amounts to 0.078 and the CDFs are estimated to be significantly different. Hence, although the visible differences appear rather small when comparing CDFs by urbanization, the urban-CDF dominates the rural-CDF at first order, suggesting inequality of opportunity in age at retirement as well for this characteristic.

4.4.3 Decomposition of the Difference in Retirement Age by Physical Demands: Circumstances versus Effort and Luck

Up to this point, we focused on retirement inequalities between different degrees of physical job demands (duration models) and inequality of opportunity in retirement (tests for stochastic dominance) separately. The ultimate aim of this study is, however, a combination of the two, i.e. an evaluation of the proportion of differences in retirement age between manual and non-manual workers that are attributed to circumstances as distinguished from effort and luck. In order to estimate this proportion, we conduct the decomposition method proposed by Blinder (1973) and Oaxaca (1973) based on the following linear model:

$$RA_g \quad = \quad \mathbf{X_g}'\beta_g + \varepsilon_g \,, \tag{5}$$

where $g = (N, M)$ denotes the groups of non-manual workers N and manual workers M, respectively, RA denotes individual retirement age expressed in years whereas varying by month, $\mathbf{X}$ contains a constant and a range of circumstance variables, and ε is an error term. The mean difference in retirement age, which is given by

$$\begin{aligned} \Delta_{NM} &= E(RA_N) - E(RA_M)\,, \\ &= E(\mathbf{X}_N)'\beta_N - E(\mathbf{X}_M)'\beta_M\,, \end{aligned} \tag{6}$$

where $E(\beta_g) = \beta_g$ and $E(\varepsilon_g) = 0$ by assumption, can generally be decomposed into

$$\{E(\mathbf{X}_N) - E(\mathbf{X}_M)\}'\beta^* + \{E(\mathbf{X}_N)'\{\beta_N - \beta^*\} + E(\mathbf{X}_M)'\{\beta^* - \beta_M\}\}\,, \tag{7}$$

such that the first summand refers to the "explained" part and the second summand represents the "unexplained" part of the outcome difference between the two groups. The literature proposes several variants of the decomposition in Equation (7) by determining β^* in different ways. Specifically, β^* can be defined as a weighted average of the group coefficient vectors:

$$\beta^* = \mathbf{\Omega}\beta_N + (\mathbf{I} - \mathbf{\Omega})\beta_M\,, \tag{8}$$

where $\mathbf{\Omega}$ denotes a weighting matrix and $\mathbf{I}$ is an identity matrix. $\mathbf{\Omega} = \mathbf{I}$ and $\mathbf{\Omega} = \mathbf{0}$ represent the special cases proposed by Oaxaca (1973) and Blinder (1973). These two decompositions provide the extreme cases of assigning the complete interaction effect between endowments and coefficients either to differences explained by endowments ("explained" part) or to differences explained by coefficients ("unexplained" part), respectively. Several authors have suggested alternatives leading to decompositions in between. Neumark (1988) suggests to estimate a pooled model over both groups to infer β^*. Cotton (1988) proposes to choose $\mathbf{\Omega} = s\mathbf{I}$, where s denotes the sample fraction of group N.

In Section 4.4, we report estimates of all four described variants of Equation (7), while our preferred decompositions are those proposed by Neumark (1988) and Cotton (1988) since they provide convincing strategies of achieving a result between the extreme cases. As the circumstances we include in our model are only a subset of all relevant circumstances (we do not observe talent, for example), the estimate of the explained part of Equation (7) must be interpreted as a lower bound of the contribution of circumstances to the retirement differential between manual and non-manual workers. The unexplained part is to be interpreted as arising from differences in the coefficients as well as differences in unobserved predictors, such as effort and luck (Jann, 2008).

4.4.4 Decomposition Results

Table 4.3 reports the results from the Oaxaca-Blinder decompositions. While on average both considered occupation types retire in their early 60s, it is predicted that manual workers retire 1.1 years earlier than non-manual workers. The decomposition results shown in the lower panel of Table 4.3 suggest that endowments explain between 0.25 and 0.46 years of this differential, depending on the choice of $\mathbf{\Omega}$, which is equivalent to a contribution of between 23.2% and 42.3%. When considering the results based on the pooled model over groups, 0.44 years of the differential (39.8%) are explained, while 0.66 years (60.2%) remain unexplained. Finally, the results for the decomposition proposed by Cotton (1988), suggest that 0.37 years of the difference (33.5%) are attributed to differences in endowments.

Table 4.3: Oaxaca-Blinder Decomposition

Non-Manual Workers:				
Average retirement age			61.42	
Obs.(Person-Month-Obs.)			499(82185)	
Manual Workers:				
Average retirement age			60.33	
Obs.(Person-Month-Obs.)			539(97363)	
Difference			1.099	
Decomposition:				
	$\mathbf{\Omega = I}$	$\mathbf{\Omega = 0}$	Pooled	Cotton
Explained	0.255	0.465	0.437	0.369
	23.18%	42.30%	39.77%	33.55%
Unexplained	0.844	0.634	0.662	0.730
	76.82%	57.70%	60.23%	66.45%

Decomposition of the difference in mean retirement age in years between non-manual and manual workers.

The findings from the latter two decompositions indicate that circumstances account for at least 33.5% to 39.8% of the differences in retirement age between manual and non-manual workers, while at most 60.2% to 66.4% can be attributed to effort and luck. The estimated contribution of circumstances can be considered as a lower bound since the range of circumstances accounted for is unlikely to be complete (e.g., we do not observe innate talents) and, in addition, circumstance effects are likely to be mediated partly through characteristics not included in the model which are potentially influenced by individual choices, such as educational attainment, occupational choice, or health behaviors, which cannot be distinctively classified as circumstance or effort variables. In sum, the decomposition results suggest that circumstances explain at least one third of the observed differences in the retirement age between workers of different degrees of physical job demands, which indicates that a considerable part of differences in retirement ages are predetermined and thus not subject to individual choice.

4.5 Conclusion

The purpose of this paper is to quantify differences in the retirement age between manual and non-manual workers and to evaluate these differences with respect to EOP. The focus is on the question how individual background during childhood transmits through physical demands of occupations on retirement ages.

Individual retrospective data from the SOEP are used to analyze labor force dynamics over the years 1984 to 2011. Discrete time duration models are estimated in the most flexible version, where age (i.e. duration time) enters the model on a monthly level and thus accounts for variation in the relevant range from age 55 to 65. The estimated hazard profile of non-manual workers is about 20% lower compared to individuals with physically demanding occupations. Non-parametric tests for stochastic dominance at first order indicate that the distribution of retirement age differs significantly between individuals across circumstances. However, the ultimate aim of this study is an evaluation of the proportion of differences in retirement age between manual and non-manual workers that are attributed to circumstances as distinguished from effort and luck. The result from a Blinder-Oaxaca decomposition suggests that circumstances explain at least one third of the observed differences in the retirement age between workers with different degrees of physical job demands. The result is a lower bound, as we do not observe the full set of individual circumstances. This finding is important because it indicates that a considerable part of differences in retirement age is predetermined and thus not subject to individual choice.

Retirement decisions are complex. Aside from general preferences for consumption and leisure, several aspects such as health-related behaviors, wealth and occupational sorting play a role in retirement choices, and most of these factors are not exogenously determined. Beyond individual choice, employer behavior and the availability of retirement benefits (i.e. social security legislation) influence the observed outcomes for retirement ages. Thus, retirement decisions are influenced by a number of factors which in sum do not clearly indicate whether retiring early or working longer is more desirable from an individual point of view. In the present paper, we apply an approach that carries on the EOP literature and decomposes differences in the retirement age into individual responsibility (i.e. effort and luck) and personal background (i.e. circumstances). Thus, the relevant quantity is the share of individual background characteristics that transmits through physical demands of occupations to retirement ages. By nature, individual circumstances are predetermined to any endogenous decision which individuals could be held responsible for. Early retirement usually implies a reduction in social security wealth and to this end, differences in retirement age between subgroups are of economic relevance. When raising the normal retirement age, policy makers must be aware of potential disadvantages in terms of reduced benefit entitlements for manual workers. While the interpretation of our result is highly normative, it helps to structure thoughts in a debate, where early retirement of manual workers is often considered to be “unfair”.

Appendix 4

Table 4.A1: Distribution of Individual Transitions across States

Origin	Transition into State				Total Net Transitions	Total
	(1)	(2)	(3)	(4)		
(1) Full-time Employment	559,944	1,656	2,977	1,323	5,956	565,900
	(98.95)	(0.29)	(0.53)	(0.23)	(1.05)	(100.00)
(2) Part-time Employment	1,595	177,439	727	583	2,905	180,344
	(0.88)	(98.39)	(0.40)	(0.33)	(1.61)	(100.00)
(3) Unemployment	2,294	738	94,057	1,130	4,162	98,219
	(2.34)	(0.75)	(95.76)	(1.15)	(4.24)	(100.00)
(4) Retirement	142	122	116	762,117	380	762,497
	(0.02)	(0.02)	(0.02)	(99.94)	(0.06)	(100.00)
Total					13,403	1,606,960

Absolute transitions are reported; relative shares in parentheses. “Net transitions” refer to all transitions into respective other states.

Table 4.A2: Number of Spells per Individual

Number of Spells	Number of Individuals	Per Cent
1	12,132	68.96
2	2,415	13.73
3	1,314	7.47
4	620	3.52
5	463	2.63
6	181	1.03
7	169	0.96
8	88	0.50
9	71	0.40
10 or more	141	0.80
Total	17,594	100.00

5 Early Life Interventions: Evidence from the Swedish Maternity and Infant Health Care Program

Abstract

This paper estimates the effects of the universal introduction of free ante- and neonatal health care in Sweden that started in 1938. Based on data from official statistics we measure the short-run effects on fertility, infant mortality, stillbirths and maternal mortality. Using the Swedish Death Index delivering death data on a majority of the treated individuals who died until 2013, we estimate mortality effects at the individual level. Finally, based on individual-level data from the 1980s, we estimate long-run effects on schooling, wage and a variety of self-assessed health variables. We find negative mortality effects of both pre- and postnatal care. There is some evidence that these effects are increasing in age. Prenatal care has beneficial effects on some health outcomes. Further, our estimates suggest considerable wage gains from prenatal care. We find some evidence for beneficial effects in maternal mortality, no effects on fertility and stillbirths, and the effects on education are ambiguous.

5.1 Introduction

In this paper, we estimate effects of a universal ante- and neonatal health care program in Sweden on health, mortality, fertility and socioeconomic status. While a bunch of recent literature points at robust and lasting gains from early health interventions,[30] our knowledge of the effectiveness of public health programs targeting infants is limited to date, particularly concerning long-term effects. Recent contributions show that an availability of publicly provided health services in early years can play a substantial role in child development even in developed countries (e.g., Bharadwaj et al., 2013; Wüst, 2012; Almond et al., 2010; Daysal et al., forthcoming; Almond and Currie, 2011; Jewell and Triunfo, 2006; Rous et al., 2004). Such services include both safe and clean birth delivery under the supervision of qualified medical staff, but positive health effects may also relate to maternal advice and health surveillance during pregnancy and after birth.

Our study makes important contributions over the existing literature. It extends the scarce evidence on long-term effects of universal early-life health interventions. The effects measured by existing studies are often confounded since health care policies are in many cases targeted at disadvantaged subgroups. Our paper is among the first to study the causal effects of a universal early-life health intervention on adult outcomes. One exception is Bütikofer et al. (2014) who find positive effects of the introduction of mother and child health care centers in Norway on education and earnings. A second exception is the study of Hjort et al. (2014) who find negative effects of a universal infant health intervention on mortality, hospitalization and cardiovascular disease.

Furthermore, we add to the growing literature following Barker (1990) which points at early-childhood conditions predetermining later-life health and socioeconomic conditions (e.g., Almond, 2006; Black et al., 2007). This literature suggests a widening of the social gradient in health over the life cycle, explained theoretically by health impairments accumulating over time (Currie and Stabile, 2003; Case et al., 2002). Supplementing this, our evidence indicates that the benefit from early-life health interventions is increasing in age. The topic is of ultimate political importance, as it suggests that the ori-

[30] Currie and Rossin-Slater (2015) survey the evidence.

gins of existing social inequalities may consist in the health conditions during pregnancy and early childhood.

Finally, we evaluate an early-life intervention in a context that is comparable to the situation in many developing countries today. Historical Sweden in the 1930s exhibited stillbirth rates, infant mortality rates and maternal mortality rates of similar size as many developing countries today.[31] The historical intervention we evaluate was conducted many decades ago and therefore allows us to measure long-term effects giving important implications to political choices taken nowadays in poor countries.

The introduction of free ante- and neonatal care services provided to all expectant mothers and infants in Sweden started in 1938. By 1960, the prenatal care utilization rate had reached about 85%, while in terms of postnatal care universal coverage was achieved already during the 1950s. The intervention included a combination of health checks for expectant mothers and infants conducted by physicians and nurses at health care centers as well as educating parents about health, parenting skills, and nutrition through home visits, executed by specially-trained nurses and midwives. Home-visiting initiatives are among the most promising policies designed to improve early-life health (Currie and Rossin-Slater, 2015).

We exploit the regional variation in the densities of health care centers during the implementation process to identify effects of pre- and postnatal care utilization. Intervention data for 24 counties and the City of Stockholm is used for the years 1940 to 1950. We measure short-run effects on fertility, infant mortality, stillbirths and maternal mortality using data from official Swedish statistics. Based on the Swedish Death Index, we estimate mortality effects at the individual level. The Death Index comprises the majority of deaths that occurred in the treated birth cohorts until 2013. Finally, using individual-level data from the 1980s, we estimate long-run effects on schooling, wage and a variety of self-assessed health variables.

We find negative effects on mortality of both pre- and postnatal care. Using aggregated data we find that an increase in the prenatal care utilization rate

[31] Current rates for developing countries can be found e.g. in Cousens et al. (2011, stillbirth rates), World Bank (2015b, infant mortality rates) and WHO (2014, maternal mortality rates). Compare Figures 5.A1, 5.A2, 5.A5 and 5.A6 in this chapter for historical descriptives for Sweden.

of 1 percentage point decreases the infant mortality rate by 0.9%. Reduced form regressions based on the Swedish Death Index suggest significant negative effects of different types of child care facilities that increase when the considered time horizon is extended. For instance, increasing the density of child care centers of type I[32] by one standard deviation decreases the probability of dying before age 1 by 0.1%, while it decreases the probability of dying before age 63 by 0.5%. A standard deviation increase in child care stations reduces the probability of dying before age 63 by even 0.8%. A difference-in-difference comparison of survival rates shows a rise in the intervention effects beyond age 50 and suggests that postnatal care affects mortality only beyond age 50, which is in line with the findings of Bütikofer et al. (2014). Reduced form regressions for health and labor income indicate significant effects of prenatal care. In particular, the densities of maternity centers of types I and II have significant negative effects on the probabilities to be disabled and to be diagnosed with a severe illness. The density of maternity centers of type I further reduces the number of long-term illnesses. With regards to labor income, a one standard deviation increase in the density of maternity centers of type I is measured to increase labor income by 7.6%, while such an increase in the density of maternity stations labor income by even 16.1%. In line with our results, Bütikofer et al. (2014) find that access to free postnatal health care leads to an increase in lifetime earnings and has beneficial effects on health. Finally, we find some evidence for beneficial effects from postnatal care on maternal mortality from eclampsia and from childbed fever after childbirth. Confirming Bütikofer et al. (2014), there are no effects on fertility. Also, there is no evidence for effects on stillbirths, and the effects on education are ambiguous.

The remainder of the paper is structured as follows. Section 5.2 outlines the national rollout of free ante- and neonatal care in Sweden. Section 5.3 describes the data sources exploited in the empirical analysis and presents descriptive statistics. Our estimation strategy is outlined in Section 5.4. In Section 5.5 our empirical results are presented. Section 5.6 concludes.

[32]Health care facilities were either centers of type I, centers of type II or health care stations. They differed by the quality of health care provided and the locations where they were established, as described in detail in Section 5.2.

5.2 The Rollout of Universal Ante- and Neonatal Health Care

The history of the Swedish midwifery system dates back to the 18th century. When in 1751 the first national statistics on maternal mortality revealed a rate of almost 900 deaths per 100,000 live births, the Swedish authorities initiated a system of thorough training and supervision of midwives for the first time (Högberg, 2004). During the 19th century, midwives were deployed particularly in areas with a shortage of midwives. Their overall number was restricted by the fact that initially there was only one midwifery school. When a second school was opened in 1856 and the health authorities started to subsidize some students, the number of midwives increased. Until the 1930s, midwives were in charge of nearly all obstetric care services since home births were the norm and hospital deliveries constituted only a very small percentage of all births (see Pettersson-Lidbom, 2014, for details).

Before the 1930s, a system called *Mjölkdroppen* was in place which relied on visits to physicians and had the purpose of distributing a nutritious cow milk mixture to poor mothers who did not breastfeed their newborns. In addition, there was a system called *Barnavårdscentralsystemet* with a focus on outreach activities to increase awareness, home calls by nurses and center-based health check-ups for children and mothers (Wallgren, 1935, 1936). Both institutions were established only in the larger cities and exhibited no eligibility restrictions, although the former aimed at low-income families. Still, none of the systems covered the entire population, not even in the cities.

In the 1920s and early 1930s Sweden experienced stagnating infant and maternal mortality rates. This trend was accompanied by a rapid decline in birth numbers. While the protection of the mother and her child before and after the birth had been legally consolidated long before, it was only when confronted with this situation that Sweden realized a necessity for more intense public support of the individual mother. The 1930s constituted a decade of change for maternity support and early-life health care in several respects. Maternity benefits were introduced in 1931 to compensate mothers enrolled in a sickness fund or with a low family income for unpaid maternity leave and a midwife's assistance. A statute of 1937 expanded this policy to apply to nearly all mothers-to-be and raised the maternity allowance to cover an estimate of three-fourths of the normal minimum costs connected with childbirth (Wangson, 1938). Additionally, a vast shift from home to institutional deliveries occurred over the decade (Statistics Sweden, various issues).

Simultaneously, the Swedish health system moved towards the universal provision of free maternity and infant health care. In a first step, a field trial was conducted in seven Swedish medical districts from 1931 to 1933.[33] The decision for the national rollout of the program was taken on July 21, 1937, and the reform was gradually implemented from 1938 onwards. The intervention included the setup of maternity and child health care centers all over the country where expectant mothers and infants were to undergo standardized health examinations. This practice was supplemented by home visits executed by nurses and midwives (Ström, 1942).

A central organization of the new ante- and neonatal health care system was complicated by the diversity of local conditions across the country. In cities, towns, municipal communities with densely populated surrounding rural areas or densely populated industrial areas, suitable facilities for the setup of health care centers were already given and qualified staff was available. There were places, however, where institutions exhibiting the preconditions to guarantee the intended extent of health care provision did not exist, as was obviously the case in the pure countryside (Population Commission, 1936).

