**Socioeconomic consequences of the fertility transition: sibling exposure and intergenerational social mobility in Stockholm 1878 – 1926.**

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## Introduction and Aim

The Fertility Transition is perhaps one of the most studied topics in historical demography, because understanding its fundamental causes and consequences could have wide-reaching implications into the future of human population development. First occurring in Europe and its offshoots in the 19th and early 20th centuries, the transition has since proceeded to parts of Asia and South America during the mid-20th century. The causes of the transition are still hotly contested, with explanations ranging from shifts in the value of time (Becker, 1960) to changing direction of wealth flows (Caldwell, 1982) to ideational transformation (Cleland & Wilson, 1987). More recently, Unified Growth Theory has contributed to the discussion by hypothesizing that fertility decline was induced by technological advancement and its consequent raising of the value of human capital (Galor, 2011; Galor & Weil, 2000).

The consequences of the transition, however, have received far less attention, especially at the micro-level. The aim of this paper is to look at how individuals' exposure to siblings during the fertility transition was associated with socioeconomic mobility later in life. More specifically, it is concerned with testing the *resource dilution hypothesis,* which posits that larger families had to distribute resources to more individuals, which lowers both tangible and intangible investments per child and eventually manifests itself in worse outcomes later in life (Blake, 1981; Becker, 1991; Downey, 1995). Similar studies have been undertaken for the Netherlands and Belgium in a historical context, while others have looked at more recent transitions in the developing world (Bras, Kok & Mandemakers, 2010; Van Bavel et al., 2011; Lam & Marteleto, 2008; Maralani, 2008).

There are several contributions of this study. First, it considers at a large, industrializing city during its fertility transition using longitudinal micro-data with detailed occupational information from Stockholm City between 1878 and 1926. Despite the dataset's impressive scope and depth, it has rarely been used for demographic research and, to my knowledge, never been used for analyzing the fertility transition. Such detailed population information can yield important findings for modern populations experiencing rapidly changing family size and can inform policy makers on the potential unanticipated effects of fertility decline.

Second, the paper approaches sibship size in a unique way. Most studies of the resource dilution hypothesis define sibling exposure in one of the following ways: surviving siblings at a given age (e.g. Van Bavel, 2011), total siblings in the home (e.g. Downey, 1995), or number of biological siblings (e.g. Lindahl, 2008). In this paper, sibling exposure is treated as a continuous measure of shared sibling years experienced until age 10. This measure relates to the spirit of the resource dilution hypothesis better than the above definitions for several reasons. First, it addresses the fact that siblings will dilute the resource pool to differing degrees based on the amount of time they share together during childhood. Second, it allows for the contribution of exposure from siblings who are born and die before recording the net siblings at a given age. This second point is highly relevant in populations where child mortality is high, as siblings may dilute family resources for several years before dying, but would fail to be represented in a measure of net sibship.

## Theory and Previous Research

The idea that larger family sizes tend to produce worse outcomes for both children and parents is not a new one. In *An Essay on the Principle of Population*, Thomas Malthus (1798) implored fertility restraint as he viewed large families as a path to destitution. Swedish economist Knut Wicksell stirred national debates by arguing along these lines in 1880, though this was perhaps more a result of his support for contraceptive devices rather than for a reduction in family size (Levin, 1994). Arséne Dumont (1890) echoed this sentiment in his theory of social capillarity, arguing that one's children will impede his chance for upward mobility. Yet in each of these cases, the arguments against larger families were mainly concerned with *parental* living standards rather than children's.

Becker's (1960) theory of fertility focuses on the elasticity of child quality versus child quantity with respect to income and views this elasticity as the mechanism through which both parent's and children's outcomes become manifest. This concept is based on the assumptions that having children comes at both a monetary and a temporal cost and parents have a preferred consumption bundle that includes both children and consumption goods from which they derive utility (Schultz, 1973; Becker, 1960). In order to maximize their utility, parents will invest in a combination of consumer goods and children. Because having more children is more cost intensive and more detrimental to satisfying tastes for consumer goods than having higher quality children, parents will substitute higher quality children for higher quantity children. When further extrapolated, one can come to the conclusion that a higher quantity of children would have a smaller quality multiplier due to a parental budget constraint. This is what is referred to as *resource dilution*. If one interprets the quality multiplier as investments in education and health, this should translate to lower social mobility for individuals with more siblings through, among other things, a lack of competitiveness in the labor market, higher morbidity and lower life expectancy.

In modern settings, the resource dilution hypothesis has attained much support. Several studies have reported negative relationships between sibship size and children's time spent reading, educational attainment, parental time spent with children, and investment in education (Mercy & Steelman, 1982; Blake, 1981; Downey, 1995; Teachman, 1987). Studies of developing countries have found supporting evidence as well. At the start of Indonesian fertility decline, it was found that the association between family size and children's education shifted from positive to negative between the urban cohort born between 1948 and 1957 and the cohort born between 1968 and 1977 (Maralani, 2008). Using a nationally representative inter-censal survey of Vietnam in 1994, it was shown that sibling size was associated with school attendance and educational attainment (Anh, Knodel, Lam, et al., 1998). For 14 modern European populations it was found that individuals from larger families had a diminished probability of receiving financial support from one's family (Emery, 2013).

