STOCKHOLM SCHOOL OF ECONOMICS
Department of Economics
5350 Master's Thesis in Economics
Academic Year 2021-2022

A Hard Pill to Swallow?

Subsidized Contraceptives and Women's Intergenerational Mobility in a Difference-in-Differences Framework

Matilda Fors (24246) and Lisa-Maria Jonsson (24257)

Abstract: The theory and methods of intergenerational mobility attempt to explain the transmission of socioeconomic status between family members of different generations. In this thesis, we study the causal effect of access to contraceptives on the intergenerational mobility of women. Our identification strategy makes use of a series of subsidies on oral contraceptives for young women, which were rolled out across Swedish localities between 1989 and 1998. We investigate the impact of these subsidies on the intergenerational elasticity of income (IGE) of Swedish women born between 1967 and 1978. Our results indicate that access to subsidized oral contraceptives did not have a significant impact on the IGE when outcomes are measured in lifetime earnings. We postulate that increasing income inequality partly explains this. Additional analyses, which instead use household disposable income as the outcome variable, suggest that access to the subsidy may have increased income persistence among women whose fathers were in the top quintile of the income distribution. All in all, our study implies that further academic attention must be devoted to understanding complementary patterns in family, fertility, gender and intergenerational economic persistence.

Keywords: intergenerational mobility, income persistence, fertility, contraceptives, birth control, natural experiment

JEL: J62, J13

Supervisor:	Jesper Roine
Date submitted:	December 6, 2021
Date examined:	January 13, 2022
Discussants:	Queennie Huang
Examiner:	Kelly Ragan

Acknowledgements

Firstly, we wish to express our utmost gratitude to our supervisor Jesper Roine. The process of writing this thesis would not have been the same without his precise, thoughtful and patient guidance. We are also deeply indebted to Markus Jäntti for his help with data access.

Furthermore, we are grateful for econometric advice from Abhijeet Singh. Additionally, Kelly Ragan has helped us in thinking closely about contraceptive technology and how to study the impact of it. Jonathan Lehne also deserves special thanks for his useful modeling advice.

Finally, we are thankful for helpful comments from our classmates Gustav Olsson and Denise Shen, Carl Widstrand and Eugénie de Jong.

Any and all errors are entirely our own.

Contents

1 Introduction

The study area of intergenerational mobility – broadly defined as the propensity for changes in social or economic status between family members of different generations – has attracted both scholarly and popular attention in recent times. The idea that industrious individuals should be rewarded for their efforts by a move up on the socioeconomic ladder is crucial for the perceived legitimacy of many social, political and economic institutions. Nowhere is this more apparent than in the rise and fall of the "American Dream" ethos in the last century. The ideas have gained traction also via the work of Piketty (2014), who popularized the notion that the distribution of wealth and subsequent income inequality is instrumental in understanding welfare, both within and between generations. Additionally, questions about whether each new generation becomes "better off" than their parents can be used as political firewood all across the ideological spectrum. The ensuing discussion about transmission of privileges and equality of opportunity often gives rise to polemics around whether high-income individuals have predominantly earned or inherited their positions.

In the Swedish context, intergenerational mobility has historically been high in comparison to many other countries. This has been taken as evidence that the Swedish welfare state has successfully achieved the goal of egalitarian outcomes for the vast majority of the population (Björklund, Roine, and Waldenström 2012). Nonetheless, considerable heterogeneity in mobility still exists within Sweden, such as mobility differences between men and women or in different parts of the income distribution (Roine and Jäntti 2021). Additionally, the intergenerational mobility literature highlights human capital accumulation and early-life circumstances as key determinants of future mobility (Chetty, Hendren, and Katz 2016; Pekkarinen, Uusitalo, and Kerr 2009; Brandén and Nybom 2020), which opens up a vast array of research areas beyond the purely descriptive studies that constitute the bulk of the literature.

In this paper, we focus on women's intergenerational mobility and the possibly causal channel of access to oral contraceptives[1] in explaining differences in mobility. Specifically, we will study whether a subsidy on the birth control pill that targeted young women and was successively enforced in the 1980s and 1990s across Swedish municipalities, had an effect on these women's intergenerational elasticity of income (IGE). We will conduct the analysis in a difference-in-differences framework, for the full sample as well as grouped by quintiles in the income distribution. To study this, we use Swedish register data on incomes, education and family ties for women born between 1967 and 1978.

1. While there exist many contraceptive methods, ranging from medical products such the pill, IUDs and hormonal implants, to condoms, to behavioral methods like abstinence, we will in this paper solely focus on the oral contraceptive pill. We will alternate between the terms "oral contraceptives", "contraceptives", "birth control" and "the pill" to refer to the same thing.

The choice of oral contraceptives is motivated by structural linkages between postponed fertility and women's educational attainments, occupational choices and lifetime earning dynamics (Bailey 2006, 2010; Goldin and Katz 2002; Blackburn, Bloom, and Neumark 1993; Sonfield et al. 2013). To exemplify, a commonly observed fertility pattern in modern-day Sweden is that young women make their sexual debut in their mid-teens, spend their twenties in various forms of partnerships, and then have two children in a relatively rapid succession beginning in their late twenties to early thirties (National Board of Health and Welfare 2001). This pattern stands in stark contrast to patterns of early marriage and childbirth, and would not be feasible without cheap, accessible and effective contraceptives. Essentially, the results have major implications for women's lifetime earnings.

Our results imply that the subsidy did not significantly alter women's IGE when income is measured in individual labor earnings. We tentatively propose that this can be explained by growing income inequality, as opposed to a null effect of the subsidy on income. Additionally, we present suggestive evidence that the subsidy may have increased income persistence at the household level for women born in the top of the income distribution.

This paper contributes to the literature in several ways. Firstly, most intergenerational mobility studies have been, for practical reasons, limited to father and son comparisons. Our paper thus contributes to the understanding of women's intergenerational mobility. Secondly, this paper is, to the best of our knowledge, the first paper that attempts to study women's income dynamics by including contraceptive technology and intergenerational mobility in the same framework. Finally, we also hope to contribute to a wider understanding of the role of oral contraceptives in an extensive welfare state, decades after they first emerged.

The structure of the paper is the following: section 2 is a review of the two parallel literatures of intergenerational mobility as well as contraceptives. Section 3 describes the institutional setting of fertility, family planning and female labor force participation in Sweden in the latter half of the 20th century. In Section 4, we reconcile the two frameworks of intergenerational mobility and contraceptives in a simple conceptual model of the impact of subsidized contraceptives on intergenerational mobility. Section 5 describes our empirical method and Section 6 our data. Our results are presented in Section 7, along with some robustness checks. Results are discussed in Section 8. Section 9 concludes.

2 Literature Review

We begin this literature review by describing the research area of intergenerational mobility, where, in spite of thorough descriptive studies conducted on the topic, less is known about causal drivers. In particular, drivers of women's intergenerational mobility have not been studied in detail. We go on to propose access to

birth control as a theoretically informed causal channel of women's intergenerational mobility. We develop our argument for this channel by summarizing the state of knowledge regarding the impact of contraceptives on women's childbearing, education, career and marriage decisions.

2.1 Income, Economic Inequality and Intergenerational Mobility

The concept of intergenerational mobility and its associated methods attempt to answer questions such as how much of an individual's income can be explained by their parents' income, and whether the opportunities of economic success are equally distributed among the members of a generation (Björklund and Jäntti 2020). Seminal work by Becker and Tomes (1979, 1986) model how parents allocate resources between present consumption and investments in their child's future income, which is also dependent on inherited endowments and luck. In equilibrium, children's incomes are thus determined by own endowments, luck, the inheritability of parents' endowments, and parents' propensity to invest in their children. Corak (2013) contextualizes the model by arguing that intergenerational mobility is by and large a product of institutional interplay between the labor market, the importance of family background, and public policies aimed at balancing the two.

Roine and Jäntti (2021) emphasize how the seemingly straightforward question of whether a new generation is economically better off than their parents can have dramatically different answers depending on the study design. For example, researchers can use absolute or relative measures of income; they can select income from labor or disposable income, at the individual or household level, as the outcome measure; there are also myriad ways to connect parents and children (sons–fathers, daughters–mothers, children–an average of both parents, etcetera). All in all, these intricacies reveal the importance of being transparent about the methods used as well as aware of their limitations.

The standard approach of measuring intergenerational mobility is to calculate the intergenerational income elasticity (IGE), captured by the β coefficient in the following regression equation:

$$y_{i,c} = \alpha + \beta y_{i,p} + \varepsilon_i \tag{2.1}$$

Where $y_{i,c}$ and $y_{i,p}$ are the log of child and parent income, respectively (Brandén and Nybom 2020). The IGE can be interpreted as how much higher than their generation's mean a child's income will be, given that their parent had an income 1 % above the mean of the parent generation; or, phrased differently, as the

transfer of income differences between members of two generations. Though the term *mobility* is frequently used in the literature, the IGE method provides no information about the *direction* of said mobility. For that reason, it is just as accurate to think of the IGE as a measure of intergenerational *persistence* of income differences. High IGE implies high rates of income persistence; hence low intergenerational mobility, and vice versa.

Naturally, the IGE model is not without limitations. Firstly, one frequent empirical shortcoming is the lack of accurate time series of income data. For that reason, average income at ages around 35 to 40 is typically used as a proxy for lifetime income. Averaging over several years smooths out possible income shocks, thereby providing a more stable proxy. Moreover, using income from around these ages has been found to reduce life-cycle bias, hence making it fairly representative of lifetime income (Black and Devereux 2010). Secondly, the parental income variable contains considerable noise that is likely correlated with child income. Examples of such unobservable variables are parents' health, education and the family's place of residence (Brandén and Nybom 2020). Worth noting is also that the IGE picks up both changes to absolute income levels (such as economic growth) as well as relative shifts in the income distribution, thereby requiring further data manipulation in order to disentangle these effects from one another (Roine and Jäntti 2021). Finally, Mitnik and Grusky (2020) note that the IGE suffers from bias stemming from individuals who report zero income, as these observations disappear when logarithmized. This is a potential cause of concern with regards to the accuracy of estimates from the lower end of the income distribution, and is in some cases solved by changing these observations to having a very low symbolic income instead.

Despite its limitations, the IGE remains the workhorse model of intergenerational mobility studies. Other methods, such as the rank-rank slope method used by, among others, Chetty et al. (2014) are more robust to measurement error and life-cycle bias in income (Nybom and Stuhler 2020). By correlating normalized ranks of the parent generation's income to normalized ranks of the child generation's income, we obtain a measurement of the role played by parental income for the income of their children. However, the rank-rank slope method may provide misleading results in situations where almost everyone has gained, as, by construction, one person must move down in rank position for another person to move up. In a situation where we expect that almost everyone in the income distribution has been affected in the same direction, such as in the case of economic growth or certain extensive policy changes, rank-rank slope estimates may be ambiguous to interpret.

Without going into depth, the tangent between economic inequality and intergenerational mobility deserves some brief attention. A negative relationship between inequality and mobility has been observed in several cross-national studies, and is often referred to as the "Great Gatsby curve". The main idea incorporated in the Great Gatsby curve is that intergenerational mobility tends to be lower in

countries characterized by substantial economic inequality. While clarifying that the relationship does not have a clear causal interpretation, Corak (2013) suggests that less concentration of wealth leads to the opportunities of economic prosperity being more evenly distributed in a population, which could explain why countries with lower economic inequality also display lower rates of intergenerational economic persistence. This relationship, as well as the fact that Sweden is characterized by comparatively low economic inequality (OECD 2021), should be kept in mind as we delve deeper into Swedish data in the coming chapters. Nonetheless, the complex dynamics of economic inequality lie outside the scope of this study.

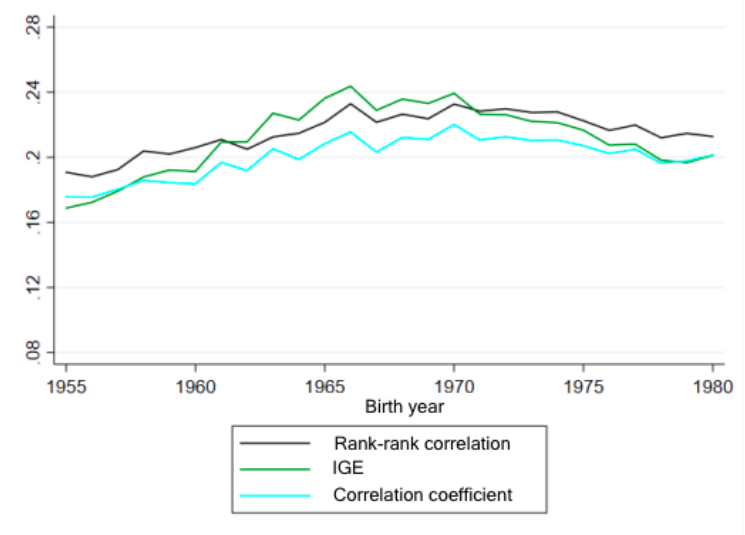
2.1.1 Intergenerational Mobility in Sweden in the Second Half of the 20th Century

Bearing in mind the many available methods of studying intergenerational mobility, it is not surprising that different studies have drawn different conclusions regarding mobility in Sweden. Nonetheless, Sweden generally displays high levels of intergenerational mobility, compared to other countries such as the US, the UK and Canada (Manduca et al. 2020).

Brandén and Nybom (2020) estimate overall mobility in terms of income elasticities, rank correlations and income correlations for cohorts born between 1955 and 1980. They conclude that though the intergenerational mobility was fairly high and stable at between 0.15 and 0.25[2], mobility was lower for cohorts born 1955-1970 and rose for cohorts born 1970-1980 (see Figure 1). This can be compared to elasticities of around 0.45 in the US, 0.5 in the UK and 0.3 in Germany, for approximately the same cohorts (Corak 2013).