To overcome these local differences and maximize the possible gains from preventive care, the organization was decentralized to the counties and cities[34] and the local agencies were instructed to implement health care institutions in any of the following forms (Population Commission, 1936):

1. Maternal or child care centers of type I should be introduced in inpatient or children's hospitals, independent maternity and paediatric clinics or in other places where specially trained women or pediatricians worked. They were to be led by specially-trained, licensed physicians or pediatricians, respectively, assisted by a nurse or midwife.

[33]Bhalotra et al. (2014) and Bhalotra et al. (2015) evaluate the field trial in terms of its effects on mortality as well as on academic performance and sickness absence in primary school, respectively.

[34]Several large cities organized the intervention independently of their counties.

2. Maternal or child care centers of type II should be set up in common, purpose-designed premises, under the direction of a licensed physician and with the necessary assistance of a nurse or midwife.

3. Maternal and child health stations should be directed by a physician, usually using his reception facilities, who is assisted by a district nurse or midwife.

As intended, type I centers were introduced in the larger cities. Centers of type II were built up in the cities where specially-trained staff was not available or in very densely populated industrial areas. Maternal and child care stations were opened in smaller communities and rural areas. For the latter no new staff was necessary since the requirements could be met by the physicians in service and the district nurses (Medicinalstyrelsens Förslag, 1935).

The reform was financed by state subsidies to the counties and cities to cover the costs for equipment of the centers and their operating costs. In comparison to the latter, the former were negligible. The operating expenditures consisted of expenses for physicians', nurses' and midwives' services as well as travel expenses for home visits. In 1940, the total intervention costs amounted to 713,885 SEK, Göteborg and Malmö excluded, of which 77% where attributed to the counties and 23% to the cities. At the same time, the annual cost per supervised individual amounted to an average of 10.28 SEK in the counties and 18.29 SEK in the cities[35] (Ström, 1942).

The health checks of the mothers consisted of medical tests complemented with urine albumin tests. Infants were monitored by physicians through examinations at clinics as well as by nurses during their home calls or in special clinics. In the latter case the service consisted mostly of weight control (Ström, 1942).

[35]The difference is partly explained by the more expensive monitoring practice adopted by the cities and the comparably low costs for health care stations in the countryside.

5.3 Data[36]

5.3.1 Data Sources and Sample Restrictions

In the empirical analysis we make use of several aggregated and individual-level data sources. Official medical, population and death statistics deliver data on the numbers of health care facilities implemented, the shares of expectant mothers and infants treated as well as population numbers, numbers of mothers, births, maternal and infant deaths and physicians over the period of treatment implementation (Statistics Sweden, various issues). These data are available for 24 counties and the City of Stockholm.The Swedish census of 1930 delivers data on occupation shares and average income at the parish and the district level, respectively.

These aggregate data are combined with the Swedish Survey of Living Conditions (ULF; Statistics Sweden, 2013). The ULF provides individual information for a random sample representative of the Swedish population. The panel study combines information collected in annual face-to-face interviews and register data. We use data from the waves 1980, 1981, 1988, 1989, 1996, 1997, 2004 and 2005. The ULF database delivers information on educational attainment, wage level and a range of self-reported health variables for the birth cohorts treated by the introduction of free ante- and neonatal health care. This data is matched with the census 1930 via the parish name.[37] Not in all cases a match is found, e.g. due to different spelling of a parish name or different regional divisions. We plan to raise the match rate from currently 88.4% to 100% of all ULF parishes for a future version of the analysis.

Finally, we combine the aggregate data available for 25 regions with the Swedish Death Index, which is provided by the Swedish Genealogical Society (2014). The dataset comprises universal information on all deaths that occured in Sweden from 1901 to 2013. Hence, it provides us with the mortality

[36] Detailed definitions of all variables used in the empirical analysis are given in Appendix Table 5.A1.

[37] For the birth cohorts until 1946, the ULF reports the actual parish of birth (i.e. the place of the hospital) instead of the parish of residence at birth. Because the numbers of institutional deliveries rose sharply during the 1930s, this provides a potential source of measurement error as described by (Fischer et al., 2013). However, as our regressor of interest exhibits a highly aggregated regional level, we consider the measurement error problem as negligible in the context of the present analysis.

information on the great majority of individuals who were born during the introduction of nationwide pre- and postnatal health care and have not survived until the end of 2013. The information in this database stems from official records such as church books.

While the pre- and postnatal care interventions began in 1938, our data on numbers of health care centers and utilization rates starts only in 1940. Because the period up to 1950 exhibited the largest variation in utilization rates we focus on the birth cohorts from 1940 to 1950 in the regressions. Consequently, our aggregated data sample for 25 regions comprises 275 observations. The individual samples are further restricted to persons born in Sweden to guarantee a potential treatment by the interventions. In addition, the ULF sample is restricted to observations without missings on the relevant outcome variables. The final ULF data sample comprises 6,990 observations. The Death Index delivers information on an individual's birth date and place only if the individual died until 2013. Since we intend to measure survival probabilities, we simulate the observations of the survivors based on actual annual birth numbers by county and sex and assign month and day of birth randomly. The resulting sample consists of 228,270 dead and 1,064,151 survivors.

5.3.2 Descriptive Statistics

The boxplots shown in Figures 5.1 and 5.2 illustrate the utilization rates of ante- and neonatal health care in the newly established centers and stations over time. In terms of its median, the proportion of mothers utilizing care rose sharply from about 20% in 1940 to 60% in 1946. Thereafter, it grew rather moderately at a nearly constant rate to roughly 90% in 1960. In comparison, the share of infants monitored grew at a much steeper rate during the early 1940s. While its median had exceeded 90% already in 1946, nearly universal coverage was reached during the 1950s.

Figures 5.1 and 5.2 also reveal the regional variation in utilization rates over time. With regards to maternal care, it remained fairly constant over the observation period, exhibiting only a slight decrease. In contrast, infant supervision rates differed strongly during the early 1940s but shrinked considerably while approaching universal coverage.

Figure 5.1: Health Care Utilization Rate of Expectant Mothers

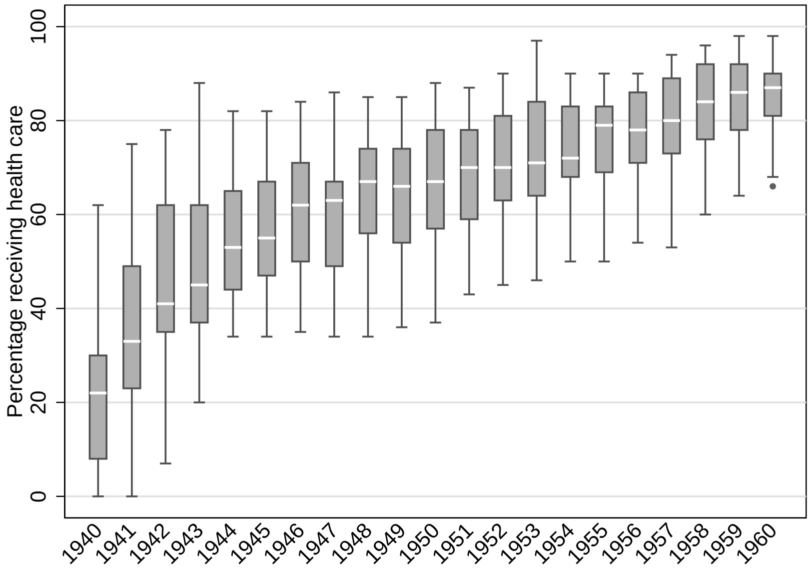
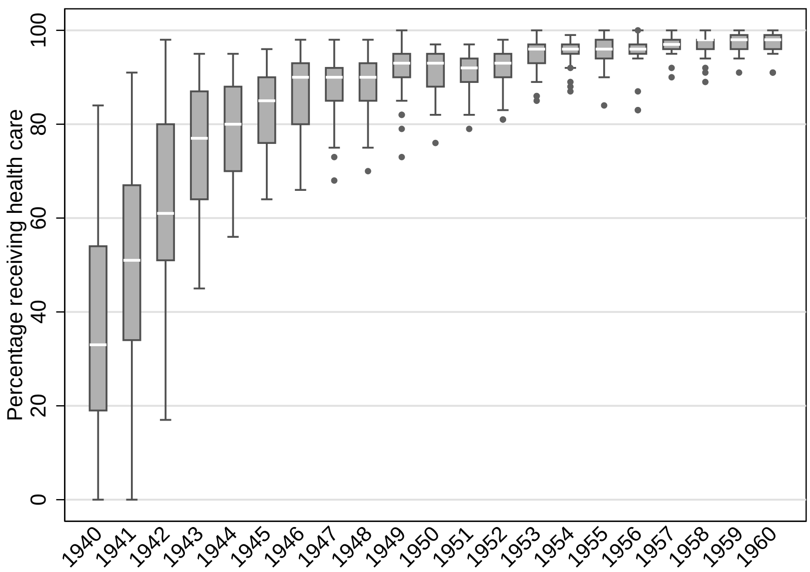


Figure 5.2: Health Care Utilization Rate of Infants



The differences in speed of implementation and variation levels resulted possibly from the way how knowledge of the interventions was spreading. It is likely that at an early state of implementation many women who became pregnant were unaware of the centers' existence and their potential utility. Slow diffusion of information might explain the only gradual uptake of prenatal care under large regional variation. Frequently, mothers learned about the newly available health care only at the time of childbirth at the hospital (Ström, 1942). Under the assumption that a mother follows the professional advise given her by the hospital staff, most mothers will have initiated the receipt of postnatal care for their newborns after delivery, even when they have not utilized prenatal care during their pregnancies. This reasoning would fuel the expectation that the proportion of infants in postnatal care grew closely as rapidly as geographic coverage by health care centers and stations did.

Table 5.1: Treatment Correlations with Population, Fertility and Stillbirths

Year	1942		1937			1942		
County	Prenatal care (%)	Postnatal care (%)	Population	Birth rate	Stillbirth rate	Population	Birth rate	Stillbirth rate
Kronobergs	8	17	152941	14.2	238.6	152133	16.8	250.8
Örebro	7	25	219800	12.4	347.6	230868	17.9	249.2
Skaraborgs	34	32	238540	14.4	270.9	241466	17.5	184.6
Malmöhus	21	34	523407	14.7	263.2	535237	17.7	196.2
Kristianstads	33	39	247963	16.0	242.1	249487	17.7	199.8
Jönköpings	35	49	238082	14.7	300.2	245458	17.7	237.4
Jämtlands	37	51	136763	16.2	319.8	141956	20.3	250.1
Hallands	43	54	152106	15.0	336.4	152355	17.3	194.0
Södermanlands	74	56	190128	13.6	310.3	194227	17.2	203.1
Östergötlands	43	56	312181	14.4	299.9	323022	18.0	225.7
Blekinge	39	60	145067	16.7	288.9	146329	18.6	202.4
Gävleborgs	41	60	278772	14.8	258.8	273863	16.6	231.2
Kalmar	47	61	230230	15.7	346.1	228411	18.0	290.0
Värmlands	41	61	270967	13.7	353.3	268895	16.0	211.9
Gotlands	24	67	58066	17.2	189.8	59275	20.3	257.3
Göteborgs/Bohus	41	72	476800	13.9	267.8	489855	17.0	208.4
Älvsborgs	47	72	324545	14.6	285.8	331570	16.8	232.0
Stockholm city	74	76	556954	11.6	250.0	613754	18.5	234.4
Kopparbergs	67	80	247407	13.7	342.5	251132	17.3	228.4
Uppsala	43	81	138726	13.7	247.2	139255	17.6	191.6
Norrbottens	62	81	209974	23.0	292.6	223117	24.6	293.4
Stockholm county	41	82	273702	13.3	297.7	295137	17.7	166.2
Västmanlands	63	84	164452	14.7	228.2	171967	17.8	167.0
Västerbottens	78	92	217156	20.5	323.4	223872	22.1	263.1
Västernorrlands	65	98	279993	16.6	277.5	275559	18.7	229.2

Prenatal (postnatal) care (%): health care utilization rate of expectant mothers (infants). Counties are sorted by postnatal care (%). The correlation coefficient between pre- and postnatal care equals 0.82 (significant at 1%). Birth rate: number of births per 1,000 inhabitants. Stillbirth rate: number of stillbirths per 10,000 births.

Tables 5.1 and 5.2 show the relationships before and during treatment implementation between pre- and postnatal health care utilization and several

Table 5.2: Treatment Correlations with Infant Mortality, Maternal Mortality and the Physician Rate

Year	1942		1937			1942		
County	Prenatal care (%)	Postnatal care (%)	Infant mortality	Maternal mortality	Physician rate	Infant mortality	Maternal mortality	Physician rate
Kronobergs	8	17	417.6	32.7	0.5	293.9	19.9	0.7
Örebro	7	25	395.2	11.2	0.6	292.8	24.5	0.7
Skaraborgs	34	32	471.9	41.4	0.7	279.2	16.8	0.6
Malmöhus	21	34	392.2	27.7	1.1	306.0	18.2	1.2
Kristianstads	33	39	451.5	25.7	0.4	304.3	6.9	0.7
Jönköpings	35	49	385.9	34.7	0.7	248.9	14.0	0.8
Jämtlands	37	51	427.9	22.9	0.5	357.8	7.0	0.8
Hallands	43	54	454.3	31.0	0.7	235.8	7.7	0.7
Södermanlands	74	56	399.5	15.7	0.7	289.7	15.2	0.8
Östergötlands	43	56	324.3	22.5	0.7	258.5	12.2	0.8
Blekinge	39	60	404.5	16.8	0.7	220.8	7.5	0.6
Gävleborgs	41	60	476.5	44.2	0.5	301.6	15.6	0.6
Kalmar	47	61	487.3	30.9	0.5	355.8	7.4	0.6
Värmlands	41	61	415.3	35.5	0.6	314.4	14.2	0.7
Gotlands	24	67	519.5	10.1	0.5	249.0	16.8	0.7
Göteborgs/Bohus	41	72	320.4	24.4	1.0	216.8	15.8	1.1
Älvsborgs	47	72	336.6	23.6	0.7	233.8	10.9	0.8
Stockholm city	74	76	375.0	35.9	1.6	221.2	22.3	1.6
Kopparbergs	67	80	490.1	23.9	0.6	281.5	7.0	0.7
Uppsala	43	81	336.7	37.0	1.2	248.7	16.5	1.5
Norrbottens	62	81	738.7	37.9	0.5	473.8	18.4	0.6
Stockholm county	41	82	405.2	36.4	0.9	267.4	17.4	1.0
Västmanlands	63	84	332.0	25.2	0.7	225.9	19.8	0.7
Västerbottens	78	92	649.0	45.7	0.6	315.7	22.6	0.6
Västernorrlands	65	98	568.0	39.2	0.6	390.4	13.7	0.7

Prenatal (postnatal) care (%): health care utilization rate of expectant mothers (infants). Counties are sorted by postnatal care (%). The correlation coefficient between pre- and postnatal care equals 0.82 (significant at 1%). Infant (maternal) mortality: number of infant (maternal) deaths per 10,000 births (mothers). Physician rate: number of physicians per 1,000 inhabitants.

of the aggregated outcome variables we will discuss in the empirical analysis of Section 5.5.1. Solely the infant mortality of 1937 seems to be slightly positively correlated with the maternal and infant care rates measured in 1942, but this correlation is not significant. There is no evidence for a systematic connection between the pre-treatment characteristics of the counties and the implementation process that could induce a reverse causality concern.

Figures 5.A1 to 5.A6 in the Appendix illustrate the evolvements of the dependent variables in the aggregated analysis of Section 5.5.1. After the interventions had started in 1938, both infant mortality and stillbirths seem to have experienced a sudden drop in the early 1940s (Figures 5.A1 and 5.A2). However, the infant mortality rate, at least, had followed a falling trend already before. With regards to fertility, Sweden experienced a phase of very low birth numbers during the 1930s followed by a strong boom starting in the late 1930s and early 1940s, which was particularly driven by the urban areas (Figures 5.A3 and 5.A4). Maternal mortality, finally, was stagnating at a high level during the 1920s and 1930s before it experienced an unparalleled drop that began in the late 1930s (Figures 5.A5 and 5.A6). During this phase, maternal mortality from all causes decreased with mortality from childbed fever sinking fastest.

Appendix Table 5.A2 reports descriptive statistics for the individual-level data samples. In the sample based on the Death Index, 2% of all individuals have died before reaching their first birthday, while only 5% did not survive until age 40. This difference of only three percentage points reflects the still high infant mortality during the 1940s. 13% of the individuals have died before reaching age 63. In the ULF sample, a good general health condition is reported for 83% of the observations, 23% receive regular medical treatment and 74% hold at least a secondary schooling degree.

5.4 Empirical Strategy

In order to identify the effects of ante- and neonatal health care on various outcomes, we exploit the regional variation in the treatments during their gradual implementation by comparing the proportions of mothers and infants treated across regions and over time. Since visiting the health care facilities was voluntary, participation in the treatment was subject to individual decisions which were possibly related to individual characteristics such as level of education. In order to identify the causal effect of the treatment, we conduct a standard 2SLS approach in which we exploit the regional variation in the densities of different health care centers as instruments for actual health care utilization.[38] Because the establishment of new centers was subject to institutional decisions, we consider our instruments as unrelated to individual preferences and therefore exogenous.

Our baseline second-stage regression equation reads as follows:

$$y_{itr} = \beta_0 + \beta_1 \hat{T}_{Mtr} + \beta_2 \hat{T}_{Itr} + \mu_t + v_r + \varepsilon_{itr} \tag{9}$$

[38] In the empirical analysis, regressions are estimated applying the Stata command `ivreg2` (Baum et al., 2010).

where y_{itr} is an outcome of individual i born in year t in parish r, T_{Mtr} is the proportion of mothers treated, T_{Itr} is the proportion of infants treated, and μ_t and v_r are birth year and region fixed effects, respectively.

$\hat{T}_{Mtr}$ and $\hat{T}_{Itr}$ are estimated from the following first-stage regressions:

$$T_{Ctr} \quad = \quad \alpha_0 + \mathbf{X}\boldsymbol{\alpha_1} + \eta_t + w_r + u_{tr} \tag{10}$$

where $C = M, I$. $\mathbf{X}$ is a vector of center densities of different types, and η_t and w_r are birth year and region fixed effects, respectively. Our first- and second-stage regression equations for the aggregated outcome variables are formulated accordingly at the regional level (omitting the individual index i).

β_1 and β_2 reflect the effects of pre- and postnatal care utilization on the outcome which we are interested in. We measure them by estimating various specifications of Equations (9) and (10) based on the datasets described in Section 5.3.

We define different estimation specifications by applying the following modifications to Equations (9) and (10). First, by replacing county fixed effects by the average income level and the occupation shares in 1930, we test whether the county fixed effects capture more than only regional differences in economic measures. Second, we add county-specific linear time trends to the regressors to account for region-specific developments over time. Third, we add the physician rate as a control variable to account for regional quality differences in the medical system. Fourth, we add socio-economic background variables to capture initial individual differences possibly affecting our outcome variables.

