# Essays in Population and Family Economics

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ABSTRACT

This dissertation consists of three empirical essays in population and family economics. The first chapter studies ethnic peer effects in the intergenerational transmission of skills. In order to determine whether the correlation between individual measures of human capital and ethnic group averages in the previous generation is not driven by omitted variables and measurement error, I develop an instrumental variables strategy that uses within-group changes in the occupational mix of new immigrants to the US as a quasi-natural experiment, and exploits variation in parental age at arrival to account for the transmission of skills within the family. I find evidence of a significant 'ethnic capital' effect, which contributes notably to the persistence of skill differentials across individuals over time. The results also suggest that geographic concentration and endogamy rates accentuate the effect of ethnic capital by promoting a higher level of interaction among individuals in a given ethnic group.

The second chapter examines the negative relationship between fertility and education. Using information on compulsory attendance and child labor laws that affected women's schooling choices in their teenage years, I identify the effect of education on total completed fertility accounting for the endogeneity of schooling, and find that women with 3-4 additional years of schooling have on average one less child than they would have otherwise. Moreover, while there is evidence that education increases childlessness, this fertility-reducing effect of education does not appear to be mediated by a reduction in marriage rates. The results also imply that rising levels of education account for a sizable fraction of the recent fertility declines observed in several Western countries.

Finally, the third chapter evaluates the labor market effects of public subsidies to families with children. Using variation in the level of benefits provided by a policy reform in the UK that affected differentially what would otherwise be comparable groups of families, I estimate the effect of family allowances (also known as child benefits) on female labor force participation. The results show evidence of negative, yet insignificant and quantitatively negligible, effects of family allowances on female labor force participation.

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## Contents

# 1. Introduction

This dissertation consists of three empirical essays in population and family economics. The first chapter studies ethnic peer effects in the intergenerational transmission of skills. The human capital of individuals appears to be correlated with ethnic group averages in the previous generation, even after controlling for the direct effect of parental investment in the human capital of children. This observed association is often interpreted as evidence for ethnic peer effects, but it might be confounded by omitted variables and measurement error in parental skills. In order to determine whether, and to what extent, this relationship is caused by ethnic peer effects, I develop the following instrumental variables strategy: (1) the occupational mix of new immigrant arrivals during the Great Migration is used to instrument for ethnic capital, and (2) age at arrival is used to instrument for parental skills. Using 1910 and 1920 US Census data on first- and second-generation Americans, I find evidence of a significant ethnic capital effect, confirming that the persistence of skill differentials across individuals is partly attributable to their belonging to particular ethnic groups through a channel independent of their respective parents' skills. As expected, the results indicate that OLS estimates significantly *understate* the role of parental skills and slightly *overstate* the magnitude of ethnic peer effects, which is consistent with the motivation for using instrumental variables. Finally, a number of specification checks support the notion that geographic concentration and endogamy rates accentuate the effect of ethnic capital by promoting a higher level of interaction among individuals in a given ethnic group.

The second chapter examines the effect of schooling on fertility. Social scientists have long observed a strong negative relationship between education and the number of children per woman. The question of whether this correlation is causal remains open, however, since the choice of schooling is not random. In this chapter, I use 1950-1990 Census data, along with information on compulsory attendance and child labor laws that affected women's schooling choices in their teenage years, to estimate the effect of education on total completed fertility accounting for the endogeneity of schooling. Instrumental variable estimates using changes in state compulsory schooling laws as a source of exogenous variation in education suggest that women with 3-4 additional years of schooling have on average one less child than they would have otherwise. Furthermore, this fertility-reducing effect of schooling does not appear to be mediated by a reduction in marriage rates, while there is evidence that education does increase the probability that a woman will reach the end of her fertile lifetime without children. These results are robust to a number of specification checks, and imply that rising levels of education can account for a sizable fraction of the decline in fertility rates for several Western countries in the second half of the $20^{th}$ century.

Finally, the third chapter evaluates the labor market effects of public subsidies to families with children. The use of policies that reduce the cost of child care to the family is seen as a means to simultaneously encourage fertility and a strong attachment of women to the labor market. These programs include child care subsidies, which have been extensively studied in the literature, and family allowances, whose consequences in terms of labor market outcomes have been mostly ignored. Unlike child care subsidies, family allowances are a universal cash payment to parents based on the presence and number of children. In the absence of 'flypaper effects', these allowances amount to an increase in the household's unearned income, so they involve a pure positive income effect which might act as a disincentive to female labor force participation, as is shown using a simple model of market work, fertility and the home production of child care. I then use data from the 1998, 1999 and 2000 waves of the British Household Panel to analyze the impact of family allowances on female participation. Exogenous variation in the level of benefits is provided by a policy reform in the UK that affected differentially what would otherwise be comparable groups of families. Fixed-Effects 2SLS estimates show evidence of negative, yet insignificant and quantitatively negligible, effects of family allowances on female labor force participation.

# 1. Does 'Ethnic Capital' Matter? Identifying Peer Effects in the Intergenerational Transmission of Ethnic Differentials

## 1.1. Introduction

Differences in socioeconomic outcomes across ethnic groups tend to persist over time. The variation observed in one generation does not completely disappear in the next. A significant part of those disparities is transmitted across cohorts, thus slowing down the process of ethnic convergence that one could expect from simple mean reversion. Table 1.1 illustrates how much of the ethnic differences in education that existed among US immigrants in 1910 persisted into the second generation, for several national origin groups. For instance, Scottish male immigrants aged 30 to 50 were 35.6 percentage points more likely to be literate than Italian immigrants in 1910, and those in turn were 17.4 points more likely to be literate than Mexican immigrants. School enrollment rates of second-generation Scots aged 6 to 18 in 1910 were 8.2 percentage points above those of Italians, which were in turn 28.4 points above the attendance rate for Mexicans. Thirty years later, there existed substantial differences in educational attainment among second-generation adults in those same groups: average years of schooling in 1940 were 10.1 for Scottish-Americans, 8.7 for Italian-Americans and only 4.1 for Mexican-Americans. Their children also had noticeably different school attendance rates: a third-generation Scottish-American of schooling age was 6 percentage points more likely to be enrolled in school than a third-generation Italian-American child, who was, in turn, 12.1 points more likely to be attending school than a third generation Mexican-American.

The persistence in ethnic differentials over time could simply be the result of the transmission of skills that takes place within the family. Parents can influence the socioeconomic development of their offspring by investing time, effort, and financial resources in their children's human capital.[1] Other individuals in their ethnic group, however, can influence children as well. Friends, relatives, and neighbors can also serve as role models, spend time helping with school homework, and transmit certain attitudes towards education and work. Hence, being exposed to an advantageous ethnic

[1] While acknowledging that the genetic transmission of ability can also be an important channel, it is not the subject of interest in this paper to determine whether the passage of parental skills to children can mostly be attributed to nurture or not.

environment while growing up can also contribute to the children's human capital accumulation process, beyond the direct role of their parents. The existence of peer effects in the ethnic group will then exacerbate the extent to which the skill level in the immigrant generation determines the socioeconomic success of the next generation. That, in turn, will have implications for overall inequality in the economy.

Following the predominant terminology in the literature, as introduced in Borjas (1992), I will refer to these ethnic peer effects as the 'ethnic capital' effect in the intergenerational transmission of skills. 'Ethnic capital' denotes the average in the ethnic group of some measure of skills or socioeconomic performance –as opposed to 'parental capital', which designates the corresponding measure for a given individual's parents. While this ethnic spillover may operate primarily through geographic concentrations of peers in the same ethnic group, ethnic capital effects are not to be confused with neighborhood effects. Even within a neighborhood, children are more likely to befriend and interact with other individuals in the same ethnic group,[2] in which case the impact of peers of the same ethnicity will outweigh that of neighbors in other groups.

The main challenge in disentangling the two channels of intergenerational transmission of skills, and therefore estimating the parental and ethnic capital effects separately, is identification. Despite the potential importance of this question for immigration and welfare policy, most studies to date have relied primarily on ordinary least squares (OLS) regression strategies to study ethnic spillovers in the transmission of skills across generations. As I argue below, however, parental skills and average skill levels in the ethnic group may be correlated for a number of reasons, so an observed association between ethnic capital and child outcomes is not necessarily causal. To solve this problem, I use instrumental variables (IV) to estimate both parental and ethnic capital effects consistently.

My identification strategy exploits variations in the occupational mix of new immigrant flows over time and across ethnic groups, much in the same way as Angrist (2002) did for the effect of changing sex ratios. The instruments for average ethnic skills are derived from official records of immigrant arrivals by year and national origin, further

[2] Alba (1992) showed this for second-, third- and higher generation children in several Caucasian European-American ethnic groups.

classified into broad occupation groups. Occupational mix of new immigrants is assumed to be exogenous to local economic conditions in the US, and therefore solves the endogeneity problem caused by ethnicity-year specific shocks encouraging further skill accumulation. The inclusion of ethnicity and year main effects and of individual characteristics as control variables ensures that my results are robust to group-specific characteristics that might be correlated with transferable skills of fathers and with the occupational mix of new immigrants, such as tastes for work or education. This strategy would fail, however, in the presence of time-ethnicity specific shocks that affected both transferable skills of fathers and occupational mix of recent arrivals *and* that were not fully captured by the covariates in my regressions. Such a situation appears unlikely, though, particularly since the results appear robust to the inclusion of additional control variables and to several other specification checks, thus weighing in in favor of a causal interpretation.

I also instrument for a second key endogenous regressor, parental skills (measured by father's literacy), with father's age at arrival interacted with a dummy variable for non-English speaking country of birth, as in Bleakley and Chin (2003). The inclusion of a father's age-at-arrival main effect controls for additional (non-literacy related) unobserved dimensions of skills that may be transmitted from parents to children.

In order to clarify this idea, consider, for example, the children of Italian-American immigrants in the US at the beginning of the 20$^{th}$ century. My strategy relates changes in school attendance rates of second-generation Italian-American children between 1910 and 1920 to changes in the fraction of recently arrived Italian immigrants who were recorded as having low-skilled occupations (agricultural workers, laborers and servants). The 'experiment' behind this approach consists, then, in observing how distinct communities of immigrants will be affected by the arrival of newcomers with a different level of skills. In fact, there is anecdotal evidence of incumbent immigrant groups at various points in time being alarmed by the arrival of what they perceived to be 'lower quality' immigrants in their ethnic groups.[3] The existence of ethnic capital effects would

[3] For instance, Thomas Sowell (1981, pp. 107-108) notes that "the relationship between the earlier arriving members of a group and those arriving later is an important factor in the history of most American ethnic groups. (...) The earlier Italian immigrants had gained a measure of acceptance and prosperity by the time the massive waves of southern Italians arrived. (...) The northern Italians openly repudiated the southern

provide some basis for the fear that new low-skilled waves of immigrants could dilute the skills of the community and have a negative impact on the next generation as well.

This strategy constitutes a good natural experiment because, as I will argue below, the resulting variation in the average skills by ethnicity was driven mainly by home country conditions in the early twentieth century (most notably World War I), which were exogenous to local US market conditions facing the existing immigrants and their children. Moreover, social interactions among individuals within each of the ethnic groups used in this analysis were indeed more important than with individuals outside the group, as evidenced by the high intra-group marriage rates that will be presented below. Finally, the immigrants (and immigrant flows) studied in this analysis constitute a major demographic episode in American history, with aliens arriving in numbers that went unmatched for almost a century.[4]

An additional contribution of this paper is the use of measures of ethnic capital that are contemporaneous with child outcomes (as opposed to using skills of immigrants in a previous period), to better reflect the actual environment facing children and reduce the potential bias from return migration in the measure of average skills in the ethnic group. Another improvement is the use of repeated cross sections, which allows me to control for ethnicity and year main effects.

The variables of interest in this research are human capital outcomes such as a proxy for literacy in English for adults and school attendance for children. Both are relevant education measures in the period being studied. Using micro data from the 1910 and 1920 US Censuses, I find evidence of significant ethnic capital effects in the intergenerational transmission of skills. The IV estimates are slightly, though not

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Italians. Many even preferred to pass for Americans." Irving Howe (1976, p229) remarks that "by the turn of the [20th] century, the tensions between the established German Jews and the insecure East European Jews had become severe –indeed, rather nasty. (…) The Germans found it hard to understand what could better serve their ill-mannered cousins than rapid lessons in civics, English, and the uses of soap." In both cases, however, the newcomers did interact with the existing communities, as evidenced by the high marriage rates within each group. Common culture, language or history could help explain why, for example, "German Jews established and financed schools, libraries, hospitals, and community centers to aid, and especially to Americanize, the eastern European Jewish immigrants." (Sowell, p.81).

[4] Borjas (1994) refers to the huge flow of immigrants between 1880 and 1924 as the *First Great Migration*, to distinguish it from the *Second Great Migration* that took place in the last twenty years of the twentieth century: the number of immigrants admitted to the United States in the decade 1901-1910 is recorded at 8.8 million (Ferenczi and Willcox (1929)), which was only exceeded nine decades later (more than 9 million legal immigrants are estimated to have arrived between 1991 and 2000), when the population of the US was much larger.

significantly, lower than the OLS estimates, which are subject to omitted variables bias and attenuation bias. IV estimates of the direct parental effect are much higher than the OLS estimates, suggesting severe measurement error in father's skills (the literacy variable). The results also suggest that ethnic spillovers are stronger where the geographic concentration of immigrants is highest. This result is consistent with ethnic peer effects that operate, at least in part, through neighborhood effects. Finally, regressions that take into account differences in endogamy rates by region also indicate that peer effects are larger for more endogamous communities, while insignificant for ethnic groups in regions where endogamy is very low.

The rest of the paper is organized as follows. Section II presents a theoretical model of ethnic peer effects, develops the estimation framework, and highlights the econometric issues involved in attempting to disentangle parental from ethnic peer effects in the intergenerational transmission of skills. Section III describes the data and presents the base empirical results. Section IV discusses some robustness checks and additional results. Section V summarizes the paper and concludes.

## 1.2. Background

### *1.2.1.. Theoretical Framework*

The idea that ethnic skills are transmitted across generations can be rationalized by Borjas' (1992) 'ethnic capital' model, a theory of human capital externalities. Similar ideas appear in the sociology literature on 'social capital': Loury (1977) first introduced this term to explain how race differences in earnings persist over time due to spillover effects within a racial group; Coleman (1988) later developed that concept and applied it to the study of peer effects in the academic performance of high school students.[5]

In this framework, utility-maximizing parents invest in the human capital of their children, while ethnic human capital has an external effect on the production of children's human capital.[6] This last assumption is meant to capture the influence that other adults in the ethnic group outside the immediate family have on the education of the next

[5] More recently, Putnam (1995) introduced the notion of 'social capital' in the political discussion of the decreasing participation in civic organizations in the US.

[6] In Loury's terms, the opportunities of young people to acquire skills depend both on "the quality of home environment" as well as "the quality of the community environment." (Loury, 1977, p.159).

generation. Other things equal, interaction with peers and exposure to the cultural norms and values (and to examples of rewarding work and achievement) that are characteristic of a particular ethnic group affect a child's human capital accumulation process. A more recent application of this idea can be found in Lundberg and Startz (1998), who use a similar model to explain the persistence of the racial wage gap.

Denote parents' human capital stock by $h_t$, a child's human capital stock by $h_{t+1}$, and the parents' own consumption by $C_t$. The household maximizes its welfare,

$$U = U(h_{t+1}, C_t) = \left[\delta_1 h_{t+1}^{\rho} + \delta_2 C_t^{\rho}\right]^{1/\rho}, \tag{1-1}$$

subject to a budget constraint,

$$C_t \leq R(1 - s_t)h_t, \tag{1-2}$$

and the production function for children's human capital is,

$$h_{t+1} = \theta h_t^{\beta_1} \, \overline{h}_t^{\beta_2}, \tag{1-3}$$

where $s_t$ is the fraction of $h_t$ devoted to the production of $h_{t+1}$, $\overline{h}_t$ is the average human capital in the parents' peer group, and $R$ is the market price of human capital stock relative to consumption goods. The model is, therefore, characterized by dynamic externalities, in the sense that the human capital of one generation contributes to the production of the next generation's human capital. The model is solved by a supply function of time allocated to investing in children's human capital: $s_t = f\left(h_t, \overline{h}_{t+1}\right)$. I take a logarithmic transformation to obtain:[7]

$$\log h_{t+1} = \alpha + \beta_1 \log h_t + \beta_2 \log \overline{h}_t \tag{1-4}$$

The parameter $\beta_2$, the coefficient on average human capital, represents a peer group effect. This concept has been an object of considerable interest among economists: Benabou (1993) analyzes how residential segregation concentrates low-skilled learners in schools, which affects the learning process and results in persistent and widening income inequality. Bertrand, Luttmer and Mullainathan (2000) investigate network effects in welfare participation; Sacerdote (2001), Zimmerman (2003), and Winston and Zimmerman (2003) study peer effects in academic outcomes among college roommates;

[7] It can also be shown that $\partial \log \overline{h}_{t+1} / \partial \log \overline{h}_t < 1$ iff $\beta_1 + \beta_2 < 1$ (condition for convergence of ethnic skill differentials).

while Hoxby (2000), and Angrist and Lang (2002) estimate peer effects in the classroom. They do not always, however, provide convincing evidence.[8]

In practice, of course, child outcomes are determined by many other factors beside parental skills and peer effects. I therefore add a stochastic error term to (4), as well as a vector of individual covariates $z_i$ that includes region effects, age, father's age, and other demographic variables. Also, I adopt notation that reflects that (1) different individuals belong to different ethnic groups and that the externality will take place at the ethnic peer group level, and (2) individuals in my data are observed in different years. The resulting equation then is:

$$y_{ijt} = \alpha + \beta_1 x_{ijt} + \beta_2 \bar{x}_{jt} + \delta_j + \delta_t + z_{it}^{'}\gamma + \varepsilon_{ijt}, \qquad (1\text{-}5)$$

where $y_{ijt}$ is an observable socio-economic outcome of child $i$ in ethnic group $j$ at time $t$ (such as school enrollment), $x_{ijt}$ is a measure of skills of the father (of child $i$ in ethnicity $j$ at time $t$), $\bar{x}_{jt}$ is the average skills of individuals in the father's generation in ethnic group $j$ at time $t$, $\delta_j$ and $\delta_t$ are ethnicity and Census year effects, respectively, and $\varepsilon_{ijt}$ is an individual error component.[9]

*1.2.2. Econometric Framework*

The most important identification problem raised by equation (1-5) is omitted variable bias from correlation between average skills in the ethnic group, $\bar{x}_{jt}$, and other ethnicity-year effects contained in the residual term $\varepsilon_{ijt}$. For example, if different ethnic groups are not distributed proportionally across occupations, industries or geographic areas, then an economic shock that increases opportunities relatively more for a given ethnic group at some point in time will encourage accumulation of skills for adults in that group, while at the same time increase the schooling of their children. Moreover, $\bar{x}_{jt}$ is

[8] To borrow Manski's (1993) terminology, the intergenerational transmission parameter $\beta_2$ in equation (1-4) expresses an "contextual or exogenous effect," as opposed to an "endogenous social effect," which is the case of the peer effects studied by Zimmerman, Sacerdote, or Hoxby.

[9] When parental level of skills is not observed, it is possible to aggregate (5) and write:

$$y_{ijt} = (\beta_1 + \beta_2)\bar{x}_{tj} + z_{ti}^{'}\gamma + v_{ijt} + \varepsilon_{ijt},$$

in which case it is only possible to recover ($\beta_1 + \beta_2$), an 'intergenerational correlation coefficient', but not the ethnic peer effect $\beta_2$ separately from the parental effect $\beta_1$.

subject to measurement error and is affected by economically motivated return migration, as well as by immigrant arrivals.