2. The reader is reminded that low income elasticity implies high intergenerational mobility, and vice versa.

Figure 1: Different Measures of Intergenerational Mobility, Birth Cohorts 1955–1980



Reproduced with permission from Brandén and Nybom (2020)

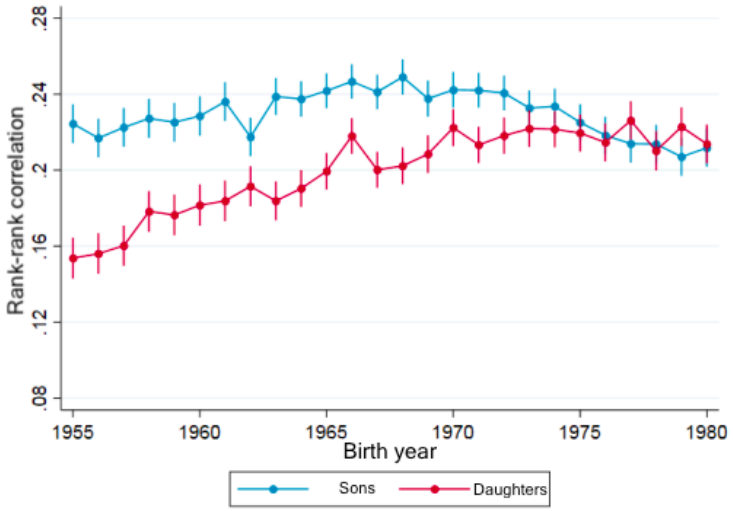
2.1.2 Heterogeneous Intergenerational Mobility: Differences Across the Income Distribution and Between Men and Women

The general trend of high and stable intergenerational mobility in Sweden masks some heterogeneity in different parts of the income distribution. Roine and Jäntti (2021) investigate intergenerational mobility in Sweden for 5-year birth cohorts born between 1948 and 1977. Using non-anonymous growth incidence curves (GICs), which disaggregate the overall growth rate by growth rates specific to the fractiles to which children's parents belong, some points can be made. The higher up in the income distribution that one's parents are located, the smaller is the median income gap between parents and children; or, conversely, children to low-income parents are more likely to move up in the income distribution, compared to children of high-income parents. In a sense, this is partly just a reflection of the mathematical property that mobility is by definition more difficult the closer one is to the distribution's upper bound. Moreover, differential mobility rates are often presented in a transition matrix, which shows the bivariate earnings distribution of parents and children, commonly split into quintiles. Children's earnings quintile are sorted conditional on their parent's earnings quintile (Black and Devereux 2010). Thus, a transition matrix can help answer questions such as "out of all children born to parents belonging to the bottom-fifth of the earnings distribution, how many end up in the bottom-fifth as adults?". In a cross-country comparison of the US, the UK and the Nordic countries, Jäntti et al. (2006) note that earnings persistence is the highest in the tails of the distribution. When only focusing on the inner 3×3 part of the matrix, differences in mobility between

countries become notably less pronounced. This suggests that the middle class operate under very different mobility dynamics compared the very rich or the very poor. In sum, disaggregating data to discover heterogeneous effects is important, particularly if the purpose is to study the impact of specific policies whose impact on different groups is *ex ante* ambiguous.

Just as mobility patterns differ across starting points in the income distribution, considerable differences also exist between men and women. For Swedish cohorts born between 1948 and 1952, only about one third of daughters reached higher labor incomes (in real and absolute terms) than their fathers, while about 60 % of sons did. However, this gap has narrowed over time; for cohorts born between 1973 and 1977, about 70 % of daughters and 80 % of sons reached higher incomes (in real and absolute terms) than their fathers. A similar relationship can be discerned also in terms of ranks in the income distribution. For the older cohorts (born 1948-1952), more than 70 % of women reached neither a higher rank nor a higher income than their fathers. The equivalent figure for men is 40 %. However, for younger cohorts (born 1973-1977), only about 30 % of women and 20 % of men reach neither higher ranks nor higher incomes than their fathers (Roine and Jäntti 2021). Brandén and Nybom (2020) engage in a similar exercise by studying rank-rank correlations for sons and daughters separately (see Figure 2). They find that the 95 % confidence interval of the rank-rank correlation between women and their parents' income is significantly separate from, as well as lower than, the rank correlation for men. This means that women's incomes were less correlated with their parents', compared to men's incomes. This trend continues up until cohorts born around 1970, where the gap between men and women starts to narrow. For younger cohorts, no statistically significant difference in rank correlation between men and women can be discerned. These studies ask subtly different questions and also draw different conclusions about the intergenerational mobility of the women in question. Nonetheless, they both suggest that the gender gap in earnings mobility has narrowed over time.

Figure 2: Intergenerational Mobility for Sons and Daughters, Born 1955–1980



Reproduced with permission from Brandén and Nybom (2020)

It has been suggested that drivers of this convergence are, among other things, women's increased labor force participation as well as wider career options (Brandén and Nybom 2020; Roine and Jäntti 2021), making it comparatively easier for women born in the 1950s to surpass their parents' average income than for women born in the 1970s. The above also indicates that intergenerational mobility for women born around 1970 may be less affected by large-scale shifts in female labor force participation, whereby variation in mobility from this point onwards will be more reflective of idiosyncrasies in family background, occupational choices and policy effects, rather than of general shifts in the structure of the labor market.

Worth noting is also that gender disparities in mobility largely disappear when instead studying disposable income on the household level. Brandén and Nybom (2020) attribute this to marital sorting; that while individual income from labor may differ between parents and children of different genders, it is common to marry into the same socioeconomic strata as one grew up in. Therefore, both sons and daughters reach comparable levels of mobility in disposable household income.

2.1.3 Causal Mechanisms of Intergenerational Mobility

The bulk of intergenerational mobility studies are descriptive, and comparatively few studies have attempted to disentangle causal effects. This is chiefly due to the "nature vs. nurture" conundrum, making it inherently difficult to separate hereditary factors from environmental ones. If a person with high-income parents also has a high income in adulthood, is this primarily because of inherited traits such as IQ, resilience and ability to delay gratification, or because of structural

factors in the economy that make wealth persistent? And if the latter is the predominant route, can families respond heterogeneously to the structures – with, for example, some parents going to great lengths to place their children in high-quality schools – thereby inducing further endogeneity?

There is no straightforward way to approach these questions, but researchers have used policy changes and natural experiments to find causal channels. Brandén and Nybom (2020) mention some recurring themes in the causal intergenerational mobility literature. Access to high-quality education and daycare, as well as social security systems, all seem to affect mobility upward. For example, Pekkarinen, Uusitalo, and Kerr (2009) utilize a Finnish school reform implemented in the 1970s to study the effect of a comprehensive academic curriculum for all students up until the age of 16 – as opposed to the previous system, which sorted students into academic or vocational schooling tracks already at age 11 – on the IGE. They find that the school reform significantly increased mobility as it reduced the IGE by approximately 23 %. Moreover, neighborhood effects also appear to play a role in breaking intergenerational patterns of poverty. Chetty, Hendren, and Katz (2016) find that children of low-income families who are randomly assigned housing vouchers, allowing them to move to lower-poverty neighborhoods, reach higher average incomes compared to children who remain in poverty-stricken housing projects. The effect becomes stronger with the length of exposure, i.e., the younger children are when they move, the more their outcomes improve. In sum, childhood and early adolescence circumstances that promote human capital accumulation are particularly important causal channels that can help explain future differences in mobility.

2.2 Female Intergenerational Mobility and the Possible Channel of Contraceptives

Even though intergenerational mobility has been studied extensively in the last decades, relatively little attention has been paid specifically to women's mobility. As a matter of fact, most studies on intergenerational mobility use exclusively samples of fathers and sons. This is primarily due to data constraints, as women's labor force participation as well as incomes have historically been lower than men's, thereby making accurate intergenerational comparisons difficult. Even less attention has been paid to female-specific causal drivers of mobility.

However, the parallel gender economics literature has placed emphasis on oral contraceptives as having a catalyst role in accelerating women's economic and social independence (Sonfield et al. 2013) as well as enabling their human capital accumulation (Bailey 2010, 2006; Goldin and Katz 2002). Given established relationships between contraceptives and women's labor force participation, education, career and marriages, as well as the explanatory power of human capital accumulation in intergenerational mobility, we believe that further investigation

into the role of contraceptives in female intergenerational mobility is warranted. The relationships between contraceptives and female human capital are described more in detail below.

2.3 Fertility Effects of the Pill

Short-to-medium term consequences of the pill's diffusion have been studied extensively in the US. Utilizing state-level idiosyncrasies in statutes regulating sales of contraceptives in the US, as well as their upheaval in the landmark *Griswold v. Connecticut* Supreme Court decision, Bailey (2010) quantifies the effect of the pill on marital fertility. She finds that the diffusion of the pill can explain as much as 40 % of the decline in marital fertility between 1955 and 1965. Moreover, Bailey (2006) notes that variation in legal access to birth control for young, unmarried women had a significant impact on the timing of the woman's first child. Women who could access the pill without parental consent before the age of 21 – due to changes in the definition of legal adulthood, which varied between US states – were 14 to 18 % less likely to have a child before age 22, according to Bailey's estimates. Utilizing the same source of exogenous variation in access to the pill, Goldin and Katz (2002) find that lenient regulation regarding access had large effects on pill take-up. Using data from the 1971 National Survey of Young Women, they find that in states where the pill was easier to obtain, usage was between 33 and 40 % higher for teenage girls, compared to states with stricter regulation. Although there is limited published research on the price elasticity of demand for contraceptives, studies have confirmed that changes in the price of the pill also appear to produce variation in take-up. For example, Kearney and Levine (2009) note that changes in the distribution of Medicaid family planning waivers in the 1990s reduced birth rates among eligible women.

All in all, the above provides evidence that differential access to birth control produces differences in fertility patterns across several settings and contexts.

2.4 Fertility Effects on Education, Wages, Careers and Marriages

For women, changing fertility patterns have potentially dramatic consequences. For instance, there is close to academic consensus regarding the existence of a wage penalty of motherhood (Gash 2009; Budig and England 2001). As an example, Lundborg, Plug, and Würtz Rasmussen (2018) use a sample of Danish women who have undergone IVF (in vitro fertilization) treatment, to create plausible randomness in childbearing in a pool of similar candidates and thereby investigate the causal effect of childbirth on women's wages. They find that the effect on wages from having children is unequivocally greater on the extensive margin (when

a woman has her first child) compared to the intensive margin (when a woman, who is already a mother, has another child). The effect on the extensive margin can be further broken down into a short-term negative effect on hours worked, and a medium-to-long term negative effect on hourly wages, perhaps due to foregone career advancements or selection into lower-paying occupations. All in all, the negative effect of childbirth on wages in the Danish sample is substantial and far-reaching. The authors also note that, with Denmark's generous family policies and maternal allowances, their estimates may represent a lower boundary for the extensive margin effect of childbirth, in comparison with other welfare states.

The role of oral contraceptives in this setting can be simplified by thinking of birth control as a mechanism that regulates the *timing* of births. As a matter of fact, Becker (1991, cited in Bailey 2006, p. 298) suggests that throughout history, the completed fertility (the number of children born per woman) has been determined largely by demand for children. Demand for children is driven by culture (Li and Zhang 2009) and socioeconomic factors (Yang et al. 2020) – and not so much by access to birth control methods. Indeed, Bailey (2006) does not find a statistically significant effect of early access to the pill on completed fertility in her sample. Instead, she finds that "the pill catalyzed changes in labor-force participation through the mechanism of birth timing" (p. 295) by delaying the *first* birth, allowing women to pursue different career trajectories than they would have without the opportunity of fertility delay. Labor force participation increased both on the extensive and intensive margin, meaning that women with early access displayed higher rates of labor force participation while also working more hours per week. Bailey concludes that the observed labor market effects "should be viewed as arising from improvements in the timing of motherhood rather than through reductions in the number of children." (p. 310). The timing condition is stressed further by Blackburn, Bloom, and Neumark (1993), who find that women who delay childbearing also earn higher wages. The wage differential between early and late childbearers can primarily be explained by the late childbearers' higher investment in human capital, implying that it is sequentially rational for women who have begun a productivity-enhancing investment in human capital to further delay having children. Underlying heterogeneity in, for example, family background do not nullify this relationship. The timing of motherhood is also the linchpin of the wage effects of the pill observed by Goldin and Katz (2002), who claim that "[t]he availability of family planning services to women *when they are in college* is a critical input to career change because it occurs when career, marriage, and family decisions are being made" (p. 742).

Analogous dynamics regarding contraceptives and delayed childbearing appear to have been at play in Sweden. In a working paper by Ragan (2011), the density of pharmacies in Swedish localities (1970–1974) is used as an instrument for exogenous variation in pill supply. Ragan finds that this variation had a positive and significant causal effect on women's earnings. The effect is not limited to young women, but can be identified for all fecund females. Improved access to the pill also increased the rate of labor force participation among young women.

Otterblad Olausson et al. (2001) confirm that the timing of motherhood appears to matter also in Sweden as they conclude that women giving birth as teenagers face elevated odds of unfavorable socioeconomic outcomes, compared to women who have their first child at age 20-24. This effect is robust to controlling for socioeconomic background, and is prevalent also in teenage mothers who come from stable economic conditions.

Since the advent of the pill in the 1960s, it is not only the number of women working that has grown, but also the number of women pursuing skilled occupations. Surely, there are many reasons for this, including – but not limited to – changing social norms (Fernández 2013), substitutes for domestic production (Albanesi and Olivetti 2007) and affirmative action in the workplace (Kurtulus 2012). Nonetheless, skilled occupations have higher returns to experience while also being more sensitive to career disruptions, particularly around ages 24-33, which are most associated with childbearing (Light and Ureta 1992; Blau and Kahn 2017). Therefore, Knowles (2007) argues that the pill and its ability to reduce the stochastic nature of fertility was likely an important explanatory factor in the development of more women pursuing skilled occupations. Similarly, Goldin and Katz (2002) develop a framework in which the pill represents a technology that reduces the costs of human capital accumulation, by removing uncertainty about the consequences of sex. The theoretical argument is thus that the pill lowers a woman's cost of making a career investment such as acquiring more education or on-the-job training, as she does not need to worry about the unplanned career interruptions associated with pregnancy. Goldin and Katz find that early access to the pill had a significant effect of delayed marriages, more women employed in professional occupations, and more women having high-investment careers such as becoming doctors and lawyers. Marrying later in life also led to a thicker marriage market with higher-quality marriages as a result.

In conclusion, childbirth tends to impact women's wages negatively, both in the short and long term. The effect is substantially larger on the extensive margin. Theoretically and empirically, the pill provides a mechanism to delay this effect while making career investments. In turn, this suggests possible income gains for women who can optimize their birth timing, particularly when contraceptives are provided in the beginning of the human capital accumulation trajectory.

3 The Institutional Setting of Fertility and Contraception in Sweden

In this section, we describe the Swedish institutional setting of female labor force participation and fertility in the second half of the 20th century. We proceed to review landmark policies relating to family planning and contraceptives, as well as their impact on Swedish women's fertility.

3.1 General Trends in Fertility and Female Labor Force Participation

Female labor force participation and fertility rates have typically been considered inversely related (Bumpass 1990); as female labor market participation has risen in Europe and the US over the latter half of the 20th century, fertility and the number of children born per woman has generally gone down. However, Sweden displayed the highest level of female labor force participation, as well as the second-highest fertility rates in Europe, throughout the late 1980s and early 1990s (Sundström and Stafford 1992). Furthermore, Sweden exhibited swings in fertility which have been described as a "rollercoaster". This indicates that the relationship between fertility and labor force participation may not be as clear-cut as commonly proposed in the literature, and that both may be altered through the introduction of public policies such as those implemented by the Swedish government from the 1960s through the 1980s. These will be described in detail below.

3.1.1 Mid 1960s - Late 1970s

The mid-1960s marked the end of what has later been dubbed "the housewife parenthesis" (Axelsson 1992), where Swedish women were generally encouraged to assume more traditional gender roles and a majority of married women opted to become housewives rather than to actively participate in the labor market (Hoem and Hoem 1996). As Sweden was facing a potential labor shortage, politicians sought to solve the issue not only through an increase in immigration, but also by facilitating the entry into the labor market for Swedish women. This was achieved by implementing public policies for combining family and work. A key example of this was the extension of the parental leave programme in 1974 which allowed fathers, in addition to mothers, to take out six months' worth of parental leave (which was extended to 15 months in 1989) with a benefit level of 90 % of prior earnings (Sundström and Stafford 1992), hence, making Sweden the first country in the world to offer paternity leave.