Finally, we take into account the timing of health care utilization. The year of pre- and postnatal care utilization is in most cases not entirely identical with the year of birth because the former takes place during the pregnancy and the latter takes place during the first year of life. To take these temporal deviations into account, we also estimate the above equations a), by replacing T_{Mtr} by the share of mothers supervised in the previous year $T_{M(t-1)r}$, and b), by replacing T_{Itr} by the share of infants supervised in the subsequent year $T_{I(t+1)r}$. The analysis of mortality conducted in Section 5.5.2.1 allows a more accurate procedure to account for the timing of health care utilization because the Death Index reports an individual's exact date of birth. By exploiting the date of birth we are able to calculate utilization probabilities which cover the individual gestational period or the first birth year, respectively. In particular, we calculate individual-specific averages over the prenatal care utilization rates in the previous year and the current year, weighting accordingly to cover the period of gestation. Analogously, we average over the postnatal care utilization rates in the current year and the subsequent year, weighting to cover the first year of birth. We apply temporal shifts to the center densities contained in vector $\mathbf{X}$ in an identical way.

5.5 Results

In this section we present estimates of the impact of the interventions on various outcomes. We begin with a discussion of the short-run effects on several aggregated outcomes, followed by an analysis of the effects on individual mortality, long-run health and socioeconomic status.

5.5.1 Aggregated Analysis

5.5.1.1 First-Stage Results

Our aggregated analysis focuses on estimates from 2SLS regressions of fertility, infant mortality and stillbirths as well as maternal mortality. All aggregated outcomes in this section have been logarithmized (ln) and are to be interpreted accordingly. Our first stage is based on the assumption that the densities of maternity and child care centers and stations, respectively, are important predictors of the utilization rates of ante- and neonatal health care during the implementation process. Since a utilization of the newly available health care became possible only after having gained access to a suitable health care center, we are confident that the plausibility of this relationship is sufficiently established.

Appendix Tables 5.A3 and 5.A4 report the results from the first-stage regressions of pre- and postnatal care utilization.[39] Our main focus is on the third and fourth columns since they control for regional time trends. In most

[39] For the regressions of fertility, center densities have been based on population numbers instead of birth numbers. Since this does not lead to qualitative changes in the results, the first-stage regressions using center densities based on population numbers are not reported here.

of the specifications for prenatal care, the density of maternity centers of type II has a significant positive effect, particularly after including linear time trends in the regressions. In the lower panel of Table 5.A3, the density of maternity stations offers an additional effect on prenatal care utilization. Considering the results for postnatal care in Table 5.A4, the densities of child care centers of type I and child care stations have positive effects in the upper panel, but their significance levels reduce to only 10% in the medium panel. In the lowest panel, at least for child care stations the positive sign returns. For both pre- and postnatal care utilization there are some significant effects for centers offering the respective other type of health care, possibly because a number of centers offers both types of health care.

Since our estimation specification comprises more than one endogenous regressor, we report the Angrist-Pischke multivariate F-test of excluded instruments for each first-stage regression. For neither of the regressions the threshold of an F-statistic of 10 is reached, implying that we cannot reject the hypothesis of under- or weak identification of one of the endogenous regressors. However, the Angrist-Pischke F-statistics reported in the last two columns of the medium and lower panels of Tables 5.A3 and 5.A4 range between 7.8 and 8.7, which is close to the threshold of 10. Our following interpretation of the second-stage results will therefore focus on these specifications.

5.5.1.2 Fertility

We hypothesize that the increasing utilization of free pre- and postnatal health care has incentivized couples to increase their own fertility. Such an effect would contribute to explaining the strong baby boom of the 1940s. Table 5.3 reports the second-stage estimates from regressions of the fertility rate in the subsequent year on utilization rates. As described above in Section 5.5.1.1, our focus is on the third and fourth columns of the medium and lower panels. Because the estimation coefficients in these specifications are insignificant, the results to not support our expectation of positive intervention effects on fertility.

Appendix Tables 5.A5 to 5.A7 report results focusing on the fertility of different groups of mothers as defined by marital status. As for overall fertility, the coefficients of interest are insignificant for married, unmarried as well as

Table 5.3: Second Stage, Fertility

Prenatal care	-0.006	-0.005*	-0.012**	-0.012**
	(0.005)	(0.003)	(0.006)	(0.006)
Postnatal care	0.005*	0.004*	0.008*	0.008*
	(0.003)	(0.002)	(0.004)	(0.004)
Observations	275	275	275	275
Prenatal care$_{(-1)}$	0.002	0.002	0.001	0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Postnatal care	0.000	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	250	250	250	250
Prenatal care	-0.000	-0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Postnatal care$_{(+1)}$	0.002	-0.000	-0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)
Observations	275	275	275	275
Avg. income 1930	√			
Occ. shares 1930	√			
County fixed effects		√	√	√
Regional time trends			√	√
Physician rate				√

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

engaged women, not supporting our expectation.

Appendix Tables 5.A10 to 5.A13 report the reduced-form regressions. The results imply insignificant coefficients or unexpected effect signs. Overall, we cannot conclude that the interventions contributed to the baby boom of the 1940s.

5.5.1.3 Infant Mortality and Stillbirths

The introduction of free ante- and neonatal health care aimed at improving the health of infants and mothers. We therefore expect the interventions to have reduced infant mortality and the number of stillbirths. Table 5.4 presents the second-stage results for effects on infant mortality. The third and fourth columns of the medium panel reveal a negative effect of prenatal care utilization in the previous year. Specifically, an increase in the prenatal care utilization rate of 1 percentage point decreases the infant mortality rate by 0.9%. Table 5.5 reports the second-stage results for stillbirths. In the lower panel, prenatal care utilization exhibits a positive sign, which is significant, however, only at the 10% level.

Table 5.4: Second Stage, Infant Mortality

Prenatal care	0.009	0.001	0.000	0.002
	(0.010)	(0.004)	(0.008)	(0.007)
Postnatal care	-0.006	-0.001	-0.000	-0.001
	(0.005)	(0.003)	(0.005)	(0.005)
Observations	275	275	275	275
Prenatal care$_{(-1)}$	0.002	-0.007*	-0.009***	-0.009***
	(0.004)	(0.004)	(0.003)	(0.003)
Postnatal care	-0.003	0.004	0.004	0.004
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	250	250	250	250
Prenatal care	0.005	-0.003	0.001	0.001
	(0.005)	(0.003)	(0.004)	(0.004)
Postnatal care$_{(+1)}$	-0.005	0.001	-0.002	-0.003
	(0.004)	(0.003)	(0.004)	(0.004)
Observations	275	275	275	275
Avg. income 1930	√			
Occ. shares 1930	√			
County fixed effects		√	√	√
Regional time trends			√	√
Physician rate				√

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix Tables 5.A8 and 5.A9 present the corresponding reduced-form regressions. Again considering the medium and lower panels only, the density of maternity centers of type I has a negative effect on infant mortality, while there is no evidence that stillbirths are affected by any of the center densities. Unexpectedly, the current density of child care centers of type II positively affects infant mortality, as reported in the medium panel of Table 5.A8.

5.5.1.4 Maternal Mortality

As for infant mortality and stillbirths, we expect negative intervention effects on maternal mortality. The second-stage results for maternal mortality are presented in Table 5.6. None of the specifications confirms our expectation, while maternity care utilization in the previous year even implies a positive effect, which is, however, only significant at the 10% level. Table 5.A14 delivers the reduced-form results. After controlling for regional time trends, there are no significant effects at all.

We also estimated the effects on maternal mortality from different causes such as childbed fever after childbirth or miscarriage and eclampsia. Because

Table 5.5: Second Stage, Stillbirths

Prenatal care	0.012	0.007	0.012	0.011
	(0.008)	(0.005)	(0.012)	(0.011)
Postnatal care	-0.006	-0.004	-0.010	-0.008
	(0.004)	(0.004)	(0.009)	(0.008)
Observations	275	275	275	275
Prenatal care$_{(-1)}$	0.002	0.004	-0.006	-0.006
	(0.005)	(0.003)	(0.005)	(0.005)
Postnatal care	-0.001	-0.004	0.002	0.002
	(0.003)	(0.003)	(0.004)	(0.004)
Observations	250	250	250	250
Prenatal care	0.010*	0.009**	0.006*	0.005*
	(0.006)	(0.004)	(0.003)	(0.003)
Postnatal care$_{(+1)}$	-0.006	-0.008*	-0.005	-0.004
	(0.005)	(0.004)	(0.003)	(0.003)
Observations	275	275	275	275
Avg. income 1930	√			
Occ. shares 1930	√			
County fixed effects		√	√	√
Regional time trends			√	√
Physician rate				√

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

all coefficients from these regressions are insignificant or, in one case, unexpectedly positive, we refrain from reporting all the regression tables here. However, with regards to the reduced-form regressions, two exceptions deserve mention. First, the density of child care centers of type I has a significant negative effect on maternal mortality from childbed fever after childbirth. Second, a higher density of child care stations significantly reduces maternal mortality from eclampsia. Although child care centers and stations were primarily designed to supervise infants, probably also mothers received advice and supervision in these localities after childbirth, which could explain these findings. The regression results are available upon request.

5.5.2 Individual Analysis

This subsection is devoted to the measurement of longer-term intervention effects on individual outcomes. As outlined above, the existing literature indicates that prenatal and early-childhood conditions predetermine later-life health and socioeconomic outcomes with initial differences reinforcing over

Table 5.6: Second Stage, Maternal Mortality

Prenatal care	0.015	0.023	-0.013	-0.009
	(0.019)	(0.016)	(0.049)	(0.046)
Postnatal care	-0.007	-0.013	0.001	-0.002
	(0.013)	(0.013)	(0.035)	(0.034)
Observations	275	275	275	275
Prenatal care$_{(-1)}$	0.026	0.029	0.029*	0.028*
	(0.019)	(0.019)	(0.015)	(0.015)
Postnatal care	-0.015	-0.018	-0.012	-0.012
	(0.017)	(0.017)	(0.018)	(0.017)
Observations	250	250	250	250
Prenatal care	-0.009	0.006	-0.011	-0.010
	(0.013)	(0.013)	(0.018)	(0.018)
Postnatal care$_{(+1)}$	0.010	0.000	0.007	0.006
	(0.010)	(0.017)	(0.017)	(0.016)
Observations	275	275	275	275
Avg. income 1930	√			
Occ. shares 1930	√			
County fixed effects		√	√	√
Regional time trends			√	√
Physician rate				√

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

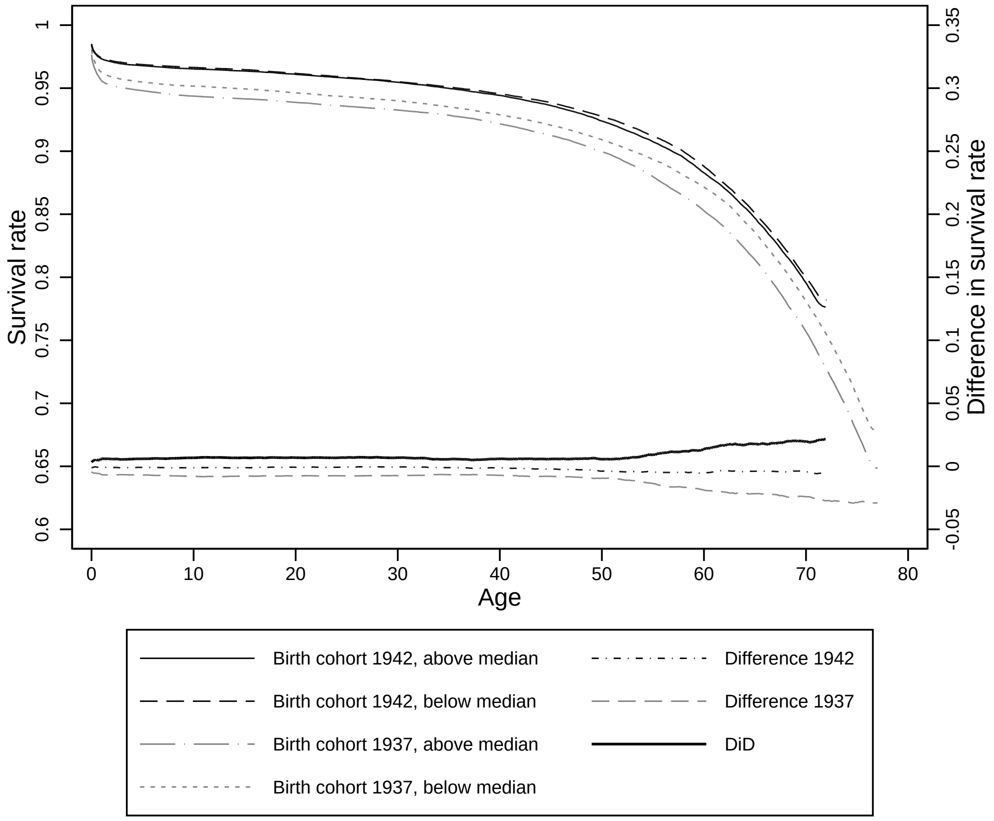
the life cycle. Against this background, we expect to find positive intervention effects on individual mortality, health and socioeconomic status, potentially increasing in age.

5.5.2.1 Mortality

Our analysis of intervention effects on individual mortality is based on the Death Index sample. In a first step, we compare mortality of children born during the implementation phase to mortality of pre-intervention children. Additionally, we compare the children with regards to the utilization levels of pre- and postnatal care in their birth regions during the implementation process. The double difference then captures the effect of being born in a region with a high utilization as compared to being born in a region with a low utilization of pre- and postnatal health care, respectively.

Figure 5.3 plots the survival curves for children born in 1937, the year before the intervention started, and children born in 1942, one of the years with the largest variation in utilization rates (compare Figures 5.1 and 5.2). We divide the two birth cohorts at the median of prenatal care utilization in

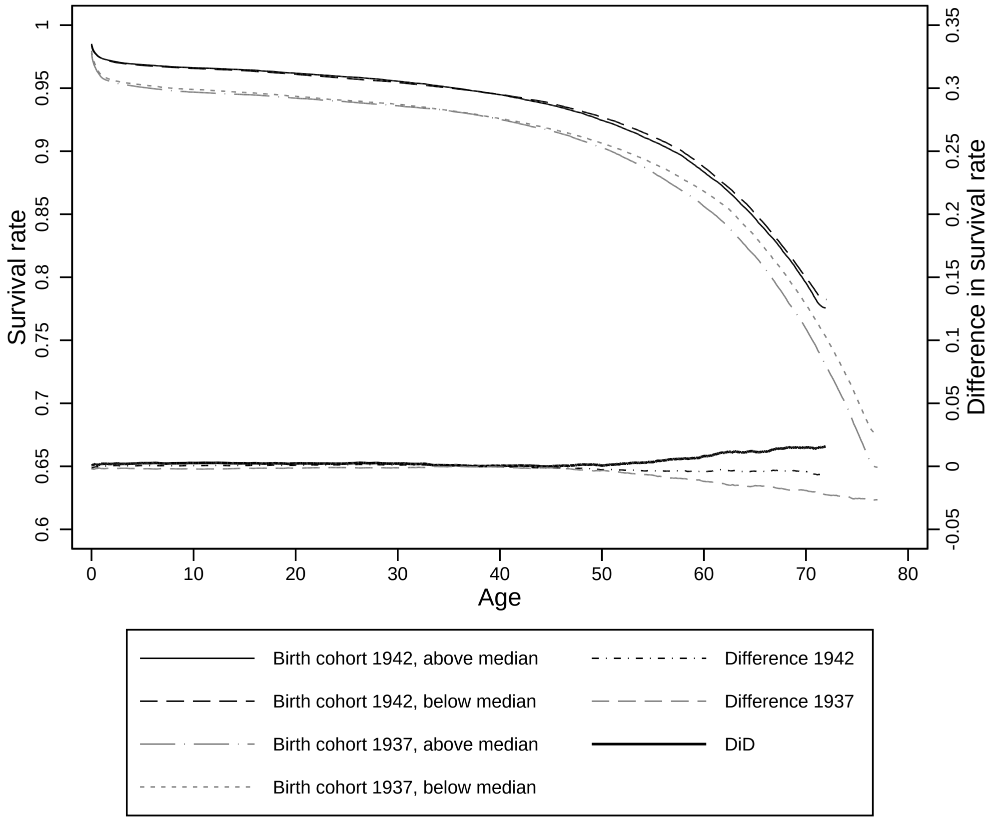
Figure 5.3: Survival Curves for Birth Cohorts 1937 and 1942 Splitted at Median of Predicted Prenatal Care Utilization 1942



1942 as predicted from regressions on center densities. Black curves refer to children born in 1942 and gray curves refer to children born in 1937. While in 1937 the survival rate of children born in high-utilization regions was below the survival rate of children born in low-utilization regions, there is nearly no visible difference between high- and low-utilization regions for children born in 1942. This is reflected in the double difference, the difference-in-difference (DiD), which we plot against the right y-axis. It implies that the effect on the survival rate of being born in a high-utilization county as compared to being born in a low-utilization county is slightly positive at all ages and starts to increase after about age 53.

Figure 5.4 plots the analogous surival rates with regards to postnatal care utilization as predicted from center densities. Within the birth cohorts, the survival curves differ barely by level of utilization, which indicates an effect of

Figure 5.4: Survival Curves for Birth Cohorts 1937 and 1942 Splitted at Median of Predicted Postnatal Care Utilization 1942



the intervention that is close to zero. This is reflected by the double difference, which, however, becomes positive and increases after age 50.

In summary, our comparison of pre- and during-intervention children indicates a positive effect of prenatal health care on survival at all ages which increases from the mid 50s onwards. Similarly, the effect of postnatal health care, while being close to zero before age 50, becomes positive and increases above age 50. These results are in line with our expectation of positive intervention effects that increase over the life cycle.

In a second step, we present 2SLS estimates of intervention effects on individual mortality focusing on different time horizons. The utilization rates of pre- and postnatal care have been calculated based on the birth year, as described above in Section 5.4. In addition, they have been divided by the factor 1,000 because in many cases the estimation coefficients are quite small, which has to be taken into account when interpreting the following results.