To the detriment of this hypothesis, empirical results of historical populations have been mixed. Two studies of Belgian cities (Antwerp and Leuven) during their demographic transitions specifically test the resource dilution hypothesis. In Leuven, larger family size was associated with higher odds of downward mobility and lower odds of upward mobility (Van Bavel, 2006); in Antwerp, smaller family size reduced the odds of downward mobility, but did not increase the odds of upward mobility (Van Bavel et al., 2011). However, other historical populations have not always found support for this hypothesis. In a study of the Dutch population between 1840 and 1925 three alternative hypotheses found empirical support, namely the household development cycle, kinship buffering, and socioeconomic development hypotheses, which seek to explain why sibship size does not always lead to resource dilution (Bras, Kok & Mandemakers, 2010). In England between the $16^{th}$ and 19th centuries it has been argued (though sample sizes were quite small) that the elite had the largest families and that their children had the highest probability of remaining in the same class than any other socioeconomic group (Boberg-Fazlic, Sharp & Weisdorf, 2011). Evidence from $20^{th}$ century

Brazil finds that larger families became more detrimental after the demographic transition, indicating a necessity to take economic development into account when researching sibship and social mobility (Marteleto, 2010).

With this mixed bag of evidence in favor and opposing the hypothesis, this paper attempts to add to the discussion with a large, heterogeneous population in a small geographical area. Using Stockholm City at the turn of the 20$^{th}$ century allows for analyzing a population that was experiencing rapid industrial and urban growth, while at the same time swift fertility and mortality decline. The data allow for utilizing detailed occupational and date information to refine how sibling exposure is defined and to estimate its effects on intergenerational occupational mobility during the fertility transition.

## Data

The data used for this study come from the Roteman's Archive, a population register kept for Stockholm City between 1878 and 1926. The Roteman System was established in order to improve the quality of record keeping for the municipality. As migration increasingly expanded Stockholm's borders and density, traditional record keepers (i.e. parish priests) experienced difficulty in recording all vital events and movements within their respective parishes. This led to the establishment of the Roteman System by the city government on January 1, 1878 (Geschwind & Fogelvik, 2000).

This longitudinal register contains all individuals ever residing in the city during this period. Our extraction, though, is based on all women ever present, and any person linked to them (i.e. children, husbands/partners, lodgers, employees). This amounts to 3.7 million observations of about 970,000 unique individuals over the 48 year period. It has detailed information on migration, occupation, fertility and mortality. There is also information on marriage, smallpox vaccination status and the exact location of an individual's residence within the city. Because of how the data were collected, there is also detailed information on individuals' movements within the city. Each individual's records were updated upon births, deaths and movements within or outside of the city and also annually at the time of census registration, allowing for the observation of individual variation in a host of observed features over time. The structure of the data is spell-based with information explaining how each spell begins and ends. For instance, it is known if a spell began with a birth and ended with out-migration.

A great advantage of these data is that they offer detailed information on occupations of individuals over time, and this is the basis of this paper. With this information one can test various hypotheses that demand a socioeconomic dimension. Occupations were pre-coded using the Historical International Standard Classification of Occupations (HISCO) (Van Leeuwen et al., 2002). Using the HISCO information, a socioeconomic class variable was created using HISCLASS, which generates a 12-category classification scheme based on required skill level, degree of supervision, manual or non-manual character of work, and whether it is an urban or rural position (Van Leeuwen & Maas, 2011). These 12 categories were then aggregated up to six categories to avoid problems with small numbers. Nevertheless, the new categorization maintains the spirit of the original classification.

## Sample Selection

The requirements of being included in the final analysis sample were fairly restrictive, and this came at the cost of reducing the sample size. The first criterion for inclusion was being born in Stockholm. This was done to ensure that exposure to siblings could be observed completely from birth. If a mother had any children who were born and died prior to those born in Stockholm, they would contribute nothing to sibling exposure. If this criterion was not included, there would be no way of being sure of the sibling exposure experienced in the first years of life.

Second, the children used in the analysis are male. This decision is one based on interpretability of results rather than data availability. It is possible to follow women over time as well, but most left the labor market upon marriage. Those who could be followed and observed as working would therefore be a highly unrepresentative group. Alternatively, if one were to assign a husband's occupation to women to estimate their socioeconomic status, the mechanisms linking resource dilution and this outcome become less clear.

Third, individuals must be followed up until they were *at least* 30 years old. This was chosen to take into account the fact that age is correlated with occupational mobility. It could be argued that observing someone until age 30 does not resolve the problem, but this is a compromise between introducing more selection bias and correctly identifying occupational mobility. In the analysis, the restriction is tightened to age 40 as a robustness check.