Furthermore, the introduction of compulsory separate taxation of spouses in 1971 drastically increased the profitability of the market work of wives, thereby significantly altering female labor force participation. Increasing the work hours of housewives from 0 to full-time implied a 67 % increase in household disposable income in 1973 (compared to 43 % in 1967) (Gustafsson and Jacobsson 1985). Additionally, multiple policies aimed at extending the education and daycare systems were implemented during this period. These included a comprehensive system of subsidized public daycare facilities in all municipalities, the right for parents with children younger than 8 to reduce working hours from full-time to 75 %, as well as an expansion of the education system (Sundström and Stafford 1992; Hoem and Hoem 1996).

All in all, the labor force participation of married women ages 20-59 during this period increased from 49.1 % in 1963 to 83.5 % in 1982, an increase that was especially large for women with children under 7 years of age. This may indicate that the aforementioned policies, along with the introduction of the pill in 1964 as well as free abortion in 1975 (more on Swedish contraceptive policies in section 3.2), had a real impact on the ability for women, especially married with children, to enter the workforce (Gustafsson and Jacobsson 1985).

3.1.2 1980s

The 1980s in Sweden was a decade characterized by great economic growth, high labor force participation for women, but also high fertility. This trend long eluded the scientific community, considering that female labor force participation and fertility rates, as previously mentioned, have been considered to be inversely related (Bumpass 1990; B. Hoem 1993). By 1990, Swedish total factor fertility (TFR) had reached 2.13, the second highest in the EU after Ireland (2.17) (Sundström and Stafford 1992). As mentioned in section 3.1.1, this could partly be attributed to the expansive family and taxation policies implemented during the 1970s. However, additional family policies were implemented during the 1980s, which shifted the incentives for women to return to work in between births, thereby contributing to the altering of fertility patterns. For instance, if a woman were to give birth to additional children prior to 1980, her benefits related to said birth would be based on her market earnings in the period between births. However, beginning in 1980, mothers were guaranteed the same level of benefits as for the first child for any additional children born, given that they were born within 24 months from the previous child. This eligibility interval was later extended to 30 months in 1986 and did not penalize women who decided to, for instance, work part-time between births to the same extent as previously (Sundström and Stafford 1992). Subsequently, this created short-term economic incentives for parents to birth children within said interval and for women to establish themselves in the labor market and increase their wages prior to the birth of their first child. On the other hand, J. M. Hoem (1990) notes that this policy change enabled women to take leaves of absence lasting years at a time without any impact on household finances. In turn, this could impact women's long-term careers and wage development negatively due to forgone work experience and skill development. Furthermore, J. M. Hoem (1990) also notes these dynamics did not affect the preferences of Swedish mothers with regards to births on the intensive margin (number of children born per woman), which remained stable at around 2 children per woman throughout the period.

To summarize, J. M. Hoem (1990) concludes that the shifting of incentives from said policy have played a role for Swedish fertility and female labor force outcomes during this period. This is exemplified by the decreased (increased) fertility among women in their early 20s (early 30s) as well as a shortening of the spacing between

births during the 80s. This has also been confirmed by Sundström and Stafford (1992), who conclude that it was the uptick in tempo of childbearing which was responsible for the high fertility at the time.

3.1.3 1990s

According to B. Hoem (2000), the rising fertility which characterized the 80s was quickly reversed during the 1990s and, by the end of the decade, Swedish fertility swings were among the greatest in Western countries. During this period, the Swedish economy had weakened as a result of the financial crisis at the beginning of the 1990s (starting in 1991) and unemployment was rising rapidly. Furthermore, as a result of the crisis, some of the expansive family policies introduced in the 1970s and 1980s, such as parental leave and child benefits, were cut for the first time. For instance, parental leave benefits were cut from 90 to 80 % of prior income in 1995 and later to 75 % in 1996, along with an overall 15 % reduction in child benefits (B. Hoem 2000; J. M. Hoem 2005).

The rise in economic uncertainty under this period caused the composition of women in the labor market to change, drastically increasing the number of women unemployed, in vocational training, or in education. Furthermore, according to Andersson (2000), the effect of earned income on Swedish women's entry into motherhood during this period was strong, meaning that low income women were likely to postpone giving birth. However, it may be noted that the income effect has been shown to primarily impact the timing of the first birth and is regarded to be a less important factor when it comes to women's propensity to birth additional children. Moreover, childless women in their early 20s had higher unemployment and were particularly impacted by the aforementioned events.

Keeping this in mind, Andersson (2000) concludes that part of the fertility decline Sweden exhibited in the 1990s can be explained by said income effect in addition to the reductions made by the public sector with regards to child and parental leave benefits. Furthermore, this effect appears to primarily impact women's fertility on the extensive, rather than intensive, margin. Consequently, it was primarily young, low-income women who decided to postpone their first births, though without a noticeable effect on total number of births. It was chiefly through this channel that Swedish TFR decreased towards the end of the 1990s (Andersson 2000; Hoem and Hoem 1996).

All in all, Swedish fertility and female labor market participation throughout the second half of the 20th century has been shown to not only exhibit pro-cyclical tendencies, but also to be highly responsive to the introduction of various public policies. Furthermore, and what is key to our investigation on the impact of contraceptives, is that all changes in fertility which have contributed to Sweden's "roller-coaster fertility" have been through changes in the timing of births, rather than changes in total number of births per woman. This signals that completed

fertility preferences have remained stable throughout the entire period, and that the impact of public policy is often noticed on the extensive margin of childbirth.

3.2 Historical Access to Birth Control in Sweden and the Introduction of Regional Subsidies

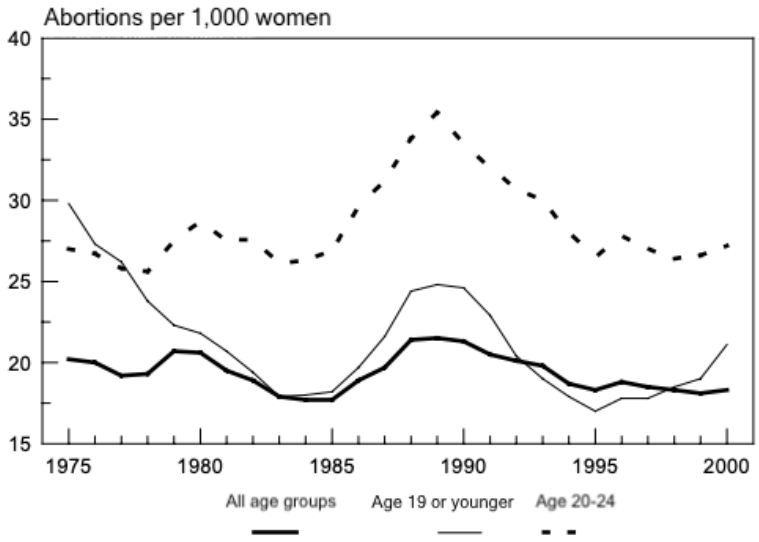
Oral contraceptives were legalized in 1964 in Sweden, following years of intense debate and illegal trading in the black market. They quickly gained popularity; a national survey conducted in 1967 found that around 60 % of adult women had used condoms, the pill or the hormonal implant at the time of their most recent intercourse. Among women aged 25 or younger, the pill was far more popular than other types of birth control. The next national survey on sexual health, conducted in 1996, found that the pill was the most common method of birth control among young women (National Board of Health and Welfare 2005). Obviously, the pill can be said to have been a major influence on young women's sexual health from the mid-60s and onward.

The gains from the revolutionary period of the 1960s were formalized in the 1970s, which were characterized by a vast expansion of family planning policies in Sweden (partially detailed in section 3.1.1). For instance, the current Swedish abortion law, which permits abortion up until the 18th week of pregnancy, came into being in 1974. Additionally, all women enjoyed access to subsidized contraceptives with the introduction of a nationwide subsidy. Funds were also directed to youth clinics (Swedish: *ungdomsmottagningar*), which were specialized in advising young people on questions regarding sexual health and relationships (Bygdeman and Lindahl 1994).

However, the 1980s were marked by a decline in birth control use, particularly among teenagers and young women. This can be attributed to the removal of the nationwide subsidy on birth control in 1984, which caused the price of the pill to quadruple (Grönqvist 2009). The removal of the subsidy was accompanied by a policy that only permitted collection of three months' worth of pill supply at the time, compared to the previous policy, which allowed collection of one year's supply at a time (National Board of Health and Welfare 1994). The combination of higher prices and collection policies that required more planning ahead contributed to a notable decline in birth control use, particularly among young women. It should be borne in mind that though the price increases were not sizable in absolute terms – after the removal of the national subsidy, the price of one year's pill supply was approximately USD 120 in 2020 prices (Grönqvist 2009) – it is a relatively large cost to bear for a teenage girl who has no income of her own and is perhaps unwilling to ask her parents for money. Furthermore, birth control must be taken each day in order to effectively protect against pregnancy, as a single missed pill compromises protection. In many cases, this is exactly what happened; there is anecdotal evidence from many midwives and youth clinics across Swedish counties,

confirming that girls had gotten pregnant because they did not have money to renew the prescription on the exact day that they were supposed to (National Board of Health and Welfare 1994; Region Västra Götaland 2000). As a result, teenage pregnancies as well as abortions began to rise from previously low levels.

Figure 3: Abortions per 1000 Women, 1975–2000, by Age Group in Sweden



Source: National Board of Health and Welfare (2001)

This uptick in teenage pregnancies and abortions motivated a pilot project in the municipality of Gävle in 1989, where contraceptives were subsidized by approximately 75 %. The project was deemed a success as teenagers' pill consumption rose by approximately 43 % (from 42 % to 60 %) and the number of teenage abortions fell by 50 % (Grönqvist 2009). Soon thereafter, others followed Gävle's line and subsidies were successively introduced across Swedish localities between the years of 1989 and 1998. By the end of the period, subsidies were in place in all of Sweden apart from the northern county of Västerbotten as well as Stockholm (except Solna municipality). Table 1 provides a full account of the implementation of the subsidies. Note also that Figure 3 clearly displays how a rise in abortions almost perfectly coincides with the removal of the nationwide subsidy in 1984, followed by a steep decline from 1989 and onwards as the subsidies for young women began to be rolled out.

Table 1: Table of Swedish counties/municipalities, in order of subsidy introduction and age of eligibility.

Location	**Starting Date**	**Eligible Cohorts**
Gävle (municipality)	Nov 01, 1989	$\leq$ 19*
Sandviken (municipality)	Nov 30, 1989	$\leq$ 19*
Partille (municipality)	Jan 01, 1990	$\leq$ 20
Hofors (municipality)	Mar 31, 1990	$\leq$ 19*
Ockelbo (municipality)	Mar 31, 1990	$\leq$ 19*
Örebro (county)	Jun 01, 1990	$\leq$ 18*
Kristianstad (county)	Nov 29, 1990	$\leq$ 18*
Kronoberg (county)	Jan 01, 1991	$\leq$ 19
Blekinge (county)	Mar 01, 1991	$\leq$ 19
Solna (municipality)	Sep 01, 1991	$\leq$ 22
Gotland (county)	Oct 01, 1991	$\leq$ 20*
Södermanland (county)	Jan 01, 1992	$\leq$ 19*
Malmöhus (county) *(except Malmö municipality)*	Jan 01, 1992	$\leq$ 19
Västernorrland (county)	Jan 01, 1992	$\leq$ 19
Älvsborg (county)	Jan 01, 1992	$\leq$ 19
Västmanland (county)	Jan 01, 1992	$\leq$ 19
Kopparberg (county)	Jan 01, 1992	$\leq$ 19
Värmland (county)	Mar 01, 1992	$\leq$ 24*
Jämtland (county)	Apr 01, 1992	$\leq$ 24
Göteborg (county)	Jul 01, 1992	$\leq$ 20
Bohuslän (county) *(except for Partille and Göteborg municipalities)*	Jul 01, 1992	$\leq$ 20
Gävleborg (county) *(except for Gävle, Sandviken, Hofors and Ockelbo)*	Nov 09, 1992	$\leq$ 19*
Uppsala (county)	Mar 01, 1993	$\leq$ 19
Malmö (municipality)	Mar 26, 1993	$\leq$ 18
Halland (county)	Jul 01, 1993	$\leq$ 19
Norrköping (municipality)	Jul 01, 1994	$\leq$ 22
Finspång (municipality)	Jul 01, 1994	$\leq$ 22
Söderköping (municipality)	Jul 01, 1994	$\leq$ 22
Valdermarsvik (municipality)	Jul 01, 1994	$\leq$ 22
Östergötland (county)	Jan 01, 1997	$\leq$ 18
	1998	$\leq$ 19
Jönköping (county)	Apr 01, 1994	$<$ 20
Kalmar (county)	Mar 15, 1994	$<$ 21
Göteborg (municipality)	Jan 01, 1998	$\leq$ 19
Skaraborg (county)	Jan 01, 1998	$\leq$ 19
Västerbotten (county)	No subsidies ever	
Norrbotten (county)	Jan 01, 1996	$\leq$ 19

* Individuals are eligible for the subsidy until the calendar year they turn this age.
Source: National Board of Health and Welfare (1994) and Madestam and Simeonova (2012)

The subsidy scheme has been subject to several follow-up analyses. To begin with, an official evaluation conducted by National Board of Health and Welfare (1994) finds that the subsidies decreased abortions by on average 20 %, though there is some variation between regions. On the local level, the region of Västra Götaland found that pill consumption increased by 41 % and 30 % in Skaraborg and Göteborg, respectively, after introduction of the subsidies. The increased uptake was visible also many years later, and similar effects could not be discerned in the areas of the region, like Älvsborg and Bohuslän, which already had subsidies in place prior to the analysis (Region Västra Götaland 2000). Additionally, Grönqvist (2009) finds that the subsidy had a significant negative impact on nationwide abortions of about 8 % as well as a significant positive impact of pill sales of between 5-7 %. Worth noting is that the effects on both abortions and sales are national averages for all fecund females, not just teenagers; there is indicative evidence from surveys that the increase in consumption was larger among young women. Grönqvist also notes that cohorts which had access to subsidized contraception from an early age are about 20 % less likely to become mothers before age 21, and that this effect is particularly prevalent in women from poor socioeconomic backgrounds. Similar effects on pill sales and abortions are also identified in a working paper by Madestam and Simeonova (2012). Additionally, Madestam and Simeonova suggest that women who were eligible for the subsidy at some point in their lives have better outcomes in terms of income and education. Though direct effects on teenage pregnancies and abortions are not considerable in the aggregate, given the low Swedish baseline prevalence of teenage pregnancies, this does not rule out substantial indirect gains for individual women, or certain groups. Consequently, the subsidy scheme was deemed a success, and contraceptives have continued to be subsidized to young women in Sweden until the present day.