Table 5.7: Second Stage, Individual Mortality

Died before reaching age 1			
Prenatal care$_w$	-0.8742**	-1.7948***	-1.7859***
	(0.3917)	(0.5731)	(0.5751)
Postnatal care$_w$	0.6490*	1.0565**	1.0458**
	(0.3664)	(0.4946)	(0.4978)
Observations	1292421	1292421	1292421
Died before reaching age 5			
Prenatal care$_w$	-0.9239**	-2.1445***	-2.1365***
	(0.3925)	(0.6879)	(0.6891)
Postnatal care$_w$	0.7212*	1.3303**	1.3198**
	(0.3771)	(0.5789)	(0.5810)
Observations	1292421	1292421	1292421
Died before reaching age 35			
Prenatal care$_w$	-1.0750**	-2.5903***	-2.5849***
	(0.4319)	(0.8439)	(0.8376)
Postnatal care$_w$	0.8063**	1.4526**	1.4418**
	(0.4102)	(0.6588)	(0.6568)
Observations	1292421	1292421	1292421
Died before reaching age 63			
Prenatal care$_w$	-2.2687**	-6.6852***	-6.6919***
	(1.0434)	(2.0112)	(1.9923)
Postnatal care$_w$	1.7217*	3.3928**	3.3781**
	(0.9663)	(1.7183)	(1.7079)
Observations	1292421	1292421	1292421
County fixed effects	√	√	√
Regional time trends		√	√
Physician rate			√

Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A15 in the Appendix reports the first-stage regressions. While several densities have significant effects on utilization rates, particularly centers of type II and health care stations, the Angrist-Pischke F-statistics are quite low for all specifications and not even exceeding a value of 4. Therefore, the second stage results need to be interpreted with caution.

In Table 5.7 we report intervention effects on the probabilities of dying before reaching ages 1, 5, 35 and 63.[40] Robustly, prenatal care utilization has a significant negative effect in all specifications. In most of the specifications,

[40]As the Death Index ends in 2013, deaths occurring in the youngest birth cohort in our sample that was born in 1950 can be observed maximally up to age 63.

postnatal care has significant positive effects on mortality, but this effect may be to small in comparison to fully compensate the negative effects measured for prenatal care.

Tables 5.A16 and 5.A17 report the reduced form regressions. The density of child care centers of type I has a significantly negative effect on mortality in the context of all considered time horizons. Furthermore, its effect seems to increase with age. While an increase of one standard deviation in the density of child care centers of type I decreases the probability of dying before age 1 by only 0.1%, it decreases the probability of dying before age 63 by 0.5%. Also the density of child care stations affects mortality in middle ages. An increase of one standard deviation in this density reduces the probability of dying before reaching age 35 by 0.2% and the probability of dying before age 63 by even 0.8%.

Overall, the 2SLS regression results are to be interpreted with severe caution. However, the reduced form regressions provide evidence for negative effects of postnatal health care on mortality which are robust across specifications and increase with age.

5.5.2.2 Health

In this subsection we present estimates of the intervention effects on various health outcomes from the ULF data sample. Appendix Tables 5.A18 and 5.A19 report the first stage regressions. Several specifications suggest significant effects of the densities of maternity centers of type II and maternity stations on prenatal care utilization and, likewise, of the densities of child care centers of both types and of child care stations on postnatal care utilization. The Angrist-Pischke test of excluded instruments indicates an F-statistic close to 10 only for the third, fourth and fifth columns of the lower panel (F-values between 7.9 and 9.5). In addition to these specifications we will cautiously interpret the third, fourth and fifth columns of the medium panel in the following discussion, where the F-statistic is at least close to 6.

Because we estimated utilization effects on a large range of health outcomes, we shift the second stage regression tables to the Appendix (Tables 5.A20 to 5.A26) and only summarize the results here. Focusing only on specifications with F-statistics larger than 5, none of the regressions of any health outcome reveals robust and significant intervention effects. At least, the coefficients of prenatal care utilization exhibit the expected signs for most of the considered outcomes and are sometimes significant at the 10% level.

While our second stage regressions provide only little evidence that the interventions have mattered for long-term health, the reduced form regressions shown in Tables 5.A27 to 5.A33 deliver some support for this hypothesis. In particular, an increase of one standard deviation in the density of type I maternity centers in the previous year reduces the probability to have been diagnosed with a severe illness by 2.4% (Table 5.A28, medium panel). Similarly, a decrease in the same outcome of 1.7% is measured for an increase in the density of maternity centers of type II by one standard deviation (Table 5.A28, lower panel). Also, increasing the density of type I maternity centers in the previous year by one standard deviation reduces the number of long-term illnesses by 0.1 (Table 5.A32, medium panel) as well as the probability of having a disability by 0.8% (Table 5.A33, medium panel). The probability of being disabled is also reduced by 1.4%, when the density of maternity centers of type II increases by one standard deviation (Table 5.A33, lower panel). Finally, there is also an unexpected effect. The medium panel of Table 5.A31 suggests that an increase in the density of maternity centers of type I in the previous year reduces the probability to be able to run 100 meters.

Overall, while there is nearly no evidence for effects of pre- and postnatal health care utilization on health outcomes in the second stage, the reduced form regressions reveal some significant coefficients. Particularly the finding that maternity centers of type I are beneficial for health exhibits some degree of robustness.

5.5.2.3 Socioeconomic Status

This subsection discusses intervention effects on socioeconomic status as measured by education and wage. Again, we will focus on the specifications reported in the third, fourth and fifth columns of the medium and lowest panels of each table (the relevant first-stage regressions, delivered in Appendix Tables 5.A18 and 5.A19, are identical to those presented in the previous section).

Table 5.8 presents the second stage results for effects on education. The coefficients in the medium panel indicate that an increase in prenatal care

Table 5.8: Second Stage, Secondary Education or Higher

Prenatal care	0.001	-0.007	0.000	-0.002	0.001
	(0.003)	(0.006)	(0.006)	(0.007)	(0.006)
Postnatal care	0.001	0.004	-0.003	-0.002	-0.003
	(0.002)	(0.004)	(0.005)	(0.005)	(0.005)
Observations	6990	6990	6990	6990	6990
Prenatal care$_{(-1)}$	0.003	0.002	0.007**	0.007**	0.008***
	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
Postnatal care	-0.000	-0.000	-0.007**	-0.006**	-0.007**
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	6524	6524	6524	6524	6524
Prenatal care	0.002	-0.001	-0.002	-0.003	-0.001
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
Postnatal care$_{(+1)}$	0.000	-0.000	-0.002	-0.002	-0.003
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	$\surd$				
Occ. shares 1930	$\surd$				
County fixed effects		$\surd$	$\surd$	$\surd$	$\surd$
Regional time trends			$\surd$	$\surd$	$\surd$
Physician rate				$\surd$	$\surd$
Social background					$\surd$

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

utilization by one percentage point increases the probability to hold at least a secondary schooling degree by 0.8%. However, the effect of postnatal care utilization points in the opposite direction, which is difficult to interpret. The lower panel does not show significant coefficients. The corresponding reduced form regressions are reported in Appendix Table 5.A35. The coefficients shown here are mostly insignicant, while an increase in the density of child care centers of type II of one standard deviation unexpectedly decreases the probability of receiving a secondary schooling degree, even after controlling for social background.

Table 5.9 reports the results from wage regressions. After controlling for social background, only the medium panel shows a positive wage effect of prenatal care in the previous year, signficant at 10%. The reduced form regressions delivered in Table 5.A34 provide more support for positive intervention effects on labor income. An increase of one standard deviation in the density of maternity centers of type I in the previous year increases the labor income by

7.6% (medium panel). Increasing the density of maternity care stations by one standard deviation increases labor income by even 16.1% (lower panel).

Table 5.9: Second Stage, ln of Gross Labor Income

Prenatal care	0.009*	0.004	0.011	0.010	0.013
	(0.005)	(0.005)	(0.013)	(0.010)	(0.013)
Postnatal care	-0.006	-0.002	-0.008	-0.007	-0.009
	(0.004)	(0.004)	(0.009)	(0.008)	(0.010)
Observations	6990	6990	6990	6990	6990
Prenatal care$_{(-1)}$	-0.002	0.005	0.008	0.008	0.012*
	(0.004)	(0.007)	(0.006)	(0.006)	(0.007)
Postnatal care	0.004	-0.002	-0.003	-0.003	-0.006
	(0.004)	(0.007)	(0.007)	(0.007)	(0.008)
Observations	6524	6524	6524	6524	6524
Prenatal care	0.014**	0.011*	0.011	0.010	0.008
	(0.006)	(0.007)	(0.008)	(0.007)	(0.006)
Postnatal care$_{(+1)}$	-0.011*	-0.014*	-0.012**	-0.011*	-0.008
	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	√				
Occ. shares 1930	√				
County fixed effects		√	√	√	√
Regional time trends			√	√	√
Physician rate				√	√
Social background					√

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Overall, the results for education effects are ambiguous. With regards to labor income, positive intervention effects are visible in the reduced form, where considerable gains are suggested.

5.6 Conclusion

In this paper, we measure the short-run effects of the universal introduction of free ante- and neonatal health care on fertility, infant mortality, stillbirths and maternal mortality using data from official Swedish statistics. Based on the Swedish Death Index which comprises a majority of the deaths of members of the treated birth cohorts that occurred until 2013, we estimate mortality effects at the individual level. Finally, using individual-level data from the 1980s, we identify long-run effects on schooling, wage and a variety of self-assessed health variables under control for social background.

We find negative effects on mortality of both pre- and postnatal care. Using aggregated data we find that an increase in the prenatal care utilization rate of 1 percentage point decreases the infant mortality rate by 0.9%. Reduced form regressions based on the Swedish Death Index suggest significant negative effects of different types of child care facilities that increase when the considered time horizon is extended. For instance, increasing the density of child care centers of type I by one standard deviation decreases the probability of dying before age 1 by 0.1%, while it decreases the probability of dying before age 63 by 0.5%. A standard deviation increase in child care stations reduces the probability of dying before age 63 by even 0.8%. A difference-in-difference comparison of survival rates shows a rise in the intervention effects beyond age 50 and suggests that postnatal care affects mortality only beyond age 50. Reduced form regressions for health and labor income indicate significant effects of prenatal care. In particular, the densities of maternity centers of types I and II have significant negative effects on the probabilities to be disabled and to be diagnosed with a severe illness. The density of maternity centers of type I further reduces the number of long-term illnesses. With regards to labor income, a one standard deviation increase in the density of maternity centers of type I is measured to increase labor income by 7.6%, while such an increase in the density of maternity stations labor income by even 16.1%. Finally, we find some evidence for beneficial effects from postnatal care on maternal mortality from eclampsia and from childbed fever after childbirth. There are no effects on fertility and stillbirths, and the effects on education are ambiguous.

Overall, beneficial effects from prenatal care are measured more frequently and more robustly across specifications compared to gains from postnatal care. This may suggest that prenatal care is the more effective type of intervention. Another lesson from our analysis is that different qualities of health checks seem to matter. As Currie and Rossin-Slater (2015) point out, there is little support for the quantity of prenatal care being a critical dimension, possibly because the quality is the more important variable. Yet, there is almost no evidence existent on the impacts of prenatal care quality.

Appendix 5

Table 5.A1: Variable Definitions

Outcome variables	
Aggregated data:	
Infant mortality	number of infant deaths per 10,000 births
Live births	number of live births per 1,000 births
Stillbirths	number of stillbirths per 10,000 births
Fertility	number of births per 1,000 inhabitants in subsequent year
Fertility, married women	number of births to married women per 1,000 inhabitants in subsequent year
Fertility, unmarried women	number of births to unmarried women (incl. engaged) per 1,000 inhabitants in subsequent year
Fertility, engaged women	number of births to engaged women per 1,000 inhabitants in subsequent year
Maternal mortality	number of maternal deaths per 10,000 mothers
Maternal mortality from childbed fever	number of maternal deaths from childbed fever (after childbirth or miscarriage) per 10,000 mothers
Maternal mortality from childbed fever after childbirth	number of maternal deaths from childbed fever after childbirth per 10,000 mothers
Maternal mortality from childbed fever after miscarriage	number of maternal deaths from childbed fever after miscarriage per 10,000 mothers
Maternal mortality from eclampsia	number of maternal deaths from eclampsia per 10,000 mothers
Maternal mortality from other cause of death	number of maternal deaths from other cause of death per 10,000 mothers
ULF data:	
Gross labor income	real gross labor income
ln of gross labor income	*ln* of real gross labor income
Secondary education or higher	dummy variable indicating if the individual holds a secondary schooling degree or higher
Good general health condition	dummy variable indicating if the individual reports a good general health condition (instead of average or bad)
Had a severe diagnosis	dummy variable indicating if the individual was diagnosed with a severe illness
Regular medical treatment	dummy variable indicating if the individual receives regular medical treatment

Normal weight	dummy variable indicating if the individual's body mass index is between 18.50 and 24.99
Only able to run less than 100 meters	dummy variable indicating if the individual is not able to run 100 meters
Number of long-term illnesses	number of long-term illnesses the individual was diagnosed with
Disabled	dummy variable indicating if the individual is disabled
Death Index:	
Probability of dying before reaching age 1	dummy variable indicating if the individual died before reaching age 1
Probability of dying before reaching age 5	dummy variable indicating if the individual died before reaching age 5
Probability of dying before reaching age 40	dummy variable indicating if the individual died before reaching age 40
Probability of dying before reaching age 63	dummy variable indicating if the individual died before reaching age 63

Independent variables

Endogenous regressors:	
Prenatal care	proportion of expectant mothers who utilize health care in %
Postnatal care	proportion of infants who receive health care in %
Instruments:	
MtypI	density of type I maternity centers, defined as current number of maternity centers of type I divided by birth number in subsequent year,† normalized by one standard deviation
MtypII	density of type II maternity centers, defined as current number of maternity centers of type II divided by birth number in subsequent year,† normalized by one standard deviation
Mstat	density of maternity stations, defined as current number of maternity stations divided by birth number in subsequent year,† normalized by one standard deviation
CtypI	density of type I child care centers, defined as number of child care centers of type I divided by number of births†, normalized by one standard deviation
CtypII	density of type II child care centers, defined as number of child care centers of type II divided by number of births†, normalized by one standard deviation
Cstat	density of child care stations, defined as number of child care stations divided by number of births†, normalized by one standard deviation

Control variables:	
Avg. income 1930	average hourly income in 1930, measured at district level (census 1930)
Ind. shares 1930	industry shares in 1930, measured at parish level (census 1930)
County fixed effects	dummy variables for 24 counties and Stockholm
Regional time trends	county-specific linear time trends
Physician rate	number of physicians per 1,000 inhabitants
Social background	dummy variables for occupation of father, foreign parental background and number of siblings (ULF)
Indices:	
$variable_{(-1)}$	*variable* in previous year
$variable_{(+1)}$	*variable* in subsequent year
$variable_{w}$	(Prenatal care, MtypI, MtypII, Mstat:) *variable* during the individual gestational period, calculated as weighted average of *variable* in previous and current year with weights being defined according to date of birth
$variable_{w}$	(Postnatal care, CtypI, CtypII, Cstat:) *variable* during the individual's first year of life, calculated as weighted average of *variable* in current and subsequent year with weights being defined according to date of birth

† For the regressions of fertility, numbers of centers are divided by current population numbers.

Figure 5.A1: Infant Mortality Rate, 1900-1960

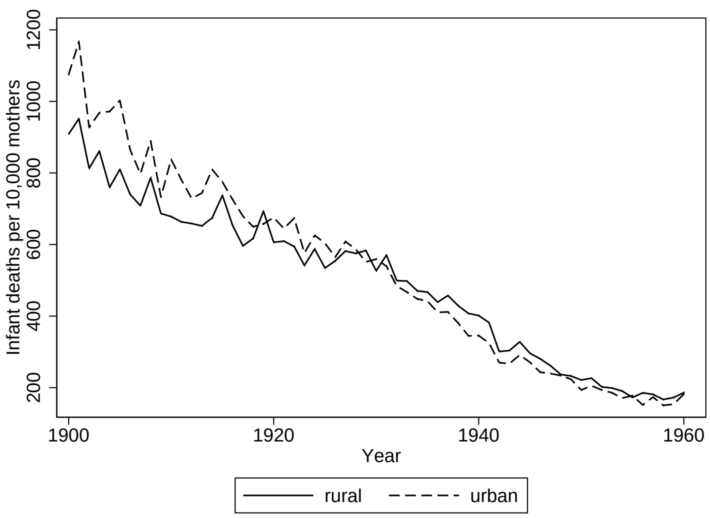


Figure 5.A2: Stillbirth Rate, 1930-1960

Figure 5.A3: Fertility Rate, 1900-1960

Figure 5.A4: Fertility Rate by Level of Urbanization, 1930-1960

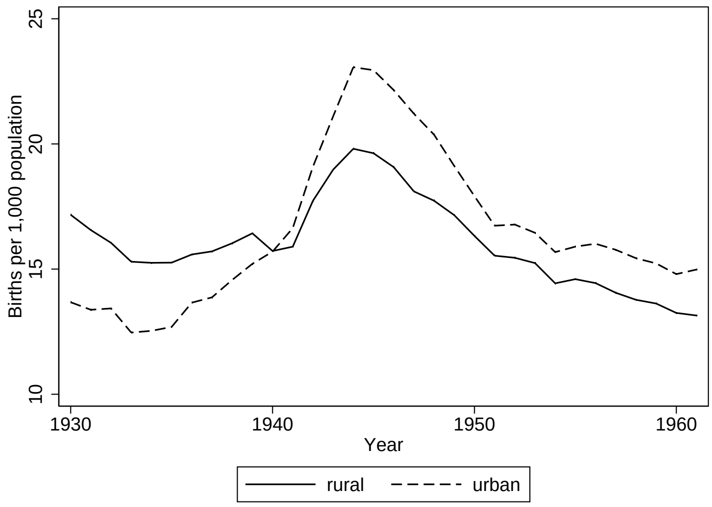


Figure 5.A5: Maternal Mortality by Cause of Death, 1900-1960

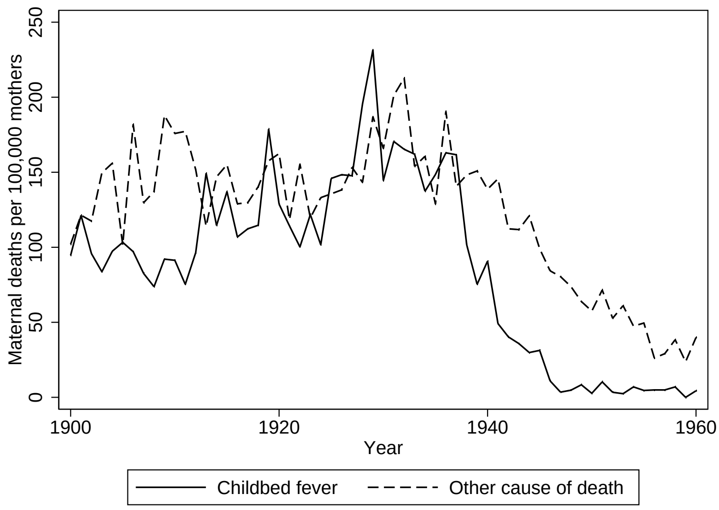


Figure 5.A6: Maternal Mortality by Cause of Death, 1931-1960

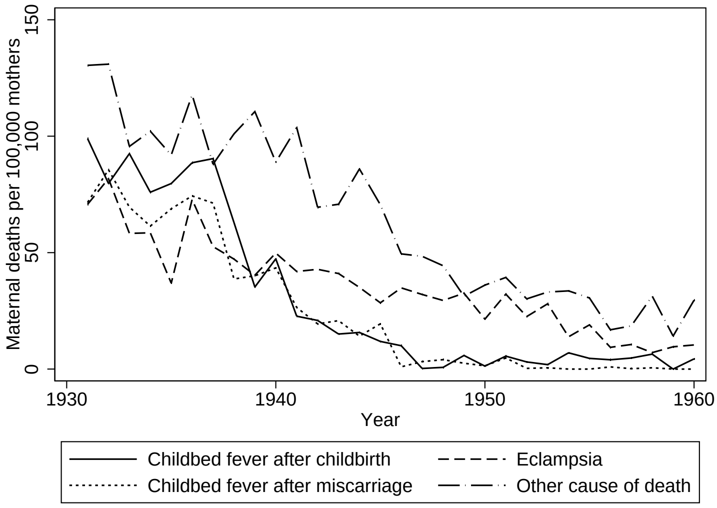


Table 5.A2: Descriptive Statistics, Individual Samples

Variable	Mean	SD	Min	Max	N
Death Index					
Year of birth	1945.43	2.87	1940.00	1950.00	1292421
Year of death	1990.05	23.75	1940.00	2013.00	228270
Died before reaching age 1	0.02	0.16	0.00	1.00	1292421
Died before reaching age 5	0.03	0.17	0.00	1.00	1292421
Died before reaching age 35	0.05	0.21	0.00	1.00	1292421
Died before reaching age 63	0.13	0.34	0.00	1.00	1292421
ULF					
Year of birth	1945.26	2.99	1940.00	1950.00	6990
Female	0.50	0.50	0.00	1.00	6990
Both parents born in Sweden	0.97	0.17	0.00	1.00	6990
At least two siblings	0.55	0.50	0.00	1.00	6990
Good general health condition	0.83	0.38	0.00	1.00	6990
Had a severe diagnosis	0.12	0.32	0.00	1.00	6990
Regular medical treatment	0.23	0.42	0.00	1.00	6990
Normal weight	0.61	0.49	0.00	1.00	6990
Only able to run less than 100m	0.06	0.24	0.00	1.00	6990
Number of long-term illnesses	0.58	0.90	0.00	6.00	6990
Disabled	0.02	0.13	0.00	1.00	6990
Gross labor income	2037.83	20718.87	1.00	999999.00	6990
Secondary education or higher	0.74	0.44	0.00	1.00	6990

SD: standard deviation. N: number of observations.