In order to solve these problems, the fraction of new immigrants of 'low skills' to which different ethnic groups in different years were exposed is used to construct an instrument for $\bar{x}_{jt}$. Recent flows of 'low-skilled' immigrants are correlated with the average skills in the ethnic group because those new immigrants (arrived in the 5 years prior to the Census year) are least likely to have returned to their home countries, and hence are not subject to economically motivated return migration that could bias the estimates.

A related consideration is that ethnic variation in the immigrant flow to the United States during this period was mainly driven by home country conditions (political instability, persecution), and hence the skill composition of that flow is unlikely to be the response to local economic conditions in the US. For example, the ethnic and skill mix of immigrants in 1920 were driven in large part by World War I, which made departure from combatant countries more difficult, particularly for individuals in low-wage occupations. Then, changes in the fraction of low-skilled immigrants arrived in 1915-1919 relative to that fraction in 1905-1909 constitute a largely exogenous source of variation in the difference between the average skills of adults in each ethnic group in 1920 relative to 1910. This is similar to the reasoning behind Angrist's (2002) study of the effects of sex ratios on marriage rates and labor market outcomes.[10]

Although omitted variables bias is the main motivation for my IV strategy, it is important to note that the OLS estimates of equation (1-5) may also be confounded by the fact that one regressor, $\bar{x}_{jt}$, is in fact close to being an average of another regressor, $x_{ij}$. In other words, the parents of children in my data are among the adults used to compute the average measure of skills in that ethnic group.

Suppose initially that $\bar{x}_{jt}$ was exactly the ethnic group mean of $x_{ij}$, then OLS estimates of the coefficient on $\bar{x}_{jt}$ in equation (1-5) would be equivalent to the augmented regression form of a Hausman (1978) specification test for the difference

[10] The instrument in Angrist (2002) was constructed from the sex mix, not the occupation mix, of recorded immigrant flows by ethnicity and year.

between OLS and IV estimates of the coefficient on $x_{ij}$ in a simple regression of $y_{ij}$ on $x_{ij}$ only, with ethnicity dummies serving as the instrument for $x_{ij}$.[11] If OLS estimates differ from the IV estimates in the bivariate regression for any reason (e.g., measurement error in $x_{ij}$), then the estimated OLS coefficient on $\bar{x}_{jt}$ in equation (1-5) would be nonzero (positive, in the errors in variables case) even in the absence of ethnic peer effects. This problem is common to a broad class of empirical exercises where an outcome variable is affected by both an average and an individual level variable, and appears in the estimation of human capital externalities (Acemoglu and Angrist (2000)).

The situation here is somewhat more complicated since $\bar{x}_{jt}$ is not the exact average of $x_{ij}$ in the sample.[12] Nevertheless, I show in Appendix A that under similar circumstances the estimate of $\beta_2$ will be non-zero even if the actual ethnic capital effect is zero. This problem is solved by treating both $x_{ij}$ and $\bar{x}_{jt}$ as endogenous in (5).

To construct instruments for $x_{ij}$, I use father's age at arrival, interacted with a non-English speaking country of birth dummy, as an instrument for $x_{ij}$. Proficiency in the dominant language of the receiving country is a particularly important component of an immigrant's work-related human capital. Because languages are easier to learn at an earlier age, an immigrant who arrived as a child from a non-English speaking country should have developed better English-language skills than one who arrived as an adult. In several studies of immigrants' language skills and earnings in Australia, Canada, Israel and the US, Chiswick and Miller (1992, 1995) and Miller and Chiswick (2002) report that, holding observable characteristics constant, language proficiency increases with years in the receiving country and is lower when immigrants have migrated at older ages. Research on cognitive science has established that the age of acquisition of a first or second language is a major determinant of ultimate proficiency (Newport (1990), Flege, Munro and MacKay (1995)).[13]

[11] See Davidson and MacKinnon (1989) for a detailed derivation of the equivalence between the Hausman specification test and its augmented (or 'artificial') regression form.
[12] The average $\bar{x}_{jt}$ also includes the foreign born who do not have children, as well as all second generation adults of working age,
[13] This is usually linked to the fact that puberty is associated with a biological reduction in the plasticity of the neural circuits that determine language learning ability (Lenneberg (1967), Flege, Yeni-Komshian and Liu (1999)).

Since immigrants originating in English-speaking countries do not face a new language upon arrival to the US, these effectively serve as a control. With my strategy, only differences in outcomes between, say, two children of the same age whose respective fathers immigrated from Germany at different ages, net of differences in outcomes for comparable children whose fathers arrived from England at parallel ages, are attributed to parental capital. A similar strategy was used in Bleakley and Chin (2003) to study the returns to English proficiency for US immigrants.

*1.2.3. Previous Research on Ethnic Peer Effects*

Most empirical research on ethnic peer effects to date looks at the intergenerational transmission of socioeconomic outcomes such as schooling and earnings. One of the earliest empirical studies is Borjas' (1992) analysis of 1970s and 1980s General Social Surveys and National Longitudinal Surveys of Youth data. He regressed education and log wages of second-generation individuals on the education of their fathers, and on the average of parents in the ethnic group, and found that ethnic capital plays as important a role as the father's skills in determining the human capital of the next generation. In essence, he estimated an equation similar to (5) and interpreted the OLS coefficients as the causal effects of parental and ethnic capital. Borjas (1995) improves upon the previous study in addressing the potential problems introduced by measurement error in $x_{ij}$, by using sibling's reports of parental skills as instruments, but still treats the level of skills in the ethnic group as exogenous and is therefore subject to omitted variable bias, as described in the previous section.[14]

A related set of papers seeks to estimate the intergenerational transmission parameter describing how the mean skills of the ethnic group change over time. Borjas (1993), using 1940 and 1970 Census data, Borjas (1994), using 1910, 1940 and 1980 Census data, and Card, DiNardo and Estes (2000), using the 1940 and 1970 Censuses along with the 1994-96 CPS, all found intergenerational correlations of education and earnings in the range of 0.4 to 0.6. While this is an interesting question in and of itself,

[14] Borjas also uses sibling's reported ethnicity as an instrument for ethnic capital. While that strategy corrects for measurement error in the assignment of the individual to an ethnic background, it does not address the potential omitted variables problem (from return migration or ethnicity-specific shocks, as explained above) that contaminates the measured level of ethnic capital in each group.

the exclusion of father's skills in the regressions makes it impossible to disentangle the ethnic peer effect from the direct transmission of skills within the family.

One additional caveat that applies to most of these studies is the fact that the measure of ethnic capital is generally constructed as the average skills in the parents generation thirty years prior (when many of the individuals observed in the next generation had not even been born yet). The resulting estimates may be especially prone to bias from measurement error and economically motivated return migration. My strategy uses characteristics of the ethnic group actually faced by the children of immigrants when growing up.

## 1.3. Data and Main Results

### *1.3.1. Data Sources*

The data used here comes from the 1910 and 1920 Census IPUMS files (documented in Ruggles and Sobek, 1997). Because information on the skills of parents is only available for the subsample of persons who still reside with their parents, which is unlikely to be a representative subsample of adults, I restrict my analysis to an extract of second-generation children of schooling age (6 to 18 years old) and their parents. For the construction of measures of average skills by ethnic group, I use an extract of foreign-born and second-generation adults of working age (19 to 65 years old). The 1910 and 1920 Censuses contain detailed information on the age at arrival of immigrants, essential for the construction of one of my instruments, and on father's and mother's country of birth and mother tongue. The latter are used to classify ethnic groups in a manner similar to that used in administrative data (Ferneczi and Willcox, (1929)) used to form the instruments. This results in twenty-six groups, plus an additional not elsewhere classified group. (See Appendix B for details on the coding scheme).

The outcome variable of interest is school attendance, the only education variable available for children but also perhaps the most relevant schooling measure in the early $20^{th}$ century.[15] Skills of parents and average skills in the ethnic group are measured using a proxy for literacy in English. Literacy in English is represented by a dummy variable

[15] Those are the years before the 'high school movement' had made attendance to secondary schooling more widespread (Goldin (1998)).

that equals one if an individual indicates he or she can read and write in some language *and* he or she can speak English. This way I aim to capture an informative dimension of human capital that is presumably valued in the labor market. As argued in the previous section, language proficiency is an important component of immigrants' skills as valued in the US labor market. The use of English literacy (henceforth referred to simply as literacy) to define ethnic capital facilitates the use of age at entry as an instrument for parental capital.

Table 1.2 gives descriptive statistics for the extract. While the distribution across regions and the average age of children and fathers remains fairly stable between 1910 and 1920, the fraction of second generation children attending school increases by about 3 percentage points, while the average literacy rate of immigrant fathers decreases slightly by almost 2 points. On the other hand, the average literacy of all first and second generation working age adults (my measure of ethnic capital) is higher in 1920 than in 1910, likely as a result of the higher average age (one and a half years older on average), the progressive accumulation of skills by previous immigrants and the higher proportion of second generation individuals over time. Finally, the high (relative to its time) fraction living in a metropolitan area reflects the fact that immigrants are disproportionately more likely to settle in urban areas than natives.

The existence of ethnic peer effects is the result of exposure to other individuals in the group who act as role models and have an influence on the skill acquisition of children. One way to measure the degree of interaction among individuals in a given community is by looking at endogamy rates. I use information on the nativity of spouses of married first and second generation women in order to compute the probability of marriage to an individual from the same (first or second generation) ethnic group, conditional on being married. The importance of intra-ethnic marriage in the groups defined in my sample is documented in Table 1.3, which reports the distribution of husbands' ethnicity separately for foreign born and second generation women. Endogamy rates are high for almost all groups even in the second generation, which suggests a strong level of individual interaction within groups. Over 80 percent of Italian women in the second generation married in the same group, and that percentage is even higher for Jewish and Japanese daughters of immigrants. In English-speaking groups such as

English/Welsh or Irish, these rates are lower, yet only half of English, and only a third of Irish women of second generation have a native husband.[16] Table 1.3 therefore supports the ethnic taxonomy used in this analysis.

The ethnicity and skill distribution of the foreign stock (first and second generation individuals) are described in Table 1.4. There is a good deal of heterogeneity across ethnic groups and over time both in adult literacy and in children's school enrollment rates. This variability is more clearly reflected in Figure 1, which plots school attendance of second generation children against the average literacy rate of first and second generation adults for all 54 ethnicity-year cells. The figure shows that higher average literacy rates are associated with higher school enrollment rates for children. Next I will turn to regression analysis in order to control for individual characteristics as well as ethnicity and year effects, and then to use instrumental variables to identify what part of this observed relationship is caused by ethnic peer effects.

### *1.3.2. OLS Estimates*

The estimating equation for second-generation individual $i$, in ethnic group $j$, observed in Census year $t$ is (5), derived in the previous section. The first stage equations relate the endogenous regressors to the instruments $a_{ijt}$, father's age at arrival interacted with a dummy for non-English speaking country of birth, and $f_{jt}$, the fraction of 'low-skilled' immigrants (laborers, servants and agricultural workers) arrived in the five years prior to the Census year:

$$x_{ijt} = \alpha_1 + \rho_{11} a_{ijt} + \rho_{12} f_{jt} + \delta_{1j} + \delta_{1t} + z_i^{'} \gamma_1 + \varepsilon_{1ijt} \text{ , and} \tag{1-6}$$

$$\bar{x}_{jt} = \alpha_2 + \rho_{21} a_{ijt} + \rho_{22} f_{jt} + \delta_{2j} + \delta_{2t} + z_i^{'} \gamma_2 + \varepsilon_{2ijt} \text{ .} \tag{1-7}$$

This system is just-identified. The covariates $z_i$ include region effects, age, father's age, and other demographics. Note that I also include a father's age-at-arrival main effect in the equation of interest. Even though the immigration decision of the father is previous to the birth of the child in my sample, and therefore could be thought of as exogenous to children's outcomes, the timing of the father's arrival to the US may be correlated with

[16] Moreover, breaking down the 'married other group' column would show that, in almost all cases, the endogamy rate for second generation women is still above the fraction of women who married in any other single group.

unobserved parental characteristics such as ambition and drive, which may then be transmitted to the next generation. I allow father's age at arrival to enter the equation and directly affect schooling of children.

Table 1.5 reports OLS estimates of equation (1-5). These suggest that parents' literacy has a modest but precisely measured effect on school attendance of children, while the average literacy in the ethnic group has a relatively large and significant impact.[17] While region of residence and metropolitan area do not appear to affect the estimates for parental capital, the ethnic peer effect declines notably (from 0.215 in column (1) to 0.135 in column(4)) after including other controls such as age, father's age, number of siblings, and father's age at arrival. According to these results, two comparable children who only differ in the literacy of their fathers are predicted to have a difference in the probability of attending school of about 5 percentage points. Two observationally equivalent children with equally skilled parents but belonging to ethnic groups that differ in their literacy rates by 30 percentage points are predicted to differ in their respective probabilities of school attendance by just over 4 percentage points.

Column 5 experiments with using an average of father's and mother's literacy, to account for the role of both parents in the transmission of skills.[18] The results are comparable to those in the previous columns: even though ethnic spillovers are estimated to be slightly lower, they still amount to twice the parental effect.

The estimates in this table are not readily comparable to other estimates in the literature. They are most relatable to Sacerdote (2003), who in his analysis of the transmission of human capital between former slaves and their children and grandchildren reports that having a mother who was born a slave decreases the probability of being enrolled in school by 12 percent, and to Weir (2000), who reports positive effects of parents' years of schooling on school enrollment of children. I am not aware, however, of any studies of the intergenerational correlation between immigrant parents and second-generation children that have looked at school enrollment as an outcome variable.

[17] Standard errors in all regressions are corrected for ethnicity-year clustering.

[18] The literature usually finds similar results when child outcomes are correlated with mother's characteristics (Card, DiNardo and Estes (2000)).

As noted in the previous section, OLS estimates of ethnic capital effects are subject to upward bias from measurement error in father's skills. In that case, not only does the measurement error attenuate the coefficient on parental capital, but it can also create a false impression of positive ethnic peer effects. To illustrate the implications of an inconsistent estimate of parental effects for the identification of ethnic effects, I estimated equation (1-5) imposing different plausible values for $\beta_1$. As reported in columns 2 and 3 of Table 1.6, the estimated peer effect is 0.144 when father's literacy is excluded from the equation, but falls to 0.106 when the parental effect is set to 0.20. On the other hand, changing the constrained value of the ethnic spillover does not have much impact on the estimated parental effect. These results support the notion that measurement error in parental skills can bias the estimation of ethnic peer effects, and therefore it is fundamental to estimate $\beta_1$ consistently in order to identify $\beta_2$.

The first-stage estimates for father's literacy rate (from estimating equation 6) are displayed in Table 1.7. There is a strong, negative relationship between the instrument $a_{ijt}$ and parental skills. Regardless of the controls used, the estimate implies that delaying arrival from a non-English speaking country to the US by three years leads to a two percentage point decline in the probability of speaking English and being literate.[19] Table 1.8 reports a set of first-stage estimates for average literacy. Even though the instrument is later used in a micro regression on tens of thousands of observations, it is insightful to estimate equation (1-7) at the aggregate level, controlling for ethnicity and year main effects only, given that both the endogenous regressor ($\overline{x}_{jt}$) and the instrument ($f_{jt}$) do not vary within ethnicity-year cells. Column 1 shows that the fraction of low-skilled recent arrivals does have a large, significantly negative effect on average literacy rates even at the macro level, on only 54 observations corresponding to the ethnicity-year cells. The point estimate reveals that a 10 percentage point rise in the fraction of new immigrants with low skills in a given ethnic group leads to a 6 percentage point decline in average literacy rates in that group. This negative relationship is illustrated in Figure 2, which

[19] These results are not directly comparable to those in Bleakley and Chin (2003). The English proficiency variable in the 1990 Census, which they use in their estimations, is coded into four different categories, whereas my measure of skills is a binary variable. It is also worth pointing out that I experimented with the non-linear function of age at arrival that Bleakley and Chin use in their definition of the instrument $a_{ijt}$, and obtained very similar results. The non-linearity likely becomes important only in distinguishing between subtle differences in language proficiency, but does not matter in predicting my binary skill indicator.

plots literacy rates and fractions of low-skilled recent immigrants, net of ethnicity and year. Columns 2 through 5 confirm that the estimate is robust to the inclusion of controls at the micro level. Interestingly, neither $a_{ijt}$ comes in significantly in equation (1-6) nor does $f_{jt}$ in equation (1-7), confirming that each instrument is a strong predictor only of one endogenous variable, along the lines of the discussion on the identification strategy outlined above.

*1.3.3. IV Estimates*

The 2SLS estimates of equation (1-5) are reported in Table 1.9. The coefficient on father's literacy after adding all the controls, as shown in column 3, is clearly higher than its OLS counterpart. The results indicate that, other things equal, having a literate father increases the probability that a child is enrolled in school by 20 percentage points. The OLS estimate from Table 1.5 appears to be downward biased, which is consistent with measurement error in the measure of parental skills. At the same time, models that treat the average skills in the ethnic group as endogenous generate a 2SLS estimate of 0.116 for the effect of ethnic capital on the probability that children are in school. It appears that most of the positive association observed in Table 1.5 and Figure 2 is indeed causal. The point estimates for ethnic spillovers are, however, slightly lower than the OLS ones, which is coherent with the omitted variables bias story whereby some ethnic groups experience positive shocks that encourage further skill accumulation and result in both higher literacy levels of adults and higher school enrollment rates of children. This difference between the OLS and 2SLS estimates of ethnic peer effects is, nevertheless, not significant. Using mean skills of both parents yields slightly lower but less precise estimates (due mostly to reduced sample size). In any case, the pattern of the estimates relative to their OLS counterparts is in line with that of all other columns, reinforcing the idea that measurement error in parental skills is a severe problem.

Table 1.10 performs the same experiment as in Table 1.6, now using instrumental variables to estimate the unconstrained coefficient. As before, the coefficient on ethnic capital shrinks when the parental effect is larger. When the latter is set to 0.20, approximately the 2SLS result from the previous table, the estimated ethnic spillover becomes equal to the unconstrained 2SLS result. This provides further proof that consistent estimation of one endogenous regressor is key to the correct identification of the other.

*1.3.4. Additional Results and Specification Checks*

I turn now to addressing the concern that my results are affected by the multi-dimensional nature of human capital. Suppose that the skill that is transmitted from one generation to the next is not a single factor, but instead comprises two different components, $x_1$ and $x_2$: $x_{ijt} = x_{1ijt} + x_{2ijt}$. Only one component, $x_1$ (literacy), is observable. In that case, the estimating equation (1-5) becomes:

$$y_{ijt} = \alpha + \beta_1 x_{1ijt} + \beta_2 \bar{x}_{1jt} + \delta_j + \delta_t + z_i' \gamma + \left(\varepsilon_{ijt} + \beta_1 x_{2ijt} + \beta_2 \bar{x}_{2jt}\right). \qquad (1\text{-}8)$$

Since only $x_1$ can be included in the regression, the residual contains $x_2$ and $\bar{x}_2$[20]. Given that $f_{jt}$, my instrument for (observed) ethnic capital is based on the occupational mix of new immigrants, it may be picking up some unobservable dimensions of skills that are not included in literacy. In that case, $f$ is correlated with $\bar{x}_2$ and hence with the residual, thus yielding inconsistent estimates. Including father's occupation in the regression, however, should control for that additional component of transferable skills not contained in the observed $x_1$. The first two columns in Table 1.11 report OLS and 2SLS models that include a dummy variable that equals one if the father is a laborer, a servant or an agricultural worker (the same criterion used to construct the instrument $f$). If unobserved skills correlated with $f$ rendered my instrumental variable strategy invalid, then these estimates should be different from those that do not include a proxy for unobserved skills. There is, however, no evidence that the inclusion of father's occupation alters the estimates in any way. These results therefore strengthen the case for interpreting the 2SLS estimate as the causal effect of ethnic capital, as measured by average literacy, on school attendance of children.