4 Conceptual Framework

In this section, we insert the empirical and theoretical arguments for contraceptives as drivers of human capital accumulation in an intergenerational mobility framework. The purpose is to answer the research question of whether subsidized contraceptives had an impact on the IGE of Swedish women belonging to the cohorts affected by the subsidy.

4.1 Conceptualization of the Impact of the Pill on Intergenerational Mobility

We think of access to contraceptives as an omnibus measure that may impact mobility through several channels, such as increased labor force participation,

selection into higher-investment occupations, higher education, improved marriage matching, delayed birth of the first child, or various combinations of all the above.

Formally, consider the following utility function:

$$U_{i,t} = (E(U_{c_{i,t}}) - C_{c_{i,t}}) - P_{p,i}(E(U_{p_{i,t}}) - C_{p_{i,t}})$$

where $E(U_c)$ is the expected utility of using contraceptives, C_c is the total cost of using contraceptives, $E(U_p)$ is the expected utility of becoming pregnant, and C_p is the cost of becoming pregnant, all for individual i at time t. $P_{p,i}$ is the probability that individual i becomes pregnant, given that she does not use any contraceptive method. For simplicity, we assume that if contraceptives are used, they are 100 % effective (in reality, effectiveness varies between 90 and 99.7 %). The cost terms C_c and C_p include both monetary and non-monetary costs such as social costs and perceived opportunity costs. Individuals update their estimation of $E(U_c)$, C_c, $E(U_p)$ and C_p with time; for example, individual i may assess the utility of becoming pregnant differently when she is thirty compared to when she is twenty.

Consider two types of women:

Type I: women who have estimations of $E(U_c)$, C_c, P_p, $E(U_p)$ and C_p such that:

$$E(U_{c_{i,t}}) - C_{c_{i,t}} \leq P_{p,i}(E(U_{p_{i,t}}) - C_{p_{i,t}})$$

Type I women will not use contraceptives, or be indifferent between using them or not using them, as they deem the utility of using contraceptives as less than or equal to the utility of becoming pregnant.

Type II: women who have estimations of $E(U_c)$, C_c, P_p, $E(U_p)$ and C_p such that:

$$E(U_{c_{i,t}}) - C_{c_{i,t}} > P_{p,i}(E(U_{p_{i,t}}) - C_{p_{i,t}})$$

Type II women will always use contraceptives, as they deem the utility of becoming pregnant as strictly less than the utility of using contraceptives.

We do not know the distribution of Type I and Type II women in the population, nor how these vary between counties or over time. As demonstrated, the cost of contraceptives is just one factor affecting why a woman may be Type I. It is obvious that a lower cost of contraceptives will not result in all women becoming Type II, reasonably because some women will have a strong desire to have children (high $E(U_{p_{i,t}})$), or consider the probability of a pregnancy occurring to be too low (low $P_{p,i}$). Nonetheless, enforcing a subsidy on contraceptives will result in a lowering of C_c which will lead to a fraction of Type I women shifting to become Type II women, *ceteris paribus*. Additionally, our model setup suggests that the marginal shifter is a woman who is financially constrained, as her price elasticity of demand

for contraceptives is likely higher. This is important, as it implies that the subsidy may disproportionately benefit women from disadvantaged backgrounds.

A few assumptions that underlie our empirical exercise should be mentioned. Firstly, we make the assumption that the women we study (born 1967–1978) are credit constrained in their youth. If capital markets were instead perfect, it would imply that girls who had a positive net present value of using contraceptives, but could not afford to pay for it, simply borrowed money. In this particular case, it would most likely entail the girl borrowing or receiving money from her parents. Anecdotal evidence from the Swedish counties (see Section 3.2) support the assumption that girls were generally reluctant to ask their parents for money for birth control. However, this does not exclude the possibility that some girls had access to credit, perhaps through their parents or partners, in which case their pill consumption would not be altered by a lowering of C_c.

Secondly, we also make the assumption that estimations of $E(U_c)$, C_c, P_p, $E(U_p)$ and C_p are fully idiosyncratic on the individual level, meaning that there is no component that is systemically shared by a group of individuals. For example, it could be the case that the cost of early pregnancy – including social costs and the perceived opportunity cost – is higher in an affluent environment where girls are expected to earn a university degree and have a career. This would imply the existence of a social gradient[3] in utilities and costs associated with pregnancy and contraceptive use. Though the existence of a social gradient appears plausible, drawing conclusions about the nature of such a gradient would require simulations of income and childbirth dynamics that lie outside the scope of this study. For that reason, we will not make further assumptions about systemic heterogeneity in the $E(U_c)$, C_c, P_p, $E(U_p)$ and C_p estimations, though we are aware such heterogeneity may exist.

The next step of our formal model is to describe the impact of contraceptives on intergenerational mobility. Consider again the prototypical IGE equation:

$$y_{i,c} = \alpha + \beta y_{i,p} + \varepsilon_i \tag{4.1}$$

where $y_{i,c}$ and $y_{i,p}$ are the log of child and parent income, and β is the intergenerational elasticity of income.

We hypothesize that the elasticity differs depending on whether the woman lived in a municipality or county where she had access to subsidized contraceptives. As the subsidy was enforced at different years, at different ages of eligibility, in different

3. The term social gradient refers to a positive linear relationship between socioeconomic status and a variable of interest. The social gradient in health is the one most frequently investigated in the literature (Marmot 2004). In our case, it would mean that the utility of using contraceptives (or, conversely, the cost of early pregnancy) is positively and linearly related to socioeconomic status.

municipalities and/or counties (hereafter referred to as treatment regions), we allow the regression coefficient to vary across cohorts and treatment regions (see Table 1 for a description of the implementation of the subsidies). Inspired by Pekkarinen, Uusitalo, and Kerr (2009) we fractionate the β coefficient into:

$$\beta_{j,m} = b_0 + \delta S_{j,m} + \eta \tag{4.2}$$

Where j,m indexes birth cohort j from treatment region m. S_{jm} is an indicator variable that takes on the value 1 if the subsidy was available for cohort j in treatment region m, and 0 otherwise. η is an error term. As we believe that access to the pill should facilitate human capital accumulation, which in turn promotes intergenerational mobility, we hypothesize that the δ coefficient should be negative (as, once again, lower IGE implies higher mobility).

We will conduct five separate estimations of β; one for each quintile of the income distribution to which the women's fathers belong. This will give us an idea of the heterogeneous effects of the subsidy and the differential effects that access may have, depending on socioeconomic background. On the one hand, the purely mechanical implication of our model suggests benefits to the financially constrained. On the other hand, there is reason to suspect that the utility of birth control is estimated based on expectations and underlying assumptions embedded in one's socioeconomic stratum, which in turn suggests that pill consumption may be higher among girls born into the upper parts of the income distribution.

5 Method

5.1 Empirical Strategy

Our identification strategy exploits the fact that the subsidies on oral contraceptive were implemented at different times, for different cohorts, in different treatment regions over a nine-year period. These differences in implementation of the subsidy thereby provides us with sources of variation in access to the pill, which can be utilized in a difference-in-differences approach. Analogously to Pekkarinen, Uusitalo, and Kerr (2009), we begin with a standard IGE specification (as presented in equations 2.1 and 4.1):

$$y_{i,c} = \alpha + \beta_{j,m} y_{i,p} + \varepsilon_i. \tag{5.1}$$

Here, the regression coefficient $\beta_{j,m}$ is the estimated IGE for a woman in treatment region m, born in cohort j. The decision to compare the women's earnings to their fathers' is motivated by the assumption that fathers were likelier to be the primary

breadwinners, and that their earnings thus give the most accurate reflection of the economic status the women grew up with.

As we are interested in the causal effect of the subsidy on the IGE estimate, we allow $\beta_{j,m}$ to vary across cohort and treatment region in a way such that

$$\beta_{j,m} = b_0 + \delta S_{j,m} + \eta \tag{5.2}$$

where $S_{j,m}$ is a dummy variable equal to 1 if the subsidy was available for cohort j in treatment region m and δ is the estimated effect of access to the subsidy on the IGE coefficient.

By substituting equation (5.2) into (5.1) and adding the main effect of the subsidy along with cohort and treatment region fixed effects, we get our final regression model:

$$y_{i,c} = \alpha + b_0 y_{i,p} + \delta(S_{j,m} \times y_{i,p}) + b_1 S_{j,m} + b_2 \theta_j + b_3 \theta_m + \varepsilon_i. \tag{5.3}$$

where θ_j and θ_m are cohort and treatment region fixed effects, respectively. The reason for including cohort fixed effects is to control for unobserved shocks affecting women born in certain cohorts which may have an impact on their lifetime earnings. Examples of such shocks are time-variant social norms surrounding female labor force participation, or the fact that women from earlier cohorts likely entered the workforce in the midst of the Swedish financial crisis of the early 90s. Furthermore, we include treatment region fixed effects to control for treatment region-specific characteristics and their impact on lifetime earnings. As such, we have a flexible model that allows for varying lifetime income trends across localities as well as birth cohorts.

It should be noted that this difference-in-differences specification rests on the assumption that, while treatment region and cohort fixed effects likely hold some explanatory power for women's lifetime income, the intergenerational elasticity of income does not also substantially vary along these dimensions. If this were the case, father's earnings would have to be interacted with the treatment region and cohort fixed effects as well. Adding these interacted fixed effects to the model presented in Equation (5.3) would likely introduce high collinearity due to absorbing a substantial part of the variation present in the subsidy dummy. This would result in highly unstable estimates, whereby we refrain from doing so. We also have reason to believe that the IGE is not influenced by major cohort or region-specific trends, which will be developed upon below.

5.2 The Implementation of Subsidies on Oral Contraceptives as a Quasi Experiment

For the purpose of studying the effect of the subsidies on intergenerational mobility, it is required that there be no variable that simultaneously affects the introduction of the subsidy policies and subsequent opportunities for intergenerational mobility in a given treatment region or for a given cohort. If this were the case, our results may be severely biased. Though this assumption is not formally testable, Heidrich (2017) offers evidence that intergenerational mobility in Sweden is relatively homogenous across regions for cohorts born 1968–1976 (which overlap largely with our cohorts of interest, born 1967–1978). For example, Heidrich finds that only 10 out of 112 local labor markets (LLMs, the geographical unit most appropriate for studying conditions regarding, for example, income and employment) have relative mobility measures that are statistically significantly different from the national average. 3 LLMs (Varberg, Skövde and Växjö) display higher-than-average relative mobility, while 7 LLMs (Stockholm, Eskilstuna, Karlstad, Linköping, Västerås, Örebro and Göteborg) display lower-than-average relative mobility. As shown in Table 2, no distinct relationship between regions' subsidy introduction and regions which are outliers in intergenerational mobility can be discerned.

Table 2: Relative Mobility Ranking and Subsidy Introduction. *Source: Heidrich (2017) and Madestam and Simeonova (2012)*

LLM	County	Relative Mobility Ranking 1–112	Year of Subsidy Introduction
Varberg	Halland	1	1993
Växjö	Kronoberg	2	1991
Skövde	Skaraborg	3	1998
...	...	...	...
Göteborg	Göteborg	106	1998
Örebro	Örebro	107	1990
Västerås	Västmanland	108	1992
Linköping	Östergötland	109	1997
Karlstad	Värmland	110	1992
Eskilstuna	Södermanland	111	1992
Stockholm	Stockholm	112	*

*The subsidy was never introduced in Stockholm county as a whole. However, it was introduced in Solna municipality, which is adjacent to the city center and in 1990 had a population of around 52,000.

The above is reassuring for the purpose of our analysis, as it indicates that there appears to be no unobservables that simultaneously impact intergenerational mobility and the decision to subsidize contraceptives in a given geographical area. Nonetheless, the timing of the subsidy implementations is likely not entirely random either; it is presumably the case that municipalities that experienced high rates of teenage pregnancies and abortions also introduced the subsidies earlier. We control for this by including treatment region fixed effects θ_m in our empirical specification.

Additionally, abortion had been legal for around fifteen years at the onset of the subsidy implementation period. Unlike Bailey (2006, 2010) and Goldin and Katz (2002), who study the effect of contraceptives in a time when matters such as abortion rights, women's rights and social norms around women's labor force participation were also in flux, we are to a larger extent able to isolate the effect of access to contraceptives.

5.2.1 Note on Using the IGE

Though useful and easy to interpret, the IGE model has several shortcomings, some of which have been laid out in Section 2.1. Firstly, the IGE captures both absolute and relative income mobility. It is easy to imagine a scenario in which everyone is better off in absolute terms (most notably, due to economic growth), but simultaneous changes in the income distribution have also made some individuals even better off relative to others. As most people care about both their absolute and relative position in the income distribution, the welfare implications of this situation are ambiguous. Secondly, the IGE coefficient only captures the explanatory power of parents' income on childern's income, and does not say anything about the *direction* of mobility. It is theoretically possible to have an IGE coefficient of zero, implying extremely high mobility, due to everyone in the child generation having lower incomes than their parents and thereby being worse off in absolute terms. Additionally, adjusting for inflation does not axiomatically make incomes comparable across generations. Levels of government consumption, taxes and interest rates that have fluctuated by more than the rate of inflation all skew comparisons over time (Roine and Jäntti 2021). To sum up, it may be tempting to equate the IGE coefficient with an intergenerational effect on welfare. However, drawing such conclusions is not possible without further specifying other economic changes that have taken place in the same time span.

6 Data

6.1 Description of the Data Material

For the purpose of our empirical analysis, we have used a dataset compiled by Statistics Sweden for a research project conducted by Jesper Roine (SITE and Department of Economics, Stockholm School of Economics) and Markus Jäntti (SOFI, Stockholm University). The project has been commissioned by the Swedish Fiscal Policy Council and regards absolute intergenerational income mobility between 1968 and 2018. We had access to the data as part of their research project.

The dataset consists of microdata such as incomes, education levels and family ties for Swedish individuals born between 1900 and 1987. However, only a subset

of this data will be used; more specifically, we will only study women born between 1967 and 1978, as well as their fathers.

Sources of the data are the following Swedish administrative registers:

- The Total Population Register, (*Registret över totalbefolkningen* (RTB))
- The Income and Taxation Register, (*Inkomst- och taxeringsregistret* (IoT))
- The Population and Housing Census, (*Folk- och bostadsräkningar* (FoB))
- The Register of Education, (*Utbildningsregistret* (UREG))

The Swedish personal identity number (*personnummer*) for each individual has been replaced with a serial number. The record of connections between personal numbers and serial numbers has been destroyed by Statistics Sweden several months prior to our analysis. As such, data is completely de-identified and a serial number cannot be linked to a specific individual.

6.2 Variable Description

6.2.1 The Subsidy Dummy

Our subsidy dummy S_{j_m} tells us whether a specific woman had access to subsidized contraceptives or not. The subsidies were implemented at different years, in different treatment regions, at different ages of eligibility. Chronological implementation of the subsidies is tabulated in Table 1. Furthermore, some treatment regions subsidized contraceptives until a woman reached a certain age, and others only until the calendar year in which she reached that age. As such, a woman's status was dependent on which treatment region she lived in, when the treatment region implemented the subsidy, and the treatment region-specific regulations governing eligibility.