Table 5.A3: First Stage for Prenatal Care, Aggregated Outcomes

MtypI	4.240	1.089	1.379	1.154
	(3.362)	(1.709)	(1.353)	(1.252)
MtypII	-0.182	1.914	4.324***	4.311***
	(3.424)	(2.327)	(1.441)	(1.320)
Mstat	1.303	-3.884	1.810	3.598
	(13.774)	(15.025)	(9.669)	(9.196)
CtypI	-0.511	0.684	8.941**	8.970***
	(3.086)	(1.839)	(3.214)	(2.972)
CtypII	3.806	5.108	2.540*	2.587*
	(3.048)	(3.266)	(1.464)	(1.420)
Cstat	5.916	16.086	7.219	5.643
	(13.329)	(16.402)	(12.013)	(11.708)
Observations	275	275	275	275
APF	0.45	1.07	0.45	0.54
$\text{MtypI}_{(-1)}$	4.238	1.433	1.437	1.401
	(3.343)	(1.644)	(1.026)	(1.009)
$\text{MtypII}_{(-1)}$	4.644**	5.974***	6.188***	6.147***
	(2.196)	(1.581)	(1.389)	(1.330)
$\text{Mstat}_{(-1)}$	9.897***	10.903***	9.126*	9.207*
	(3.152)	(2.817)	(4.633)	(4.650)
CtypI	-2.326	-0.315	2.338	2.298
	(2.642)	(1.885)	(3.067)	(3.076)
CtypII	-1.062	1.390	-1.122	-1.116
	(2.423)	(2.368)	(1.415)	(1.426)
Cstat	-2.230	2.066	1.119	1.207
	(3.691)	(3.150)	(2.485)	(2.492)
Observations	250	250	250	250
APF	1.93	3.63	8.07	7.80
MtypI	4.463	1.713	2.211	1.989
	(3.346)	(1.820)	(1.451)	(1.362)
MtypII	3.149	5.179***	6.338***	6.349***
	(2.344)	(1.745)	(1.226)	(1.120)
Mstat	9.277***	10.319***	10.190**	10.473**
	(3.112)	(2.618)	(4.015)	(4.067)
$\text{CtypI}_{(+1)}$	-1.467	0.259	1.834	1.634
	(2.721)	(1.981)	(3.465)	(3.296)
$\text{CtypII}_{(+1)}$	0.054	1.546	-1.178	-1.298
	(2.209)	(2.038)	(1.019)	(1.048)
$\text{Cstat}_{(+1)}$	-2.050	1.425	2.197	2.001
	(3.594)	(3.170)	(2.486)	(2.469)
Observations	275	275	275	275
APF	1.85	3.97	8.71	8.20
Avg. income 1930	$\surd$			
Occ. shares 1930	$\surd$			
County fixed effects		$\surd$	$\surd$	$\surd$
Regional time trends			$\surd$	$\surd$
Physician rate				$\surd$

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. APF: Angrist-Pischke multivariate F test of excluded instruments.

Table 5.A4: First Stage for Postnatal Care, Aggregated Outcomes

MtypI	8.180**	3.178	2.493	2.306
	(3.069)	(2.035)	(1.728)	(1.643)
MtypII	3.962*	7.422***	4.573**	4.562***
	(2.092)	(2.449)	(1.637)	(1.532)
Mstat	-23.472	-38.954***	-13.648	-12.163
	(15.013)	(12.122)	(8.747)	(8.525)
CtypI	-4.946**	-3.455*	7.591***	7.615***
	(1.925)	(1.912)	(2.270)	(2.132)
CtypII	0.548	2.378	2.956	2.995
	(2.617)	(2.428)	(2.168)	(2.157)
Cstat	34.768**	53.147***	27.187***	25.878**
	(15.318)	(12.592)	(9.521)	(9.353)
Observations	275	275	275	275
APF	0.45	1.07	0.45	0.54
$\text{MtypI}_{(-1)}$	6.269**	1.297	1.413	1.353
	(2.992)	(1.372)	(0.999)	(0.943)
$\text{MtypII}_{(-1)}$	5.774***	7.613***	4.066***	3.997***
	(1.600)	(2.271)	(1.382)	(1.376)
$\text{Mstat}_{(-1)}$	1.757	1.728	6.423*	6.558*
	(2.860)	(3.077)	(3.159)	(3.185)
CtypI	-2.853*	-0.405	4.533*	4.467*
	(1.594)	(2.260)	(2.284)	(2.315)
CtypII	-1.551	3.654*	4.093*	4.103*
	(1.901)	(1.959)	(2.176)	(2.185)
Cstat	9.099***	10.484**	11.919*	12.067*
	(3.240)	(4.541)	(6.123)	(6.088)
Observations	250	250	250	250
APF	1.93	3.63	8.07	7.80
MtypI	6.273**	2.394	1.906	1.936
	(2.759)	(1.655)	(1.288)	(1.319)
MtypII	4.758***	7.247***	4.520***	4.519***
	(1.344)	(2.137)	(1.408)	(1.413)
Mstat	1.172	0.504	6.383**	6.345**
	(2.827)	(3.164)	(2.946)	(2.978)
$\text{CtypI}_{(+1)}$	-2.855*	-0.483	2.735	2.761
	(1.408)	(2.288)	(2.392)	(2.393)
$\text{CtypII}_{(+1)}$	-0.870	3.032	3.527*	3.543*
	(1.819)	(1.801)	(1.897)	(1.909)
$\text{Cstat}_{(+1)}$	8.416**	9.324*	12.536**	12.562**
	(3.176)	(4.562)	(5.780)	(5.811)
Observations	275	275	275	275
APF	1.85	3.97	8.71	8.20
Avg. income 1930	√			
Occ. shares 1930	√			
County fixed effects		√	√	√
Regional time trends			√	√
Physician rate				√

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. APF: Angrist-Pischke multivariate F test of excluded instruments.

Table 5.A5: Second Stage, Fertility of Married Women

Prenatal care	-0.006	-0.005*	-0.012**	-0.012**
	(0.006)	(0.003)	(0.006)	(0.006)
Postnatal care	0.005*	0.004*	0.008*	0.008*
	(0.003)	(0.002)	(0.004)	(0.004)
Observations	275	275	275	275
Prenatal care$_{(-1)}$	0.004*	0.002	0.001	0.001
	(0.002)	(0.002)	(0.001)	(0.001)
Postnatal care	-0.001	-0.001	-0.000	-0.000
	(0.002)	(0.001)	(0.001)	(0.001)
Observations	250	250	250	250
Prenatal care	0.001	-0.000	-0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)
Postnatal care$_{(+1)}$	0.001	-0.000	-0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.001)
Observations	275	275	275	275
Avg. income 1930	√			
Occ. shares 1930	√			
County fixed effects		√	√	√
Regional time trends			√	√
Physician rate				√

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A6: Second Stage, Fertility of Unmarried Women

Prenatal care	-0.008	-0.004	-0.014*	-0.014*
	(0.013)	(0.003)	(0.008)	(0.008)
Postnatal care	0.006	0.003	0.011*	0.011*
	(0.007)	(0.002)	(0.006)	(0.006)
Observations	275	275	275	275
Prenatal care$_{(-1)}$	-0.007	-0.001	-0.002	-0.002
	(0.006)	(0.002)	(0.002)	(0.002)
Postnatal care	0.009	0.000	0.001	0.001
	(0.006)	(0.002)	(0.002)	(0.002)
Observations	250	250	250	250
Prenatal care	-0.007*	-0.000	0.001	0.001
	(0.004)	(0.002)	(0.003)	(0.003)
Postnatal care$_{(+1)}$	0.009*	-0.000	-0.002	-0.002
	(0.005)	(0.002)	(0.002)	(0.002)
Observations	275	275	275	275
Avg. income 1930	√			
Occ. shares 1930	√			
County fixed effects		√	√	√
Regional time trends			√	√
Physician rate				√

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A7: Second Stage, Fertility of Engaged Women

Prenatal care	-0.053	-0.007	0.002	0.001
	(0.051)	(0.010)	(0.011)	(0.011)
Postnatal care	0.031	0.008	0.000	0.001
	(0.029)	(0.007)	(0.008)	(0.008)
Observations	225	225	225	225
Prenatal care$_{(-1)}$	-0.008	0.004	-0.000	-0.000
	(0.009)	(0.004)	(0.004)	(0.004)
Postnatal care	0.005	-0.000	0.001	0.001
	(0.008)	(0.003)	(0.004)	(0.004)
Observations	200	200	200	200
Prenatal care	-0.010	0.006	0.002	0.002
	(0.011)	(0.006)	(0.005)	(0.005)
Postnatal care$_{(+1)}$	0.008	-0.001	-0.003	-0.003
	(0.010)	(0.005)	(0.003)	(0.003)
Observations	225	225	225	225
Avg. income 1930	$\surd$			
Occ. shares 1930	$\surd$			
County fixed effects		$\surd$	$\surd$	$\surd$
Regional time trends			$\surd$	$\surd$
Physician rate				$\surd$

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A8: Reduced Form, Infant Mortality

MtypI	-0.021	-0.017	-0.024	-0.027
	(0.018)	(0.017)	(0.019)	(0.021)
MtypII	-0.021	-0.001	-0.011	-0.011
	(0.037)	(0.025)	(0.034)	(0.037)
Mstat	-0.069	0.037	-0.005	0.019
	(0.175)	(0.130)	(0.151)	(0.148)
CtypI	-0.010	0.025	-0.030	-0.029
	(0.031)	(0.020)	(0.081)	(0.080)
CtypII	0.079*	0.046	0.064*	0.065*
	(0.041)	(0.028)	(0.036)	(0.037)
Cstat	0.008	-0.080	-0.014	-0.035
	(0.183)	(0.123)	(0.136)	(0.135)
Observations	275	275	275	275
$\text{MtypI}_{(-1)}$	-0.028	-0.032***	-0.041***	-0.042***
	(0.020)	(0.011)	(0.011)	(0.011)
$\text{MtypII}_{(-1)}$	-0.034	-0.021	-0.023	-0.024
	(0.028)	(0.014)	(0.032)	(0.030)
$\text{Mstat}_{(-1)}$	-0.060**	-0.019	-0.042	-0.040
	(0.027)	(0.031)	(0.040)	(0.041)
CtypI	-0.008	0.021	-0.026	-0.028
	(0.030)	(0.021)	(0.074)	(0.072)
CtypII	0.091***	0.066***	0.068***	0.068***
	(0.031)	(0.015)	(0.019)	(0.019)
Cstat	-0.005	-0.038	0.002	0.005
	(0.037)	(0.044)	(0.034)	(0.034)
Observations	250	250	250	250
MtypI	-0.015	-0.016	-0.019	-0.022
	(0.018)	(0.016)	(0.015)	(0.016)
MtypII	0.005	0.014	0.022	0.022
	(0.024)	(0.018)	(0.026)	(0.027)
Mstat	-0.013	0.032	-0.006	-0.002
	(0.032)	(0.032)	(0.037)	(0.037)
$\text{CtypI}_{(+1)}$	-0.018	0.008	-0.093*	-0.096*
	(0.030)	(0.020)	(0.050)	(0.051)
$\text{CtypII}_{(+1)}$	0.054*	0.030	0.030	0.028
	(0.030)	(0.028)	(0.031)	(0.031)
$\text{Cstat}_{(+1)}$	-0.048	-0.113***	-0.082	-0.085
	(0.048)	(0.039)	(0.051)	(0.051)
Observations	275	275	275	275
Avg. income 1930	√			
Occ. shares 1930	√			
County fixed effects		√	√	√
Regional time trends			√	√
Physician rate				√

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A9: Reduced Form, Stillbirths

MtypI	-0.032	0.008	0.003	0.007
	(0.024)	(0.014)	(0.015)	(0.016)
MtypII	0.005	-0.003	0.032	0.032
	(0.041)	(0.060)	(0.069)	(0.066)
Mstat	0.391***	0.303**	0.317**	0.284**
	(0.126)	(0.118)	(0.124)	(0.125)
CtypI	0.001	-0.005	-0.026	-0.027
	(0.015)	(0.013)	(0.034)	(0.038)
CtypII	0.023	-0.010	-0.021	-0.022
	(0.047)	(0.053)	(0.068)	(0.066)
Cstat	-0.386***	-0.269**	-0.293**	-0.264*
	(0.125)	(0.127)	(0.142)	(0.147)
Observations	275	275	275	275
$\text{MtypI}_{(-1)}$	-0.028	0.013	0.008	0.010
	(0.027)	(0.011)	(0.012)	(0.011)
$\text{MtypII}_{(-1)}$	-0.033	-0.037	-0.035	-0.033
	(0.028)	(0.031)	(0.027)	(0.026)
$\text{Mstat}_{(-1)}$	-0.004	0.008	-0.045	-0.048
	(0.049)	(0.043)	(0.076)	(0.076)
CtypI	-0.011	-0.018	-0.038	-0.036
	(0.014)	(0.013)	(0.047)	(0.051)
CtypII	0.052	-0.004	-0.001	-0.001
	(0.033)	(0.028)	(0.037)	(0.036)
Cstat	0.002	0.040	0.040	0.037
	(0.035)	(0.046)	(0.066)	(0.064)
Observations	250	250	250	250
MtypI	-0.030	0.003	-0.001	0.003
	(0.024)	(0.013)	(0.014)	(0.014)
MtypII	-0.005	-0.009	0.015	0.015
	(0.024)	(0.028)	(0.039)	(0.037)
Mstat	0.070**	0.080**	0.032	0.026
	(0.028)	(0.029)	(0.080)	(0.078)
$\text{CtypI}_{(+1)}$	-0.003	-0.014	0.021	0.024
	(0.013)	(0.012)	(0.026)	(0.029)
$\text{CtypII}_{(+1)}$	0.029	-0.018	-0.015	-0.012
	(0.031)	(0.024)	(0.026)	(0.025)
$\text{Cstat}_{(+1)}$	-0.066**	-0.055	-0.063	-0.059
	(0.032)	(0.033)	(0.060)	(0.061)
Observations	275	275	275	275
Avg. income 1930	√			
Occ. shares 1930	√			
County fixed effects		√	√	√
Regional time trends			√	√
Physician rate				√

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A10: Reduced Form, Fertility

MtypI	0.016	-0.005	-0.002	-0.001
	(0.013)	(0.007)	(0.009)	(0.008)
MtypII	0.012	-0.013	-0.011	-0.011
	(0.015)	(0.009)	(0.008)	(0.007)
Mstat	-0.180*	-0.165***	-0.156***	-0.163***
	(0.096)	(0.042)	(0.036)	(0.036)
CtypI	-0.044***	-0.041***	-0.045***	-0.045***
	(0.011)	(0.006)	(0.015)	(0.015)
CtypII	0.004	0.004	-0.005	-0.005
	(0.018)	(0.008)	(0.007)	(0.007)
Cstat	0.169	0.146***	0.156***	0.162***
	(0.099)	(0.042)	(0.036)	(0.037)
Observations	275	275	275	275
$\text{MtypI}_{(-1)}$	0.018	0.002	0.005	0.005
	(0.014)	(0.004)	(0.003)	(0.003)
$\text{MtypII}_{(-1)}$	0.014	0.001	-0.003	-0.003
	(0.011)	(0.006)	(0.005)	(0.005)
$\text{Mstat}_{(-1)}$	-0.015	-0.002	0.016	0.016
	(0.013)	(0.013)	(0.014)	(0.014)
CtypI	-0.042***	-0.039***	-0.022**	-0.021**
	(0.009)	(0.007)	(0.010)	(0.010)
CtypII	0.006	-0.006	-0.007	-0.007
	(0.015)	(0.005)	(0.006)	(0.006)
Cstat	0.008	-0.018	0.004	0.004
	(0.015)	(0.012)	(0.011)	(0.011)
Observations	250	250	250	250
MtypI	0.014	-0.006	-0.005	-0.004
	(0.013)	(0.006)	(0.009)	(0.009)
MtypII	0.012	-0.006	-0.009	-0.009
	(0.012)	(0.006)	(0.007)	(0.006)
Mstat	-0.021*	-0.006	-0.006	-0.006
	(0.010)	(0.007)	(0.011)	(0.011)
$\text{CtypI}_{(+1)}$	-0.033***	-0.034***	-0.027***	-0.027***
	(0.008)	(0.008)	(0.009)	(0.009)
$\text{CtypII}_{(+1)}$	0.009	0.002	-0.001	-0.001
	(0.015)	(0.007)	(0.006)	(0.006)
$\text{Cstat}_{(+1)}$	0.012	-0.027*	-0.011	-0.011
	(0.016)	(0.014)	(0.012)	(0.012)
Observations	275	275	275	275
Avg. income 1930	√			
Occ. shares 1930	√			
County fixed effects		√	√	√
Regional time trends			√	√
Physician rate				√