Finally, columns 3 and 4 in Table 1.11 deal with the possibility that imprecise estimation of averages for small ethnic groups may be biasing my estimates. For that purpose, I re-estimate equation (1-5) after excluding observations belonging to the following groups: African, Spanish, Romanian, Armenian and Ruthenian (the five

[20] To the extent that different components of skills may be correlated, that alone creates an additional source of bias for OLS estimates of equation (1-9).

smallest ethnic groups in my sample, as evidenced by the counts in Table 1.3). Again, my findings also survive this robustness check.

## 1.4. Ethnic Capital Effects and Measures of Ethnic Concentration and Interaction

The ethnic peer effects hypothesis has a number of implications that can be checked. First, ethnicity is likely to play a more important role among individuals who grow up in an environment with a higher concentration of people in their ethnic group. In regions where one's ethnic group only represents a very small fraction of the population, children will probably be exposed to, and influenced by, less frequent social and cultural intragroup contacts. The analysis in the preceding section ignored this because it assumed that the coefficient on ethnic capital was the same across individuals. In order to explore whether ethnic clustering affects the magnitude of ethnic spillovers, I interact average skills with a measure of concentration in the ethnic group. For each child in my sample, I compute the proportion of working age adults in the region who share the same ethnic background. A dummy variable indicating whether that fraction is above or below the average across observations is interacted with both the parental capital variable and the ethnic capital variable.[21] Admittedly, a sharper exercise would compare individuals in highly segregated neighborhoods against those in more homogeneous districts. The 1910 and 1920 Census data, however, does not include such detail of information on place of residence, so I use Census region instead.[22]

The findings are summarized in Table 1.12. Column 1 reproduces the baseline OLS estimates from column 4 in Table 1.5. Column 2 shows that the ethnic peer effect is larger among persons who live in highly concentrated areas (0.261 versus 0.122 for children in low concentration regions), even though the standard errors are too high to claim the difference is statistically significant. The loss in precision occurs because not

[21] The average fraction of the working age adult population in the same ethnic group as the child in my sample is just under 12%. I therefore define my dummy variable for 'high' ('low') concentration as being in a region with more (less) than 12% of adults in the same ethnic group. In order to compute that fraction, I look at both first and second generation adults aged 19 to 65 (which are the most likely to interact with the parents of the child).

[22] I do not use state of residence, because the number of first and second generation adults of working age by state in 1910 is too small and introduces too much sampling error in the measures of concentration.

all ethnic groups are represented in both high and low concentration regions, and hence estimation of each of the parental and capital effects no longer uses all ethnicity-year cells. The last two columns repeat the same exercise for 2SLS. While column 3 shows the benchmark 2SLS estimates from column 3 in Table 1.9, Column 4 reports the coefficients separately for high and low concentration areas. Again, despite the loss in precision, the coefficient on ethnic capital is higher where concentration is greater. These results are suggestive that ethnic environment has a stronger impact on children in areas where ethnic groups are more concentrated.

Another check on the peer effects story looks at differences in the magnitude of the coefficient on ethnic capital as a function of a different measure of social interactions within groups. As has been argued in Section II, endogamy rates provide a good measure of the extent to which individuals in an ethnic group are in close contact to other people in the group as opposed to people in other groups. Communities where most women marry within their ethnic group are typically more cohesive and closed to outside influences. On the other hand, children in those communities where a large proportion of women marry outside their ethnic group are more likely to interact with neighbors or relatives of different ethnicities, and should be less frequently exposed to the particular role models and values associated with their own ethnic group. If that assumption is correct, ethnic peer effects in more endogamous communities must be stronger than in less endogamous groups.

To determine whether ethnic peer effects are associated with high endogamy rates, I allow the coefficient on ethnic capital to vary according to the fraction of married second-generation women in the region who wedded in the same ethnic group. I use second generation women because endogamy rates for the first generation might simply reflect the fact that many immigrants married before arrival to the US, whereas the marriage decisions of their US-born children provide a more accurate measure of the actual level of interaction among members of the same ethnic group.[23]

Table 1.13 reports regressions where father's literacy and average literacy in the ethnic group are interacted with dummy variables indicating whether the endogamy rate in the region was above or below 55 percent, which is roughly the average second-

[23] Endogamy rates for second-generation women were presented in column 5 of Table 3.

generation endogamy rate in the sample. OLS estimates in column (2) indicate that ethnic capital externalities are larger in highly endogamous ethnic groups. This is further confirmed by the 2SLS estimates in column (4). The estimated ethnic peer effect is insignificant and very close to zero for those in low endogamy communities, and 0.140 for those in high endogamy groups.

Aside from providing further support to my ethnic group classification, these results imply that ethnic spillovers operate mainly through the strength of the ethnic social fabric, as measured by endogamy rates. There is evidence that as cultural and socioeconomic assimilation takes place, cross-ethnicity marriage rates increase and endogamy rates decline (Spickard (1989)). Those communities with both few endogamous unions and low ethnic spillovers are thus likely to be more integrated in the US. In such groups, then, exposure to ethnic role models and behavioral norms becomes more infrequent, and the importance of ethnic capital in the transmission of skills across generations diminishes.

## 1.5. Conclusions

Previous attempts to identify the link between average skills of immigrants and the socioeconomic outcomes of their children have paid little attention to problems of omitted variables bias and measurement error. My research underscores the potential importance of endogenous ethnic and parental skills in intergenerational skill transmission equations and of their sensitivity to the estimation procedure used in the analysis.

Estimates using an exogenous source of variation in skills among immigrant groups, while simultaneously instrumenting for the skills of parents to reduce attenuation bias, provide strong evidence for the existence of ethnic capital effects, albeit not of a stronger magnitude than the direct effect that parents have on their children. Moreover, a number of specification checks support the notion that ethnic peer effects operate partly through the geographic concentration of ethnic groups and the higher level of interaction among individuals in those groups.

The persistence of ethnic differentials across generations and over time has relevant implications for welfare and immigration policy. While the outcome variable studied here is school enrollment, the estimated ethnic capital effects have far-reaching consequences. A lower probability that a child attends school implies reduced opportunities for social mobility and ultimately translates into lower earnings. The existence of ethnic peer effects in the human capital accumulation process of children has long-lasting effects on inequality, and shows that incumbent ethnic communities are correct to be concerned about the dilution of skills resulting from the arrival of new immigrants to the group. On the other hand, it also indicates that government interventions in the form of aid programs specifically targeted at particular ethnic groups can be a very effective means to reduce inequality in the short *and* in the long run, for that same reason.

## Appendix 1

*A.1.1. Mathematical Appendix*

This section attempts to develop more formally the point that a positive estimated coefficient on ethnic capital can be obtained even in the absence of ethnic peer effects. Consider a simplified version of equation (1-5), where $\beta_2 = 0$ and all covariates have been dropped or 'partialed out.' Moreover, assume that $x$ is only a noisy measure of the true parental skill variable, $x^*$. The model then becomes:

$$y_i = \beta_1 x_i^* + \varepsilon_i, \tag{A1-1}$$

where $x_i = x_i^* + v_i$ , the measurement error term $v_i$ has mean zero and variance $\sigma_v^2$, and it satisfies $E[v_i x_i^*] = E[v_i \varepsilon_i] = 0$ (classical measurement error).

In addition to $x$, another variable $w$ is available such that $E[w_i x_i^*] \equiv \sigma_{wx} > 0$ and $E[w_i \varepsilon_i] = 0$. Without loss of generality, then, this new variable $w$ is positively correlated with parental skills. A regression that includes both $x$ and $w$ will yield:

$$y_i = \pi_1 x_i + \pi_2 w_i + u_i, \tag{A1-2}$$

with:

$$p\lim \pi_2 = \beta_1 \left[ \frac{\sigma_{wx}(1-\lambda)}{\sigma_w^2 - \lambda(\sigma_{xw}^2 / \sigma_x^2)} \right], \tag{A1-3}$$

where $\sigma_w^2 = E[w_{ij}^2]$ and $\sigma_x^2 = E[x_{ij}^2]$ (all variables are measured in deviations from their means) and $\lambda = [\sigma_x^2 / (\sigma_x^2 + \sigma_v^2)]$, or the 'reliability ratio', a measure of the goodness of $x$ as a measure of $x^*$. Since $0<\lambda<1$, the coefficient on $w$ in this regression does not converge to zero. In words, the introduction of an additional regressor that is correlated with the mismeasured parental capital results in biased coefficients and the misleading appearance that the new regressor 'belongs' in the equation, when in fact it is not present in the true model (A1). The sign of the probability limit of $\pi_2$ is that of the covariance between $w$ and $x$ (positive). Of course, if no measurement error is present, then $\pi_2$ is asymptotically zero.

Ethnic capital is an example of such a regressor $w$. To be more precise, consider $w_i = x_i^* + \eta_i$, where $\eta_i$ has mean zero and variance $\sigma_\eta^2$, and $E[\eta_i x_i^*] \equiv \sigma_{x\eta}$ which does not necessarily equal zero. In that case, $\sigma_{wx} = \sigma_x^2 + \sigma_{x\eta}$ and $\sigma_w^2 = \sigma_x^2 + \sigma_\eta^2 + 2\sigma_{x\eta}$, so (A3) becomes:

$$p\lim \pi_2 = \beta_1 \left[ \frac{(\sigma_x^2 + \sigma_{x\eta})(1-\lambda)}{(\sigma_x^2 + \sigma_{x\eta})\left[1 - \lambda\left(\frac{\sigma_x^2 + \sigma_{x\eta}}{\sigma_x^2}\right)\right] + \sigma_\eta^2 + \sigma_{x\eta}} \right]. \tag{A1-4}$$

When $w$ does not vary within a group $j$, but only across groups (this is, when $w_{ij} \equiv \bar{x}_j = x_{ij}^* + \eta_{ij}$), then $\sigma_{x\eta} = -\sigma_\eta^2$ (intuitively: the covariance must be negative because relatively high realizations of $x^*$ will require relatively low values of $\eta$ in order for all observations in group $j$ to have the same value of $w$). Hence $\sigma_w^2 = \sigma_x^2 - \sigma_\eta^2$ and therefore the coefficient will converge to:

$$p\lim \pi_2 = \beta_1 \left[ \frac{1-\lambda}{1 - \lambda\left(\frac{\sigma_w^2}{\sigma_x^2}\right)} \right]. \tag{A1-5}$$

Since $\sigma_w^2 < \sigma_x^2$, the above formula is bounded between 0 and $\beta_1$.

The derivations above show that the OLS estimate of the coefficient on ethnic capital ($\bar{x}_j$) in equation (1-5) is inconsistent, because $\bar{x}_j$ is some ethnicity-specific summary measure of skills that is correlated with $x_{ij}$, even if it is not the exact average of the fathers in the sample. One should then expect the coefficient to be positive even in the absence of ethnic spillovers, just from the fact that the ethnic mean is correlated with the true parental skills, which are observed with error.

Finally, note that in the particular case where $\bar{x}_j$ is actually computed for each ethnic group as the average of $x_{ij}$ in the sample, then the term $(\sigma_w^2/\sigma_x^2)$, or $(\sigma_{\bar{x}}^2/\sigma_x^2)$, in (A5) can be read as the R-squared of the first stage regression of $x$ on a full set of ethnicity dummies. The better the fit in that first stage (this is, the better ethnicity predicts $x_{ij}$), the larger the bias, and the stronger the spurious 'ethnic capital effect' will appear to be.

*A.1.2. Data Appendix*

Ferenczi and Willcox (1929) report administrative data on alien arrivals collected by the United States immigration authorities. This source shows numbers of immigrants admitted by year, broad occupation categories (agriculture, laborers and servants, professionals, commerce and finance, industry, and miscellaneous), and "race or people." Additional tables classify immigrants by "race or people" into their countries of origin, which is the information I used to match the groups in the administrative data to the ethnic groups I identified in the Census data. Every Census from 1880 to 1970 collected information on country of birth that identifies the foreign born, and the foreign-birth status of both parents. Moreover, the nativity variables were recoded in the IPUMS to give a fairly consistent categorization for all years.

Classification of the foreign born (first generation) individuals in my sample into ethnic groups was made using country of birth or a combination of country of birth and mother tongue or race, in order to match the "race or people" categories in Ferenczi and Willcox as closely as possible. The coding scheme was as follows. [When different, the Ferenczi and Willcox categories appear in brackets].

1. African (Black): born in Africa or the West Indies, and of black race.
2. Armenian: born in the former Russian Empire/Soviet Union or the Middle East, with Armenian as their mother tongue.
3. Bulgarian/Serbian/Croatian/Slovenian: born in Bulgaria or the former Yugoslavia, or elsewhere in Central/Eastern Europe with Bulgarian, Serbo-Croatian or Slovene as their mother tongue. ["Bulgarian, Serbian and Montenegrin," and "Croatian and Slovenian," in Ferenczi and Willcox (1929)].
4. Czech: born in Bohemia or Moravia, or elsewhere in Central/Eastern Europe with Czech (Bohemian or Moravian) as their mother tongue. ["Bohemian and Moravian," in Ferenczi and Willcox (1929)].
5. Dutch/Flemish: born in the Netherlands, or in Belgium with Dutch (Flemish) as their mother tongue.

6. English/Welsh: born in England, Wales or British Canada, or in Canada with English as their mother tongue.[24]

7. Finnish: born in Finland.

8. French/Canadian: born in French Canada (Québec) or France, or in Canada,[25] Belgium or Switzerland with French as their mother tongue.

9. German/Austrian: born in German or Austria, or elsewhere in Central/Eastern Europe, with German as their mother tongue ["German," in Ferenczi and Willcox (1929)].

10. Greek: born in Greece.

11. Hungarian: born in Central/Eastern Europe, with Hungarian/Magyar as their mother tongue. ["Magyar," in Ferenczi and Willcox (1929)].

12. Irish: born in Ireland, or in British Canada with Irish as their mother tongue.

13. Italian: born in Italy.

14. Japanese: born in Japan.

15. Jewish: born in Central/Eastern Europe, with Yiddish as their mother tongue. ["Hebrew," in Ferenczi and Willcox (1929)].

16. Lithuanian: born in Lithuania or elsewhere in Central/Eastern Europe with Lithuanian as their mother tongue.

17. Mexican: born in Mexico.

18. Polish: born in Poland or elsewhere in Central/Eastern Europe, with Polish as their mother tongue.

19. Portuguese: born in Portugal, or in South America with Portuguese as their mother tongue.

20. Romanian: born in Romania, or elsewhere in Central/Eastern Europe with Romanian as their mother tongue.

21. Russian: born in the Russian Empire/Soviet Union, or elsewhere in Central/Eastern Europe, with Russian as their mother tongue.

22. Ruthenian: born in Central/Eastern Europe with Ruthenian as their mother tongue.

23. Scandinavian: born in Norway, Iceland, Denmark or Sweden.

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[24] In 1910 all individuals born in Canada are given the same code for birthplace, hence the use of mother tongue to distinguish English from French Canadians.

[25] See previous footnote.

24. Scottish: born in Scotland, or in British Canada with Scottish Gaelic as their mother tongue.
25. Slovak: born in Slovakia, or elsewhere in Central/Eastern Europe with Slovak as their mother tongue.
26. Spanish: born in Spain, or in South America with Spanish as their mother tongue.
27. NEC: Not Elsewhere Classified.

Ethnicity of the second generation was assigned as above, but using father's country of birth and father's mother tongue, except for those with a foreign mother only, in which case mother's country of birth and mother's mother tongue were used.[26]

[26] I experimented with a definition of second generation based on the mother's country of birth and mother tongue, and using the father's information for those with a foreign father only. The results were not sensitive to the definition of the second generation.

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**Figure 1**
**Children School Attendance versus Average Literacy in Ethnic Group.**
**No Ethnicity or Year Effects.**

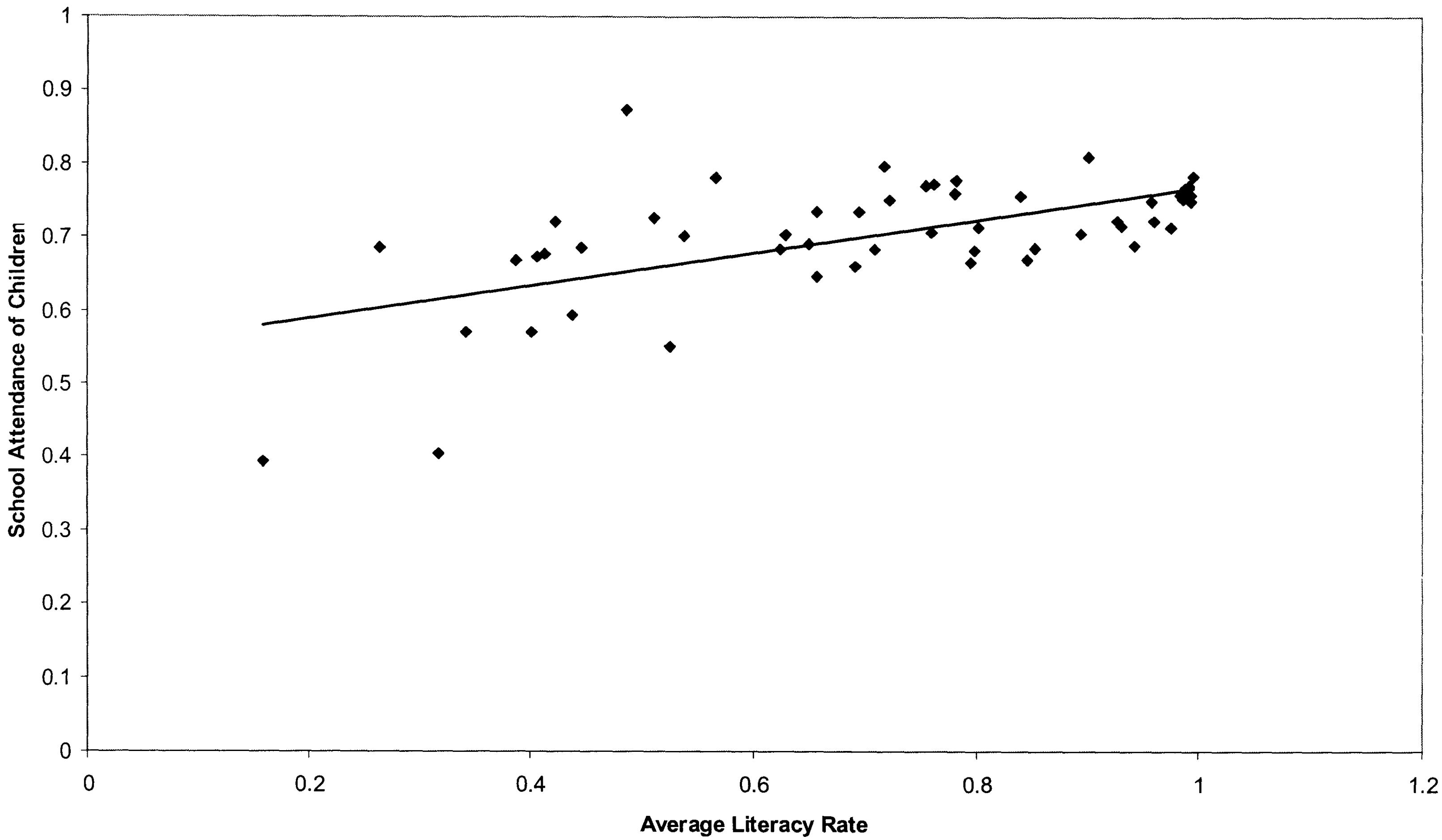

**Figure 2**
**First Stage for Ethnic Capital: Average Literacy versus Fraction of Low-Skilled Recent Immigrants in Ethnic Group, with Ethnicity and Year Effects**