The first step in creating our subsidy dummy S_{j_m} is to match women to the municipalities where they lived as teenagers. Municipalities are then assigned to treatment regions, which can be either counties or municipalities depending on the level in which the subsidy was implemented. Next, we impose a birth date cutoff for each treatment region. Women born above this cutoff are considered eligible for the subsidy. The full process of creating the subsidy dummy is described in detail in Appendix A.

After constructing the subsidy dummy, we note that in our final sample of 492,064 women, 197,750 women (approximately 40 %) had access to subsidized contraceptives.

An important clarification is that $S_{j,m}$ only captures whether a woman had access to subsidized contraceptives, not whether she actually used them or not. Thus, any effect captured by the subsidy dummy is technically an intention-to-treat (ITT) effect.

6.2.2 Income Variable

Different Measures of Income

The main income variable of interest is earned income (*sammanräknad förvärvsinkomst, CSFVI*) (see Appendix B for comprehensive definitions of this income measure). This measure of income captures the primary pre-tax incomes from labor, such as wages, business income and social benefits related to labor (mainly unemployment, parental and disability benefits). Capital income is not included. The motivation behind studying mobility in earned income is that we are primarily interested in income changes that are mediated by changes in human capital (education, career paths, occupational choice, etcetera), which should reasonably be reflected in an individual's earned income.

As an extension of the analysis, we will also study the effect of access to contraceptives on mobility in household disposable income (*disponibel hushållsinkomst, CDISPH*). Disposable income differs from earned income in several ways. It includes after-tax income from both capital and labor, as well as transfer payments. Moreover, we have manipulated the *CDISPH* measure, which is originally on the household level, by distributing it via an equivalence scale to account for the household structure. This is the standard method in income mobility studies (Roine and Jäntti 2021). As such, the purpose of the *CDISPH* analysis is to test whether a potential effect of contraceptives on intergenerational mobility is mediated not through human capital accumulation, but by better marriage matching and higher-quality marriages, as proposed by Goldin and Katz (2002).

For both *CSFVI* and *CDISPH*, income is converted to 2018 prices according to the Consumption Price Index from Statistics Sweden.

Income Estimation in IGE Models

IGE models frequently utilize income around age 40 as a proxy for lifetime income. For example, Roine and Jäntti (2021) use averages of income at ages 39–41 as their income variable. It is, however, not clear whether age 40 is also the most suitable for measuring women's income, given the empirically established wage penalty of motherhood. However, no other age seems unanimously more or less appropriate (Roine and Jäntti 2021). We choose to measure income when our women, born 1967–1978, are between age 37 and 41 years old. As 2018 is the last year in which

incomes are registered in our data set, we are unable to study women's incomes past age 41.[4] Nonetheless, averaging income over five years likely provides a fairly good proxy for lifetime income.

For the women's fathers, however, we adopt a slightly different method. Instead of measuring their income at around age 40, we measure their income when their children – the women we study – were between 7 and 13 years old. There are two main reasons for this. Firstly, as we attempt to proxy the economic status that the woman grew up with, and compare it to her own economic status in adulthood, it is intuitively appealing to look at her father's economic conditions during the formative years of her childhood. If we instead study her father's income conditional on him being around age 40, the economic situation may instead be reflective of a point in time when the woman of interest may have left her childhood home.[5] This approach becomes yet more attractive when considering what previous research says about early childhood circumstances, such as parents' investments in their children's human capital, as determinants of intergenerational mobility (Chetty, Hendren, and Katz 2016; Brandén and Nybom 2020). Additionally, Björklund, Roine, and Waldenström (2012) argue that parents' income during crucial years in their offspring's childhood is a solid channel for intergenerational transmissions. The second reason for measuring fathers' income conditional on their child's age is that we prefer to only use income data from 1974 and onwards. This is primarily due to two major tax reforms that were rolled out in 1971 and 1974, which by and large replaced joint taxation of married couples with individual taxation, as well as made social benefits such as disability checks taxable. Using income data from between 1974 and 1991 ensures that we have a consistent definition of income for all fathers in our sample.

Going back to the "zeroes problem" mentioned by Mitnik and Grusky (2020), it turns out is not straightforward to replace zero income with, for example, SEK 1. This is because of the uneven distribution of zero income between the father and daughter generations. It would inflate the number of daughters with very low incomes, and hence skew mobility in a problematic way. For that reason, observations of zero income have been dropped. A complete overview of sample attrition is detailed in Table 3.

4. Our last cohort, born in 1978, turned 41 in 2019 and can therefore not be observed at age 41. Their lifetime income is thus based on incomes from ages 37-40.

5. Of course, one could argue that measuring parents' income conditional on their child's age is problematic if parents are of different ages at the time of measurement, thereby introducing life-cycle bias. Mazumder (2005) has highlighted how lifetime income estimates are highly sensitive to measurement error due to, for example, age. However, we believe that this is not a major cause of concern, as the average age of fathers at the midpoint of income measurement is 39.5 (see Table 4).

6.2.3 Income Quintiles

We divide both the women and their fathers into quintiles according to their position in the income distribution. The quintiles are constructed over the average incomes of the father and daughter subsets of the sample, respectively. Surely, this introduces puzzling questions such as "is one's position in the distribution of *averages of incomes from different years* truly representative of one's position in the income distribution *the year in which incomes are measured*?". However, given that we are primarily interested in whether potential effects of the subsidy on intergenerational mobility differ between groups of women of different *absolute* economic and material standard, this approach appears resonable.[6]

Furthermore, we trim our sample by removing the top and bottom 1 % of earnings among both fathers and daughters. This is to exclude the high variance of earnings in the tails of the distribution, which adds a high degree of heteroscedasticity and makes results from the first and fifth quintile difficult to interpret. The distribution of incomes within the first and fifth quintiles is drastically different from the distribution within the second, third and fourth quintiles (Appendix C displays the quintile cutoffs in the trimmed and untrimmed sample, respectively). Removing the very high and very low incomes ensures that the standard errors of our elasticity coefficients are not inflated due to very high variance at the tails. We do not believe that trimming the sample in this fashion is a serious data violation as we are interested in the effect of the subsidy in the population as a whole, as well as in the five quintiles. If we were instead interested in the effect among the poorest or richest 1 %, another model specification may be more suitable.

6. The alternative would be to construct "quasi quintiles" by assigning each person to their respective quintile of the income distribution, for each year that they are observed, and take the median or mode of all years as their lifetime income quintile. Surely, this better summarizes one's economic status relative to a relevant peer group, namely those who had incomes in the same year. This would be a suitable approach if one believes that it is primarily one's *relative* economic status in childhood that matters for intergenerational mobility. However, this approach requires equating median income quintiles from different periods. For example, we would have to say that those in the third quintile 1974–1980 have the same standard relative to their peers, as those in the third quintile 1985–1991 have to their peers. Given rising inequality in the 1980s, this approach poses serious concerns.

6.2.4 Attrition Table

Table 3: Structure of Attrition

		Number of Observations
1.	All daughters born between 1967-1978 who do not have a reused personal identity number and who did not die before age 41.	727,004
2.	All daughters in 1 who were registered as living in a Swedish municipality according to the Swedish Census during their teens.	631,539
3.	All daughters in 2 with positive values for CSFVI (>SEK 10,000/year, in 2018 prices) for ages 37-41.	567,908
4.	All daughters in 3 with a registered father.	555,778
5.	All daughters in 4 with a father with positive CSFVI values (>SEK 10,000/year, in 2018 prices) for all years when the daughter's age was 7-13.	512,141
6.	All daughters in 5, after removing the top and bottom 1% of CSFVI earners among the daughters and fathers in 5.	492,064

Presented above in in Table 3 is the structure of attrition for our sample. After imposing the restrictions necessary in order to employ our empirical strategy detailed in 5.1, our sample consists of 492,064 women. This is a reduction of 234,940 women from the first to the sixth step. To begin with, we have chosen to exclude all women with a suspected reused personal identity number in order to deal with duplicate IDs in the data. Additionally, we need to be able connect the daughters with a registered father as well as a municipality of residence, to establish which treatment region the women belonged to during the subsidy implementation period. Lastly, observations where incomes (measured in CSFVI, as described in Section 6.2.2) for women and fathers cannot be measured in a reliable way are removed.

All in all, these restrictions will disproportionately impact those with low incomes, who are born or have lived abroad, or with parents from foreign countries. Therefore, this attrition can be viewed as problematic. However, as our sample still contains data on nearly half a million Swedish women and their fathers, we believe that the main effect of the subsidy may still be reliably estimated.

6.2.5 Descriptive Statistics

Below we include descriptive statistics for all women and fathers in our sample. We also report descriptive statistics separately depending on women's ITT status.

Table 4: Descriptive Statistics for Our Sample of Women Born 1967-1978

	No. of obs.	Mean	Standard Dev.	Min	Max
No Access to Subsidy					
Daughter Year of Birth	294314	1970.632	2.990728	1967	1978
Daughter CSFVI	294314	307919	120090.5	93853	824588
Father CSFVI	294314	279281.2	102330.4	86137	751157
log(Daughter CSFVI)	294314	12.56539	.382725	11.44948	13.62264
log(Father CSFVI)	294314	12.4798	.3445027	11.36369	13.52937
Daughter CDIPSH	284682	292626	105526.6	105907	774566
Father CDISPH	284682	132487.6	40361.05	53603	275409
log(Daughter CDISPH)	294314	13.06467	.481369	11.01676	18.01404
log(Father CDISPH)	294314	12.39663	.3233642	10.4119	15.22951
Daughter Education Level	293943	4.548545	1.314779	1	7
Father Eduction Level	286912	3.08916	1.724467	1	7
Number of Children	294314	1.946547	1.070089	0	19
Daughter's Age at First Birth	259057	28.07718	5.366376	13	51
Father's Age, When Daughter is 10	294314	39.43092	6.029109	22	79
Access to Subsidy					
Daughter Year of Birth	197750	1974.776	2.361432	1967	1978
Daughter CSFVI	197750	328398.2	120326.6	93848	824262
Father CSFVI	197750	265161.9	92662.52	86137	751159
log(Daughter CSFVI)	197750	12.6352	.3725897	11.44943	13.62224
log(Father CSFVI)	197750	12.4341	.3257611	11.36369	13.52937
Daughter CDIPSH	192249	311430.9	106404.5	105928	774559
Father CDISPH	192249	142767.5	39769.77	53613	275392
log(Daughter CDISPH)	197750	13.13386	.4739596	11.0369	18.74607
log(Father CDISPH)	197750	12.49951	.3268837	10.82767	14.65125
Daughter Education Level	197261	4.859273	1.237644	1	7
Father Eduction Level	196109	3.169054	1.662528	1	7
Number of Children	197750	1.913163	1.048401	0	15
Daughter's Age at First Birth	173385	28.6588	5.014918	12	49
Father's Age, When Daughter is 10	197750	39.47229	5.544253	24	84
Total					
Daughter Year of Birth	492064	1972.297	3.423368	1967	1978
Daughter CSFVI	492064	316149.2	120603.9	93848	824588
Father CSFVI	492064	273606.9	98801.86	86137	751159
log(Daughter CSFVI)	492064	12.59344	.3802275	11.44943	13.62264
log(Father CSFVI)	492064	12.46144	.3378395	11.36369	13.52937
Daughter CDIPSH	476931	300206.2	106282.3	105907	774566
Father CDISPH	476931	136631.4	40439.32	53603	275409
log(Daughter CDISPH)	492064	13.09247	.479606	11.01676	18.74607
log(Father CDISPH)	492064	12.43798	.3286765	10.4119	15.22951
Daughter Education Level	491204	4.673329	1.293359	1	7
Father Eduction Level	483021	3.121597	1.700043	1	7
Number of Children	492064	1.933131	1.061552	0	19
Daughter's Age at First Birth	432442	28.31038	5.236058	12	51
Father's Age, When Daughter is 10	492064	39.44754	5.839126	22	84
Observations	492064				

7 Results

7.1 T-Test for Differences in Mean Income

Table 5: T-Test for Difference in Mean CSFVI for Treated and Untreated

	(1)	
Difference in Daughters' CSFVI	-20479.2***	(-58.60)
Observations	492064	

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

As an initial validation before our main analysis, we run a t-test for differences in mean income between treated and untreated women. We find that women who had access to the subsidy earn, on average, SEK 20,479 more per year than women who did not have access. The result is significant at the 0.1 % level. Though far from capturing the causal effect of the subsidy, this result provides suggestive evidence of a positive relationship between the subsidy and income. This is reassuring for our main analysis.

7.2 Main Results

7.2.1 Full Sample Results

Table 6: Regression Results for all Women Born 1967-1978 using CSFVI

	(1)	(2)	(3)	(4)
	log(Daughter CSFVI)	log(Daughter CSFVI)	log(Daughter CSFVI)	log(Daughter CSFVI)
log(Father CSFVI)	0.208***	0.220***	0.191***	0.197***
	(0.00167)	(0.0151)	(0.00439)	(0.00374)
Subsidy		0.237	-0.000157	0.0271
		(0.170)	(0.0638)	(0.0563)
Subsidy × log(Father CSFVI)		-0.0126	0.0114**	-0.00159
		(0.0154)	(0.00512)	(0.00444)
Region Dummies	no	no	yes	yes
Cohort Dummies	no	no	no	yes
Observations	492064	492064	492064	492064
R-squared	0.0342	0.0447	0.0630	0.0835

Standard errors in parentheses

* $p < .10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the log of the daughter's average earnings (measured in CSFVI) at ages 37-41. Father CSFVI is measured as the average CSFVI when the daughter was 7-13 years old. Subsidy refers to the subsidy dummy and is equal to 1 if the woman had access to the subsidy at any point during her teens. Region dummies refer to 36 treatment region dummies, while cohort dummies refer to birth year dummies. Standard errors, reported within parentheses, are robust to clustering at the treatment region level for all specifications except (1), where robust standard errors are used instead.

We note that baseline IGE is approximately 0.21. This is consistent with other findings in the Swedish intergenerational mobility literature (Brandén and Nybom 2020; Jäntti et al. 2006).

Further, the interaction of the subsidy and the log of father's income – which captures the effect of the subsidy on the IGE and thus generates our coefficient of interest – initially produces a significant coefficient of 0.0114 (column 3). This would indicate that the subsidy has increased the IGE by 1.14 percentage points, thereby increasing the rate of economic persistence between generations. However, when also introducing cohort dummies (column 4), the coefficient is no longer significant.