Fourth, the children must come from a home with a father present. Without this information, it would be impossible to get any idea of intergenerational mobility, because, as already mentioned, so few women worked beyond their mid-20s. Those that did were almost exclusively working in low skilled positions, such as maids and seamstresses. This requirement does not, however, preclude illegitimate children from being included in the analysis. The data indicate whether a child was born out of wedlock and also identifies whether an illegitimate child belonged to the father's household or was born outside of the household. Thus there is no requirement that the parents must have been married.

Fifth, both the father and son must have had non-missing occupational information. Using the aforementioned five-class scheme, each individual was assigned the maximum socioeconomic class that was attained by their father before they were age ten. This forms their childhood class variable. Sons' adult class was defined as the maximum socioeconomic class they achieved at or above age 30. This transition between the two, or lack thereof, represents the outcome of interest for this study. Table 1 reveals a simple cross-tabulation of father's and son's classes. It becomes quickly apparent that children born into the higher occupations and lower managers groups tended to replicate their father's class more than children born into the lower classes. Furthermore, more than a third of those born to the lower managers group experienced downward mobility compared to roughly a quarter of the skilled workers' sons, though these moves were mostly into the adjacent skilled workers category. On the other hand, children born into the lower skilled and unskilled groups tended to be upwardly mobile despite the fact that these were classes with very high levels of fertility during this time, as can be seen in figure 1. These aggregate statistics seem to contradict the resource dilution hypothesis, but one must keep in mind that the hypothesis is concerned with the *individual* and not the group.

The above restrictions amount to a total of over 5,500 males who can be observed between birth and age 30. Of the 42,460 male births observed between 1878 and 1896, this amounts to about 17% of all those born in the city. Of course, not all of the lack of follow-up is due to migration. Some of it is simply due to mortality, which can be seen in table 2. After adjusting for the number of *observed* deaths of these cohorts (i.e. those dying in Stockholm prior to age 30) the percentage of survivors that can be followed increases to about 25%. It is unclear how many of the out-migrants died before age 30, but the percentage of the cohort survivors that can be followed should nevertheless be even higher.

When imposing such restrictive, data-intensive conditions on the sample, there is obviously a concern about introducing selection bias. The primary question that comes to mind is: who are the individuals that remain in the same place for 30 years or more and how do they differ from those that leave? To address this, a logistic regression was used estimate predicted probabilities of remaining under observation for the analysis. The dependent variable was whether a male met all of the above criteria for inclusion and the dependent variables were characteristics assigned from birth. The results may be found in table 3. The results indicate that children born into the upper classes were more likely to be included in the sample than the sons of the unskilled. While this difference is not very large, it may bias the results as the upper classes were more likely to experience downward mobility than upward mobility. In fact, because of how we have defined socioeconomic groups, the highest occupation cannot be upwardly mobile at all. To take this into account, I will only consider individuals born into the three middle groups: Lower Managers, Skilled Workers, and Lower Skilled Workers. The difference in inclusion probabilities is smaller between these groups and if this introduces bias into the model, it should appear as if the probability of downward mobility is more likely.

## Method

*Defining Sibling Exposure*

This paper defines sibship size differently than any study I am aware of. Although one's number of siblings is certainly a discrete variable, the exposure to those siblings is not. For instance, an individual with two siblings at, say, age 15 could, on the one hand, share little of his childhood with them if he had been born to a young, unwed mother. On the other extreme, he could be born as a triplet and share the entirety of his childhood with these siblings. Resource dilution is often treated as the quotient of parental resources divided by an unweighted number of siblings. But the above example shows that this may be misleading. In a low mortality population, it may be sufficient to simply weight each sibling's exposure by their age, though this still may underestimate exposure depending on, for instance, the normative age of leaving the household. Furthermore, defining the divisor as an unweighted surviving sibling count completely omits children who detracted from the resource pool, yet died before being counted as a surviving sibling, therefore assigning them no weight whatsoever. If, on the other hand, the gross number of siblings was included as a measure of sibship size, those siblings who died early would contribute too much.

With these considerations in mind I have defined sibling exposure as a continuous measure, the number of person-years an individual shared with his siblings until age 10. The cutoff at age 10 was an arbitrary decision and could be extended to any age. This definition accounts for differential exposure contributed by siblings, including siblings who only survive for a shorter period of time, and thus better captures the amount of exposure contributing to resource dilution.

Figure 2 demonstrates how this measure better approximates exposure. In this figure, the first two children survive until age 10, while children 3 and 4 die at ages 2.5 and 4, respectively. If sibling exposure were defined purely as the number of siblings a child had at age 10, then both of the first children would be considered to have the same exposure when in fact they did not. Child 2 had one more year of exposure than child 1. If one chose a later cutoff age, this difference could be mitigated, but this would depend on whether the mother had any subsequent children. Furthermore, at some point parental resources become inconsequential to individuals' development, and as such it probably would not be reasonable to raise this cutoff beyond the late teens. More important than this point, however, is the fact that this method clearly allows for exposure contributed by non-surviving siblings. Child 1 had two surviving siblings at age 10, but one of them died only one year after this cutoff. He also had a sibling who was born when he was four years old and died when he was six and a half. By using person-years as a measure of sibship, this information is not discarded. Instead, it contributes to the potential of resource dilution.