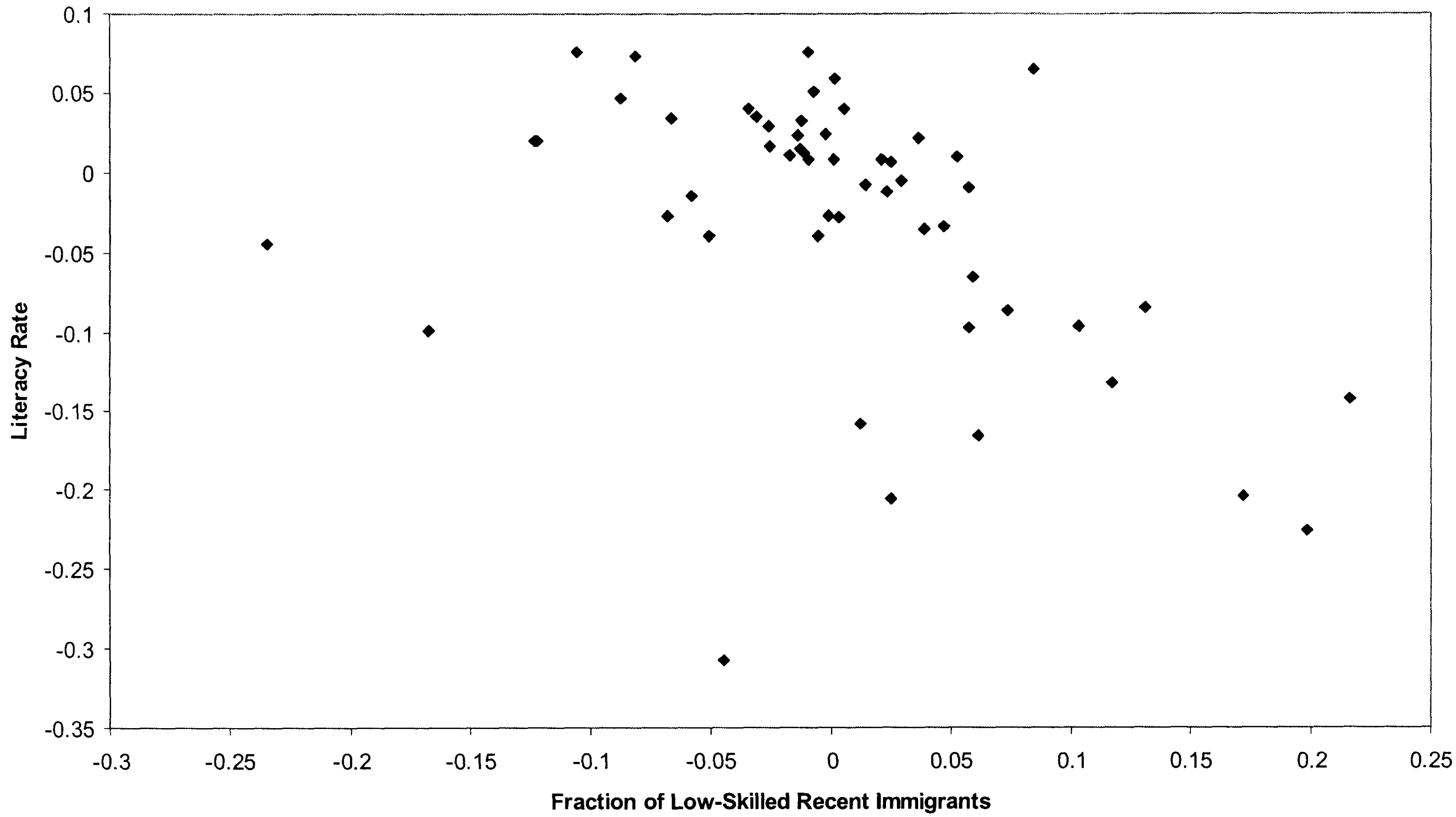

**Table 1.1**
Summary Education of Immigrants in 1910, of the Second-Generation in 1910 and 1940, and of the Third Generation in 1940, by Ethnic Group

| | 1910 | | | | 1940 | | | |
|---|---|---|---|---|---|---|---|---|
| Ethnic Group | Literacy Rate of First-Gener. Adults 30-50 | Adults Sample Size | School Attend. Rate of Second-Gener. Kids 6-18 | Kids Sample Size | Years of School. of Second-Gener. Adults 30-50 | Adult Sample Size | School Attend. Rate of Third-Gener. Kids 6-18 | Kids Sample Size |
| African | .754 | 49 | .667 | 18 | 6.0 | 464 | .853 | 526 |
| Armenian | .871 | 39 | .875 | 8 | 9.7 | 7 | .911 | 31 |
| Bulg/Ser/Cro | .699 | 216 | .571 | 98 | 8.2 | 54 | .890 | 321 |
| Czech | .928 | 225 | .683 | 498 | 8.8 | 179 | .881 | 462 |
| Dutch/Flemis | .944 | 143 | .705 | 277 | 9.1 | 118 | .856 | 424 |
| English/Wels | .989 | 1,008 | .768 | 2,390 | 10.0 | 874 | .887 | 1,630 |
| Finnish | .913 | 150 | .692 | 124 | 8.8 | 113 | .878 | 187 |
| French/Cana | .977 | 135 | .670 | 823 | 9.0 | 429 | .851 | 929 |
| German/Aust | .910 | 3,079 | .690 | 5,307 | 8.9 | 2,940 | .868 | 5,757 |
| Greek | .826 | 150 | .571 | 14 | 11.0 | 14 | .883 | 298 |
| Hungarian | .874 | 526 | .673 | 165 | 9.4 | 125 | .826 | 476 |
| Irish | .972 | 1,104 | .753 | 1,954 | 9.7 | 1,242 | .904 | 1,912 |
| Italian | .636 | 1,476 | .677 | 1,348 | 8.7 | 902 | .857 | 4,282 |
| Japanese | .775 | 223 | .721 | 43 | 10.8 | 8 | .935 | 130 |
| Jewish | .868 | 815 | .771 | 1,153 | 10.8 | 472 | .884 | 723 |
| Lithuanian | .719 | 164 | .686 | 121 | 8.8 | 71 | .863 | 279 |
| Mexican | .458 | 205 | .393 | 201 | 4.1 | 134 | .736 | 891 |
| Polish | .736 | 338 | .594 | 1,188 | 8.3 | 685 | .846 | 2,184 |
| Portuguese | .558 | 68 | .702 | 114 | 7.5 | 50 | .820 | 197 |
| Romanian | .810 | 58 | .727 | 11 | 11.2 | 37 | .866 | 178 |
| Russian | .748 | 721 | .782 | 116 | 10.6 | 411 | .882 | 1,791 |
| Ruthenian | .465 | 43 | .686 | 35 | 7.2 | 8 | .781 | 58 |
| Scandinavian | .985 | 1,385 | .750 | 2,339 | 9.4 | 1,365 | .896 | 2,402 |
| Scottish | .992 | 252 | .759 | 399 | 10.1 | 252 | .917 | 480 |
| Slovak | .814 | 250 | .669 | 248 | 8.4 | 115 | .889 | 219 |
| Spanish | .886 | 44 | .552 | 30 | 9.2 | 20 | .891 | 101 |
| Native | .910 | 28,031 | .725 | 40,576 | 8.8 | 53,755 | .841 | 60,395 |

**Notes:** The table shows the fraction of foreign-born men aged 30 to 50 who can read and write in any language in 1910, the fraction of second-generation children (i.e.: born in the US to a foreign-born parent) aged 6 to 18 who are enrolled in school in 1910, the average years of schooling for second-generation men aged 30-50 in 1940, and the fraction of third-generation children (i.e.: born in the US to a second-generation parent) who are enrolled in school in 1940. For comparison purposes, the last row shows the corresponding measures for third- and higher- generation adults (this is, US-born adults with US-born parents), and for fourth- and higher-generation children (this is, US-born children of US-parents and grandparents). *Source:* Author's tabulations from the 1910 and 1940 Census IPUMS files.

**Table 1.2**
Descriptive Statistics for the 1910 and 1920 Census IPUMS samples

| Variables | 1910-1920 | 1910 | 1920 |
|---|---|---|---|
| A. Children (Second-Generation Americans of Schooling Age) | | | |
| | *Dependent Variable* | | |
| In School | .724 | .709 | .737 |
| | (.447) | (.454) | (.440) |
| | *Covariates* | | |
| Age | 10.93 | 11.18 | 10.73 |
| | (3.98) | (4.00) | (3.95) |
| Female | .496 | .493 | .498 |
| | (.500) | (.500) | (.500) |
| Number of Siblings | 3.68 | 3.78 | 3.60 |
| | (2.12) | (2.10) | (2.12) |
| In Metropolitan Area | .611 | .567 | .648 |
| | (.487) | (.496) | (.477) |
| In Region: | | | |
| New England | .123 | .121 | .125 |
| | (.328) | (.326) | (.330) |
| Middle Atlantic | .335 | .296 | .368 |
| | (.472) | (.457) | (.482) |
| East North Central | .244 | .260 | .231 |
| | (.430) | (.439) | (.421) |
| West North Central | .150 | .180 | .125 |
| | (.357) | (.384) | (.331) |
| South Atlantic | .020 | .018 | .022 |
| | (.141) | (.133) | (.148) |
| East South Central | .008 | .008 | .007 |
| | (.087) | (.088) | (.085) |
| West South Central | .032 | .034 | .030 |
| | (.175) | (.181) | (.171) |
| Mountain | .030 | .029 | .030 |
| | (.170) | (.169) | (.171) |
| Pacific | .058 | .054 | .061 |
| | (.233) | (.226) | (.240) |
| N | 76,847 | 19,202 | 57,645 |

**Notes:** The data are from the Census IPUMS for 1910 and 1920. In Panels A and B, the sample is restricted to second-generation Americans (i.e.: born in the US to a foreign-born parent) of schooling age (6 to 18 years old) who reside with their parents. In Panel C, the sample is restricted to first- and second-generation Americans (i.e.: foreign-born, or born in the US to a foreign-born parent) of working age (19 to 60 years old). Standard deviations are in parentheses. All other entries are means (weighted by the IPUMS sample-line weight).

**Table 1.2 (continued)**
Descriptive Statistics for the 1910 and 1920 Census IPUMS samples

| Variables | 1910-1920 | 1910 | 1920 |
|---|---|---|---|
| B. Fathers (First-Generation Americans with Children of Schooling Age) | | | |
| | *Regressor of Interest: Parental Capital* | | |
| Literacy | .843 | .853 | .835 |
| | (.364) | (.354) | (.371) |
| | *Covariate* | | |
| Age | 44.84 | 45.35 | 44.42 |
| | (8.46) | (8.27) | (8.59) |
| | *Instrument* | | |
| Age at Arrival | 20.61 | 20.75 | 20.49 |
| | (8.52) | (8.52) | (8.52) |
| N | 76,847 | 19,202 | 57,645 |
| C. Adults (First- and Second-Generation Americans of Working Age) | | | |
| | *Regressor of Interest: Ethnic Capital* | | |
| Literate | .863 | .832 | .890 |
| | (.344) | (.374) | (.312) |
| | *Demographic Characteristics* | | |
| Age | 36.71 | 35.67 | 37.09 |
| | (11.29) | (11.17) | (11.33) |
| Fraction in Metropolitan Area | .625 | .595 | .653 |
| | (.480) | (.491) | (.476) |
| | *Instrument* | | |
| Low-Skilled New Immigrants as Fraction of Population | .043 | .079 | .011 |
| | (.106) | (.146) | (.021) |
| New Immigrants as Fraction of Population | .082 | .142 | .029 |
| | (.148) | (.196) | (.040) |
| N | 305,842 | 81,649 | 224,193 |

**Notes:** The data are from the Census IPUMS for 1910 and 1920. In Panels A and B, the sample is restricted to second-generation Americans of schooling age (6 to 18 years old) who reside with their parents. In Panel C, the sample is restricted to first- and second-generation Americans of working age (19 to 60 years old). Standard deviations are in parentheses. All other entries are means (weighted by the IPUMS sample-line weight).

**Table 1.3**
Endogamy Rates in the First and Second Generation (1910-1920), by Ethnic Group

| | First-Generation Women | | | Second-Generation Women | | |
|---|---|---|---|---|---|---|
| Ethnic Group | Married Native (1) | Married Same Group (2) | Married Other Group (3) | Married Native (4) | Married Same Group (5) | Married Other Group (6) |
| African (Black) | 25.4 | 60.8 | 13.9 | 71.4 | 22.6 | 5.9 |
| Armenian | 1.0 | 97.9 | 1.0 | 0.0 | 100.0 | 0.0 |
| Bulg/Serb/Croa | 0.2 | 95.8 | 4.0 | 18.0 | 64.1 | 18.0 |
| Czech | 2.6 | 87.5 | 9.9 | 13.6 | 61.1 | 25.3 |
| Dutch/Flemish | 6.0 | 83.6 | 10.5 | 27.0 | 45.8 | 27.3 |
| English/Welsh | 27.1 | 52.9 | 19.9 | 53.2 | 23.7 | 23.1 |
| Finnish | 0.8 | 92.3 | 6.9 | 10.5 | 62.7 | 26.9 |
| French/Canadian | 9.4 | 74.5 | 16.2 | 33.9 | 34.3 | 31.8 |
| German/Austrian | 7.2 | 82.9 | 9.9 | 31.0 | 52.3 | 16.6 |
| Greek | 0.4 | 95.5 | 4.1 | 14.3 | 57.1 | 28.6 |
| Hungarian | 0.8 | 87.2 | 12.1 | 11.8 | 41.2 | 47.1 |
| Irish | 11.2 | 71.3 | 17.5 | 33.9 | 39.3 | 26.9 |
| Italian | 0.2 | 98.9 | 0.8 | 7.6 | 82.4 | 10.0 |
| Japanese | 0.0 | 99.6 | 0.4 | 0.0 | 95.5 | 4.6 |
| Jewish | 0.2 | 97.8 | 2.0 | 2.8 | 84.2 | 13.0 |
| Lithuanian | 0.2 | 96.9 | 3.0 | 5.7 | 75.5 | 18.9 |
| Mexican | 4.2 | 93.5 | 2.3 | 11.7 | 82.8 | 5.5 |
| Polish | 0.5 | 94.3 | 5.3 | 6.4 | 76.1 | 17.5 |
| Portuguese | 1.7 | 95.1 | 3.2 | 14.5 | 64.0 | 21.5 |
| Romanian | 0.6 | 83.4 | 16.0 | 5.3 | 36.8 | 57.9 |
| Russian | 1.2 | 88.3 | 10.6 | 12.0 | 56.7 | 31.3 |
| Ruthenian | 0.0 | 88.9 | 11.1 | 0.0 | 71.4 | 28.6 |
| Scandinavian | 6.2 | 85.0 | 8.8 | 29.5 | 49.4 | 21.0 |
| Scottish | 20.3 | 47.4 | 32.4 | 51.5 | 9.8 | 38.7 |
| Slovak | 0.4 | 92.3 | 7.3 | 3.3 | 75.8 | 20.9 |
| Spanish | 7.1 | 80.1 | 12.8 | 26.7 | 36.0 | 37.3 |
| NEC | 8.0 | 74.1 | 18.0 | 40.2 | 24.0 | 35.8 |

**Notes:** The table shows the distribution of husband's ethnicity for married women in the 1910 and 1920 Censuses with spouse present in the household. Columns (1)-(3) show the ethnicity distribution of husbands for foreign-born women, while columns (4)-(6) do the same for second-generation women. Columns (2) and (5) refer to husbands, either first or second generation, of the same ethnic group as the wife. Columns (3) and (6) refer to husbands, either first or second generation, of some ethnic group other than that of the wife.
*Source:* Author's tabulations from the 1910 and 1920 Census IPUMS files.

**Table 1.4**
Descriptive Statistics for the 1910 and 1920 Census IPUMS samples, by Ethnicity

| | 1910 | | | | | 1920 | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Ethnicity | Fraction Children in School | Fraction Literate Fathers | Children Sample Size | Average Literacy Rate | Adults Sample Size | Fraction Children in School | Fraction Literate Fathers | Children Sample Size | Average Literacy Rate | Adults Sample Size |
| African (Black) | .667 | .722 | 18 | .795 | 223 | .687 | .812 | 128 | .852 | 991 |
| Armenian | .875 | .875 | 8 | .486 | 91 | .797 | .812 | 69 | .717 | 328 |
| Bulg/Serb/Croa/Slov | .571 | .724 | 98 | .401 | 670 | .735 | .789 | 837 | .694 | 1,921 |
| Czech | .683 | .783 | 498 | .798 | 1,258 | .717 | .876 | 1,132 | .930 | 3,744 |
| Dutch/Flemish | .705 | .931 | 277 | .894 | 841 | .723 | .942 | 774 | .959 | 2,858 |
| English/Welsh | .768 | .983 | 2,390 | .988 | 11,812 | .771 | .981 | 4,926 | .991 | 31,270 |
| Finnish | .692 | .871 | 124 | .650 | 473 | .757 | .806 | 724 | .839 | 1,392 |
| French/Canadian | .67 | .744 | 823 | .845 | 3,269 | .723 | .832 | 1,861 | .926 | 8,655 |
| German/Austrian | .69 | .898 | 5,307 | .942 | 22,791 | .714 | .943 | 9,930 | .975 | 56,601 |
| Greek | .571 | .643 | 14 | .342 | 475 | .715 | .860 | 200 | .801 | 1,574 |
| Hungarian (Magyar) | .673 | .848 | 165 | .406 | 858 | .773 | .809 | 1,215 | .762 | 2,867 |
| Irish | .753 | .973 | 1,954 | .985 | 13,606 | .75 | .986 | 3,235 | .992 | 30,188 |
| Italian | .677 | .602 | 1,348 | .413 | 4,847 | .752 | .653 | 8,695 | .722 | 15,987 |
| Japanese | .721 | .256 | 43 | .422 | 543 | .704 | .577 | 362 | .628 | 1,360 |
| Jewish | .771 | .864 | 1,153 | .755 | 3,343 | .81 | .892 | 4,860 | .901 | 11,188 |
| Lithuanian | .686 | .595 | 121 | .445 | 489 | .735 | .698 | 859 | .657 | 1,802 |
| Mexican | .393 | .144 | 201 | .159 | 840 | .405 | .291 | 762 | .317 | 3,749 |
| Polish | .594 | .599 | 1,188 | .437 | 3,563 | .684 | .696 | 5,953 | .709 | 12,304 |
| Portuguese | .702 | .395 | 114 | .538 | 352 | .685 | .540 | 531 | .624 | 1,301 |
| Romanian | .727 | .909 | 11 | .510 | 132 | .76 | .901 | 161 | .780 | 643 |
| Russian | .782 | .836 | 116 | .566 | 430 | .778 | .796 | 1,572 | .782 | 4,081 |
| Ruthenian | .686 | .457 | 35 | .264 | 106 | .648 | .739 | 119 | .657 | 236 |
| Scandinavian | .75 | .962 | 2,339 | .957 | 6,626 | .758 | .974 | 5,217 | .983 | 19,886 |
| Scottish | .759 | .989 | 399 | .993 | 1,984 | .784 | .990 | 834 | .995 | 5,462 |
| Slovak | .669 | .628 | 248 | .387 | 753 | .707 | .772 | 1,809 | .759 | 2,962 |
| Spanish | .552 | .567 | 30 | .525 | 200 | .661 | .576 | 132 | .691 | 798 |
| Not Elsewhere Clas. | .752 | .761 | 180 | .513 | 1,074 | .737 | .733 | 748 | .746 | 3,788 |

**Table 1.5**
OLS Estimates of the Effect of Parental and Ethnic Capital on Individual Skills

| | Using Father's Literacy | | | | Using average of Father's and Mother's Literacy |
|---|---|---|---|---|---|
| | In School (1) | In School (2) | In School (3) | In School (4) | In School (5) |
| Father's Literacy (Parental Capital) | .054** (.011) | .052** (.011) | .051** (.009) | .049** (.008) | .060** (.008) |
| Average Literacy (Ethnic Capital) | .215** (.034) | .214** (.035) | .137** (.022) | .135** (.022) | .119** (.022) |
| Region, Metro Effects? | No | Yes | No | Yes | Yes |
| Age Dummies? | No | No | Yes | Yes | Yes |
| Father's Age and Age-Squared? | No | No | Yes | Yes | Yes |
| Female | | | .002 (.005) | .001 (.005) | -.003 (.005) |
| Father's Age at Arrival | | | -.0012** (.0003) | -.0011** (.0002) | -.0008** (.0003) |
| N | 69,864 | 69,864 | 69,864 | 69,864 | 56,308 |

**Notes:** Standard errors corrected for ethnicity-year clustering are reported in parentheses. The data are from the Census IPUMS for 1910 and 1920, with the sample being restricted to second-generation Americans of schooling age (6 to 18 years old) who reside with their parents. Ethnicity-year-specific average literacy rates are computed from a sample restricted to first- and second-generation Americans of working age (19 to 60 years old). All regressions include Census year and ethnicity main effects. In column 5, the sample is restricted to children whose parents are both first-generation Americans and are both present in the household. Single (double) asterisk denotes statistical significance at the 90% (95%) level of confidence in a one-tailed test.