7.2.2 Quintile Results

Table 7: Regression Results for all Women Born 1967-1978 Belonging to Q1 Using CSFVI

	(1) log(Daughter CSFVI)	(2) log(Daughter CSFVI)	(3) log(Daughter CSFVI)	(4) log(Daughter CSFVI)
log(Father CSFVI)	0.0113**	0.00787	0.0152*	0.0149
	(0.00566)	(0.00701)	(0.00825)	(0.00968)
Subsidy		0.0762	0.193	0.0559
		(0.129)	(0.128)	(0.137)
Subsidy × log(Father CSFVI)		-0.000391	-0.00564	-0.00401
		(0.0106)	(0.0106)	(0.0114)
Region Dummies	no	no	yes	yes
Cohort dummies	no	no	no	yes
Observations	98414	98414	98414	98414
R-squared	0.0000411	0.00912	0.0241	0.0413

Standard errors in parentheses

* $p < .10$, ** $p < 0.05$, *** $p < 0.01$

Note: All notes in Table 6 apply with the addition that the regressions above have only been run on daughters to fathers in quintile 1 (the lowest quintile). Standard errors, reported within parentheses, are robust to clustering at the treatment region level for all specifications except (1), where robust standard errors are used instead.

Table 8: Regression Results for all Women Born 1967-1978 Belonging to Q2 Using CSFVI

	(1) log(Daughter CSFVI)	(2) log(Daughter CSFVI)	(3) log(Daughter CSFVI)	(4) log(Daughter CSFVI)
log(Father CSFVI)	0.187***	0.171***	0.171***	0.207***
	(0.0274)	(0.0273)	(0.0294)	(0.0289)
Subsidy		-0.866	-0.695	-0.343
		(0.560)	(0.561)	(0.527)
Subsidy × log(Father CSFVI)		0.0774*	0.0676	0.0280
		(0.0453)	(0.0456)	(0.0429)
Region Dummies	no	no	yes	yes
Cohort dummies	no	no	no	yes
Observations	98414	98414	98414	98414
R-squared	0.000472	0.0153	0.0311	0.0535

Standard errors in parentheses

* $p < .10$, ** $p < 0.05$, *** $p < 0.01$

Note: All notes in Table 6 apply with the addition that the regressions above have only been run on daughters to fathers in quintile 2. Standard errors, reported within parentheses, are robust to clustering at the treatment region level for all specifications except (1), where robust standard errors are used instead.

Table 9: Regression Results for all Women Born 1967-1978 Belonging to Q3 Using CSFVI

	(1) log(Daughter CSFVI)	(2) log(Daughter CSFVI)	(3) log(Daughter CSFVI)	(4) log(Daughter CSFVI)
log(Father CSFVI)	0.306***	0.309***	0.265***	0.277***
	(0.0311)	(0.0439)	(0.0320)	(0.0324)
Subsidy		-0.247	-0.551	-0.155
		(0.818)	(0.752)	(0.734)
Subsidy × log(Father CSFVI)		0.0272	0.0563	0.0131
		(0.0665)	(0.0605)	(0.0590)
Region Dummies	no	no	yes	yes
Cohort dummies	no	no	no	yes
Observations	98411	98411	98411	98411
R-squared	0.000993	0.0167	0.0359	0.0594

Standard errors in parentheses

* $p < .10$, ** $p < 0.05$, *** $p < 0.01$

Note: All notes in Table 6 apply with the addition that the regressions above have only been run on daughters to fathers in quintile 3. Standard errors, reported within parentheses, are robust to clustering at the treatment region level for all specifications except (1), where robust standard errors are used instead.

Table 10: Regression Results for all Women Born 1967-1978 Belonging to Q4 Using CSFVI

	(1) log(Daughter CSFVI)	(2) log(Daughter CSFVI)	(3) log(Daughter CSFVI)	(4) log(Daughter CSFVI)
log(Father CSFVI)	0.378***	0.439***	0.368***	0.366***
	(0.0215)	(0.0232)	(0.0283)	(0.0318)
Subsidy		1.713***	0.900	0.769
		(0.529)	(0.569)	(0.598)
Subsidy × log(Father CSFVI)		-0.129***	-0.0592	-0.0602
		(0.0424)	(0.0452)	(0.0476)
Region Dummies	no	no	yes	yes
Cohort dummies	no	no	no	yes
Observations	98413	98413	98413	98413
R-squared	0.00315	0.0167	0.0375	0.0608

Standard errors in parentheses

* $p < .10$, ** $p < 0.05$, *** $p < 0.01$

Note: All notes in Table 6 apply with the addition that the regressions above have only been run on daughters to fathers in quintile 4. Standard errors, reported within parentheses, are robust to clustering at the treatment region level for all specifications except (1), where robust standard errors are used instead.

Table 11: Regression Results for all Women Born 1967-1978 Belonging to Q5 Using CSFVI

	(1) log(Daughter CSFVI)	(2) log(Daughter CSFVI)	(3) log(Daughter CSFVI)	(4) log(Daughter CSFVI)
log(Father CSFVI)	0.202***	0.222***	0.187***	0.186***
	(0.00657)	(0.00794)	(0.00945)	(0.00823)
Subsidy		0.619***	0.297*	0.185
		(0.164)	(0.166)	(0.157)
Subsidy × log(Father CSFVI)		-0.0425***	-0.0119	-0.0138
		(0.0119)	(0.0130)	(0.0122)
Region Dummies	no	no	yes	yes
Cohort dummies	no	no	no	yes
Observations	98412	98412	98412	98412
R-squared	0.00993	0.0165	0.0346	0.0547

Standard errors in parentheses

* $p < .10$, ** $p < 0.05$, *** $p < 0.01$

Note: All notes in Table 6 apply with the addition that the regressions above have only been run on daughters to fathers in quintile 5 (the highest quintile). Standard errors, reported within parentheses, are robust to clustering at the treatment region level for all specifications except (1), where robust standard errors are used instead.

We note that the baseline IGE differs between the quintiles, with lower baseline IGE (column 1) in the first and second quintiles. Moreover, the subsidy originally appears to have decreased mobility by 12.9 and 4.25 percentage points in the fourth and fifth quintiles respectively (column 2 in Table 10 and Table 11). However, this effect also disappears when also controlling incomes for region and cohort.

7.3 Robustness Checks and Secondary Analyses

7.3.1 Excluding Stockholm

Recalling Table 2, Stockholm is an outlier both in mobility and due to the fact that subsidies were not introduced on the county level (as Solna was the only municipality which introduced subsidies). The lower mobility is likely driven by the higher concentration of wealth in the capital.

Therefore, as a robustness check, we run a regression on the full sample, excluding observations from Stockholm. Results are reported in Table 12. We note that the main results presented in 7.2.1 still hold.

Table 12: Regression Results for all Women Born 1967-1978 using CSFVI (Excluding Stockholm)

	(1) log(Daughter CSFVI)	(2) log(Daughter CSFVI)	(3) log(Daughter CSFVI)	(4) log(Daughter CSFVI)
log(Father CSFVI)	0.195***	0.188***	0.186***	0.194***
	(0.00186)	(0.00768)	(0.00478)	(0.00471)
Subsidy		-0.133	-0.0535	-0.00697
		(0.0936)	(0.0656)	(0.0646)
Subsidy × log(Father CSFVI)		0.0196**	0.0157***	0.000950
		(0.00757)	(0.00527)	(0.00515)
Region Dummies	no	no	yes	yes
Cohort Dummies	no	no	no	yes
Observations	415675	415675	415675	415675
R-squared	0.0290	0.0512	0.0591	0.0769

Standard errors in parentheses

* $p < .10$, ** $p < 0.05$, *** $p < 0.01$

Note: All notes in Table 6 apply, except that region dummies now refer to 35 treatment region dummies as Stockholm has been excluded from the analysis. Standard errors, reported within parentheses, are robust to clustering at the treatment region level for all specifications except (1), where robust standard errors are used instead.

7.3.2 Transition Matrix for Women Born 1967-1978

Table 13: Quintile Transition Matrix using CSFVI

	Father Quintile (CSFVI)				
Daughter Quintile (CSFVI)	1	2	3	4	5
1	25.44%	22.28%	20.41%	17.58%	14.28%
2	22.47%	22.90%	21.34%	18.63%	14.67%
3	20.61%	21.79%	21.17%	20.01%	16.41%
4	17.98%	19.19%	20.21%	21.50%	21.12%
5	13.49%	13.84%	16.87%	22.28%	33.52%

The purpose of the transition matrix above is to complement our IGE analyses by briefly demonstrating the limitations of the IGE measure. For example, even if the

IGE is very low (mobility is high) for women born to fathers in the bottom quintile, it does not mean that all of these women have moved up in the overall income distribution as adults. Notably, 25.44 % of them still remain in the bottom-fifth of their generation's corresponding distribution, and only 13.49 % have managed to move from the bottom to the very top. For women born to fathers in the top quintile, 33.52 % remain in the top quintile as adults – the highest rate of persistence anywhere in the matrix.

Clearly, this reveals how the many methods in the income mobility toolbox do not always overlap, which may produce results that initially appear contradictory. An example of this is IGE close to zero, accompanied by low inter-quintile transition rates. Once more, this demonstrates the need for acknowledging the subtle differences between income mobility models, and how no single model is able to exhaustively determine whether the new generation is "better off".

7.3.3 Testing the Effect of Mobility in Household Disposable Income

Table 14: Regression Results for all Women Born 1967-1978 Using CDISPH

	(1) Full	(2) Q1	(3) Q2	(4) Q3	(5) Q4	(6) Q5
log(Father CDISPH)	0.258***	0.159***	0.133***	0.153***	0.236***	0.330***
	(0.00418)	(0.0123)	(0.0102)	(0.0172)	(0.0196)	(0.0122)
Subsidy	-0.0713	0.117	-0.151	-0.0455	-0.490	-0.630***
	(0.0566)	(0.238)	(0.232)	(0.291)	(0.362)	(0.195)
Subsidy × log(Father CDISPH)	0.00502	-0.0104	0.0120	0.00350	0.0383	0.0485***
	(0.00480)	(0.0193)	(0.0188)	(0.0231)	(0.0285)	(0.0151)
Region Dummies	yes	yes	yes	yes	yes	yes
Cohort dummies	yes	yes	yes	yes	yes	yes
Observations	492064	94219	96383	96653	96464	93212
R-squared	0.0639	0.0241	0.0238	0.0256	0.0291	0.0389

Standard errors in parentheses

* $p < .10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the log of the daughter's average earnings (measured in CDISPH) at ages 37-41. Father CDISPH is measured as the average CDISPH when the daughter was 7-13 years old. Subsidy refers to the subsidy dummy and is equal to 1 if the woman had access to the subsidy at any point during her teens. Region dummies refer to 36 treatment region dummies, while cohort dummies refer to birth year dummies. Specification (1) is run on the full sample, whereas (2)-(6) are run at the quintile level (based on father CDISPH, with 1 being the lowest quintile). Standard errors, reported within parentheses, are robust to clustering at the treatment region level for all specifications.

Column 1 in Table 14 reports results from our full regression specification (including region and cohort dummies) in the full sample. Columns 2-6 report quintile results. Overall IGE, as well as IGE at the tails of the distribution, is higher when using disposable household income as the outcome measure. This is consistent with observations such as those by Brandén and Nybom (2020), who emphasize

the important role of marital sorting in explaining earnings persistence in household income.

Column 6 deserves special attention. Here, we observe that the subsidy significantly increased the IGE by 4.85 percentage points. It appears that the subsidy increased household income persistence for the women born in the top-fifth of the income distribution. From this, a proposition can be formulated: the main effect of the subsidy on intergenerational mobility is mediated not by human capital accumulation, but by assortive mating. Women born in high-income households understand the importance of having a high-income partner in adulthood, if they want to maintain the same economic standard as they had growing up. Using the pill allows them to have casual and non-committal relationships, essentially "shopping around" for suitable partners. Seen in this way, the pill can be thought of as a mechanism that reinforces income persistence for those at the top. However, this is of course a tentative proposition. This relationship should be studied in closer detail before any conclusion can be drawn.

8 Analysis

8.1 Findings

Starting from a purely descriptive perspective, there exists some heterogeneity in the IGE across the income distribution. Baseline IGE, without any added controls for subsidy access, cohort or treatment region, is very low among women born to the poorest fathers, fairly high in the middle, and medium-low at the top (see baseline IGEs summarized in Table 15). The high mobility at the bottom is likely driven by real wage increases between the mid-70s and the early 2010s, making even the lowest-income women better off, in absolute terms, than the lowest-income fathers.

Table 15: Baseline Quintile IGEs for all Women Born 1967-1978 Using CSFVI

	(1) Q1	(2) Q2	(3) Q3	(4) Q4	(5) Q5
log(Father CSFVI)	0.0113**	0.187***	0.306***	0.378***	0.202***
	(0.00566)	(0.0274)	(0.0311)	(0.0215)	(0.00657)
Region Dummies	no	no	no	no	no
Cohort dummies	no	no	no	no	no
Observations	98414	98414	98411	98413	98412
R-squared	0.0000411	0.000472	0.000993	0.00315	0.00993

Standard errors in parentheses

* $p < .10$, ** $p < 0.05$, *** $p < 0.01$

Note: Regressions (1)-(5) represent specification (1) in tables 6-11. Standard errors, reported within parentheses, are robust.

Initially, we see in the full sample regression what appears to be a positive effect on the IGE from the subsidy (Table 6). It is interesting to note that on the quintile level, there is initially a negative effect from the subsidy on the IGE in the fourth and fifth quintiles (Table 10 and 11). This could be interpreted as indicative evidence that the subsidy has *decreased* mobility in the sample as a whole, because the subsidy *increased* mobility at the top. However, no definitive conclusions can be drawn from this, as these effects disappear when also controlling the women's incomes for region and cohort effects. When doing this, there is no longer any discernible effect of the subsidy on the IGE, neither in the full sample nor in any of the quintiles. For that reason, we conclude that access to subsidized contraceptives did not affect women's intergenerational elasticity of income, as measured in earned income.

8.2 Discussion

In light of the findings in 8.1, access to subsidized contraceptives does not appear to have had an impact on the IGE in our sample, thereby answering our original research question. However, this in turn introduces new questions regarding possible drivers of said result. Considering that the IGE is a measure of the relationship between parent and child income, reasons for the null effect of the subsidy can theoretically be broken down into two categories. Either, the subsidy did not significantly change women's incomes, in which case there can be no effect on the IGE. Alternatively, the subsidy did change incomes, but not by a magnitude large enough to ultimately shift the IGE. Interactions between the two are of course also possible. Establishing the true causal drivers would require further analyses which lie beyond the scope of this thesis. However, using our results and previous research, we conjecture on the two scenarios and their likelihoods below.

8.2.1 The Impact of the Subsidy on Income

Using terminology from Section 4, it is conceivable that the subsidy did not noticeably alter the distribution of Type I and Type II women. For example, the credit constraint assumption does perhaps not hold in reality; maybe women who wanted to use contraceptives could borrow money to afford them even under the pre-subsidy price scheme. It is also possible that the price elasticity of demand for contraceptives is very low for the majority of women. Furthermore, it should be borne in mind that the effect captured by our subsidy dummy is an ITT effect, meaning that it does not accurately reflect the actual pill consumption of a given woman. Therefore, we cannot claim with certainty that women who enjoyed access to subsidized contraceptives also consumed more contraceptives. However, all the above should be viewed against the backdrop of Section 3.2, which presents rather convincing evidence that the subsidy did in fact increase pill consumption and delay first births among young women.