*Multivariate Analysis*

To analyze how the amount of sibling exposure was associated with socioeconomic mobility, multinomial logistic regressions are employed. The categorical dependent variable could take on three values representing upward mobility, non-mobility and downward mobility. The main independent variable of interest is the cumulated person-years an individual shared with other siblings by age 10. The model controls for one's birth order, illegitimacy, cohort, mother's age at birth, district of residence as a child, age at recording of maximum socioeconomic class and father's maximum achieved socioeconomic class. To account for correlation between brothers standard errors were clustered at the family level. Because not all sons could be upwardly or downwardly mobile due to their fathers coming from the highest and lowest socioeconomic groups, the models only consider the sons of fathers from the middle three groups.

The analysis is then extended to analyze how the age and sex composition of sibling exposure influenced mobility. This was done by calculating the number of shared person-years that came from younger versus older siblings and the number that came from male versus female siblings and including these in separate models.

## Results

The results of the multinomial logistic regression (table 4) indicate that the number of shared person-years a child experienced by age 10 was negatively associated with upward mobility and had no statistically significant association with downward mobility. The coefficients' magnitudes are small, but this is due to the scale of the independent variable. A one person-year increase was associated with a 1% decrease in the probability of being upwardly mobile relative to being non-mobile and this result was significant at the 1% level. The results thus provide some support to the resource dilution hypothesis.

To better visualize how sibling exposure was associated with mobility predicted probabilities were generated of each outcome, holding all other variables at their means. Doing this reveals that children with no shared person-years, which does not necessarily mean singletons, were most likely to experience upward mobility compared to downward or non-mobility. As sibling exposure increases, however, the probability of upward mobility quickly declines. Interestingly, this is not because individuals were more likely to be downwardly mobile, but rather they were more likely to remain in the same class as their father. The probability of downward mobility remains nearly constant for all levels of sibling exposure.

The model was modified to include the number of person-years shared by younger siblings and older siblings instead of total person-years to better understand how the age composition of sibling exposure influenced resource dilution. The results consistently point toward greater exposure to younger siblings as the driver of these results. Once again, the only statistically significant associations were found for upward mobility. The exposure to younger siblings was significant at the 1% level while the exposure to older siblings was insignificant at all conventional levels. A Wald test supported that the coefficients of these two variables were significantly different from each other. This result has interesting implications for research of the fertility transition, as it shows that there were certainly advantages for children belonging to small families. Whether these were intentional, however, remains to be seen.

Then, the model considered differential effects from male and female sibling exposure. No statistically significant effect was found for the exposure to male siblings, but the coefficient for female sibling exposure was statistically significant. A Wald test showed, however, that the coefficients of male and female sibling exposure were not significantly different from one another.

The results supported the previous models' findings in that there was only an effect of sibling exposure on upward mobility, but not on downward mobility. Furthermore, exposure to younger male siblings and younger female siblings both had statistically significant negative associations with upward mobility, while older sibling exposure from either sex showed no significant association.

As a robustness check, the sample was further restricted to only include individuals who could be observed until age 40. It would be a fair argument to say that one cannot measure occupational mobility at age 30, as these individuals may continue to work for another 15 to 20 years and experience professional advancement. By restricting the minimum follow-up age to 40, the chances that the individual have reached their occupational "peak" should be higher. The results (table 4) largely support the findings of the models with the lower follow-up age.

## Discussion

This paper has sought to test the resource dilution hypothesis in a historical setting, which posits that children with more siblings will have worse outcomes than those with fewer. This paper contributes to the literature by offering a more refined measure of sibling exposure than other studies concerning resource dilution. Sibling exposure was measured as the total shared person-years a child experienced until his tenth birthday. This measure has two main advantages. First, it accounts for the fact that exposure to resource dilution can vary substantially among individuals who have the same number of surviving siblings. Second, it allows children who die prematurely to contribute to the dilution of parental resources, a detail that is important to consider in high mortality contexts.

The analysis is not without its weaknesses, however. First, testing the resource dilution hypothesis requires that we find individuals who have worse socioeconomic outcomes because they had greater exposure to siblings. This is not achieved in this paper. The results show consistent associations, but they do not allow for a causal interpretation. A causal interpretation requires some sort of exogenous increase in sibling size, which is difficult to come by. One could use multiple births, for instance, as an indicator of an exogenous change in exposure, but there are simply too few individuals who meet all of the selection requirements and also had a set of twins born into their family.

Second, while I believe the measure of sibling exposure used here is superior to conventional sibling measurements, it is fairly data intensive. It requires detailed date information to calculate exposure, including for previously dead siblings. This should not be a problem for those using historical or modern registers, but may be too much to ask of survey data. To approximate sibling exposure, one could weight sibling counts by ages of siblings as done in Öberg (2014). Though this is not completely compatible to the metric used here, it does take into account the fact that individuals are differently exposed to their siblings and may be more easily calculated from survey data.