**Table 1.6**
OLS Estimates of the Effect of Parental and Ethnic Capital on Individual Skills:
Additional Results

| | Benchmark | Coefficient on Parental Capital =0 | Coefficient on Parental Capital =.2 | Coefficient on Ethnic Capital =.1 | Coefficient on Ethnic Capital =0 |
|---|---|---|---|---|---|
| | In School | In School | In School | In School | In School |
| | (1) | (2) | (3) | (4) | (5) |
| Father's Literacy (Parental Capital) | .049** (.008) | .000 | .200 | .049** (.009) | .048** (.009) |
| Average Literacy (Ethnic Capital) | .135** (.022) | .144** (.024) | .106** (.018) | .100 | .000 |
| N | 69,864 | 69,864 | 69,864 | 69,864 | 69,864 |

**Notes:** Standard errors corrected for ethnicity-year clustering are reported in parentheses. The data are from the Census IPUMS for 1910 and 1920, with the sample being restricted to second-generation Americans of schooling age (6 to 18 years old) who reside with their parents. Ethnicity-year-specific average literacy rates are computed from a sample restricted to first- and second-generation Americans of working age (19 to 60 years old). All regressions include Census year, ethnicity, region and female main effects as well as father's age, father's age squared, father's age at arrival, number of siblings, a dummy indicating residence in a metropolitan area, and a vector of age dummies. Single (double) asterisk denotes statistical significance at the 90% (95%) level of confidence in a one-tailed test.

**Table 1.7**
Age at Arrival from Non-English Speaking Country as Instrument for Parental Capital: First-Stage Estimates

| | Father's Literacy (1) | Father's Literacy (2) | Father's Literacy (3) | Father's Literacy (4) | Average of Father's and Mother's Literacy (5) |
|---|---|---|---|---|---|
| Father's Age at Arrival * Non-English Speaking Country of Origin | -.0065**<br>(.0012) | -.0065**<br>(.0012) | -.0066**<br>(.0012) | -.0065**<br>(.0012) | -.0053**<br>(.0017) |
| Father's Age at Arrival | -.0007**<br>(.0002) | -.0008**<br>(.0002) | -.0007**<br>(.0003) | -.0007**<br>(.0003) | -.0030**<br>(.0011) |
| Region, Metro Effects? | No | Yes | Yes | Yes | Yes |
| Age Dummies? | No | No | Yes | Yes | Yes |
| Father's Age and Age-Squared, Female Dummy, and Number of Siblings? | No | No | Yes | Yes | Yes |
| Fraction of New Immigrants in Low-Skilled Occupations | | | | -.058<br>(.070) | -.119<br>(.096) |
| N | 69,864 | 69,864 | 69,864 | 69,864 | 56,308 |

**Notes:** Standard errors corrected for ethnicity-year clustering are reported in parentheses. The data are from the Census IPUMS for 1910 and 1920, with the sample being restricted to second-generation Americans of schooling age (6 to 18 years old) who reside with their parents. All regressions include Census year and ethnicity main effects. Single (double) asterisk denotes statistical significance at the 90% (95%) level of confidence in a one-tailed test.

**Table 1.8**
Recent Immigrant Flows as Instrument for Ethnic Capital:
First-Stage Estimates

| | Average Literacy (1) | Average Literacy (2) | Average Literacy (3) | Average Literacy (4) | Average Literacy (5) |
|---|---|---|---|---|---|
| Fraction of New Immigrants in Low-Skilled Occupations | -.599** (.300) | -.605** (.197) | -.605** (.197) | -.603** (.197) | -.603** (.197) |
| Region, Metro Effects? | No | Yes | Yes | Yes | Yes |
| Age Dummies? | No | No | Yes | Yes | Yes |
| Father's Age and Age-Squared, Female Dummy, and Number of Siblings? | No | No | No | Yes | Yes |
| Father's Age at Arrival * Non-English Speaking Country of Origin | | | | | -.0002 (.0002) |
| N | 54 | 69,864 | 69,864 | 69,864 | 69,864 |

**Notes:** Standard errors corrected for ethnicity-year clustering are reported in parentheses. The data are from the Census IPUMS for 1910 and 1920, with the sample being restricted to first- and second-generation Americans of working age (19 to 60 years old). The fraction of new immigrant arrivals, in the 5 years prior to the Census year, who were laborers or servants or agricultural workers, by ethnicity and year, are computed from the immigration records in Ferenczi and Willcox (1929). The mean fraction of new immigrants who were laborers/servants across ethnicity-year cells is .483, and the interquartile range (from .312 to .683) is .371. (See Appendix for more details). All regressions include Census year and ethnicity main effects. Single (double) asterisk denotes statistical significance at the 90% (95%) level of confidence in a one-tailed test.

**Table 1.9**
2SLS Estimates of the Effect of Parental and Ethnic Capital on Individual Skills

| | Using Father's Literacy | | | Using average of Father's and Mother's Literacy |
|---|---|---|---|---|
| | In School | In School | In School | In School |
| | (1) | (2) | (3) | (4) |
| Father's Literacy (Parental Capital) | .280** (.078) | .281** (.080) | .203** (.089) | .149** (.069) |
| Average Literacy (Ethnic Capital) | .138** (.066) | .138** (.066) | .116** (.041) | .094* (.051) |
| Region, Metro Effects? | No | Yes | Yes | Yes |
| Age Dummies? | No | No | Yes | Yes |
| Father's Age and Age-Squared? | No | No | Yes | Yes |
| Female | | | .002 (.005) | -003 (.005) |
| Father's Age at Arrival | | | -.0003 (.0005) | -.0001 (.0007) |
| N | 69,864 | 69,864 | 69,864 | 56,308 |

**Notes:** Standard errors corrected for ethnicity-year clustering are reported in parentheses. The data are from the Census IPUMS for 1910 and 1920, with the sample being restricted to second-generation Americans of schooling age (6 to 18 years old) who reside with their parents. The excluded instruments are the father's age at arrival interacted with a dummy for non-English speaking country of origin, and the fraction of new immigrant arrivals, in the 5 years prior to the Census year, who were laborers or servants or agricultural workers, by ethnicity and year. Ethnicity-year-specific average literacy rates are computed from a sample restricted to first- and second-generation Americans of working age (19 to 60 years old). All regressions include Census year and ethnicity main effects, a female dummy, father's age at arrival, number of siblings, a dummy indicating residence in a metropolitan area, and a full set of age dummies. Single (double) asterisk denotes statistical significance at the 90% (95%) level of confidence in a one-tailed test.

**Table 1.10**
2SLS Estimates of the Effect of Parental and Ethnic Capital on Individual Skills:
Additional Results

| | Benchmark | Coefficient on Parental Capital =0 | Coefficient on Parental Capital =.2 | Coefficient on Ethnic Capital =.1 | Coefficient on Ethnic Capital =0 |
|---|---|---|---|---|---|
| | In School | In School | In School | In School | In School |
| | (1) | (2) | (3) | (4) | (5) |
| Father's Literacy (Parental Capital) | .203** (.089) | .000 | .200 | .204** (.088) | .211** (.087) |
| Average Literacy (Ethnic Capital) | .116** (.041) | .153** (.045) | .117** (.035) | .100 | .000 |
| N | 69,864 | 69,864 | 69,864 | 69,864 | 69,864 |

**Notes:** Standard errors corrected for ethnicity-year clustering are reported in parentheses. The data are from the Census IPUMS for 1910 and 1920, with the sample being restricted to second-generation Americans of schooling age (6 to 18 years old) who reside with their parents. The excluded instruments are the father's age at arrival interacted with a dummy for non-English speaking country of origin, and the fraction of new immigrant arrivals, in the 5 years prior to the Census year, who were laborers or servants or agricultural workers, by ethnicity and year. Ethnicity-year-specific average literacy rates are computed from a sample restricted to first- and second-generation Americans of working age (19 to 60 years old). All regressions include Census year, ethnicity, region and female main effects as well as father's age, father's age squared, father's age at arrival, number of siblings, and a vector of age dummies. Single (double) asterisk denotes statistical significance at the 90% (95%) level of confidence in a one-tailed test.

**Table 1.11**
OLS and 2SLS Estimates of the Effect of Parental and Ethnic Capital on Individual Skills: Specification Checks

| | Including Father's Occupation | | Excluding Five Smallest Ethnic Groups | |
|---|---|---|---|---|
| | OLS | 2SLS | OLS | 2SLS |
| | (1) | (2) | (3) | (4) |
| Father's Literacy<br>(Parental Capital) | .047**<br>(.008) | .198**<br>(.093) | .050**<br>(.009) | .205**<br>(.091) |
| Average Literacy<br>(Ethnic Capital) | .147**<br>(.021) | .122**<br>(.042) | .138**<br>(.023) | .116**<br>(.043) |
| Father is in a Low-Skilled Occupation | -.016**<br>(.004) | -.008<br>(.006) | | |
| N | 69,864 | 69,864 | 69,266 | 69,266 |

**Notes:** Standard errors corrected for ethnicity-year clustering are reported in parentheses. The data are from the Census IPUMS for 1910 and 1920, with the sample being restricted to second-generation Americans of schooling age (6 to 18 years old) who reside with their parents. The excluded instruments are the father's age at arrival interacted with a dummy for non-English speaking country of origin, and the fraction of new immigrant arrivals, in the 5 years prior to the Census year, who were laborers or servants or agricultural workers, by ethnicity and year. Ethnicity-year-specific average literacy rates are computed from a sample restricted to first- and second-generation Americans of working age (19 to 60 years old). The low-skilled occupations used in Columns 1 and 2 are agriculture, laborers and servants (the same ones used in the construction of the instrument for ethnic capital). The five ethnic groups excluded in Columns 3 and 4 are African, Spanish, Romanian, Armenian and Ruthenian, and correspond to the 5 rows with the smallest counts in Table 1.2. All regressions include Census year, ethnicity, region and female main effects as well as father's age, father's age squared, father's age at arrival, number of siblings, and a vector of age dummies. Single (double) asterisk denotes statistical significance at the 90% (95%) level of confidence in a one-tailed test.

**Table 1.12**
OLS and 2SLS Estimates of the Effect of Parental and Ethnic Capital on Individual Skills: Exploring the Role of Geographic Concentration

| | OLS<br>(1) | OLS<br>(2) | 2SLS<br>(3) | 2SLS<br>(4) |
|---|---|---|---|---|
| Father's Literacy<br>(Parental Capital) | .049**<br>(.008) | | .203**<br>(.089) | |
| Father's Literacy<br>(Parental Capital)<br>* High Concentration | | .047**<br>(.013) | | .231<br>(.142) |
| Father's Literacy<br>(Parental Capital)<br>* Low Concentration | | .048**<br>(.009) | | .182**<br>(.076) |
| Average Literacy<br>(Ethnic Capital) | .135**<br>(.022) | | .116**<br>(.041) | |
| Average Literacy<br>(Ethnic Capital)<br>* High Concentration | | .261*<br>(.157) | | .141<br>(.195) |
| Average Literacy<br>(Ethnic Capital)<br>* Low Concentration | | .122**<br>(.030) | | .109<br>(.084) |
| N | 69,864 | 69,864 | 69,864 | 69,864 |

**Notes:** Standard errors corrected for ethnicity-year clustering are reported in parentheses. The data are from the Census IPUMS for 1910 and 1920, with the sample being restricted to second-generation Americans of schooling age (6 to 18 years old) who reside with their parents. The concentration index is computed as the fraction of all adults of working age in the region who are first- or second- generation and who have the same ethnicity, and averages approximately .12 for the entire sample. High (Low) concentration is then defined as a dummy that equals one if the individual lives in a region where their ethnic group (first and second generation) comprises 12% or more (less than 12%) of the population of working age, zero otherwise. The excluded instruments are the father's age at arrival interacted with a dummy for non-English speaking country of origin, and the fraction of new immigrant arrivals, in the 5 years prior to the Census year, who were laborers or servants or agricultural workers, by ethnicity and year. Ethnicity-year-specific average literacy rates are computed from a sample restricted to first- and second-generation Americans of working age (19 to 60 years old). All regressions include Census year and ethnicity main effects, a female dummy, father's age at arrival, number of siblings, a dummy indicating residence in a metropolitan area, and a full set of age dummies. Single (double) asterisk denotes statistical significance at the 90% (95%) level of confidence in a one-tailed test.

**Table 1.13**
OLS and 2SLS Estimates of the Effect of Parental and Ethnic Capital on Individual Skills: Using Regional Endogamy Rates as a Measure of the Level of Interaction Within Groups

| | OLS (1) | OLS (2) | 2SLS (3) | 2SLS (4) |
|---|---|---|---|---|
| Father's Literacy (Parental Capital) | .049** (.008) | | .203** (.089) | |
| Father's Literacy (Parental Capital) * High Endogamy Rate | | .041** (.011) | | .171** (.075) |
| Father's Literacy (Parental Capital) * Low Endogamy Rate | | .062** (.006) | | .259** (.118) |
| Average Literacy (Ethnic Capital) | .135** (.022) | | .116** (.041) | |
| Average Literacy (Ethnic Capital) * High Endogamy Rate | | .141** (.023) | | .140** (.040) |
| Average Literacy (Ethnic Capital) * Low Endogamy Rate | | .095** (.028) | | .017 (.082) |
| N | 69,864 | 69,864 | 69,864 | 69,864 |

**Notes:** Standard errors corrected for ethnicity-year clustering are reported in parentheses. The data are from the Census IPUMS for 1910 and 1920, with the sample being restricted to second-generation Americans of schooling age (6 to 18 years old) who reside with their parents. The endogamy rate is computed as the fraction of married women in the region whose husband belongs to the same ethnic group, and averages approximately .55 for the entire sample. High (Low) Endogamy Rate is then defined as a dummy that equals one if the individual lives in a region where the endogamy rate for their ethnic group equals 55% or more (less than 55%), zero otherwise. The excluded instruments are the father's age at arrival interacted with a dummy for non-English speaking country of origin, and the fraction of new immigrant arrivals, in the 5 years prior to the Census year, who were laborers or servants or agricultural workers, by ethnicity and year. Ethnicity-year-specific average literacy rates are computed from a sample restricted to first- and second-generation Americans of working age (19 to 60 years old). All regressions include Census year and ethnicity main effects, a female dummy, father's age at arrival, number of siblings, a dummy indicating residence in a metropolitan area, and a full set of age dummies. Single (double) asterisk denotes statistical significance at the 90% (95%) level of confidence in a one-tailed test.

# 2. The Effect of Education on Fertility: Evidence From Compulsory Schooling Laws

## 2.1. Introduction

Social scientists have long observed a strong relationship between education and fertility. Both across countries and over time, higher levels of schooling appear associated with fewer children per woman. In particular, the last forty years have seen widespread fertility declines accompanied by increases in educational attainment levels in most Western countries. The question of whether this correlation is causal remains open, however. Despite the various reasons to expect a causal relationship between schooling and fertility, empirical research to date has not provided a definitive and satisfactory answer.

The key challenge in estimating the effect of education on fertility decisions is that unobserved characteristics affecting schooling choices are potentially correlated with unobservable factors influencing the decision to have children. For instance, women with high ability levels, stronger tastes for work or low discount rates are relatively more likely to finish high school and attend college. At the same time, for any given level of education, they are likely to be more inclined to pursue a professional career and delay having children. Therefore, one might expect a negative relationship between years of schooling and number of children even in the absence of any causal effect of education on fertility. On the other hand, women with better access to credit markets may be more likely both to attend school and to have children, whereas females coming from less affluent households may lack the opportunities or incentives to get an education as well as the means to raise children and support an extended family. As a result, a positive spurious correlation between education and fertility is possible too. The presence of error in available measures of schooling can also introduce a bias towards zero, thus creating the appearance of a weaker correlation between the two variables than may exist in reality.

Estimating the impact of schooling on fertility may help clarify the role of education in demographic transitions. Moreover, this effect can be seen as yet another dimension of the social return to schooling. To the extent that schooling choices of individuals have social consequences in the form of fiscal costs or welfare benefits, which are not taken into account by individuals, then this constitutes another reason why the social return to education may be different from its private return.[27]

An analysis of the impact of schooling on fertility is also motivated by important fiscal and welfare policy implications. Falling fertility rates, along with longer life expectancies, result in population ageing. Unless immigration flows are large enough to offset this process (and countries are typically reluctant to let that happen), this tends to reduce the labor force relative to the elderly population. In other words, it raises the dependency ratio of retirees to working-age adults, thus putting pressure on public spending in pensions and health care. This is a major concern in many industrialized countries, where such fiscal burdens constitute a major threat to their current social security systems. If secondary- and post-secondary education enrollment rates are growing in those countries, no analysis of the sustainability of the welfare system can be complete without an assessment of the impact of higher education on fertility, which in turn affects the future size of the labor market. In developing countries, on the other hand, decreasing fertility may reduce health risks for both women and children, and contribute to improving welfare conditions, especially of rural households. Programs like the World Bank's Female Secondary Schooling Assistance Project aim to achieve these goals precisely by encouraging education of girls. The effectiveness of such ventures depends on whether, and to what extent, female education stimulates reductions in fertility.