We cannot, however, be certain that the subsidy also resulted in higher incomes. Tentative evidence for this is presented in Table 5, but the difference in mean incomes between treated and untreated women may very well be driven also by underlying region or cohort trends. Furthermore, it is possible that access to subsidized contraceptives did not determine future earned income because of the extensive Swedish welfare state. With its free daycare, generous maternal leave schemes, anti-discrimination policies in the workplace and considerable social insurance, it is possible that the Swedish welfare system is able to adjust individual's outcomes for unfavorable circumstances such as having children at a young age. Nonetheless, this argument is weakened by the findings of Otterblad Olausson et al. (2001), who assert that childbirth in adolescence substantially raises the odds of socioeconomic disadvantage also in Sweden. Similarly, the wage penalty of motherhood is still very tangible in comparable welfare states such as Denmark (Lundborg, Plug, and Würtz Rasmussen 2018). Therefore, it cannot be plausibly claimed that the Swedish welfare state is able to fully neutralize the effect of early childbirth on income, though it is presumably able to lessen it.

On the other hand, the degree of substitutability between abortion and contraceptives has not been formally established. During the subsidy implementation period, abortion had been legal for around fifteen years. It was also freely available at a very low cost. Therefore, it is conceivable that women without access to the subsidy, who became unintendedly pregnant and did not want to have a child, simply had an abortion. This could explain why we do not observe an effect of the subsidy on the IGE, and should be investigated further.

In sum, we are unable to present conclusive evidence of whether the subsidy affected women's incomes. Nonetheless, theory suggests many channels through which pill access could increase income (Goldin and Katz 2002; Bailey 2006, 2010). Naïve tests (Table 5) and several working papers (Madestam and Simeonova 2012; Ragan 2011) point in the same direction. We thereby postulate that the lack of overall effect on IGE is rather driven by difficulties in converting these income gains to IGE shifts. This, in turn, can presumably be explained by the relative slightness of the income gains, the shortness of exposure to the subsidy, as well as the limited proportion of women whose lifetime outcomes were drastically altered by the subsidy, as briefly described in Section 3.2.

8.2.2 The Relationship Between Income and IGE

Indeed, it is plausible that the subsidy's effect on incomes may not have been substantial enough to shift the IGE. In particular, simultaneous changes in the income distribution such as rising income inequality may have rendered small income changes insignificant in overall mobility. As observed by Björklund and Jäntti (2009), rising income inequality requires more sizeable income changes to account for mobility. This is a material consideration in light of increasing income inequality from the 1980s and onwards (Roine and Jäntti 2021).

Further, the transition matrix presented in Table 13 demonstrates that those born to low- and high-income fathers are overrepresented in the low and high ends of their corresponding distributions. This confirms that real income gains are only part of the overall dynamics of income persistence. For that reason, we posit that any absolute income gains from the subsidy may not have been enough to also impact mobility relative to one's peers, which explains why we do not observe an effect of the subsidy on the IGE.

Moreover, it may also be problematic to estimate the IGE between fathers and daughters, considering their differential earnings profiles. If fathers were the primary breadwinners, but their daughters are often working part-time, their income differentials are not strictly comparable. For example, a potentially large marginal effect of the subsidy on daughters' incomes does not translate into large mobility with regards to her father's income, if their labor force participation differs largely on the intensive margin.

8.3 Areas for Future Research

Our suggestions for future research can be divided into three categories: investigations into the effect of increasing income inequality on the IGE, model adjustments, and analytic extensions.

Firstly, we suggest that one attempts to disentangle absolute from relative effects in the IGE measure. By manipulating the underlying income distributions of fathers and daughters, as done by Roine and Jäntti (2021), it is possible to see how much of the IGE that can be explained by rising income inequality. If the reason for the null effect of the subsidy on the IGE is due to changes in the income distribution, this exercise may shed new light on the effect of the subsidy. Income mobility methods more suited for comparisons across differential distributions, such as the correlation methods proposed by Corak, Lindquist, and Mazumder (2014), are presumably relevant. Additionally, estimating IGE for normalized levels of hours worked may reveal the true effect of the subsidy on the IGE, if fathers and daughters had comparable rates of labor force participation on the intensive margin.

Our second category of suggestions are related to data and model suggestions, such as adding linear abortion trends to the model. This would put the substitutability between abortion and contraceptives into full view. Additionally, other researchers may consider the trade-offs between the ITT approach employed in this paper and a more traditional average treatment effect from pill consumption. The second alternative may be pursued by applying our model to, for example, local pharmacy sales data. However, such an approach requires having to deal with endogeneity and self-selection issues which the ITT approach conveniently circumvents. In all likelihood, self-selection into using birth control is driven by underlying heterogeneity in, for example, career preferences and in how individuals

discount the future. These factors reasonably hold some explanatory power also in terms of future earnings. Thus, estimating the effect of direct pill consumption on the IGE will likely produce deeply biased results. On the other hand, this opens up for different, but tangential, research endeavors such as simulating contraceptive preferences depending on perceived cost of pregnancy. Such an exercise would perhaps reveal a social gradient in demand for contraceptives, as mentioned in Section 4. This may then be applied to pill consumption data to disclose interesting details about the joint dynamics of childbirth, contraceptive use and intergenerational economic persistence.

Finally, we suggest investigating more closely our findings related to intergenerational mobility in household disposable income, as presented in Section 7.3.3. A working paper by Holmlund (2006) studies the effect of a Swedish school reform on intergenerational mobility through assortive mating. Holmlund's argument for this setup is that the school reform changed the composition of the peer group in which individuals find future partners, hence breaking patterns of intergenerational economic persistence. A similar approach could be utilized for our research question. The need for taking a fuller family structure into account when studying intergenerational economic patterns has been acknowledged also by Adermon, Lindahl, and Palme (2021) and Fernández, Fogli, and Olivetti (2002). All in all, the above sheds light on the need for more comprehensive income mobility models that are able to accommodate also the gender, family and social network dynamics that underpin income persistence.

9 Conclusion

Oral contraceptives remain one of the major innovations of the 20th century. They have played a pivotal role in helping women shape and govern their own lives with regards to childbearing, education, work and marriage. In this paper, we have used regional and temporal variation in a subsidy scheme as a natural experiment to study whether access to subsidized contraceptives altered the intergenerational elasticity of income for Swedish women born 1967–1978. The hypothesized causal channel of such an effect is women's higher investments in human capital as a consequence of postponed childbirth. We find, however, that the access to subsidized contraceptives did not alter the IGE in a significant way. We tentatively propose that growing inequality between the generations is partly driving this result. Nonetheless, we present indicative evidence that the subsidy may have contributed to a higher degree of assortive mating for women born in the top quintile of the income distribution. Though merely suggestive, this is clearly an interesting avenue for future research.

As we have answered our main research question, many questions remain. More than half a century after its invention, the full impact of oral contraceptives on the economic welfare of men, women and children is still not understood. While it

is highly plausible that the marginal benefits of oral contraceptives were largest at around the time of their introduction, as found by Bailey (2006, 2010) and Goldin and Katz (2002), it does not mean that they have no role to play in a modern welfare state such as Sweden. One may even posit that we do not observe an effect of contraceptive technology on intergenerational economic patterns, precisely *because of* decades' worth of cumulatively improving norms and prospects for women, partly catalyzed by the pill's advent in the 1960s.

Clearly, a prerequisite for economic equality between men and women is a societal structure in which women's opportunities are not disproportionately delimited by their biological functions. Matters such as abortion rights, maternal benefits as well as safe, accessible and efficient contraceptives must therefore be thoroughly understood and vigilantly defended. Moving forward, this will surely remain an imperative and significant task for academics across many disciplines.

References

Adermon, A., M. Lindahl, and M. Palme. 2021. "Dynastic Human Capital, Inequality, and Intergenerational Mobility." *American Economic Review* 111 (5): 1523–48.

Albanesi, S., and C. Olivetti. 2007. "Gender Roles and Technological Progress." *NBER Working Paper Series,* no. 13179.

Andersson, G. 2000. "The Impact of Labour-Force Participation on Childbearing Behaviour: Pro-cyclical Fertility in Sweden during the 1980s and the 1990s." *European journal of population* (Dordrecht) 16 (4): 293–333.

Axelsson, C. 1992. *Hemmafrun som försvann : övergången till lönearbete bland gifta kvinnor i Sverige 1968-1981.* Stockholm.

Bailey, M. J. 2006. "More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply." *The Quarterly Journal of Economics* 121 (1): 289–320.

———. 2010. ""Momma's Got the Pill": How Anthony Comstock and Griswold v. Connecticut Shaped US Childbearing." *The American Economic Review* 100 (1): 98–129.

Becker, G. S., and N. Tomes. 1979. "An Equilibrium Theory of the Distribution of Income and Intergenerational Mobility." *Journal of Political Economy* 87 (6): 1153–1189.

———. 1986. "Human Capital and the Rise and Fall of Families." *Journal of Labor Economics* 4 (3 (Part 2)): S1–S39.

Björklund, A., and M. Jäntti. 2009. "Intergenerational Income Mobility and the Role of Family Background." In *The Oxford Handbook of Economic Inequality,* edited by B. Nolan, W. Salverda, and T. M. Smeeding. Oxford: Oxford University Press.

———. 2020. "Intergenerational Mobility, Intergenerational Effects, Sibling Correlations, and Equality of Opportunity: A Comparison of Four Approaches." *Research in Social Stratification and Mobility* 70 (2020): 100455.

Björklund, A., J. Roine, and D. Waldenström. 2012. "Intergenerational Top Income Mobility in Sweden: Capitalist Dynasties in the Land of Equal Opportunity?" *Journal of Public Economics* 96 (2012): 474–484.

Black, S. E., and P. J. Devereux. 2010. "Recent Developments in Intergenerational Mobility." *NBER Working Paper Series,* no. 15889.

Blackburn, M. L., D. E. Bloom, and D. Neumark. 1993. "Fertility Timing, Wages, and Human Capital." *Journal of Population Economics* 6 (1): 1–30.

Blau, F. D., and L. M. Kahn. 2017. "The Gender Wage Gap: Extent, Trends, and Explanations." *Journal of Economic Literature* 55 (3): 789–865.

Brandén, G., and M. Nybom. 2020. *Intergenerationell rörlighet: Tidigare forskning och utvecklingen i Sverige.* IFAU - Institute for Evaluation of Labour Market and Education Policy.

Budig, M. J., and P. England. 2001. "The Wage Penalty for Motherhood." *American Sociological Review* 66 (2): 204–225.

Bumpass, L. L. 1990. "What's Happening to the Family? Interactions Between Demographic and Institutional Change." *Demography* (Chicago, IL) 27 (4): 483–498.

Bygdeman, M., and K. Lindahl. 1994. *Sexualupplysning och reproduktiv hälsa under 1900-talet i Sverige.* Statens Offentliga Utredningar 1994:37. Swedish Foreign Ministry.

Chetty, R., N. Hendren, and L. F. Katz. 2016. "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment." *American Economic Review* 106, no. 4 (April): 855–902.

Chetty, R., N. Hendren, P. Kline, and E. Saez. 2014. "Where Is the Land of Opportunity? The Geography of Intergenerational Mobility in the United States." *The Quarterly Journal of Economics* 129 (4): 1553–1624.

Corak, M. 2013. "Income Inequality, Equality of Opportunity, and Intergenerational Mobility." *The Journal of Economic Perspectives* 27 (3): 79–102.

Corak, M., M. J. Lindquist, and B. Mazumder. 2014. "A comparison of upward and downward intergenerational mobility in Canada, Sweden and the United States." *Labour Economics* 30:185–200.

Fernández, R. 2013. "Cultural Change as Learning: The Evolution of Female Labor Force Participation over a Century." *The American Economic Review* 103 (1): 472–500.

Fernández, R., A. Fogli, and C. Olivetti. 2002. "Marrying Your Mom: Preference Transmission and Women's Labor and Education Choices." *NBER Working Paper Series,* no. 9234.

Gash, V. 2009. "Sacrificing Their Careers for Their Families? An Analysis of the Penalty to Motherhood in Europe." *Social Indicators Research* 93 (3): 569–586.

Goldin, C., and L. F. Katz. 2002. "The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions." *Journal of Political Economy* 110 (4): 730–770.

Grönqvist, H. 2009. *Putting Teenagers on the Pill: The Consequences of Subsidized Contraception.* Working Paper Series 2009:8. IFAU - Institute for Evaluation of Labour Market and Education Policy, April.

Gustafsson, S., and R. Jacobsson. 1985. "Trends in Female Labor Force Participation in Sweden." *Journal of Labor Economics* (United States) 3 (1): S256–S274.

Heidrich, S. 2017. "Intergenerational Mobility in Sweden: A Regional Perspective." *Journal of Population Economics* (Berlin/Heidelberg) 30 (4): 1241–1280.

Hoem, B. 1993. "The Compatibility of Employment and Childbearing in Contemporary Sweden." *Acta Sociologica* (Thousand Oaks, CA) 36 (2): 101–120.

———. 2000. "Entry into Motherhood in Sweden: The Influence of Economic Factors on the Rise and Fall In Fertility, 1986-1997." *Demographic Research* 2.

Hoem, B., and J. M. Hoem. 1996. "Sweden's Family Policies and Roller-Coaster Fertility." *Jinko Mondai Kenkyu (Journal of Population Problems)* 52 (3-4): 1–22.

Hoem, J. M. 1990. "Social Policy and Recent Fertility Change in Sweden." *Population and Development Review* (New York) 16 (4): 735–748.

———. 2005. "Why Does Sweden Have Such High Fertility?" *Demographic Research* 13:559–572.

Holmlund, H. 2006. *Intergenerational Mobility and Assortive Mating: Effects of an Educational Reform.* Working paper 4/2006. Swedish Institute for Social Research (SOFI), Stockholm University.

Jäntti, M., K. Røed, R. Naylor, A. Björklund, B. Bratsberg, O. Raaum, E. Österbacka, and T. Eriksson. 2006. *American Exceptionalism in a New Light: A Comparison of Intergenerational Earnings Mobility in the Nordic Countries, the United Kingdom and the United States.* IZA Discussion Paper 1938. IZA Institute of Labor Economics.

Kearney, M. S., and P. B. Levine. 2009. "Subsidized Contraception, Fertility and Sexual Behavior." *The Review of Economics and Statistics* 91 (1): 137–151.

Knowles, J. 2007. *Birth Control and the Rise in Female LFP; What Can We Learn from Women's Occupational Choices?* Working paper. March.

Kurtulus, F. A. 2012. "Affirmative Action and the Occupational Advancement of Minorities and Women During 1973–2003." *Industrial Relations* 51 (2): 213–46.

Li, H., and J. Zhang. 2009. "Testing the External Effect of Household Behavior: The Case of the Demand for Children." *The Journal of Human Resources* 44 (4): 890–915.

Light, A., and M. Ureta. 1992. "Panel Estimates of Male and Female Job Turnover Behavior: Can Female Nonquitters Be Identified?" *Journal of Labor Economics* 10 (2): 156–81.