Finally, the issue of selection is one that is difficult to assess. At least 25% of the births born between 1878 and 1895 were followed until age 30 or later. How these individuals differ from those that migrated out of the city is not always clear. The selection model shown in table 3 revealed that individuals who remained in the city for this amount of time were more likely to be from the upper classes, illegitimately born, had a mother born in Stockholm , and were born in certain parts of the city, but the difference in probabilities was usually not very large between any categories. There were certainly many unobservable reasons that a certain proportion remained in the city, but any explanation for this would be merely speculative.

The results of the multinomial logistic regressions showed that children with higher sibling exposure were less likely to experience upward mobility and more likely to remain in the same socioeconomic class as their father. However, there was no significant association between sibling exposure and downward mobility, a finding that is contrary to those found by Van Bavel (2006) and

Van Bavel et al. (2011). Virtually the entire association between sibling exposure and mobility was due to the amount of time spent with *younger* siblings in the household rather than older ones. There was no association between the sex composition of exposure and mobility. All of these results held when the sample was restricted to only include males who could be observed between birth and age 40 or higher.

These findings have interesting implications for studying the effects of the fertility transition. In the language of economic theories of fertility, we can see evidence of higher quality children coming from lower exposure environments. Individuals growing up in large families, where children tended to be more tightly spaced, were much more likely to remain in the same socioeconomic class as their fathers. This fact then confronts researchers with a difficult task of untangling the relationship between child outcomes and parental fertility decisions. Researchers and theorists of fertility have long pointed to family size as a path toward social and economic advancement. But this path has often been considered to be one created for parents, not children. The theories of Malthus (1798), Dumont (1890), Westoff (1953), Becker (1960), and Caldwell (1976), and Johannson (1987) have all promoted this idea in one form or another: fewer children allows parents to be upwardly mobile, consume more, and generally have better living standards.

Yet the outcomes of the children of the fertility transition are seldom discussed. A role for the link between parental fertility behavior and their aspirations for their children in explanations of the fertility transition is unclear. It would be very difficult to test empirically and, without substantial ethnographic evidence, may be completely irrelevant. But this does not mean the observable association of socioeconomic outcomes and sibling exposure would be meaningless. While it may not be a cause for the transition, it may be a mechanism through which the transitional process reinforced itself. As children with lower sibling exposure moved upward, they likely gained access to higher incomes and more information via schooling and socialization. This, in turn, likely led to smaller family sizes. Because sibling exposure was not associated with downward mobility, the mechanism would only serve to either further reduce fertility levels or to maintain their current trend. This is one plausible way that the resource dilution mechanism may have played a role in the fertility transition without necessarily being its proximate cause.

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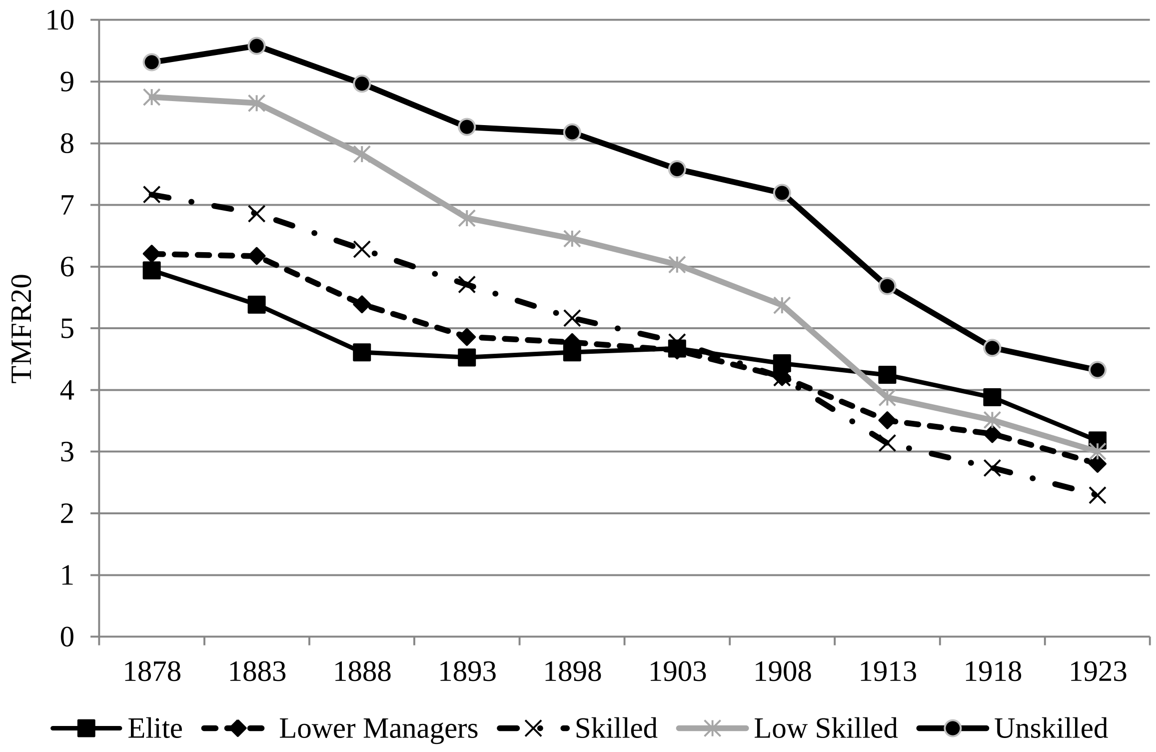
**APPENDIX**