In order to solve the identification problem outlined above and estimate the effect of education on fertility, I use changes in state compulsory schooling laws over time as a source of exogenous variation in individual schooling choices. Female teenagers typically faced compulsory attendance and child labor laws enacted by their state legislatures several years or even decades prior to those women's fertility decisions. Moreover, legislators appeared to be concerned with raising education levels and preventing children from entering the labor force too young, and did not seem to be acting in response to contemporaneous or anticipated changes in fertility patterns. Recent literature

[27] Most of the research on social returns to education to date has been focused on the effect of an individual's schooling on the *wages* of other workers in her social group (Acemoglu and Angrist (2000), Heckman and Klenow (1998)), although some recent studies analyze the impact of individual schooling on other outcomes with social repercussions, such as crime (Lochner and Moretti (2002)), or mortality (Lleras-Muney (2002)).

studying the causal links between education and labor market outcomes has used supply-side instruments such as child schooling laws: Angrist and Krueger (1991) first documented a relationship between quarter of birth and individual schooling and used it to analyze private returns to education; Acemoglu and Angrist (2000) studied wage spillovers; Lochner and Moretti (2004) analyzed effects on crime; and Lleras-Muney (2004) studied mortality. Very little or no work has been done linking schooling and fertility through the study of a natural experiment, however. An exception is McCrary and Royer (2003), who use birthday information for Texan and Californian women in the 1990s and a regression discontinuity approach to study the effect of mothers' education on infant mortality, by first establishing no impact of schooling on the probability of becoming a mother (an age-specific fertility rate). Instead of relying solely on school age entry laws (which is the rationale behind a strategy based on date of birth), this paper also uses information on other regulations such as minimum school dropout age, and minimum schooling requirements for leaving school and for entering the workforce, in order to obtain a more complete picture of the institutional constraints affecting education of women in all fifty contiguous states during five decades in the early- and mid-twentieth century, a period when high school attendance rates rose dramatically.[28]

Instrumental variable (IV) estimates using data on women aged 40-49 from the 1950-1990 US Censuses suggest a strong, negative relationship between education and fertility. A one-year increase in schooling is associated with a 0.33 reduction in the average number of children. The magnitude of this effect appears to be larger than the relationship uncovered by simple ordinary least squares (OLS) regressions, which suggests the presence of measurement error in schooling. Further analysis indicates that part of this difference is due to the existence of heterogeneity in the fertility return to schooling across individuals and to non-linearity across education levels. Since the OLS and IV estimators are different weighted sums of the distinct impacts of each additional year of schooling on the number of children, with the IV placing more "weight" on the levels of education which are most affected by the instruments,[29] the estimates using

[28] This is the education expansion known as the 'high school movement'. See Goldin (1998) for details.
[29] Angrist and Imbens (1995) show that 2SLS and OLS estimates can be written as weighted averages of individual IV estimators, and Lochner and Moretti (2004) derive the corresponding 2SLS and OLS weights as a function of observable quantities.

compulsory schooling laws as instruments provide an accurate approximation to the effect of additional schooling on the fertility of women who are induced to increase their education because of those laws.

Because education can affect fertility by reducing the likelihood that a woman will marry and start a family, I also study the impact of schooling on marriage rates. Estimates uncover no statistically significant relationship, which suggests that education may be reducing fertility by delaying, but not by preventing, the decision to get married. This hypothesis is further supported by estimates showing that schooling does indeed raise the probability that a woman will reach the end of her fertile life-cycle with no children. Finally, I use these estimates to calculate the contribution of education expansion to the dramatic fertility declines observed in several Western countries, and find that about a third of the documented reductions in fertility between 1960 and 1990 can be attributed to the observed increases in female schooling in those countries.

The remainder of the paper is organized as follows. Section II discusses the theory, develops the estimation framework, and highlights the econometric issues involved in attempting to identify the effect of education on fertility. Section III describes the data and presents and interprets the base empirical results. Section IV discusses some robustness checks and additional results and applications. Section V summarizes the paper and concludes.

## 2.2. Theoretical and Econometric Framework

### *2.2.1. Fertility and Schooling: Theory*

The most accepted theories in the Demography and Economics literature (Willis (1973), Barro and Becker (1988), Livi-Baci (1997)) suggest that female education lowers fertility through an increase in the opportunity cost of women's time where the productive technology for children is time-intensive relative to the parents' technology for their standard of living. In fact, theoretical models that seek to explain the number of children born over the life-cycle highlight female wages as the key element in the opportunity cost of childbearing. The canonical one-period, full-certainty model of fertility (Montgomery and Trussell (1986)) where children are a normal good and their care requires time as well as money expenditures yields a shadow price of children that is a function of the wage rate.[30] Other models seek to explain fertility histories as stochastic processes, where the woman is assumed to solve a sequential decision problem under uncertainty. These include Wolpin (1984), Newman (1988), and Hotz and Miller (1988), and have not yet produced a consensus about an appropriate empirical specification for life cycle fertility. In any case, since returns to schooling are positive, this induces a negative relationship between education and fertility. There are, however, other channels through which schooling can affect a woman's decision to have children.

To borrow from Easterlin and Crimmins (1985)'s terminology, the above is the 'demand' component of the educational effect on fertility.[31] Schooling can also affect the 'supply' of children, however. More educated women may have better information about health. By increasing awareness of the importance of food care, balanced nutrition, personal hygiene or cleaning standards, education can raise the fecundity, or potential reproductive capacity, of women. In this framework, schooling can have an additional impact on fertility by reducing the psychological cost of fertility control, since education may increase the ability or willingness to adopt new birth-control methods. In the limit, by raising knowledge of the existence and functioning of contraceptives in the first place, schooling may even bring the 'price' of fertility control down from infinity, thus allowing women to have the chance to exercise some control on family size that would not have been available otherwise.

The theories briefly reviewed above do not consider some additional channels that can mediate the relationship between schooling and fertility. Completing additional years of education necessarily entails spending more time in school. There is naturally a rather

[30] This is just the (compensated) substitution effect. An increase in mothers' schooling also brings about an increase in parents' income that encourages spending in all normal goods, including children, but it appears safe to assume that this income effect must be small enough and hence the wage effect dominates. In fact, more complex theories produce an even weaker income effect on the sheer number of children: Becker (1960) and Becker and Lewis (1973) incorporate the quality dimension of reproduction decisions in their "child quality" fertility model. Their model predicts that any increase in parents' income raises both the quantity and the quality of children. Since the income elasticity of the former is small compared to the income elasticity of the latter, Becker contends, then the resulting increase in the amount spent on children mainly takes the form of higher quality, thus allowing for the substitution (wage) effect to clearly dominate the income effect on the number of children.

[31] By 'demand for children', Easterlin and Crimmins (1985) refer to the number of children parents would want in order to achieve their desired family size, in the absence of any natural constraints and under the assumption that birth control mechanisms were known, available and costless. 'Supply of children' is, then, the number of children a couple would have, were they to make no deliberate attempt to limit family size.

mechanical effect of schooling on fertility if women tend not to have children while continuing to attend high school or college, thus delaying the beginning of (and effectively shortening) their reproductive life. Other mechanisms for education to affect fertility include changes in tastes for children versus work. Schooling may alter or shape the views that women have on their traditionally assigned role in society, encouraging some women to devote themselves to a professional career to the expense of creating an extensive family (or even of having children at all).

To sum up, theory suggests that there are several channels how schooling impacts on fertility, all of them being negative expect for the 'supply' argument regarding health conditions and fecundity. The combined sign of the overall effect is ambiguous, although it seems reasonable to expect a negative relationship. In any case, the magnitude of such effect is a purely empirical question.

### *2.2.2. Empirical Specification*

In order to capture the causal relationship of interest, consider the regression model:

$$y_{it} = \alpha + \beta \cdot s_{it} + z_{it}^{'}\gamma + \delta_t + \varepsilon_{it}, \qquad (2\text{-}1)$$

where $y_{it}$ is a measure of total completed fertility, the total number of children ever born to woman $i$ observed in the Census year $t$, $s_{it}$ is her schooling, $\delta_t$ are Census year fixed effects, $z_i$ is a vector of individual covariates that includes state-of-birth effects, year-of-birth effects, and other demographic variables, and $\varepsilon_{ist}$ is an individual error component. Standard OLS estimates of equation (2-1) will be biased if schooling, $s_{it}$, is correlated with unobserved determinants of individual fertility choices contained in the residual term $\varepsilon_{it}$. As argued above, this can be the case if ability, patience or tastes for work encourage schooling and produce a low demand for children, which creates a negative omitted variable bias, or if access to economic opportunities facilitates both education and raising children, in which case there is a source of positive confounding bias, or in the presence of measurement error, which creates attentuation bias towards zero (a positive bias, if $\beta$ is indeed negative). Naturally, all these possibilities are not mutually exclusive.

To address the endogeneity of education and eliminate those sources of bias, I use compulsory schooling laws as instruments that exogenously affect schooling choices. The use of valid instruments for education should produce consistent estimates of $\beta$ in equation (2-1). It is important to recognize, though, that the effect of education on fertility may be non-linear in schooling and may vary across individuals. In that case, the IV estimates must be interpreted as some weighted average of the heterogeneous marginal effects of schooling on the fertility of those women most induced to raise their education by the compulsory schooling laws being used as instruments.[32]

## 2.3. Data and Main Results

### *2.3.1. Data Sources and Descriptive Statistics*

The analysis uses data on US-born white women from the 1950-1990 Census microdata extracts for whom all the relevant variables are reported. Individuals born in Hawaii or Alaska are excluded, since these states did not enter the Union until 1959, and no information on compulsory attendance or child labor laws during the early twentieth century is available. The sample is further restricted to women aged 40 to 49, who are reaching or are already past the end of their fertile lifetime and who were 14 years old between 1914 and 1964 (years for which information on compulsory schooling laws in effect in each of the 50 contiguous states is available). The schooling variable is highest grade completed, capped at 17 years to impose a uniform top-code across survey years. Other technical details on the extracts and the definitions of the variables are documented in the Data Appendix.

Table 2.1 provides the descriptive statistics for the extract. The average age is constant across censuses, while mean schooling increases by about 0.6 or 0.7 years between 1950-60, 1960-70 and 1970-80, and then by slightly more than a year between 1990 and 2000. Total completed fertility, measured as the number of children per woman, increases steadily from an average of 2.2 for individuals in the 1950 Census to 2.9 in 1980 (these are the mothers of the 'baby boom' children), and then goes back down to 2.2

[32] The monotonicity assumption (namely, that compulsory schooling laws only have a positive or no effect on individual schooling) is necessary for this interpretation of the IV estimator. See Angrist and Imbens (1995), Imbens and Angrist (1994), Heckman (1997), Heckman and Vytlacil (1997) for more details.

by 1990. Table 2.2 reports total completed fertility by educational attainment. In each of the sample years higher education levels are associated with substantially lower average numbers of children. The key feature to notice in Table 2.2 is that, while the average moves around over time, the differences in fertility by education level are sizable and persistent over time. This is further evidenced in Figure 1, which shows total completed fertility by educational attainment for all cohorts of women born between 1885 and 1954. For example, women born in 1925 who did not complete high school had an average of 3.4 children, whereas those in the same cohort who obtained a college degree gave birth to 2.4 children on average. Looking at total fertility rates —the average number of children a woman could expect to bear in her lifetime if she were to experience current age-specific fertility rates— also reveals a similar pattern. As illustrated in Figures 2a and 2b, college graduates aged 27 or younger were less likely to give to birth to a child in than high school dropouts both in 1960 and in 1990. Between ages 28 and 40, college graduates are slightly more likely to have given birth during the reference year, but that tiny advantage does not make up for the big difference in fertility rates for teenagers and women in their early-to-mid twenties.[33] In conclusion, it appears that college graduates typically have about one less children on average than high school dropouts. Next I will turn to regression analysis in order to control for state of birth, cohort of birth, and year effects, and then to use instrumental variables to identify whether this observed relationship can be interpreted as a causal effect of schooling.

### *2.3.2. OLS Estimates*

Table 2.3 shows OLS estimates of equation (2-1) for the entire sample, and also separately by Census year. Education appears to be negatively correlated with total completed fertility after controlling for cohort of birth, state of birth, and year effects using OLS. Column (2) adds state of residence fixed effects in order to absorb potential heterogeneity across states in fertility patterns. Doing that leaves the point estimates practically unchanged. While the estimates using single censuses (columns 4 to 8) show a

[33] This translates into an estimated (using US Census data) TFR for high school dropouts of 3.7 in 1960 and 1.9 in 1990, compared to 2.7 and 1.3 for college graduates in 1960 and 1990, respectively.

slight degree of variation across years,[34] the table suggests that on average an additional year of schooling appears to be associated with a reduction of about 0.13 in the average number of children. Put another way, women with four additional years of schooling appear to have on average 0.5 less children.

The OLS estimates presented here are consistent with the hypothesis that schooling reduces completed fertility. If so, the effect appears to be quantitatively and statistically significant, and fairly persistent over time. However, these estimates may reflect the impact of unobserved characteristics that influence the probability of completing higher levels of schooling and the decision to have children. For example, as discussed in the previous section, women with lower discount rates are relatively less likely to invest in education and a professional career, and more likely to marry and start having children early. To the extent that variation in unobserved discount rates is important, OLS estimates could be overstating the effect of schooling on fertility. On the other hand, OLS could be underestimating that effect if measurement error in the schooling variable is significant, and/or if there are significant disparities in background and access to economic opportunities that promote education and child rearing.

*2.3.3. Compulsory Schooling Laws as Instruments for Individual Schooling*

The ideal instrumental variable induces exogenous variation in years of schooling while being uncorrelated with measurement error, discount rates, ability, tastes for work, or any other individual characteristics that can affect both education and fertility. I use state-mandated restrictions on child labor and compulsory school attendance laws as instruments for schooling. Compulsory attendance laws are condensed as the minimum number of years a child had to be in school before being allowed to drop out, and are the maximum of either the explicitly mandated minimum years of schooling in the state, or the difference between the minimum dropout age and the maximum enrollment age. Child labor laws are summarized as the minimum years of schooling required before obtaining a work permit. Since the major reason to leave school typically was to work, these act as constraints on schooling choices as well. Child labor laws are defined as the

[34] These deviations may be simply reflecting time variation in omitted state characteristics in the relationship between schooling and fertility, since these single-Census regressions do not include state of residence effects.

larger of the explicitly mandated minimum schooling required for a work permit, and the difference between the minimum age for work and the maximum school enrollment age.

Compulsory schooling laws in the first half of the twentieth century have been extensively studied by Acemoglu and Angrist (2000), Lleras-Muney (2004) and Lochner and Moretti (2004). Lleras-Muney (2002) documents their effectiveness for both men and women, and finds that they did not affect blacks. For this reason, I restrict my attention to white women. The compulsory attendance laws and child labor laws in effect in each of the 50 contiguous states were assigned to individuals in the sample based on the year in which they turned 14 (which is calculated from year of birth, estimated using age on Census day) and their state of birth (since no information on state of residence during adolescence is available in the Census).

In the years relevant for my sample, 1914-1964, states changed child labor and compulsor attendance laws several times, and generally upward.[35] This resulted in an increase over time in the fraction of women being exposed to more restrictive laws, as is evident from observing the bottom eight rows in Table 2.1. For example, while no woman in the sample from the 1950 Census had been exposed to laws requiring 9 or more years in school before obtaining a work permit, by 1990 such laws had been in place at age 14 in the state of birth of 43.5% of all women.

There is a sizable and statistically significant relationship between individual schooling and sets of dummies for both types of compulsory schooling laws. This is shown in Table 2.4, which display estimates for the first-stage regressions of years of schooling on dummies for child labor laws requiring 7, 8, and 9 or more years in school, and/or dummies for compulsory attendance laws mandating 9, 10, and 11 or more years of schooling (the omitted categories are the least restrictive groups for child labor and compulsory schooling laws); equations also include controls for state of birth, year of birth and Census year. For instance, the entries in column 1 show that women born in states requiring 9 or more years in school to issue a work permit ended up with .38 more years of schooling completed than those born in states with a child labor law that required 6 or less years. In general, the estimated coefficients are consistent with the notion that the more stringent the legislation, the stronger is its effect on average years of

[35] See Lleras-Muney (2002) and Lochner and Moretti (2004).

schooling.[36] Moreover, the hypothesis of no joint significance of the estimated coefficients is soundly rejected in every column (F-statistics are reported in the table).

The identifying assumption is that the timing of within-state changes in compulsory schooling laws over time is orthogonal to unobservable characteristics of women that may affect their fertility decisions years later, once other potential confounding factors have been taken into account by conditioning on state of birth, cohort of birth and Census year (and, in some specifications, also state of residence). This hypothesis is reinforced by the results shown in Table 2.5. The columns in this table report the estimated coefficients from regressions of a dummy for whether a woman completed discrete levels of education as indicated in the column heading. The effect of compulsory schooling laws is strongest for completion of high school levels of schooling, and is smaller, and in most cases statistically insignificant, in columns corresponding to higher levels of education. Finding that the laws increased the proportion attending college as well as the fraction completing high school would have suggested that they might have been correlated with underlying trends in education or other omitted factors such as tastes or family background, and are therefore not exogenous, thus invalidating them as instruments for schooling. Instead, the results indicate that this is not a problem in the data, showing that the laws were not endogenous during this period.[37]

The different columns in Table 2.6, which report estimated coefficients from the first-stage regression for subsamples of the data, show that the impact of the child labor and compulsory attendance laws dummies remain significant after excluding each Census year, therefore ensuring that the effectiveness of compulsory schooling laws is consistent over time and across cohorts in the sample. Overall, the evidence seems to support the validity of these laws as instruments for schooling.

[36] Such effects were first documented for men in Acemoglu and Angrist (2000). The effects for women hereby reported are qualitatively and quantitatively comparable.

[37] It is interesting to note that, up to $12^{th}$ grade, the laws have a significant positive effect on schooling above required levels. Possible explanations include peer effects, educational sorting (Lang and Kropp (1986)), or the fact that educational decisions are "lumpy" (Acemoglu and Angrist (2000)). On the other hand, the appearance of quantitatively small, but statistically significant *negative* effects of compulsory schooling laws on higher levels of schooling might reflect shifts in state resources away from local colleges and to high schools concurrently with the enactment of laws requiring additional years of schooling.

*2.3.4. IV Estimates and Interpretation*

The 2SLS estimates of equation (2-1) are reported in Table 2.7. Controlling for state of residence, state of birth, year, and year of birth main effects, and using compulsory schooling laws as instruments generates an estimate of the effect of education on total completed fertility of -0.330 (with a standard error of .061) when dummies for both child labor and compulsory attendance regulations are employed.[38] This is still negative, and significantly larger in magnitude than the OLS estimate (the 95 percent confidence interval for this coefficient is [-0.445,-0.210] and comfortably excludes the OLS estimate of -0.131), and indicates that, other things equal, having completed three additional years of schooling reduces the number of children ever born by one. Using compulsory attendance laws alone yields somewhat smaller (in absolute value) estimates, although less precise and not significantly different from those including child labor laws or both types of laws in the set of instruments.