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A11: Reduced Form, Fertility of Married Women

MtypI	0.021	-0.004	-0.002	-0.001
	(0.015)	(0.007)	(0.008)	(0.007)
MtypII	0.003	-0.015	-0.012	-0.012
	(0.017)	(0.010)	(0.009)	(0.008)
Mstat	-0.232**	-0.176***	-0.140***	-0.148***
	(0.108)	(0.039)	(0.039)	(0.039)
CtypI	-0.055***	-0.043***	-0.047***	-0.047***
	(0.014)	(0.006)	(0.016)	(0.016)
CtypII	0.016	0.005	-0.007	-0.007
	(0.018)	(0.009)	(0.008)	(0.007)
Cstat	0.210*	0.157***	0.144***	0.151***
	(0.110)	(0.038)	(0.037)	(0.038)
Observations	275	275	275	275
$\text{MtypI}_{(-1)}$	0.023	0.003	0.005	0.005
	(0.016)	(0.004)	(0.004)	(0.004)
$\text{MtypII}_{(-1)}$	0.009	0.000	-0.003	-0.003
	(0.013)	(0.006)	(0.005)	(0.005)
$\text{Mstat}_{(-1)}$	-0.014	-0.002	0.021	0.021
	(0.013)	(0.014)	(0.013)	(0.014)
CtypI	-0.050***	-0.041***	-0.021**	-0.020*
	(0.013)	(0.008)	(0.010)	(0.010)
CtypII	0.013	-0.006	-0.009	-0.009
	(0.016)	(0.006)	(0.006)	(0.006)
Cstat	-0.005	-0.016	0.006	0.006
	(0.016)	(0.012)	(0.011)	(0.011)
Observations	250	250	250	250
MtypI	0.019	-0.005	-0.004	-0.004
	(0.016)	(0.006)	(0.008)	(0.008)
MtypII	0.008	-0.008	-0.011	-0.011*
	(0.014)	(0.006)	(0.007)	(0.007)
Mstat	-0.021*	-0.007	-0.002	-0.003
	(0.011)	(0.008)	(0.013)	(0.013)
$\text{CtypI}_{(+1)}$	-0.040***	-0.036***	-0.027***	-0.027***
	(0.012)	(0.009)	(0.009)	(0.009)
$\text{CtypII}_{(+1)}$	0.018	0.003	-0.001	-0.000
	(0.016)	(0.009)	(0.008)	(0.008)
$\text{Cstat}_{(+1)}$	0.001	-0.027*	-0.010	-0.009
	(0.022)	(0.015)	(0.014)	(0.014)
Observations	275	275	275	275
Avg. income 1930	√			
Occ. shares 1930	√			
County fixed effects		√	√	√
Regional time trends			√	√
Physician rate				√

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A12: Reduced Form, Fertility of Unmarried Women

MtypI	-0.014	-0.012	-0.006	-0.007
	(0.029)	(0.016)	(0.017)	(0.018)
MtypII	0.092*	0.008	-0.002	-0.002
	(0.045)	(0.014)	(0.012)	(0.012)
Mstat	0.098	-0.103	-0.308**	-0.301**
	(0.231)	(0.148)	(0.122)	(0.127)
CtypI	0.018	-0.019*	-0.026	-0.026
	(0.039)	(0.009)	(0.028)	(0.030)
CtypII	-0.101*	-0.008	0.010	0.010
	(0.059)	(0.015)	(0.014)	(0.015)
Cstat	-0.023	0.088	0.300**	0.294**
	(0.244)	(0.154)	(0.114)	(0.118)
Observations	275	275	275	275
$\text{MtypI}_{(-1)}$	-0.018	-0.014	-0.007	-0.008
	(0.030)	(0.010)	(0.009)	(0.009)
$\text{MtypII}_{(-1)}$	0.052	0.002	-0.000	-0.001
	(0.034)	(0.015)	(0.021)	(0.021)
$\text{Mstat}_{(-1)}$	-0.027	-0.009	-0.005	-0.004
	(0.034)	(0.023)	(0.032)	(0.033)
CtypI	0.012	-0.021	-0.026	-0.026
	(0.042)	(0.012)	(0.036)	(0.039)
CtypII	-0.064	0.002	0.015	0.015
	(0.047)	(0.012)	(0.015)	(0.015)
Cstat	0.097	-0.025	0.012	0.013
	(0.069)	(0.027)	(0.036)	(0.037)
Observations	250	250	250	250
MtypI	-0.019	-0.015	-0.006	-0.008
	(0.032)	(0.016)	(0.018)	(0.019)
MtypII	0.055*	0.006	0.013	0.013
	(0.032)	(0.011)	(0.018)	(0.018)
Mstat	-0.011	0.007	-0.008	-0.006
	(0.034)	(0.024)	(0.031)	(0.032)
$\text{CtypI}_{(+1)}$	0.012	-0.015	-0.021	-0.022
	(0.039)	(0.013)	(0.029)	(0.028)
$\text{CtypII}_{(+1)}$	-0.067	-0.002	-0.003	-0.004
	(0.042)	(0.018)	(0.022)	(0.022)
$\text{Cstat}_{(+1)}$	0.079	-0.038	-0.021	-0.022
	(0.072)	(0.028)	(0.031)	(0.031)
Observations	275	275	275	275
Avg. income 1930	√			
Occ. shares 1930	√			
County fixed effects		√	√	√
Regional time trends			√	√
Physician rate				√

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A13: Reduced Form, Fertility of Engaged Women

MtypI	0.066	0.015	-0.027	-0.026
	(0.043)	(0.021)	(0.018)	(0.016)
MtypII	0.167**	0.071*	0.010	0.010
	(0.066)	(0.038)	(0.030)	(0.031)
Mstat	0.046	-0.042	-0.277	-0.279
	(0.399)	(0.278)	(0.334)	(0.351)
CtypI	-0.032	-0.087**	0.094	0.094
	(0.087)	(0.034)	(0.105)	(0.104)
CtypII	-0.244***	-0.041	0.027	0.026
	(0.080)	(0.048)	(0.039)	(0.039)
Cstat	-0.001	0.051	0.211	0.213
	(0.401)	(0.277)	(0.300)	(0.320)
Observations	225	225	225	225
$\text{MtypI}_{(-1)}$	0.064	0.026*	0.008	0.008
	(0.043)	(0.015)	(0.015)	(0.015)
$\text{MtypII}_{(-1)}$	0.071	0.034	-0.011	-0.011
	(0.046)	(0.030)	(0.029)	(0.030)
$\text{Mstat}_{(-1)}$	-0.040	0.016	0.015	0.015
	(0.071)	(0.045)	(0.073)	(0.073)
CtypI	-0.013	-0.067**	0.043	0.043
	(0.092)	(0.030)	(0.150)	(0.149)
CtypII	-0.162**	-0.004	0.034	0.034
	(0.067)	(0.039)	(0.038)	(0.039)
Cstat	0.061	-0.035	-0.027	-0.027
	(0.097)	(0.055)	(0.088)	(0.088)
Observations	200	200	200	200
MtypI	0.054	0.010	-0.021	-0.021
	(0.045)	(0.020)	(0.017)	(0.016)
MtypII	0.095*	0.063*	0.032	0.032
	(0.048)	(0.031)	(0.036)	(0.036)
Mstat	-0.068	-0.006	-0.042	-0.042
	(0.062)	(0.050)	(0.079)	(0.079)
$\text{CtypI}_{(+1)}$	-0.064	-0.108***	-0.042	-0.042
	(0.076)	(0.019)	(0.069)	(0.069)
$\text{CtypII}_{(+1)}$	-0.188**	-0.044	-0.012	-0.012
	(0.069)	(0.032)	(0.027)	(0.027)
$\text{Cstat}_{(+1)}$	0.085	-0.013	-0.016	-0.017
	(0.113)	(0.056)	(0.069)	(0.070)
Observations	225	225	225	225
Avg. income 1930	√			
Occ. shares 1930	√			
County fixed effects		√	√	√
Regional time trends			√	√
Physician rate				√

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A14: Reduced Form, Maternal Mortality

MtypI	-0.018	-0.027	0.002	-0.004
	(0.048)	(0.071)	(0.091)	(0.089)
MtypII	-0.148	-0.183	-0.049	-0.050
	(0.146)	(0.184)	(0.200)	(0.203)
Mstat	0.829*	0.932**	0.413	0.465
	(0.404)	(0.451)	(0.834)	(0.879)
CtypI	-0.172**	-0.174*	-0.367	-0.366
	(0.070)	(0.090)	(0.362)	(0.353)
CtypII	0.112	0.252	0.140	0.141
	(0.147)	(0.182)	(0.302)	(0.306)
Cstat	-0.857*	-0.980*	-0.511	-0.556
	(0.437)	(0.501)	(0.781)	(0.828)
Observations	275	275	275	275
$\text{MtypI}_{(-1)}$	0.042	0.045	0.088	0.086
	(0.050)	(0.054)	(0.065)	(0.064)
$\text{MtypII}_{(-1)}$	-0.070	0.026	0.189	0.186
	(0.117)	(0.112)	(0.123)	(0.121)
$\text{Mstat}_{(-1)}$	0.148	0.234	0.156	0.161
	(0.194)	(0.206)	(0.255)	(0.259)
CtypI	-0.195**	-0.189	-0.655	-0.657
	(0.091)	(0.117)	(0.401)	(0.391)
CtypII	0.030	-0.000	0.024	0.024
	(0.116)	(0.126)	(0.159)	(0.160)
Cstat	-0.179	-0.283	-0.150	-0.145
	(0.165)	(0.186)	(0.268)	(0.267)
Observations	250	250	250	250
MtypI	-0.013	-0.019	-0.014	-0.019
	(0.053)	(0.071)	(0.098)	(0.097)
MtypII	-0.099	-0.060	0.014	0.014
	(0.088)	(0.067)	(0.070)	(0.071)
Mstat	-0.113	-0.030	-0.128	-0.122
	(0.106)	(0.114)	(0.246)	(0.247)
$\text{CtypI}_{(+1)}$	-0.181***	-0.180***	-0.121	-0.125
	(0.046)	(0.064)	(0.311)	(0.307)
$\text{CtypII}_{(+1)}$	0.040	0.048	0.006	0.004
	(0.085)	(0.125)	(0.150)	(0.150)
$\text{Cstat}_{(+1)}$	0.102	0.049	0.110	0.106
	(0.110)	(0.178)	(0.168)	(0.165)
Observations	275	275	275	275
Avg. income 1930	✓			
Occ. shares 1930	✓			
County fixed effects		✓	✓	✓
Regional time trends			✓	✓
Physician rate				✓

Regressions include birth year fixed effects and are weighted by birth numbers. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A15: First Stage, Individual Mortality

Prenatal care			
MtypI_w	-0.0007	0.0001	0.0001
	(0.0009)	(0.0004)	(0.0004)
MtypII_w	0.0027**	0.0012**	0.0011**
	(0.0010)	(0.0006)	(0.0005)
Mstat_w	0.0055**	0.0021	0.0022
	(0.0024)	(0.0014)	(0.0014)
CtypI_w	-0.0003	0.0010	0.0009
	(0.0012)	(0.0008)	(0.0008)
CtypII_w	0.0032***	0.0015**	0.0015**
	(0.0011)	(0.0007)	(0.0007)
Cstat_w	0.0069**	0.0033**	0.0033**
	(0.0028)	(0.0016)	(0.0016)
Observations	1292421	1292421	1292421
APF	1.21	3.31	3.57
Postnatal care			
MtypI_w	-0.0007	0.0002	0.0002
	(0.0011)	(0.0003)	(0.0003)
MtypII_w	0.0048***	0.0018**	0.0018**
	(0.0015)	(0.0007)	(0.0007)
Mstat_w	0.0041	0.0037**	0.0037**
	(0.0026)	(0.0015)	(0.0015)
CtypI_w	-0.0006	0.0007	0.0007
	(0.0015)	(0.0005)	(0.0005)
CtypII_w	0.0051***	0.0026**	0.0026**
	(0.0017)	(0.0010)	(0.0010)
Cstat_w	0.0056*	0.0046**	0.0046**
	(0.0031)	(0.0019)	(0.0019)
Observations	1292421	1292421	1292421
APF	1.21	3.31	3.57
County fixed effects	$\surd$	$\surd$	$\surd$
Regional time trends		$\surd$	$\surd$
Physician rate			$\surd$

Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A16: Reduced Form, Died Before Reaching Age 1/5

Died before reaching age 1			
MtypI_w	-0.0002	-0.0004	-0.0004
	(0.0004)	(0.0004)	(0.0004)
MtypII_w	0.0002	-0.0003	-0.0003
	(0.0004)	(0.0006)	(0.0006)
Mstat_w	-0.0012*	0.0003	0.0003
	(0.0006)	(0.0009)	(0.0009)
CtypI_w	-0.0009*	-0.0013**	-0.0013**
	(0.0005)	(0.0006)	(0.0006)
CtypII_w	0.0009	0.0001	0.0001
	(0.0006)	(0.0007)	(0.0007)
Cstat_w	-0.0030***	-0.0012*	-0.0012*
	(0.0007)	(0.0007)	(0.0007)
Observations	1292421	1292421	1292421
Died before reaching age 5			
MtypI_w	-0.0003	-0.0003	-0.0003
	(0.0005)	(0.0005)	(0.0005)
MtypII_w	0.0004	-0.0002	-0.0002
	(0.0003)	(0.0006)	(0.0006)
Mstat_w	-0.0010*	0.0005	0.0005
	(0.0006)	(0.0009)	(0.0009)
CtypI_w	-0.0013**	-0.0015**	-0.0015**
	(0.0005)	(0.0006)	(0.0006)
CtypII_w	0.0011	0.0002	0.0002
	(0.0006)	(0.0007)	(0.0008)
Cstat_w	-0.0029***	-0.0011	-0.0011
	(0.0006)	(0.0007)	(0.0007)
Observations	1292421	1292421	1292421
County fixed effects	√	√	√
Regional time trends		√	√
Physician rate			√

Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A17: Reduced Form, Died Before Reaching Age 35/63

Died before reaching age 35			
MtypI_w	-0.0003	-0.0004	-0.0004
	(0.0005)	(0.0004)	(0.0004)
MtypII_w	0.0004	-0.0002	-0.0002
	(0.0006)	(0.0007)	(0.0007)
Mstat_w	-0.0012	-0.0002	-0.0002
	(0.0007)	(0.0010)	(0.0010)
CtypI_w	-0.0015***	-0.0018***	-0.0018***
	(0.0005)	(0.0003)	(0.0003)
CtypII_w	0.0008	-0.0001	-0.0001
	(0.0008)	(0.0008)	(0.0008)
Cstat_w	-0.0033***	-0.0021**	-0.0021**
	(0.0008)	(0.0009)	(0.0009)
Observations	1292421	1292421	1292421
Died before reaching age 63			
MtypI_w	-0.0019*	-0.0010	-0.0010
	(0.0010)	(0.0007)	(0.0007)
MtypII_w	0.0008	-0.0001	-0.0001
	(0.0011)	(0.0011)	(0.0011)
Mstat_w	-0.0016**	-0.0034*	-0.0034*
	(0.0006)	(0.0019)	(0.0019)
CtypI_w	-0.0060***	-0.0047***	-0.0047***
	(0.0011)	(0.0005)	(0.0005)
CtypII_w	0.0010	-0.0003	-0.0003
	(0.0015)	(0.0017)	(0.0017)
Cstat_w	-0.0068***	-0.0082**	-0.0082**
	(0.0013)	(0.0030)	(0.0030)
Observations	1292421	1292421	1292421
County fixed effects	√	√	√
Regional time trends		√	√
Physician rate			√

Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A18: First Stage for Prenatal Care, Health and Socioeconomic Status

MtypI	1.895	1.260	0.763	0.520	0.527
	(3.365)	(1.733)	(1.676)	(1.370)	(1.373)
MtypII	-2.251	2.161	3.695**	4.006***	4.005***
	(3.658)	(1.995)	(1.683)	(1.294)	(1.294)
Mstat	11.588	-3.693	3.676	9.436	9.416
	(12.968)	(13.904)	(10.607)	(10.421)	(10.349)
CtypI	3.743	0.547	12.113***	10.977***	10.993***
	(3.004)	(2.151)	(4.279)	(3.358)	(3.342)
CtypII	4.332	4.829*	2.876*	3.050*	3.096*
	(3.308)	(2.796)	(1.613)	(1.521)	(1.511)
Cstat	-2.012	15.478	5.918	0.829	0.758
	(13.695)	(15.351)	(11.377)	(11.338)	(11.272)
Observations	6990	6990	6990	6990	6990
APF	1.42	1.24	0.89	0.89	0.89
$\text{MtypI}_{(-1)}$	1.591	1.146	0.821	0.938	0.959
	(2.851)	(1.444)	(1.125)	(1.054)	(1.046)
$\text{MtypII}_{(-1)}$	2.876	5.948***	6.659***	6.750***	6.726***
	(2.524)	(1.465)	(1.084)	(0.966)	(0.957)
$\text{Mstat}_{(-1)}$	15.458***	10.823***	10.752***	11.215***	11.186***
	(4.454)	(2.862)	(3.488)	(3.504)	(3.475)
CtypI	3.391	0.348	6.929*	5.899*	5.889*
	(3.302)	(2.106)	(3.826)	(3.354)	(3.340)
CtypII	-0.547	1.597	0.509	0.533	0.551
	(2.973)	(2.199)	(1.047)	(1.087)	(1.073)
Cstat	-4.552	2.273	0.919	1.109	1.052
	(4.810)	(3.183)	(2.734)	(2.698)	(2.711)
Observations	6524	6524	6524	6524	6524
APF	1.54	2.86	5.89	5.80	5.92
MtypI	2.285	1.892	1.492	1.261	1.269
	(3.254)	(1.797)	(1.571)	(1.332)	(1.343)
MtypII	1.203	5.381***	6.002***	6.391***	6.418***
	(2.506)	(1.516)	(1.300)	(0.949)	(0.960)
Mstat	14.964***	10.475***	9.380***	10.160***	10.072***
	(4.551)	(2.653)	(2.885)	(3.009)	(2.972)
$\text{CtypI}_{(+1)}$	2.945	0.111	8.534*	6.516*	6.511*
	(2.825)	(2.290)	(4.902)	(3.713)	(3.722)
$\text{CtypII}_{(+1)}$	0.467	1.413	0.284	0.169	0.178
	(2.612)	(1.452)	(0.773)	(0.749)	(0.756)
$\text{Cstat}_{(+1)}$	-5.005	0.656	0.941	0.796	0.780
	(4.126)	(3.160)	(2.819)	(2.769)	(2.770)
Observations	6990	6990	6990	6990	6990
APF	1.71	4.03	7.91	9.36	9.49
Avg. income 1930	√				
Occ. shares 1930	√				
County fixed effects		√	√	√	√
Regional time trends			√	√	√
Physician rate				√	√
Social background					√