**Table 1.**
Comparison of Father's and Son's Social Class.

| Father's Class | Son's Class: Higher Occupations | Lower Managers | Skilled | Lower Skilled | Unskilled | Number of Fathers |
|---|---|---|---|---|---|---|
| Higher Occupations | 54.8 | 19.9 | 22.3 | 2.4 | 0.6 | 166 |
| Lower Managers | 20.6 | 45.4 | 19.8 | 7.6 | 6.7 | 632 |
| Skilled | 9.0 | 28.2 | 36.5 | 14.2 | 12.1 | 720 |
| Low Skilled | 4.2 | 25.8 | 29.8 | 25.2 | 15.0 | 473 |
| Unskilled | 2.2 | 27.4 | 28.5 | 22.9 | 19.1 | 362 |
| Number of Sons | 314 | 744 | 669 | 356 | 270 | |

*Note:* All rows sum to 100%. Shaded boxes in the diagonal indicate non-mobility.

**Figure 1.**
TMFR20 by socioeconomic class.




*Note:* TMFR20 is the hypothetical number of births a married woman would have had between the ages of 20 and 49 if she had experienced the same age-specific marital fertility rates that were present in the period and remained married until at least 49 years old.

**Table 2.**
Percentage of males observed from birth until age 30 adjusting for mortality**.**

| Birth Year | (I)<br>Number of Males Born | (II)<br>Observed Deaths before Age 30 | (III)<br>Maximum Possible Survivors at age 30 | (IV)<br>Number of Men Observed until Age 30 | (V)<br>**Percentage of Survivors Observed until Age 30** |
|---|---|---|---|---|---|
| 1878 | 1576 | 683 | 893 | 192 | **21.5** |
| 1879 | 1795 | 665 | 1130 | 347 | **30.7** |
| 1880 | 1845 | 657 | 1188 | 385 | **32.4** |
| 1881 | 2001 | 656 | 1345 | 396 | **29.4** |
| 1882 | 2048 | 711 | 1337 | 419 | **31.3** |
| 1883 | 2187 | 738 | 1449 | 457 | **31.5** |
| 1884 | 2341 | 842 | 1499 | 472 | **31.5** |
| 1885 | 2394 | 791 | 1603 | 468 | **29.2** |
| 1886 | 2592 | 872 | 1720 | 496 | **28.8** |
| 1887 | 2372 | 771 | 1601 | 463 | **28.9** |
| 1888 | 2382 | 775 | 1607 | 468 | **29.1** |
| 1889 | 2410 | 790 | 1620 | 451 | **27.8** |
| 1890 | 2471 | 767 | 1704 | 456 | **26.8** |
| 1891 | 2451 | 732 | 1719 | 417 | **24.3** |
| 1892 | 2351 | 666 | 1685 | 368 | **21.8** |
| 1893 | 2363 | 630 | 1733 | 370 | **21.4** |
| 1894 | 2291 | 682 | 1609 | 307 | **19.1** |
| 1895 | 2261 | 656 | 1605 | 265 | **16.5** |

*Note*: Column II refers to observed deaths within Stockholm and does not count if individuals who migrated out of the city died before reaching age 30. Column III simply is the difference between column I and column II and therefore refers to the number of survivors there would be if absolutely no mortality occurred after individuals migrated from the city. Column IV is the number of males included in the analysis from each cohort. Column V is the difference between columns III and IV and is therefore an underestimate of the true proportion being observed.

**Table 3.**
Predicted probabilities of being included in analysis sample by characteristics at birth.

| Variable | Predicted Probability | Variable | Predicted Probability |
|---|---|---|---|
| Mother's Age at Birth: | | Father's Socioeconomic Group: | |
| 15 to 19 | 0.275 | Higher Occupations | 0.293 |
| 20 to 24 | 0.261 | Lower Managers | 0.295 |
| 25 to 29 | 0.256 | Skilled | 0.250 |
| 30 to 34 | 0.256 | Lower Skilled | 0.253 |
| 35 to 39 | 0.258 | Unskilled | 0.229 |
| 40 to 44 | 0.261 | Legitimacy: | |
| 45 to 49 | 0.269 | Born within Wedlock | 0.253 |
| Birth District: | | Born outside Wedlock | 0.347 |
| Gamla Stan | 0.296 | Birth order: | |
| Norrmalm | 0.251 | 1 | 0.266 |
| Kungsholmen | 0.233 | 2 | 0.259 |
| Östermalm | 0.234 | 3 | 0.257 |
| Södermalm-East | 0.271 | 4 | 0.255 |
| Södermalm-West | 0.283 | 5 | 0.254 |
| Mother's Birth Place: | | 6 | 0.251 |
| Stockholm City | 0.261 | 7 | 0.248 |
| Stockholm County | 0.085 | 8 | 0.246 |
| Other Sweden | 0.075 | 9 | 0.246 |
| Outside Sweden | 0.119 | 10 | 0.246 |
| Undefined | 0.220 | | |