The difference between OLS and 2SLS estimates appears to suggest that OLS understates the magnitude of the causal relationship of interest. In previous sections, I pointed at measurement error in schooling, and at unobserved differences in access to economic opportunities encouraging both education and fertility, as possible explanations for a positive bias in OLS estimates. Measurement error is probably not a candidate in this case, however, since the results in Table 2.3 showed that pulling 1990 Census data from the sample does not affect the OLS estimates. If attenuation bias were responsible for the discrepancy between the OLS and 2SLS coefficients, and given that the measure of schooling is noisier in 1990, then one should expect the OLS estimate from a sample that excludes that year to be significantly closer to the 2SLS estimate than the regular OLS from using all of the data. That does not appear to be the case, however, which indicates that a positive correlation between schooling and fertility induced by some unobserved factor such as access to credit is likely to have caused the OLS estimate to be biased upwards.[39]

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[38] All standard errors reported in this paper are corrected for state-of-birth/year clustering.

[39] Similarly, if more educated women tend to marry relatively more educated, wealthier men, and demand for the sheer number of children decreases with income, then women with high levels of schooling are less likely to show higher fertility rates on average, thus creating a negative spurious correlation between these two variables. To the extent that compulsory schooling laws do not alter matching decisions in the marriage

Another plausible reason for the 2SLS estimates to differ from their OLS counterparts lies in the difference between the weighing function underlying each estimator. IV estimates reflect a weighted average of causal responses to each single-year change in completed schooling, with the weights depending on the fraction of individuals who are induced to make each transition by the compulsory schooling laws used as instruments, while OLS estimates weigh individuals in proportion to their contribution to the total variation in schooling, irrespective of the instruments.[40] In the presence of non-linearity and/or individual heterogeneity in the effect of schooling on fertility, the IV will almost surely differ from the OLS estimate. Figure 3a plots the OLS and 2SLS weights for the case where child labor laws as used as instruments, as well as the difference between the two. These weights are computed using the formulae derived in Lochner and Moretti (2004). As expected given the results shown in Table 2.5 (indicating that the instruments induce changes in schooling at the secondary education level, but not at post-high school levels), the 2SLS weights are larger than the OLS weights for levels of education corresponding to high school, and comparable if not lower for levels beyond high school. To the extent that the greatest impact of schooling on fertility decisions is associated with changes at the secondary level of education, one should expect the 2SLS estimates to be larger than OLS estimates. In fact, using the 2SLS weights to re-weight the observed fertility responses to each additional year of schooling (i.e.: the year-by-year changes in regression-adjusted fertility means net of state, year and cohort effects) produces an estimate that is larger (more negative) than the OLS estimate and closer to the 2SLS estimate, although not by much.[41] This suggests that a small part of the reason why 2SLS are more negative than OLS estimates lies in differences in the fertility return to schooling across levels of education.[42] Indeed, given the evidence that suggests that

market, then, 2SLS estimates are more negative than OLS because the instruments eliminate this additional source of positive endogeneity bias.

[40] In other words, IV uses only the variation in schooling that is correlated with the instrument. See Angrist and Imbens (1995) for more details.

[41] The changes in the regression-adjusted average number of children by years of schooling (obtained from regressing number of children ever born on state of birth, state of residence, Census year and year of birth dummies and using the predicted values evaluated at the means), weighted by the 2SLS weights, produce an estimate of -0.17, compared with -0.13 from using the OLS weights.

[42] The fact that the heterogeneity in of fertility returns to schooling across education levels is small does not necessarily imply that there is no heterogeneity in the effect of schooling *across individuals*. If, for example, the women most affected by compulsory schooling laws happen to be those with the highest

compulsory schooling laws encouraged further schooling of women who would have otherwise dropped out from high school, the IV estimates are a better assessment than OLS estimates of the likely fertility outcomes of education expansions that increase female graduation rates from high school.

In order to further analyze the causal effect of education on fertility, and given that Table 2.2 seems to suggest that the impact of schooling on fertility may be greatest at high school completion, I estimate a model of fertility using an indicator for high school graduation rather than total years of schooling. This will answer the question: how does a change in the fraction of women graduating from high school affect fertility? The answer to such question should be interesting also because fertility declines have been associated historically with education expansions that increased female rates of enrollment in, and of completion of, secondary education. Arguably, these are also the most relevant levels of schooling for policy intervention.

The first two columns in Table 2.8 report OLS estimates of a model where the regressor of interest is a dummy for having graduated from high school. The change in fertility, net of state, year and cohort effects, associated with completion of secondary school is -0.63. This is fairly consistent with the previous estimates for the linear model in schooling, given that high school graduates have on average more than just one extra year of schooling than high school dropouts. Next I move to IV estimation. Since Table 2.4 documented that child labor and compulsory attendance laws did induce an increase in the fraction of women graduating from high school, the same identification strategy remains valid in this case as in the model linear in years of schooling. The estimated effect of high school graduation on fertility using the instruments is close to -1. The most precisely estimated, which uses both sets of compulsory schooling laws, is -0.9. This implies that completing secondary education leads women to having, on average, approximately one less child. This supports the notion that the effect of schooling on fertility is likely to be largest at twelfth grade, and reinforces the finding that estimates that account for endogeneity show a sizable negative effect of education on fertility.

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discount rates and/or with the lowest access to credit, who have a larger fertility return to schooling relative to the average individual in the distribution, then the IV estimator will be capturing the average marginal effect for those women, and will not be an estimate of the average effect in the population.

An alternative approach for examining the relationship of interest for women with education levels between 8 and 12 years of schooling consists in using completed years of high school as the endogenous regressor. Unlike the model that uses a dummy for high school graduation, this method has the advantage that it is not vulnerable to the miscoded binary treatment problem (see Imbens and Agrist (1994)). Table 2.9 presents estimates of the effect of an additional year of high school on fertility. As shown in columns 1 and 2, the OLS estimates are now higher in value (around -0.22) and much closer to the IV. In particular, the coefficients obtained by including compulsory attendance laws in the set of instruments (also -0.22, and -0.26 when including all instruments), while comparable to those obtained in the model with total years of schooling, become no longer significantly different from the OLS estimates in columns 1 and 2 (even the IV estimates from using only child labor laws –columns 3 and 4– are not statistically distinguishable from the OLS estimates, although in this case this is just due to the loss in precision). This reconciliation of the OLS and IV coefficients in this specification is further evidence that the effect of education on fertility is largest for women who completed at least some high school education, which are the individuals most affected by the instruments.

## 2.4. Additional Results and Applications

### *2.4.1. The Impact of Schooling on Marriage and Childlessness*

One of the possible ways for education to affect fertility is through marriage status. If schooling reduces the likelihood that a woman will marry, then naturally higher schooling levels will bring about reductions in the number of children. I explore this possibility by estimating models of the probability of having ever been married on years of schooling. Panel A of Table 2.10 reports OLS and 2SLS estimates of the impact of education on marriage status. While OLS shows a small, but statistically significant, negative coefficient, the 2SLS estimates show no systematic relationship and are all insignicant. Hence there is no evidence that schooling affects the probability that a woman will marry. The documented fertility-reducing effect of education is not due to more educated women being significantly more likely to remain single.

Since the negative impact of schooling on fertility does not seem to operate through marriage, it seems natural to ask whether schooling does affect the probability that a woman will remain childless until the end of her fertile years. Panel B of Table 2.10 shows estimates of models of the probability of not having had any children on years of schooling. In this case, both 2SLS and OLS regressions produce statistically and quantitatively significant coefficients. Using compulsory attendance laws yields an estimate of 0.18, which implies that an additional year of schooling raises the probability of not having any children by almost 2 percentage points. Other IV specifications produce somewhat smaller but not very precisely estimated, and hence not significantly different coefficients. Overall, this constitutes suggestive evidence that schooling may be reducing fertility partly by increasing the proportion of women who reach the end of their fertile lives without children, even though these women may still be about as likely to get married as their less educated counterparts.

*2.4.2. An Application: The Role of Education Expansions in Fertility Declines*

To put the above estimates of the impact of schooling on fertility into perspective, it is useful to look at several countries that experienced dramatic fertility declines and education expansions in the second half of the twentieth century, and compare the actual reductions in fertility with those implied by these estimates from the observed increases in education levels of women.

Although still high in many parts of the world, total fertility rates have been decreasing dramatically during the twentieth century, mostly after 1960.[43] As populations become more educated, the number of children per woman falls. In particular, the last forty years have seen widespread fertility declines accompanied by increases in educational attainment levels in most Western countries. While in 1960 the average woman in North America or in industrialized Europe would have 3.4 and 2.6 children respectively, fertility rates in every developed nation are now below the replacement rate of 2.1, ranging from 2.0 in the United States to 1.6 in France and Canada, 1.4 in Japan, and just 1.2 in Italy.[44] In some cases where education expansion has been relatively more

[43] The worldwide Total Fertility Rate fell from around 6 children per woman in 1900 to 2.7 in 2003. In 1960, this rate was 4.9. *Source*: Population Division of the Department of Economic and Social Affairs of the United Nations Secretariat, *World Population Prospects: The 2002 Revision* and *World Urbanization Prospects: The 2001 Revision*, http://esa.un.org/unpp

[44] According to the latest *Population Bulletin* of the Population Reference Bureau (March 2004), as of 2003 all industrialized countries have fertility rates below 2.1 children per woman, the level needed to ensure the

recent and initial levels were particularly low, the decline in fertility between 1960 and 2000 has been even more dramatic: it went from 2.9 to 1.1 in Spain, from 3.2 to 1.4 in Portugal, and from 3.8 to 1.9 in Ireland.

Table 2.11 reports data from six European countries that underwent significant reductions in fertility between 1960 and 1990. At the same time, average female education increased in all of them. It is natural to ask: to what extent did education expansion contribute to the decline in fertility in each one of those countries? Using the 2SLS estimates from Table 2.6 it is possible to provide an answer to that question. The predicted fall in fertility from the observed rise in female education is computed for each country and presented in columns (5) and (6), each using one of the extremes in the range of 2SLS estimates obtained from the different specifications in Table 2.7. The fact that these countries saw increases in education at or around the high school levels makes the exercise particularly meaningful if the 2SLS estimates are capturing the marginal effect of education on fertility for women at those education levels.

The estimated effects of schooling on fertility imply, for example, that between 21% and 28% of the fertility drop in Italy between 1960 and 1990 can be explained by the increase in education experienced by its female population during that period. In general, about a quarter of the fertility decline in Germany, Italy and Ireland, a third of the drop in Portugal and Spain, and as much as half of the fall in Greece can be attributed to the effect of rising female education.

## 2.5. Concluding Remarks

There are a number of reasons to expect that education reduces fertility. By raising wages, education increases the opportunity cost of having children and spending time away from work. Education may also make women more aware of methods of birth control, and more accepting of alternative lifestyles that do not necessarily include marrying early and having children. It is also possible that more educated women enjoy

long-term replacement of the population. Moreover, the United Nations projected in 2002 that fertility levels will likely fall below replacement in three out of four developing countries by 2050 (UN Press Release POP/850).

higher husband's earnings, if there is assertive matching, and that may encourage demand for children. Empirical evidence on the effect of education on fertility has implications for welfare and fiscal policy and is also of interest for economic theory.

In order to identify the magnitude of the relationship between schooling and total completed fertility, this paper uses changes in state compulsory attendance and child labor laws over time to generate exogenous variation in schooling of women in an extended sample from the US Census that includes 1950 through 1990 data. The finding that three additional years of schooling result in one less children per woman on average is consistent and robust to a number of specification checks. Instrumental variables estimates also suggest that most of this effect is not channeled through lower marriage rates. Educated women are not less likely to marry, however they are more likely to reach the end of their fertile lifecycle without having any children, which is consistent with the hypothesis that education delays marriage and family formation.

I further argue that the estimated impact of schooling on the decision to have children can shed some light on the dramatic demographic changes experienced in the last few decades by countries with large expansions in education. As much as a third or more of the fertility decline observed in several Western countries such as Spain or Ireland can be attributed to a rise in their female education levels. This suggests that the fertility-reducing impact of schooling, while constituting an external benefit in less developed countries with high population densities and growth rates, can be regarded as an external cost in more advanced societies where the fertility rate is already significantly below replacement, the dependency rates of workers to pension recipients is already low, and the expected contribution of the average young person to the public budget is clearly positive, as is currently the case in most of the industrialized world. In those countries, the schooling effects on fertility represent a negative external effect of education that should contribute to better understand the overall social impacts of human capital accumulation.

## Data Appendix

This study uses data from the 1950 General (1/330 sample), 1960 General (1% sample), 1970 Form 1 State and Form 2 State (both 1% samples), 1980 5% State A (a 5% sample), and 1990 1% unweighted (a 1% random self-weighted sample created by IPUMS) Census IPUMS files. See Ruggles and Sobek (1997) for more details on the IPUMS system.

The extracts include all US-born (except Alaska and Hawaii) white women aged 40-49 at the time of the Census survey. The 1950 sample is limited to "sample line" individuals (this is, those with long-form responses), the only for whom information on children born is available. The schooling variable used for 1950 through 1980 is the Census extracts variable HIGRADE (general), the IPUMS recode of the highest grade completed. The 1990 Census only reports schooling in broader categories; therefore it is not directly comparable with the information from previous surveys. Years of schooling in that year were assigned from group means for white women in each category reported in Park (1994, Table 5), who uses a one-time overlap questionnaire from the February 1990 Current Population Survey to construct averages for the categories found in the 1990 Census. Finally, in order to ensure consistency of the schooling measure across all five Census years, the resulting variable was capped at 17, the highest grade completed available in the 1950 Census.

The compulsory attendance laws and child labor laws in effect in each of the 50 contiguous states in the years 1914-1964 were assigned to all individuals in the sample on the basis of their state of birth and the year in which they turned 14 (which is calculated from year of birth, estimated using age on Census day). More details on the data sources for these laws are given in Appendix B of Acemoglu and Angrist (2000). Baseline regressions that use compulsory schooling laws matched by age at 16 produced qualitatively similar results.

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***Figure 1*: Total Completed Fertility, by education and cohort: 1885-1954**