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors (SE) are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A19: First Stage for Postnatal Care, Health and Socioeconomic Status

MtypI	8.161***	3.234	1.757	1.699	1.728
	(2.487)	(1.917)	(1.503)	(1.432)	(1.420)
MtypII	0.875	6.559***	4.334**	4.408**	4.358**
	(2.471)	(2.017)	(1.777)	(1.754)	(1.741)
Mstat	-9.583	-38.856***	-10.666	-9.300	-9.375
	(13.251)	(12.093)	(9.949)	(10.137)	(10.029)
CtypI	0.868	-4.603**	9.348***	9.078***	9.118***
	(2.811)	(2.014)	(2.947)	(2.818)	(2.815)
CtypII	3.325	3.038	6.098**	6.139***	6.181***
	(3.395)	(2.005)	(2.185)	(2.170)	(2.141)
Cstat	22.198	51.713***	21.425**	20.218**	20.201**
	(13.336)	(12.430)	(8.918)	(8.841)	(8.751)
Observations	6990	6990	6990	6990	6990
APF	1.42	1.24	0.89	0.89	0.89
$\text{MtypI}_{(-1)}$	6.214***	1.519	0.636	0.720	0.686
	(1.941)	(1.198)	(0.755)	(0.775)	(0.767)
$\text{MtypII}_{(-1)}$	3.393*	7.006***	4.286***	4.351***	4.340***
	(1.867)	(1.883)	(1.227)	(1.202)	(1.185)
$\text{Mstat}_{(-1)}$	6.601**	2.145	3.355	3.686	3.704
	(2.438)	(2.745)	(2.888)	(2.904)	(2.879)
CtypI	1.387	-1.007	7.824**	7.085**	7.108**
	(2.418)	(2.267)	(3.120)	(2.748)	(2.739)
CtypII	0.734	3.853**	6.607***	6.624***	6.631***
	(2.500)	(1.761)	(1.815)	(1.787)	(1.771)
Cstat	5.923*	9.088**	7.573*	7.710*	7.665*
	(2.895)	(4.201)	(4.412)	(4.392)	(4.329)
Observations	6524	6524	6524	6524	6524
APF	1.54	2.86	5.89	5.80	5.92
MtypI	6.572***	2.849*	1.632**	1.626**	1.637**
	(1.773)	(1.397)	(0.790)	(0.767)	(0.757)
MtypII	2.807*	6.731***	4.452***	4.461***	4.434***
	(1.508)	(1.797)	(1.409)	(1.407)	(1.406)
Mstat	4.935*	-0.005	1.466	1.484	1.412
	(2.613)	(3.161)	(3.040)	(3.149)	(3.141)
$\text{CtypI}_{(+1)}$	0.479	-1.757	5.950*	5.904*	5.933*
	(2.043)	(2.296)	(3.368)	(3.129)	(3.134)
$\text{CtypII}_{(+1)}$	0.977	3.083*	5.288***	5.285***	5.304***
	(2.299)	(1.528)	(1.390)	(1.395)	(1.382)
$\text{Cstat}_{(+1)}$	5.617**	7.730*	5.824	5.821	5.825
	(2.389)	(4.276)	(4.732)	(4.721)	(4.704)
Observations	6990	6990	6990	6990	6990
APF	1.71	4.03	7.91	9.36	9.49
Avg. income 1930	√				
Occ. shares 1930	√				
County fixed effects		√	√	√	√
Regional time trends			√	√	√
Physician rate				√	√
Social background					√

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors (SE) are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A20: Second Stage, Good General Health Condition

Prenatal care	-0.000	0.001	-0.002	-0.002	-0.001
	(0.002)	(0.003)	(0.005)	(0.004)	(0.004)
Postnatal care	-0.001	0.000	0.003	0.003	0.002
	(0.001)	(0.002)	(0.004)	(0.003)	(0.003)
Observations	6990	6990	6990	6990	6990
Prenatal care$_{(-1)}$	0.000	-0.003	-0.002	-0.002	-0.001
	(0.001)	(0.002)	(0.003)	(0.003)	(0.003)
Postnatal care	-0.002	0.003	0.002	0.003	0.002
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	6524	6524	6524	6524	6524
Prenatal care	0.000	-0.000	-0.002	-0.002	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Postnatal care$_{(+1)}$	-0.001	0.002	0.003	0.003	0.003
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	$\surd$				
Occ. shares 1930	$\surd$				
County fixed effects		$\surd$	$\surd$	$\surd$	$\surd$
Regional time trends			$\surd$	$\surd$	$\surd$
Physician rate				$\surd$	$\surd$
Social background					$\surd$

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A21: Second Stage, Had a Severe Diagnosis

Prenatal care	-0.001	-0.000	-0.001	-0.003	-0.004
	(0.002)	(0.003)	(0.004)	(0.004)	(0.004)
Postnatal care	0.001	-0.000	0.000	0.002	0.002
	(0.001)	(0.002)	(0.003)	(0.002)	(0.003)
Observations	6990	6990	6990	6990	6990
Prenatal care$_{(-1)}$	0.002	-0.000	-0.000	-0.000	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Postnatal care	-0.001	-0.001	-0.001	-0.001	-0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	6524	6524	6524	6524	6524
Prenatal care	-0.001	-0.002	-0.001	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Postnatal care$_{(+1)}$	0.001	0.000	0.000	0.001	0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	$\surd$				
Occ. shares 1930	$\surd$				
County fixed effects		$\surd$	$\surd$	$\surd$	$\surd$
Regional time trends			$\surd$	$\surd$	$\surd$
Physician rate				$\surd$	$\surd$
Social background					$\surd$

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A22: Second Stage, Regular Medical Treatment

Prenatal care	-0.006	-0.009**	-0.012	-0.013	-0.013
	(0.004)	(0.004)	(0.010)	(0.010)	(0.010)
Postnatal care	0.003	0.005	0.008	0.009	0.009
	(0.003)	(0.003)	(0.008)	(0.007)	(0.007)
Observations	6990	6990	6990	6990	6990
Prenatal care$_{(-1)}$	-0.004	-0.005	-0.004	-0.004	-0.004
	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
Postnatal care	0.001	0.002	0.002	0.002	0.002
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	6524	6524	6524	6524	6524
Prenatal care	-0.004	-0.005	-0.002	-0.002	-0.002
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Postnatal care$_{(+1)}$	0.002	0.002	0.001	0.001	0.001
	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	√				
Occ. shares 1930	√				
County fixed effects		√	√	√	√
Regional time trends			√	√	√
Physician rate				√	√
Social background					√

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A23: Second Stage, Normal Weight

Prenatal care	0.001	-0.000	0.003	0.002	0.005
	(0.003)	(0.003)	(0.006)	(0.006)	(0.007)
Postnatal care	0.000	0.001	-0.001	-0.001	-0.003
	(0.002)	(0.002)	(0.005)	(0.004)	(0.005)
Observations	6990	6990	6990	6990	6990
Prenatal care$_{(-1)}$	0.001	0.001	-0.001	-0.002	-0.002
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
Postnatal care	-0.001	0.001	0.003	0.003	0.003
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
Observations	6524	6524	6524	6524	6524
Prenatal care	0.002	0.004	0.004	0.004	0.005
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Postnatal care$_{(+1)}$	-0.001	-0.002	-0.004	-0.004	-0.005*
	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	✓				
Occ. shares 1930	✓				
County fixed effects		✓	✓	✓	✓
Regional time trends			✓	✓	✓
Physician rate				✓	✓
Social background					✓

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A24: Second Stage, Only Able to Run Less than 100 Meters

Prenatal care	0.000	0.001	0.001	0.001	0.001
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Postnatal care	-0.000	-0.002	-0.002	-0.002	-0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Observations	6990	6990	6990	6990	6990
Prenatal care$_{(-1)}$	-0.001	0.002	0.002	0.002	0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Postnatal care	0.001	-0.002	-0.003*	-0.003*	-0.002*
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Observations	6524	6524	6524	6524	6524
Prenatal care	-0.001	-0.001	-0.002	-0.002	-0.003
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Postnatal care$_{(+1)}$	0.001	0.001	0.001	0.001	0.002
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	√				
Occ. shares 1930	√				
County fixed effects		√	√	√	√
Regional time trends			√	√	√
Physician rate				√	√
Social background					√

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A25: Second Stage, Number of Long-term Illnesses

Prenatal care	-0.007	-0.016	-0.019	-0.022	-0.023
	(0.005)	(0.011)	(0.016)	(0.016)	(0.017)
Postnatal care	0.005	0.009	0.012	0.014	0.015
	(0.004)	(0.008)	(0.013)	(0.013)	(0.013)
Observations	6990	6990	6990	6990	6990
Prenatal care$_{(-1)}$	-0.001	-0.005	-0.002	-0.002	-0.003
	(0.005)	(0.008)	(0.007)	(0.006)	(0.006)
Postnatal care	0.001	0.003	0.001	0.001	0.001
	(0.004)	(0.007)	(0.006)	(0.005)	(0.005)
Observations	6524	6524	6524	6524	6524
Prenatal care	-0.004	-0.009	-0.007	-0.007	-0.008
	(0.004)	(0.008)	(0.006)	(0.006)	(0.006)
Postnatal care$_{(+1)}$	0.003	0.003	0.004	0.004	0.004
	(0.003)	(0.006)	(0.005)	(0.005)	(0.005)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	$\surd$				
Occ. shares 1930	$\surd$				
County fixed effects		$\surd$	$\surd$	$\surd$	$\surd$
Regional time trends			$\surd$	$\surd$	$\surd$
Physician rate				$\surd$	$\surd$
Social background					$\surd$

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A26: Second Stage, Disabled

Prenatal care	-0.000	-0.000	-0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Postnatal care	0.000	-0.000	-0.000	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Observations	6990	6990	6990	6990	6990
Prenatal care$_{(-1)}$	0.001	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Postnatal care	-0.000	-0.001	-0.002	-0.002	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	6524	6524	6524	6524	6524
Prenatal care	-0.001	-0.002	-0.003	-0.003	-0.003*
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Postnatal care$_{(+1)}$	0.001	0.001	0.002	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	$\surd$				
Occ. shares 1930	$\surd$				
County fixed effects		$\surd$	$\surd$	$\surd$	$\surd$
Regional time trends			$\surd$	$\surd$	$\surd$
Physician rate				$\surd$	$\surd$
Social background					$\surd$

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A27: Reduced Form, Good General Health Condition

MtypI	-0.010	0.002	-0.003	-0.002	-0.001
	(0.014)	(0.017)	(0.021)	(0.019)	(0.018)
MtypII	-0.006	-0.000	-0.006	-0.008	-0.006
	(0.013)	(0.015)	(0.013)	(0.012)	(0.012)
Mstat	-0.032	-0.008	0.007	-0.020	0.005
	(0.067)	(0.106)	(0.114)	(0.122)	(0.119)
CtypI	0.007	0.007	0.030	0.036	0.040
	(0.007)	(0.011)	(0.035)	(0.032)	(0.031)
CtypII	0.013	0.027	0.032	0.032	0.031
	(0.016)	(0.022)	(0.021)	(0.020)	(0.019)
Cstat	0.020	0.009	-0.009	0.015	-0.003
	(0.064)	(0.103)	(0.114)	(0.120)	(0.116)
Observations	6990	6990	6990	6990	6990
$\text{MtypI}_{(-1)}$	-0.018**	-0.015**	-0.016**	-0.017**	-0.014*
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)
$\text{MtypII}_{(-1)}$	0.005	0.021	0.025	0.024	0.026
	(0.016)	(0.016)	(0.017)	(0.017)	(0.016)
$\text{Mstat}_{(-1)}$	-0.016	-0.015	-0.030	-0.034	-0.027
	(0.016)	(0.019)	(0.021)	(0.021)	(0.021)
CtypI	0.008	0.006	0.007	0.015	0.019
	(0.005)	(0.009)	(0.023)	(0.024)	(0.024)
CtypII	0.004	0.006	0.005	0.004	0.002
	(0.017)	(0.020)	(0.020)	(0.020)	(0.019)
Cstat	-0.002	-0.012	0.002	-0.000	0.000
	(0.019)	(0.022)	(0.024)	(0.023)	(0.023)
Observations	6524	6524	6524	6524	6524
MtypI	-0.007	0.005	0.002	0.003	0.004
	(0.013)	(0.016)	(0.017)	(0.016)	(0.015)
MtypII	-0.002	0.007	0.007	0.006	0.008
	(0.010)	(0.013)	(0.010)	(0.010)	(0.010)
Mstat	0.021	0.021	0.014	0.011	0.016
	(0.027)	(0.025)	(0.025)	(0.025)	(0.026)
$\text{CtypI}_{(+1)}$	0.002	-0.003	-0.023	-0.016	-0.016
	(0.007)	(0.008)	(0.033)	(0.037)	(0.035)
$\text{CtypII}_{(+1)}$	0.011	0.024	0.024	0.024	0.023
	(0.013)	(0.016)	(0.015)	(0.015)	(0.014)
$\text{Cstat}_{(+1)}$	-0.035	-0.035	-0.025	-0.025	-0.022
	(0.031)	(0.039)	(0.039)	(0.039)	(0.039)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	√				
Occ. shares 1930	√				
County fixed effects		√	√	√	√
Regional time trends			√	√	√
Physician rate				√	√
Social background					√

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A28: Reduced Form, Had a Severe Diagnosis

MtypI	0.003	0.008	0.010	0.010	0.009
	(0.006)	(0.008)	(0.009)	(0.008)	(0.008)
MtypII	-0.006	-0.034	-0.023	-0.022	-0.025
	(0.017)	(0.021)	(0.021)	(0.021)	(0.021)
Mstat	-0.134**	-0.121	-0.161*	-0.150	-0.166*
	(0.062)	(0.084)	(0.085)	(0.089)	(0.088)
CtypI	-0.001	0.009	0.014	0.012	0.012
	(0.006)	(0.008)	(0.031)	(0.031)	(0.031)
CtypII	0.001	0.017	0.003	0.004	0.005
	(0.017)	(0.025)	(0.025)	(0.025)	(0.025)
Cstat	0.138**	0.120	0.152*	0.143	0.155*
	(0.062)	(0.086)	(0.087)	(0.091)	(0.088)
Observations	6990	6990	6990	6990	6990
$\text{MtypI}_{(-1)}$	-0.012*	-0.020***	-0.023***	-0.023***	-0.024***
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
$\text{MtypII}_{(-1)}$	0.006	-0.007	-0.003	-0.002	-0.004
	(0.013)	(0.011)	(0.012)	(0.011)	(0.012)
$\text{Mstat}_{(-1)}$	-0.001	0.002	0.005	0.008	0.004
	(0.015)	(0.016)	(0.017)	(0.018)	(0.018)
CtypI	0.007	0.020**	0.011	0.004	0.005
	(0.007)	(0.008)	(0.032)	(0.029)	(0.029)
CtypII	-0.007	0.008	0.002	0.003	0.003
	(0.013)	(0.014)	(0.016)	(0.015)	(0.015)
Cstat	0.002	-0.013	-0.018	-0.017	-0.016
	(0.016)	(0.019)	(0.020)	(0.020)	(0.020)
Observations	6524	6524	6524	6524	6524
MtypI	0.004	0.011	0.011	0.010	0.010
	(0.006)	(0.008)	(0.009)	(0.008)	(0.008)
MtypII	0.002	-0.023**	-0.017**	-0.016**	-0.017**
	(0.011)	(0.010)	(0.008)	(0.008)	(0.007)
Mstat	-0.026	-0.025	-0.025*	-0.023*	-0.027*
	(0.017)	(0.016)	(0.013)	(0.013)	(0.014)
$\text{CtypI}_{(+1)}$	-0.001	0.010	0.023	0.018	0.017
	(0.006)	(0.008)	(0.020)	(0.022)	(0.021)
$\text{CtypII}_{(+1)}$	-0.005	0.010	0.002	0.002	0.002
	(0.011)	(0.017)	(0.013)	(0.013)	(0.013)
$\text{Cstat}_{(+1)}$	0.028*	0.027*	0.022*	0.022*	0.022
	(0.014)	(0.014)	(0.012)	(0.013)	(0.014)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	√				
Occ. shares 1930	√				
County fixed effects		√	√	√	√
Regional time trends			√	√	√
Physician rate				√	√
Social background					√

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A29: Reduced Form, Regular Medical Treatment

MtypI	0.004	-0.026*	-0.017	-0.017	-0.014
	(0.013)	(0.013)	(0.019)	(0.018)	(0.017)
MtypII	-0.001	-0.000	0.014	0.015	0.015
	(0.015)	(0.015)	(0.020)	(0.020)	(0.021)
Mstat	-0.290***	-0.281**	-0.364**	-0.360**	-0.350**
	(0.080)	(0.105)	(0.149)	(0.149)	(0.152)
CtypI	-0.019	-0.019	-0.061*	-0.062*	-0.058*
	(0.012)	(0.013)	(0.030)	(0.031)	(0.033)
CtypII	-0.014	-0.015	-0.023	-0.023	-0.023
	(0.014)	(0.013)	(0.022)	(0.022)	(0.022)
Cstat	0.270***	0.238**	0.323**	0.319**	0.307**
	(0.081)	(0.103)	(0.143)	(0.143)	(0.146)
Observations	6990	6990	6990	6990	6990
$\text{MtypI}_{(-1)}$	0.005	-0.014	-0.013	-0.012	-0.011
	(0.010)	(0.012)	(0.010)	(0.010)	(0.010)
$\text{MtypII}_{(-1)}$	-0.007	-0.015	-0.002	-0.002	-0.003
	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
$\text{Mstat}_{(-1)}$	-0.045	-0.031	-0.030	-0.027	-0.027
	(0.027)	(0.027)	(0.029)	(0.029)	(0.031)
CtypI	-0.019*	-0.005	-0.049*	-0.056*	-0.048
	(0.009)	(0.011)	(0.027)	(0.029)	(0.030)
CtypII	-0.004	0.021	0.018	0.018	0.018
	(0.013)	(0.016)	(0.018)	(0.018)	(0.018)
Cstat	0.024	-0.019	-0.021	-0.020	-0.024
	(0.026)	(0.035)	(0.036)	(0.035)	(0.035)
Observations	6524	6524	6524	6524	6524
MtypI	0.003	-0.027*	-0.024	-0.024	-0.021
	(0.013)	(0.013)	(0.016)	(0.016)	(0.014)
MtypII	0.008	0.007	0.014	0.015	0.016
	(0.011)	(0.013)	(0.016)	(0.017)	(0.017)
Mstat	-0.065*	-0.057	-0.055	-0.054	-0.054
	(0.032)	(0.034)	(0.037)	(0.037)	(0.034)
$\text{CtypI}_{(+1)}$	-0.014	-0.006	0.001	-0.004	-0.006
	(0.009)	(0.009)	(0.031)	(0.032)	(0.035)
$\text{CtypII}_{(+1)}$	-0.018*	-0.019*	-0.020	-0.020	-0.021
	(0.010)	(0.010)	(0.013)	(0.013)	(0.014)
$\text{Cstat}_{(+1)}$	0.044	0.008	0.002	0.002	-0.000
	(0.035)	(0.037)	(0.036)	(0.036)	(0.033)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	√				
Occ. shares 1930	√				
County fixed effects		√	√	√	√
Regional time trends			√	√	√
Physician rate				√	√
Social background					√