*Note*: The above predicted probabilities were taken from a logistic regression, where the dependent variable was equal to one if a male could be observed from birth until age 30. Neither females nor individuals with missing socioeconomic information were included in this analysis.

**Figure 2.**
Distribution of Shared-Person Years in a Hypothetical Family.

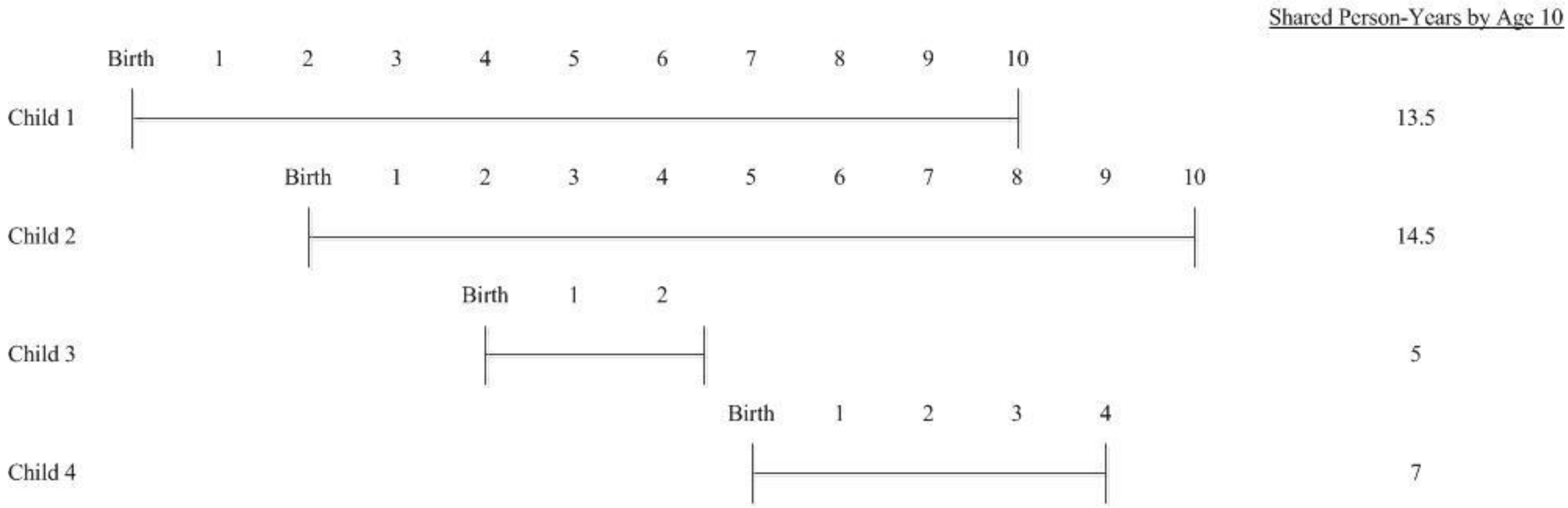


**Table 4.**
Multinomial Logistic Regressions of Social Mobility.

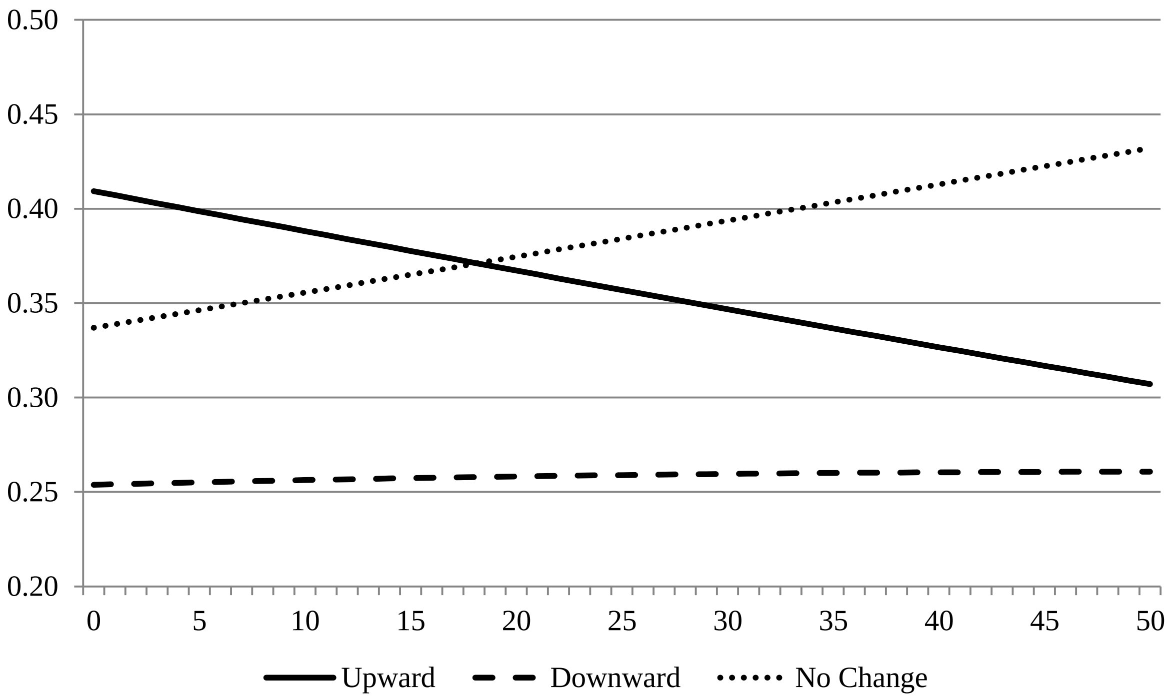
| | Upward Mobility | | | | | |
|---|---|---|---|---|---|---|
| | Age at Exit>=30 | | | Age at Exit>=40 | | |
| | RRR | Huber-White SE | p-value | RRR | Huber-White SE | p-value |
| Total Shared Sibling-Years | 0.99 | 0.003 | 0.002 | 0.99 | 0.006 | 0.028 |
| from younger siblings | 0.98 | 0.004 | 0.000 | 0.98 | 0.007 | 0.002 |
| from older siblings | 1.00 | 0.006 | 0.788 | 1.01 | 0.012 | 0.463 |
| Shared Male Sibling-Years | 0.99 | 0.004 | 0.249 | 0.99 | 0.008 | 0.098 |
| from younger siblings | 0.99 | 0.006 | 0.025 | 0.98 | 0.010 | 0.022 |
| from older siblings | 1.01 | 0.007 | 0.370 | 1.00 | 0.013 | 0.823 |
| Female Sibling-Years | 0.99 | 0.005 | 0.002 | 0.99 | 0.008 | 0.209 |
| from younger siblings | 0.98 | 0.006 | 0.002 | 0.98 | 0.011 | 0.030 |
| from older siblings | 0.99 | 0.007 | 0.213 | 1.01 | 0.014 | 0.565 |

| | Downward Mobility | | | | | |
|---|---|---|---|---|---|---|
| | Age at Exit>=30 | | | Age at Exit>=40 | | |
| | RRR | Huber-White SE | p-value | RRR | Huber-White SE | p-value |