Lundborg, P., E. Plug, and A. Würtz Rasmussen. 2018. *Kan kvinnor ha både barn och karriär? Resultat från data på assisterade befruktningar.* IFAU - Institute for Evaluation of Labour Market and Education Policy.

Madestam, A., and E. Simeonova. 2012. *Children of the Pill: The Effect of Subsidizing Oral Contraceptives on Children's Health and Wellbeing.* Working paper. November.

Manduca, R., M. Hell, A. Adermon, J. Blanden, E. Bratberg, A. C. Gielen, H. van Kipepersluis, et al. 2020. *Trends in Absolute Income Mobility in North America and Europe.* IFAU - Institute for Evaluation of Labor Market and Education Policy.

Marmot, M. 2004. *Status Syndrome.* London: Bloomsbury.

Mazumder, B. 2005. "Fortunate Sons: New Estimates of Intergenerational Mobility in the United States Using Social Security Earnings Data." *The Review of Economics and Statistics* 87 (2): 235–255.

Mitnik, P. A., and D. B. Grusky. 2020. "The Intergenerational Elasticity of What? The Case for Redefining the Workhorse Measure of Econonomic Mobility." *Sociological Methodology* 50 (1): 47–95.

National Board of Health and Welfare. 1994. *Minskar tonårsaborter vid subventionering av p-piller? Ett försök till utvärdering av en abortförebyggande insats.* Scientific Report 1994-18-2. The National Board of Health and Welfare, Centre for Epidemiology.

———. 2001. *Statistics - Health and Diseases: Abortions 2000.* Statistical Report 2001-42-8. The National Board of Health and Welfare, Centre for Epidemiology.

———. 2005. *Reproduktiv hälsa i ett folkhälsoperspektiv.* Scientific Report 2005-112-5. The National Board of Health and Welfare, Centre for Epidemiology.

Nybom, M., and J. Stuhler. 2020. *Biases in Standard Measures of Intergenerational Income Dependence.* Institute for Evaluation of Labor Market and Education Policy.

OECD. 2021. *Income Inequality.* https://data.oecd.org/inequality/income-inequality.htm. Accessed: 29th October 2021.

Otterblad Olausson, P., B. Haglund, G. Ringback Weitoft, and S. Cnattingius. 2001. "Teenage Childbearing and Long-Term Socioeconomic Consequences: A Case Study in Sweden." *Family Planning Perspectives* 33 (2): 70–74.

Pekkarinen, T., R. Uusitalo, and S. Kerr. 2009. "School Tracking and Intergenerational Income Mobility: Evidence from the Finnish Comprehensive School Reform." *Journal of Public Economics* 93 (7-8): 965–973.

Piketty, T. 2014. *Capital in the 21st Century.* Cambridge: Harvard University Press.

Ragan, K. S. 2011. *How Powerful Was the Pill? Quantifying a Contraceptive Technology Shock.* Working paper. September.

Region Västra Götaland. 2000. *Utvärdering av subventionerade avgifter för preventivmedel till tonåringar.* Assessment Report. Region Västra Götaland.

Roine, J., and M. Jäntti. 2021. *Får varje generation högre inkomster än sina föräldrar? – Frågor och svar om absolut inkomströrlighet i Sverige.* Swedish Fiscal Policy Council.

Sonfield, A., K. Hasstedt, M. L. Kavanaugh, and R. Anderson. 2013. *The Social and Economic Benefits of Women's Ability To Determine Whether and When to Have Children.* Guttmacher Institute.

Sundström, M., and F. P. Stafford. 1992. "Female Labour Force Participation, Fertility and Public Policy in Sweden." *European Journal of Population* (Netherlands) 8 (3): 199–215.

Yang, X., Q. Wen, J. Ma, and J. Li. 2020. "Upward Mobility and the Demand for Children: Evidence From China." *China Economic Review* 60 (2020): 101393.

Appendix A: The Subsidy Eligibility Dummy

Here, we describe in detail how we have constructed the dummy variable S_{j_m} which indicates whether a woman had access to the subsidy, given her birth year, birth month and municipality of residence in youth. A full account of the subsidy implementation scheme is to be found in Table 1.

The first step in generating S_{j_m} is to match women to the municipalities where they lived as teenagers. Ideally, we would have liked to assign women to the municipality where they lived at age 16, at which age some women may have moved from their home municipality to attend high school elsewhere (a practice that is not uncommon in some rural areas of Sweden). This would work as a fairly accurate proxy for where the woman resided in her teens. Even better, we would have liked to observe which municipality the woman resided in for each year between the age of 13 and 20, and assign her to the municipality where she lived for a majority of the years. Unfortunately, we do not have access to annual census data. Instead, the censuses we have access to (*The Population and Housing Censuses*) were conducted in five-year intervals, with 1990 being the last year. For our cohorts, we will thus use the 1985 and 1990 censuses. We impose a cutoff after 1973, where those born before 1973 will be assigned to municipalities according to the information in the 1985 census, and those born after 1973 will be assigned according to the 1990 census. For clarity, the women's ages in 1985 and 1990 for our birth cohorts are tabulated in Table 16. Though we observe most of the women at representative ages, some cohorts are as young as ten when we match them to municipalities. Thus, we cannot exclude the possibility that some women, particularly in the younger cohorts, may have lived elsewhere at the time when they wanted to access contraceptives. This introduces some potential measurement error. However, it is not clear in what direction, if any, this may bias our results.

Table 16: Ages and Census Data Used for Birth Cohorts 1967–1978.

Birth cohort	**Age in 1985**	**Age in 1990**	**Census Year Used**
1967	18	23	1985
1968	17	22	1985
1969	16	21	1985
1970	15	20	1985
1971	14	19	1985
1972	13	18	1985
1973	12	17	1985
1974	11	16	1990
1975	10	15	1990
1976	9	14	1990
1977	8	13	1990
1978	7	12	1990

Once women are matched to municipalities, we must construct a birth date cutoff above which women were eligible. To exemplify, Blekinge county implemented the subsidy on March 1st, 1991, for all women aged 19 or younger. This means that anyone who turned 20 on the day of the subsidy implementation or earlier was ineligible for the subsidy. Thus, women born on or before March 1st, 1971 are ineligible, and women born after March 1st, 1971 are eligible.

Usually, the subsidy was implemented on the first day of the month, in which case we have counted everyone born in the month of the implementation as eligible (in the Blekinge example, anyone born in March 1971 or later are considered eligible). In some cases, the subsidy applied to everyone until the calendar year in which they reached a certain age. For example, Värmland implemented the subsidy on March 1st, 1992 and it applied to everyone until the calendar year in which they turned 24. Thus, women born on or before December 31st, 1968 were ineligible (as 1992 was the calendar year they were due to turn 24) and women born on January 1st, 1969 or later were eligible. In these cases, the decision rule has been that everyone born in January or later of a certain year are considered eligible.

There are also a few special cases. In Malmö municipality (but not Malmöhus county), the subsidy was introduced on March 26th, 1993 and applied to anyone 18 or younger. As we can only observe birth months, we have been conservative and considered anyone born in April 1974 or later as eligible (though technically, anyone born after March 26th, 1974 was eligible). Additionally, Jönköping and Kalmar county made the subsidy available to those younger than 20 or 21, respectively. As we once again cannot observe women's birth dates, we set conservative month limits.

Appendix B: Income Definitions

In this appendix, we describe the various components that make up our variable of interest, CSFVI or total earned income. The exact definition of the variable has varied over the years, primarily due to changes in tax regulation. Nonetheless, the main idea is to construct a comparable measure of an individual's total earned income. The below list has been recreated from a report by Roine and Jäntti (2021).

1968-1970: $CSFVI = INJO + INAF + INRO + INDR + INTJ + INTF + SJOIN$

$INJO$ - Agricultural property income
$INAF$ - Other property income
$INRO$ - Business income
$INDR$ - Income from joint ownership of small business etc.
$INTJ$ - Income from employment
$INTF$ - Income from temporary professional activity
$SJOIN$ - Taxable maritime income

1971-1977: $CSFVI = AINJO + BINJO + INAF + AINRO + BINRO + AINTJ + BINTJ + INTF + SJOIN$

$AINJO$ - Agricultural property income, type A
$BINJO$ - Agricultural property income, type B
$INAF$- Other property income
$AINRO$ - Business income, type A
$BINRO$ - Business income, type B
$AINTJ$ - Income from employment, type A
$BINTJ$ - Income from employment, type B
$INTF$- Income from temporary professional activity
$SJOIN$ - Taxable maritime income

1978: $CSFVI = INATJ + INSFAST + INSTFF + INASJOR + INBSJOR + INASROR + INBSROR + INSJO$

$INATJ$ - Income from employment, type A
$INSFAST$ - Other property income
$INSTFF$ - Income from temporary professional activity
$INASJOR$ - Public agricultural property income, type A
$INBSJOR$ - Public agricultural property income, type B
$INASROR$ - Business income, type A
$INBSROR$ - Business income, type B

$INSJO$ - Taxable maritime income

1979-1982: $CSFVI = INATJ+INBTJ+INSFAST+INSTFF+INASJOR+INBSJOR+INASROR+INBSROR+INSJO$

$INATJ$ - Income from employment, type A
$INBTJ$ - Income from employment, type B
$INSFAST$ - Other property income
$INSTFF$- Income from temporary professional activity
$INASJOR$ - Public agricultural property income, type A
$INBSJOR$ - Public agricultural property income, type B
$INASROR$ - Business income, type A
$INBSROR$ - Business income, type B
$INSJO$ - Taxable maritime income

1983-86: $CSFVI = INTJ+INSFAST+INSTFF+INSJOR+INSROR+INSJO$

$INTJ$ - Income from employment
$INSFAST$ - Other property income
$INSTFF$- Income from temporary professional activity
$INASJOR$ - Public agricultural property income, type A
$INBSJOR$ - Public agricultural property income, type B
$INASROR$ - Business income, type A
$INBSROR$ - Business income, type B
$INSJO$ - Taxable maritime income

1987-1990: $CSFVI = INTJ+INSFAST+INSTFF+INSJOR+INSROR+INSJO$

$INTJ$ - Income from employment
$INSFAST$ - Other property income
$INSTFF$ - Income from temporary professional activity
$INASJOR$ - Public agricultural property income, type A
$INBSJOR$ - Public agricultural property income, type B
$INSROR$ - Business income
$INSJO$ - Taxable maritime income

1991-1993: $CSFVI = SINKSJO$

$SINKSJO$ - Total earned income, including maritime income

1993-1997: $CSFVI = CSFVISJ$

$CSFVISJ$ - Total earned income, including maritime income

1998-: $CSFVI$ - Total earned income

(Maritime income is part of the regular taxation from 1998 and onwards)

Appendix C: Quintile Descriptives in the Trimmed and Untrimmed Sample

Table 17: Descriptive Statistics for Untrimmed Sample by Daughter Quintiles

	count	mean	sd	min	max
Quintile 1					
Daughter CSFVI	102429	167845.8	41162.58	11695	220474
Father CSFVI	102429	258652.1	114937.7	21067	4998630
Quintile 2					
Daughter CSFVI	102428	248666.6	15262.7	220475	273876
Father CSFVI	102428	260622.8	104581	16443	5034193
Quintile 3					
Daughter CSFVI	102429	297955.7	14161.97	273877	323483
Father CSFVI	102429	267302.2	109803.2	21396	4117137
Quintile 4					
Daughter CSFVI	102427	357524.6	22041.67	323484	401220
Father CSFVI	102427	282628.9	125293.8	12919	5147771
Quintile 5					
Daughter CSFVI	102428	536637.1	175389.1	401221	7045744
Father CSFVI	102428	327809.4	174124.1	17912	1.16e+07
Total					
Daughter CSFVI	512141	321725.5	148610.3	11695	7045744
Father CSFVI	512141	279403	130770.8	12919	1.16e+07
Observations	512141				

Table 18: Descriptive Statistics for Trimmed Sample by Daughter Quintiles

	count	mean	sd	min	max
Quintile 1					
Daughter CSFVI	98413	174517.2	35683.15	93848	222505
Father CSFVI	98413	255701.5	88928.35	86146	751155
Quintile 2					
Daughter CSFVI	98414	249679.8	14805.07	222506	274193
Father CSFVI	98414	258954.7	86631.49	86137	751057
Quintile 3					
Daughter CSFVI	98413	297682.4	13808.59	274194	322530
Father CSFVI	98413	264851.6	89583.53	86137	750858
Quintile 4					
Daughter CSFVI	98412	355319.9	21094.32	322531	396877
Father CSFVI	98412	277811	98093.13	86157	751157
Quintile 5					
Daughter CSFVI	98412	503549.4	95533.56	396878	824588
Father CSFVI	98412	310716.2	117226.4	86236	751159
Total					
Daughter CSFVI	492064	316149.2	120603.9	93848	824588
Father CSFVI	492064	273606.9	98801.86	86137	751159
Observations	492064				

Table 19: Descriptive Statistics for Untrimmed Sample by Father Quintiles

	count	mean	sd	min	max
Quintile 1					
Daughter CSFVI	102431	291173.3	122585.4	13950	4745766
Father CSFVI	102431	161527.7	37451.78	12919	204638
Quintile 2					
Daughter CSFVI	102427	297653.2	118282	11695	2877494
Father CSFVI	102427	222219	9462.736	204639	237985
Quintile 3					
Daughter CSFVI	102429	309515.3	129402.8	16396	5721554
Father CSFVI	102429	254028.1	9701.377	237986	271855
Quintile 4					
Daughter CSFVI	102426	331076.7	146172.7	14765	5804535
Father CSFVI	102426	298174	17451.78	271856	333363
Quintile 5					
Daughter CSFVI	102428	379210.2	195557.6	14951	7045744
Father CSFVI	102428	461069.8	180139.9	333364	1.16e+07
Total					
Daughter CSFVI	512141	321725.5	148610.3	11695	7045744
Father CSFVI	512141	279403	130770.8	12919	1.16e+07
Observations	512141				

Table 20: Descriptive Statistics for Trimmed Sample by Father Quintiles

	count	mean	sd	min	max
Quintile 1					
Daughter CSFVI	98414	291053.9	107963.1	93913	823642
Father CSFVI	98414	167591.5	31560.21	86137	205841
Quintile 2					
Daughter CSFVI	98414	296898.9	105482.7	93853	824037
Father CSFVI	98414	222749.1	9161.06	205842	238068
Quintile 3					
Daughter CSFVI	98411	307097.4	111023.8	93848	824359
Father CSFVI	98411	253671.1	9426.285	238069	271003
Quintile 4					
Daughter CSFVI	98413	325395	121204.9	93858	824588
Father CSFVI	98413	296084.1	16565.51	271004	329313
Quintile 5					
Daughter CSFVI	98412	360301.4	140749.7	93962	824587
Father CSFVI	98412	427941.6	93905.42	329314	751159
Total					
Daughter CSFVI	492064	316149.2	120603.9	93848	824588
Father CSFVI	492064	273606.9	98801.86	86137	751159
Observations	492064				