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A30: Reduced Form, Normal Weight

MtypI	0.020	0.034	0.036	0.038*	0.039*
	(0.018)	(0.022)	(0.024)	(0.021)	(0.019)
MtypII	-0.016	0.022	0.011	0.009	0.009
	(0.026)	(0.031)	(0.035)	(0.033)	(0.033)
Mstat	0.075	0.016	0.073	0.039	0.099
	(0.092)	(0.122)	(0.147)	(0.150)	(0.147)
CtypI	0.011	-0.000	-0.011	-0.004	0.003
	(0.010)	(0.013)	(0.033)	(0.040)	(0.038)
CtypII	0.026	-0.025	-0.015	-0.016	-0.014
	(0.020)	(0.024)	(0.033)	(0.032)	(0.032)
Cstat	-0.068	0.009	-0.046	-0.017	-0.061
	(0.092)	(0.132)	(0.152)	(0.155)	(0.155)
Observations	6990	6990	6990	6990	6990
$\text{MtypI}_{(-1)}$	-0.001	-0.023**	-0.025**	-0.026**	-0.014
	(0.009)	(0.010)	(0.010)	(0.011)	(0.011)
$\text{MtypII}_{(-1)}$	-0.020*	0.011	-0.006	-0.007	-0.007
	(0.011)	(0.014)	(0.015)	(0.015)	(0.015)
$\text{Mstat}_{(-1)}$	0.019	0.007	0.007	0.002	-0.003
	(0.026)	(0.026)	(0.029)	(0.026)	(0.027)
CtypI	0.023*	0.011	0.031	0.041	0.042
	(0.012)	(0.017)	(0.037)	(0.050)	(0.047)
CtypII	0.027**	0.008	0.023	0.023	0.015
	(0.012)	(0.019)	(0.020)	(0.020)	(0.018)
Cstat	-0.010	0.033	0.031	0.030	0.043
	(0.027)	(0.037)	(0.035)	(0.035)	(0.032)
Observations	6524	6524	6524	6524	6524
MtypI	0.023	0.031	0.035	0.036*	0.037*
	(0.017)	(0.022)	(0.022)	(0.021)	(0.019)
MtypII	-0.004	0.014	0.009	0.007	0.009
	(0.016)	(0.018)	(0.022)	(0.020)	(0.019)
Mstat	0.064*	0.047	0.049	0.045	0.065*
	(0.037)	(0.037)	(0.036)	(0.035)	(0.037)
$\text{CtypI}_{(+1)}$	0.005	-0.011	-0.054*	-0.045	-0.044
	(0.008)	(0.012)	(0.030)	(0.030)	(0.028)
$\text{CtypII}_{(+1)}$	0.011	-0.022	-0.018	-0.018	-0.021
	(0.015)	(0.020)	(0.022)	(0.022)	(0.021)
$\text{Cstat}_{(+1)}$	-0.057*	-0.045	-0.044	-0.043	-0.047
	(0.031)	(0.036)	(0.038)	(0.037)	(0.037)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	√				
Occ. shares 1930	√				
County fixed effects		√	√	√	√
Regional time trends			√	√	√
Physician rate				√	√
Social background					√

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A31: Reduced Form, Only Able to Run Less than 100 Meters

MtypI	0.005	-0.006	-0.003	-0.004	-0.003
	(0.008)	(0.013)	(0.016)	(0.016)	(0.014)
MtypII	0.004	-0.007	0.000	0.000	-0.001
	(0.005)	(0.010)	(0.008)	(0.008)	(0.008)
Mstat	0.074*	0.122**	0.083	0.086	0.081
	(0.037)	(0.055)	(0.062)	(0.063)	(0.061)
CtypI	-0.010	-0.007	-0.003	-0.004	-0.002
	(0.006)	(0.007)	(0.015)	(0.015)	(0.015)
CtypII	-0.012**	0.006	-0.004	-0.004	-0.003
	(0.005)	(0.010)	(0.010)	(0.010)	(0.011)
Cstat	-0.071*	-0.133**	-0.099	-0.101	-0.101
	(0.036)	(0.055)	(0.062)	(0.063)	(0.059)
Observations	6990	6990	6990	6990	6990
$\text{MtypI}_{(-1)}$	0.014***	0.012**	0.014**	0.014**	0.012**
	(0.005)	(0.006)	(0.006)	(0.006)	(0.005)
$\text{MtypII}_{(-1)}$	0.005	-0.003	0.004	0.004	0.003
	(0.007)	(0.009)	(0.012)	(0.012)	(0.012)
$\text{Mstat}_{(-1)}$	0.013	0.017	0.013	0.013	0.010
	(0.013)	(0.013)	(0.015)	(0.015)	(0.015)
CtypI	-0.014***	-0.014**	-0.024	-0.024	-0.019
	(0.003)	(0.006)	(0.015)	(0.016)	(0.016)
CtypII	-0.010	-0.006	-0.013	-0.013	-0.012
	(0.006)	(0.008)	(0.010)	(0.010)	(0.011)
Cstat	-0.007	-0.029	-0.034*	-0.034*	-0.036*
	(0.013)	(0.020)	(0.019)	(0.019)	(0.020)
Observations	6524	6524	6524	6524	6524
MtypI	0.005	-0.006	-0.003	-0.003	-0.002
	(0.007)	(0.012)	(0.016)	(0.016)	(0.014)
MtypII	-0.007	-0.014	-0.008	-0.008	-0.009
	(0.007)	(0.010)	(0.010)	(0.010)	(0.010)
Mstat	-0.020	-0.016	-0.022	-0.022	-0.027
	(0.015)	(0.016)	(0.018)	(0.019)	(0.019)
$\text{CtypI}_{(+1)}$	-0.009*	-0.012*	-0.011	-0.011	-0.012
	(0.005)	(0.006)	(0.013)	(0.015)	(0.015)
$\text{CtypII}_{(+1)}$	-0.002	0.012	0.006	0.006	0.007
	(0.006)	(0.013)	(0.011)	(0.011)	(0.010)
$\text{Cstat}_{(+1)}$	0.023	0.015	0.016	0.016	0.016
	(0.014)	(0.015)	(0.016)	(0.016)	(0.015)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	√				
Occ. shares 1930	√				
County fixed effects		√	√	√	√
Regional time trends			√	√	√
Physician rate				√	√
Social background					√

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A32: Reduced Form, Number of Long-term Illnesses

MtypI	0.019	-0.034	-0.027	-0.027	-0.027
	(0.020)	(0.025)	(0.028)	(0.029)	(0.026)
MtypII	-0.038	-0.048	-0.013	-0.014	-0.015
	(0.024)	(0.031)	(0.037)	(0.038)	(0.038)
Mstat	-0.600***	-0.661**	-0.665**	-0.684**	-0.699**
	(0.203)	(0.289)	(0.280)	(0.283)	(0.267)
CtypI	-0.022	-0.038	-0.083	-0.079	-0.076
	(0.022)	(0.024)	(0.070)	(0.073)	(0.072)
CtypII	0.004	0.003	-0.016	-0.017	-0.017
	(0.026)	(0.032)	(0.034)	(0.034)	(0.032)
Cstat	0.608***	0.592*	0.608**	0.625**	0.631**
	(0.208)	(0.298)	(0.285)	(0.288)	(0.271)
Observations	6990	6990	6990	6990	6990
$\text{MtypI}_{(-1)}$	0.005	-0.064***	-0.076***	-0.077***	-0.083***
	(0.022)	(0.020)	(0.018)	(0.018)	(0.018)
$\text{MtypII}_{(-1)}$	0.001	0.006	0.024	0.024	0.019
	(0.030)	(0.036)	(0.040)	(0.040)	(0.040)
$\text{Mstat}_{(-1)}$	-0.018	-0.024	0.001	0.000	-0.002
	(0.046)	(0.054)	(0.054)	(0.053)	(0.056)
CtypI	-0.008	-0.004	-0.076	-0.074	-0.063
	(0.024)	(0.032)	(0.073)	(0.076)	(0.074)
CtypII	-0.016	0.027	0.021	0.021	0.024
	(0.030)	(0.045)	(0.052)	(0.052)	(0.051)
Cstat	0.032	-0.051	-0.053	-0.053	-0.060
	(0.043)	(0.059)	(0.059)	(0.058)	(0.059)
Observations	6524	6524	6524	6524	6524
MtypI	0.021	-0.030	-0.035	-0.035	-0.035
	(0.019)	(0.025)	(0.024)	(0.024)	(0.021)
MtypII	-0.018	-0.033	-0.023	-0.023	-0.025
	(0.023)	(0.030)	(0.031)	(0.033)	(0.032)
Mstat	-0.069	-0.085	-0.052	-0.051	-0.058
	(0.076)	(0.088)	(0.086)	(0.086)	(0.085)
$\text{CtypI}_{(+1)}$	-0.014	-0.015	0.003	0.002	-0.005
	(0.016)	(0.028)	(0.099)	(0.104)	(0.104)
$\text{CtypII}_{(+1)}$	-0.001	0.016	0.023	0.023	0.025
	(0.021)	(0.027)	(0.032)	(0.032)	(0.030)
$\text{Cstat}_{(+1)}$	0.074	-0.013	-0.039	-0.039	-0.042
	(0.084)	(0.091)	(0.088)	(0.088)	(0.086)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	$\surd$				
Occ. shares 1930	$\surd$				
County fixed effects		$\surd$	$\surd$	$\surd$	$\surd$
Regional time trends			$\surd$	$\surd$	$\surd$
Physician rate				$\surd$	$\surd$
Social background					$\surd$

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A33: Reduced Form, Disabled

MtypI	0.003	0.000	0.001	0.001	0.001
	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)
MtypII	-0.003	-0.015**	-0.011**	-0.012**	-0.012**
	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)
Mstat	0.005	0.027	-0.008	-0.013	-0.014
	(0.027)	(0.044)	(0.050)	(0.048)	(0.046)
CtypI	-0.004	-0.009*	0.004	0.004	0.005
	(0.003)	(0.004)	(0.010)	(0.010)	(0.010)
CtypII	0.001	0.009	0.003	0.003	0.003
	(0.005)	(0.008)	(0.006)	(0.006)	(0.006)
Cstat	-0.004	-0.037	-0.009	-0.005	-0.005
	(0.026)	(0.046)	(0.051)	(0.049)	(0.048)
Observations	6990	6990	6990	6990	6990
$\text{MtypI}_{(-1)}$	-0.001	-0.007*	-0.008**	-0.008**	-0.008**
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
$\text{MtypII}_{(-1)}$	0.001	-0.003	-0.001	-0.001	-0.002
	(0.005)	(0.006)	(0.008)	(0.008)	(0.008)
$\text{Mstat}_{(-1)}$	0.015	0.011	0.009	0.008	0.006
	(0.011)	(0.009)	(0.009)	(0.008)	(0.008)
CtypI	-0.002	-0.009**	0.002	0.003	0.005
	(0.004)	(0.003)	(0.008)	(0.009)	(0.009)
CtypII	-0.003	-0.000	-0.006	-0.006	-0.005
	(0.004)	(0.006)	(0.007)	(0.007)	(0.007)
Cstat	-0.011	-0.023*	-0.028*	-0.029*	-0.029**
	(0.009)	(0.012)	(0.014)	(0.014)	(0.014)
Observations	6524	6524	6524	6524	6524
MtypI	0.004	0.002	0.002	0.002	0.003
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
MtypII	-0.006	-0.015***	-0.014***	-0.014***	-0.014***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)
Mstat	-0.022	-0.025*	-0.031*	-0.031*	-0.033*
	(0.013)	(0.014)	(0.017)	(0.017)	(0.017)
$\text{CtypI}_{(+1)}$	-0.003	-0.009***	0.002	0.004	0.004
	(0.003)	(0.003)	(0.009)	(0.010)	(0.010)
$\text{CtypII}_{(+1)}$	0.004	0.012*	0.009*	0.010*	0.010*
	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)
$\text{Cstat}_{(+1)}$	0.023	0.026	0.024	0.025	0.025
	(0.014)	(0.016)	(0.017)	(0.017)	(0.017)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	√				
Occ. shares 1930	√				
County fixed effects		√	√	√	√
Regional time trends			√	√	√
Physician rate				√	√
Social background					√

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A34: Reduced Form, ln of Gross Labor Income

MtypI	-0.039*	-0.047	-0.067	-0.066	-0.063
	(0.019)	(0.041)	(0.044)	(0.045)	(0.045)
MtypII	-0.077**	-0.080	-0.082	-0.084	-0.068
	(0.032)	(0.052)	(0.059)	(0.061)	(0.058)
Mstat	-0.241	-0.359**	-0.083	-0.110	-0.040
	(0.168)	(0.166)	(0.200)	(0.203)	(0.221)
CtypI	0.032	0.018	0.059	0.064	0.080
	(0.022)	(0.025)	(0.099)	(0.103)	(0.096)
CtypII	0.045	0.049	0.058	0.057	0.039
	(0.030)	(0.050)	(0.060)	(0.061)	(0.061)
Cstat	0.263	0.410**	0.182	0.206	0.126
	(0.173)	(0.191)	(0.222)	(0.224)	(0.231)
Observations	6990	6990	6990	6990	6990
$\text{MtypI}_{(-1)}$	0.050**	0.095***	0.095***	0.096***	0.076***
	(0.022)	(0.019)	(0.023)	(0.022)	(0.018)
$\text{MtypII}_{(-1)}$	0.022	0.030	0.044	0.044	0.040
	(0.024)	(0.032)	(0.031)	(0.031)	(0.029)
$\text{Mstat}_{(-1)}$	0.006	0.016	0.011	0.014	0.076
	(0.045)	(0.053)	(0.056)	(0.056)	(0.050)
CtypI	-0.000	0.054	-0.079	-0.085	-0.046
	(0.019)	(0.032)	(0.093)	(0.099)	(0.102)
CtypII	-0.029	-0.064	-0.067	-0.067	-0.062
	(0.028)	(0.049)	(0.053)	(0.053)	(0.051)
Cstat	0.035	0.082	0.138**	0.139**	0.084
	(0.045)	(0.056)	(0.060)	(0.059)	(0.060)
Observations	6524	6524	6524	6524	6524
MtypI	-0.038*	-0.045	-0.068	-0.066	-0.062
	(0.021)	(0.043)	(0.041)	(0.042)	(0.042)
MtypII	-0.035	-0.032	-0.047	-0.049	-0.051
	(0.034)	(0.060)	(0.066)	(0.067)	(0.055)
Mstat	0.150*	0.141*	0.192**	0.188**	0.161**
	(0.078)	(0.081)	(0.089)	(0.087)	(0.076)
$\text{CtypI}_{(+1)}$	0.027	0.034	0.104	0.114	0.113
	(0.019)	(0.036)	(0.094)	(0.110)	(0.095)
$\text{CtypII}_{(+1)}$	0.006	0.002	0.010	0.011	0.020
	(0.029)	(0.044)	(0.053)	(0.053)	(0.042)
$\text{Cstat}_{(+1)}$	-0.126	-0.146	-0.151	-0.150	-0.116
	(0.081)	(0.099)	(0.096)	(0.096)	(0.090)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	√				
Occ. shares 1930	√				
County fixed effects		√	√	√	√
Regional time trends			√	√	√
Physician rate				√	√
Social background					√

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5.A35: Reduced Form, Secondary Education or Higher

MtypI	0.010	0.021	0.021	0.024	0.025
	(0.017)	(0.015)	(0.018)	(0.016)	(0.016)
MtypII	-0.009	-0.039	-0.006	-0.009	-0.000
	(0.025)	(0.027)	(0.031)	(0.028)	(0.031)
Mstat	-0.107	-0.185	-0.166	-0.220	-0.149
	(0.138)	(0.170)	(0.188)	(0.197)	(0.210)
CtypI	-0.006	-0.058***	-0.057	-0.046	-0.035
	(0.013)	(0.014)	(0.044)	(0.051)	(0.046)
CtypII	-0.014	-0.004	-0.058*	-0.059*	-0.061*
	(0.024)	(0.024)	(0.032)	(0.030)	(0.031)
Cstat	0.131	0.173	0.158	0.205	0.156
	(0.134)	(0.181)	(0.195)	(0.203)	(0.216)
Observations	6990	6990	6990	6990	6990
$\text{MtypI}_{(-1)}$	0.006	-0.003	-0.009	-0.010	-0.001
	(0.013)	(0.018)	(0.018)	(0.019)	(0.018)
$\text{MtypII}_{(-1)}$	0.036*	0.045*	0.055*	0.054*	0.052*
	(0.020)	(0.025)	(0.028)	(0.028)	(0.027)
$\text{Mstat}_{(-1)}$	0.030	0.014	0.022	0.017	0.032
	(0.032)	(0.035)	(0.031)	(0.031)	(0.032)
CtypI	0.004	-0.031*	-0.060*	-0.050	-0.035
	(0.014)	(0.017)	(0.034)	(0.046)	(0.045)
CtypII	-0.043*	-0.036	-0.064***	-0.064***	-0.064***
	(0.023)	(0.027)	(0.023)	(0.022)	(0.021)
Cstat	0.005	-0.016	-0.007	-0.009	-0.008
	(0.028)	(0.044)	(0.049)	(0.049)	(0.042)
Observations	6524	6524	6524	6524	6524
MtypI	0.009	0.020	0.014	0.016	0.018
	(0.016)	(0.013)	(0.015)	(0.014)	(0.013)
MtypII	-0.023	-0.043**	-0.047*	-0.049**	-0.041
	(0.018)	(0.020)	(0.024)	(0.023)	(0.025)
Mstat	0.007	-0.010	-0.001	-0.007	0.008
	(0.061)	(0.055)	(0.058)	(0.059)	(0.055)
$\text{CtypI}_{(+1)}$	-0.002	-0.053***	-0.036	-0.021	-0.019
	(0.012)	(0.014)	(0.029)	(0.029)	(0.031)
$\text{CtypII}_{(+1)}$	0.006	0.013	0.005	0.006	0.002
	(0.015)	(0.017)	(0.022)	(0.023)	(0.024)
$\text{Cstat}_{(+1)}$	0.015	-0.020	-0.023	-0.022	-0.009
	(0.066)	(0.076)	(0.075)	(0.075)	(0.070)
Observations	6990	6990	6990	6990	6990
Avg. income 1930	√				
Occ. shares 1930	√				
County fixed effects		√	√	√	√
Regional time trends			√	√	√
Physician rate				√	√
Social background					√

Social background variables: occupation of father, parental foreign background, number of siblings. Regressions include birth year fixed effects. Standard errors are clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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