| Shared Sibling-Years | 1.00 | 0.003 | 0.204 | 1.00 | 0.006 | 0.704 |
| from younger siblings | 0.99 | 0.004 | 0.184 | 1.00 | 0.007 | 0.997 |
| from older siblings | 1.00 | 0.006 | 0.722 | 0.99 | 0.012 | 0.482 |
| Shared Male Sibling-Years | 1.00 | 0.005 | 0.428 | 1.00 | 0.008 | 0.928 |
| from younger siblings | 0.99 | 0.006 | 0.362 | 1.00 | 0.010 | 0.879 |
| from older siblings | 1.00 | 0.007 | 0.886 | 1.00 | 0.013 | 0.711 |
| Shared Female Sibling-Years | 1.00 | 0.005 | 0.371 | 0.99 | 0.008 | 0.537 |
| from younger siblings | 0.99 | 0.006 | 0.336 | 1.00 | 0.011 | 0.877 |
| from older siblings | 1.00 | 0.008 | 0.786 | 0.98 | 0.014 | 0.227 |
| | | | | | | |
| Number of Individuals | 5552 | | | 1824 | | |
| | | | | | | |
| Wald Tests for Upward Mobility | | | | | | |
| | Chi2 | p-value | | Chi2 | p-value | |
| Female=Male SY | 0.53 | 0.467 | | 0.00 | 0.993 | |
| Young=Older SY | 4.39 | 0.036 | | 5.94 | 0.015 | |
| Female Young=Female Older | 1.20 | 0.273 | | 3.61 | 0.058 | |
| Male Young=Male Older | 4.71 | 0.030 | | 2.71 | 0.099 | |

*Note:* The reference category of the outcome variable is non-mobility. The relative risk ratio for upward mobility is interpreted as follows: a one unit increase in shared sibling-years would decrease the multinomial log-odds of upward mobility relative to non-mobility by 0.01 unit while holding all other variables in the model constant.

**Figure 3.**

Predicted probabilities of mobility by shared-person years.




*Note*: The predicted probabilities were generated holding all control variables at their means.