Source: Author's calculations from 1950-1990 Census IPUMS data

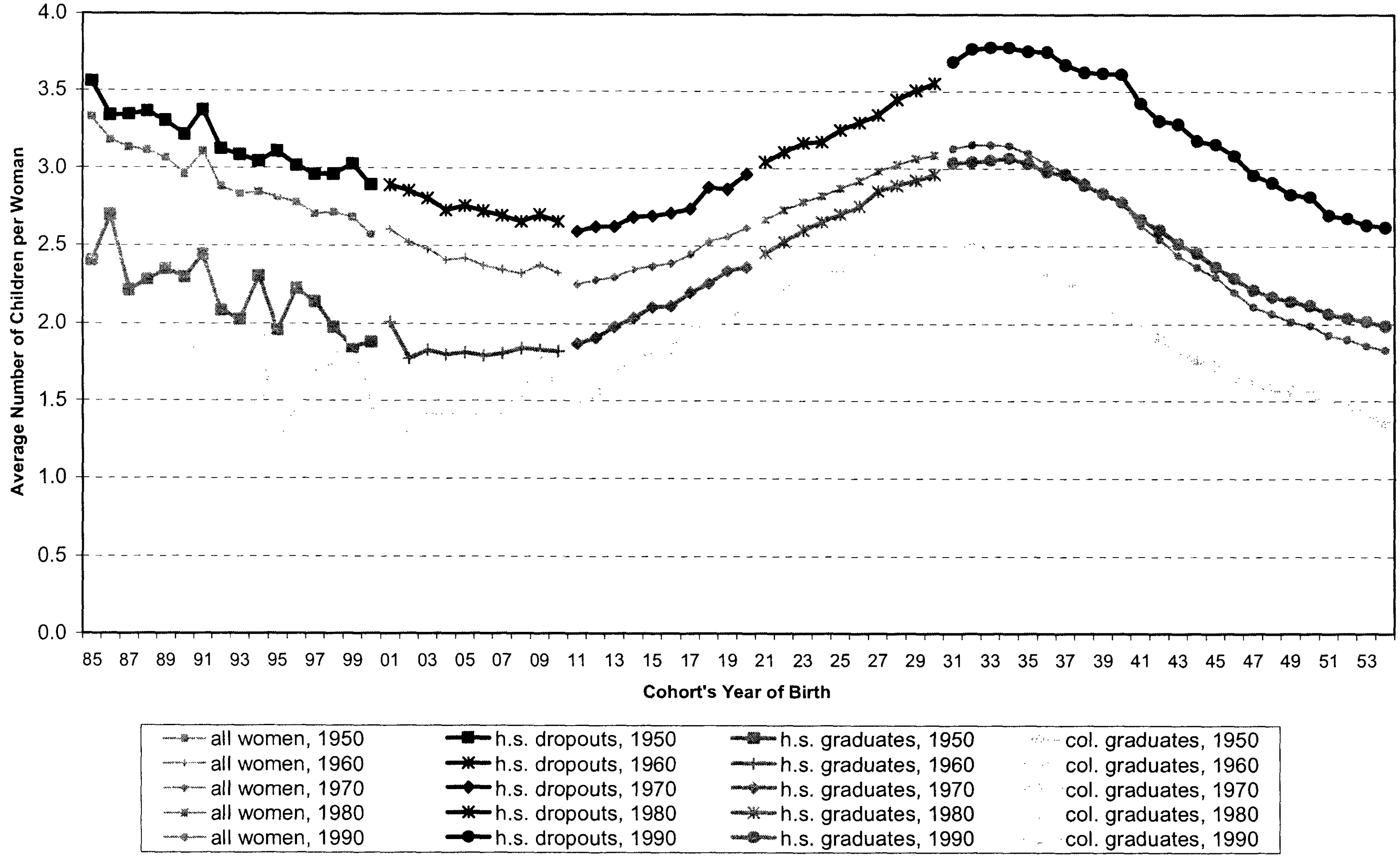

***Figure 2a*: Age-Specific Fertility Rates, by Education: 1960**
Source: Author's calculations from 1960 Census IPUMS

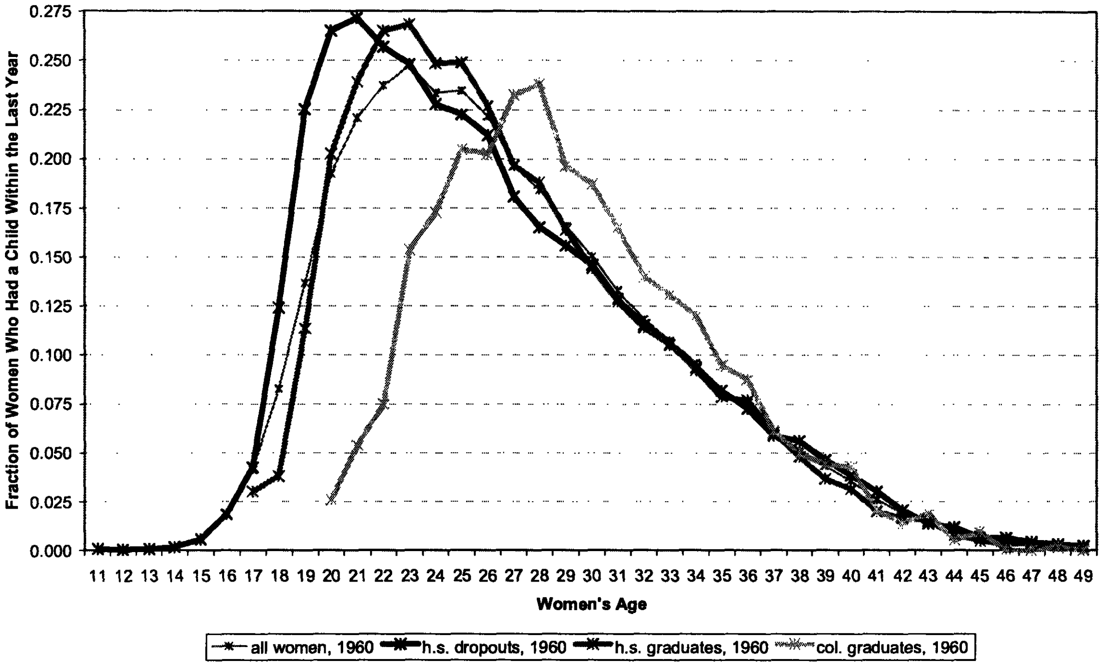


***Figure 2b*: Age-Specific Fertility Rates, by Education: 1990**
Source: Author's calculations from 1990 Census IPUMS

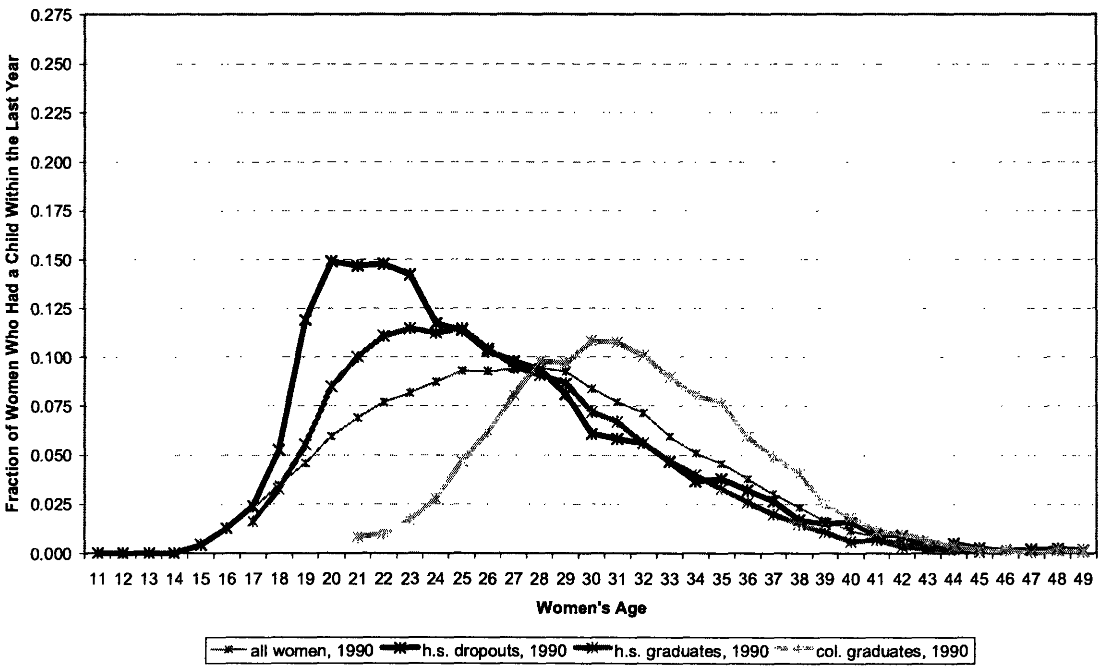

***Figure 3a*: 2SLS Weights and OLS Weights**
Instruments: Child Labor Laws

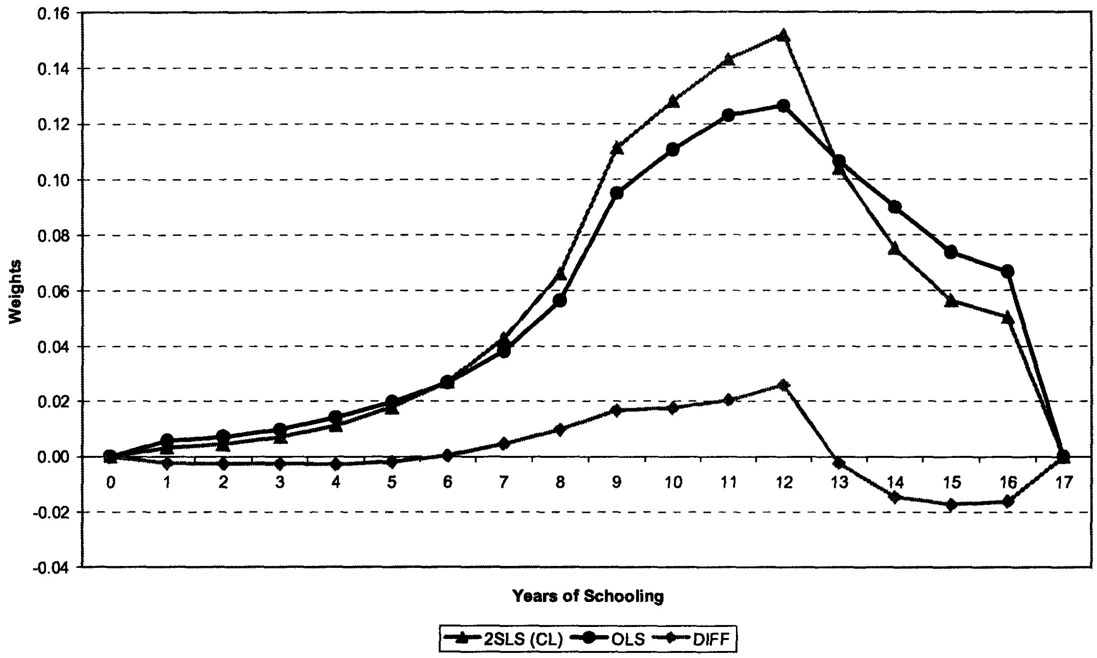


***Figure 3b*: 2SLS Weights and OLS Weights**
Instruments: Compulsory Attendance Laws

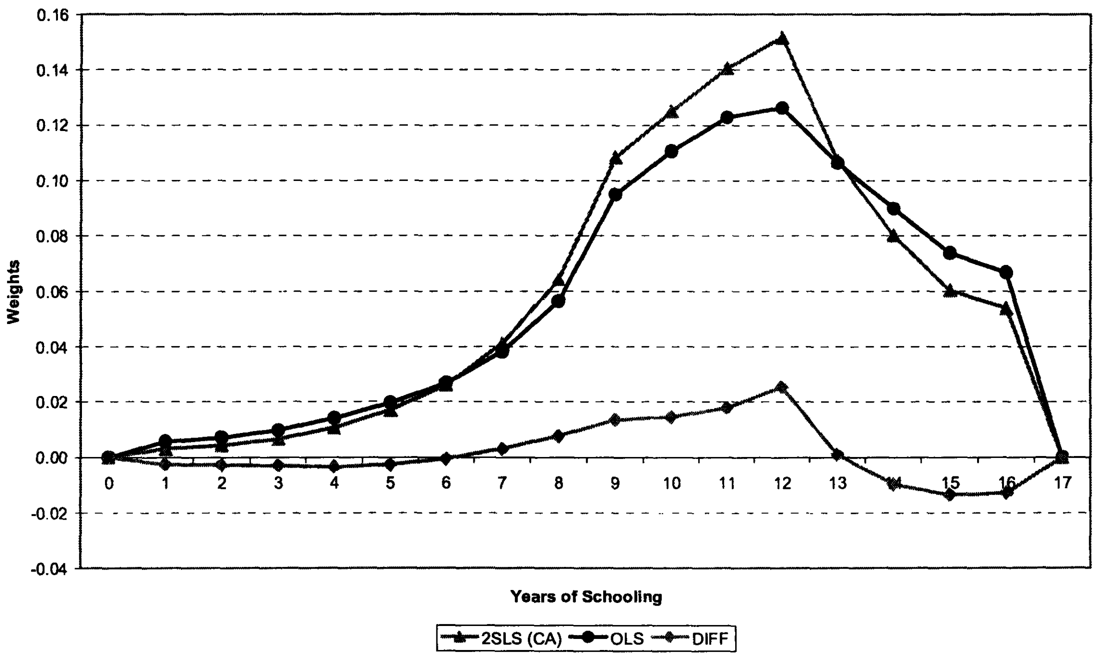

**Table 2.1**
Descriptive Statistics for the Census IPUMS extraction

| Variables | 1950-90 | 1950 | 1960 |
|---|---|---|---|
| | *Dependent Variable* | | |
| #Children Ever Born (Completed Fertility) | 2.53 (1.95) | 2.21 (2.26) | 2.31 (2.00) |
| Childless | .154 (.361) | .252 (.434) | .194 (.396) |
| Ever Married | .948 (.222) | .928 (.258) | .944 (.230) |
| | *Regressor* | | |
| Years of Schooling | 11.56 (2.89) | 9.96 (3.24) | 10.61 (2.89) |
| | *Covariates* | | |
| Age | 44.37 (2.87) | 44.23 (2.89) | 44.33 (2.85) |
| | *Instruments* | | |
| Percent Child Labor 6 or less | .1801 | .4805 | .2267 |
| Percent Child Labor 7 | .2753 | .4498 | .3731 |
| Percent Child Labor 8 | .3759 | .0698 | .3487 |
| Percent Child Labor 9+ | .1686 | .0000 | .0515 |
| Percent Compulsory Attendance 8 or less | .2642 | .6066 | .3507 |
| Percent Compulsory Attendance 9 | .4445 | .3753 | .5225 |
| Percent Compulsory Attendance 10 | .0650 | .0181 | .0605 |
| Percent Compulsory Attendance 11+ | .2263 | .0000 | .0663 |
| N | 888,420 | 23,315 | 93,743 |

**NOTE**: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white women born in the US (except Alaska and Hawaii) and aged 40-49 in the Census Year. Standard deviations are in parentheses. All other entries are means.

**Table 2.1 (continued)**
Descriptive Statistics for the Census IPUMS extraction

| Variables | 1970 | 1980 | 1990 |
|---|---|---|---|
| | *Dependent Variable* | | |
| #Children Ever Born (Completed Fertility) | 2.80<br>(2.02) | 2.94<br>(1.86) | 2.16<br>(1.45) |
| Childless | .125<br>(.330) | .101<br>(.302) | .148<br>(.355) |
| Ever Married | .985<br>(.208) | .960<br>(.195) | .944<br>(.230) |
| | *Regressor* | | |
| Years of Schooling | 11.38<br>(2.65) | 12.10<br>(2.51) | 13.19<br>(2.34) |
| | *Covariates* | | |
| Age | 44.55<br>(2.86) | 44.46<br>(2.91) | 44.13<br>(2.85) |
| | *Instruments* | | |
| Percent Child Labor 6 or less | .1942 | .0533 | .0295 |
| Percent Child Labor 7 | .2445 | .2408 | .1667 |
| Percent Child Labor 8 | .4956 | .4141 | .3687 |
| Percent Child Labor 9+ | .0657 | .2917 | .4351 |
| Percent Compulsory Attendance 8 or less | .2524 | .1190 | .1115 |
| Percent Compulsory Attendance 9 | .4403 | .4374 | .4399 |
| Percent Compulsory Attendance 10 | .0783 | .0894 | .0585 |
| Percent Compulsory Attendance 11+ | .2289 | .3542 | .3902 |
| N | 194,279 | 454,712 | 122,371 |

**Table 2.2**
Total Completed Fertility by Educational Attainment

| | 1950-90 | 1950 | 1960 | 1970 | 1980 | 1990 |
|---|---|---|---|---|---|---|
| All women | 2.53 | 2.21 | 2.31 | 2.80 | 2.94 | 2.16 |
| College graduates | 1.93 | 1.28 | 1.77 | 2.39 | 2.29 | 1.67 |
| Some College | 2.31 | 1.68 | 1.99 | 2.69 | 2.78 | 2.10 |
| High School Graduates | 2.48 | 1.69 | 2.06 | 2.67 | 2.90 | 2.29 |
| High School Dropouts | 2.89 | 2.60 | 2.62 | 3.09 | 3.45 | 2.82 |

**NOTE**: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white women born in the US (except Alaska and Hawaii) and aged 40-49 in the Census Year. All entries are means. 'College Graduates' is defined as having completed 16 or more years of schooling; 'Some College' as having completed more than 12 but less than 16 years of schooling; 'High School Graduates' as having completed exactly 12 years of schooling, and 'High School Dropouts' as having completed less than 12 years of schooling.

**Table 2.3**
OLS Estimates of the Effect of Schooling on Total Completed Fertility

| | 1950-90 (1) | 1950-90 (2) | 1950-80 (3) | 1950 (4) | 1960 (5) | 1970 (6) | 1980 (7) | 1990 (8) |
|---|---|---|---|---|---|---|---|---|
| Years of Schooling | -.131<br>(.002) | -.128<br>(.002) | -.131<br>(.002) | -.166<br>(.006) | -.133<br>(.005) | -.104<br>(.005) | -.138<br>(.003) | -.131<br>(.003) |
| State of Residence Main Effects | No | Yes | Yes | No | No | No | No | No |
| R-squared | .074 | .078 | .068 | .086 | .055 | .034 | .048 | .076 |
| N | 888,420 | 888,420 | 766,049 | 23,315 | 93,743 | 194,279 | 454,712 | 122,371 |

**NOTE**: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to women aged 40-49 in the Census Year. Standard errors corrected for state-year clustering are shown in parentheses. Entries are estimates of the effect of years of schooling on the measure of Total Completed Fertility, namely the discrete choice variable 'Children Ever Born'. All regressions contain Census year, year of birth and state of birth main effects.

**Table 2.4**
Compulsory Schooling Laws as Instruments for Years of Schooling: First-Stage Estimates

| Variables | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | | | *Child Labor Laws* | | | |
| CL7 (Percent Child Labor 7) | .175 (.028) | .184 (.027) | | | .155 (.030) | .164 (.030) |
| CL8 (Percent Child Labor 8) | .163 (.027) | .174 (.026) | | | .137 (.029) | .148 (.029) |
| CL9 (Percent Child Labor 9+) | .386 (.034) | .385 (.034) | | | .336 (.037) | .340 (.037) |
| | | | *Compulsory Attendance Laws* | | | |
| CA9 (Percent Compulsory Attendance 9) | | | .079 (.023) | .092 (.023) | .024 (.023) | .034 (.023) |
| CA10 (Percent Compulsory Attendance 10) | | | .126 (.031) | .118 (.031) | .094 (.033) | .086 (.033) |
| CA11 (Percent Compulsory Attendance 11+) | | | .222 (.029) | .208 (.028) | .115 (.031) | .099 (.031) |
| State of Residence Main Effects | No | Yes | No | Yes | No | Yes |
| F-statistic (p-value) | 47.02 (.000) | 45.29 (.000) | 20.61 (.000) | 18.09 (.000) | 27.52 (.000) | 25.43 (.000) |
| R-squared | .171 | .178 | .171 | .177 | .171 | .178 |

**NOTE**: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white women born in the US (except Alaska and Hawaii) and aged 40-49 in the Census Year. Standard errors corrected for state-of-birth/year-of-birth clustering are shown in parentheses. All regressions contain Census year, year of birth and state of birth main effects. The sample size is 888,420.

**Table 2.5**
Effects of Compulsory Schooling Laws on Discrete Levels of Schooling

| Variables | Completed 8+ Years of Schooling (1) | Completed 10+ Years of Schooling (2) | Completed 12+ Years of Schooling (3) | Completed 14+ Years of Schooling (4) | Completed 16+ Years of Schooling (5) |
|---|---|---|---|---|---|
| Dependent Variable Mean | .922 | .777 | .652 | .186 | .115 |
| | | | *A. Child Labor Laws* | | |
| CL7 (Percent Child Labor 7) | .031 (.004) | .023 (.004) | .015 (.005) | .004 (.003) | .002 (.002) |
| CL8 (Percent Child Labor 8) | .037 (.004) | .025 (.004) | .026 (.005) | -.008 (.004) | -.010 (.003) |
| CL9 (Percent Child Labor 9+) | .064 (.005) | .057 (.005) | .057 (.006) | -.005 (.005) | -.006 (.004) |
| F-statistic (p-value) | 60.02 (.0000) | 44.28 (.0000) | 35.84 (.0000) | 6.08 (.0004) | 10.08 (.0001) |
| R-squared | .085 | .135 | .146 | .048 | .050 |
| | | | *B. Compulsory Attendance Laws* | | |
| CA9 (Percent Compulsory Attendance 9) | .029 (.003) | .012 (.003) | .019 (.004) | -.009 (.003) | -.009 (.002) |
| CA10 (Percent Compulsory Attendance 10) | .018 (.004) | .024 (.006) | .039 (.006) | -.005 (.003) | -.007 (.002) |
| CA11 (Percent Compulsory Attendance 11+) | .025 (.004) | .044 (.006) | .051 (.005) | -.003 (.004) | -.007 (.003) |
| F-statistic (p-value) | 27.48 (.0000) | 26.04 (.0000) | 34.81 (.0000) | 4.32 (.0048) | 6.04 (.0004) |
| R-squared | .084 | .135 | .146 | .048 | .050 |

**NOTE:** The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white women born in the US (except Alaska and Hawaii) and aged 40-49 in the Census Year. Standard errors corrected for state-of-birth/year-of-birth clustering are shown in parentheses. All regressions contain Census year, year of birth, state of birth and state of residence main effects. The sample size is 888,420.

**Table 2.6**
Effects of Compulsory Schooling Laws on Years of Schooling: Robustness Checks

| Variables | Excluding 1950<br>(1) | Excluding 1960<br>(2) | Excluding 1970<br>(3) | Excluding 1980<br>(4) | Excluding 1990<br>(5) |
|---|---|---|---|---|---|
| | | | *A. Child Labor Laws* | | |
| CL7 | .133 | .203 | .239 | .180 | .153 |
| (Percent Child Labor 7) | (.025) | (.033) | (.033) | (.030) | (.029) |
| CL8 | .154 | .181 | .204 | .191 | .121 |
| (Percent Child Labor 8) | (.023) | (.030) | (.036) | (.031) | (.029) |
| CL9 | .344 | .374 | .473 | .425 | .259 |
| (Percent Child Labor 9+) | (.031) | (.037) | (.041) | (.043) | (.040) |
| F-statistic | 46.28 | 37.10 | 51.50 | 34.30 | 15.54 |
| (p-value) | (.000) | (.000) | (.000) | (.000) | (.000) |
| R-squared | .156 | .181 | .224 | .189 | .113 |
| | | | *B. Compulsory Attendance Laws* | | |
| CA9 | .107 | .097 | .148 | .087 | .027 |
| (Percent Compulsory Attendance 9) | (.021) | (.026) | (.029) | (.026) | (.025) |
| CA10 | .155 | .064 | .114 | .130 | .103 |
| (Percent Compulsory Attendance 10) | (.026) | (.035) | (.045) | (.036) | (.036) |
| CA11 | .229 | .180 | .307 | .206 | .133 |
| (Percent Compulsory Attendance 11+) | (.025) | (.031) | (.039) | (.032) | (.033) |
| F-statistic | 27.71 | 13.43 | 21.54 | 13.61 | 6.78 |
| (p-value) | (.000) | (.000) | (.000) | (.000) | (.000) |
| R-squared | .156 | .180 | .223 | .188 | .113 |
| N | 865,105 | 794,677 | 694,141 | 433,708 | 766,049 |

**NOTE:** The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to white women born in the US (except Alaska and Hawaii) and aged 40-49 in the Census Year. Standard errors corrected for state-of-birth/year-of-birth clustering are shown in parentheses. All regressions contain Census year, year of birth and state of birth main effects.

**Table 2.7**
2SLS Estimates of the Effect of Schooling on Total Completed Fertility

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Instruments | CL | CL | CA | CA | CL & CA | CL & CA |
| Years of Schooling | -.327<br>(.065) | -.344<br>(.066) | -.264<br>(.085) | -.295<br>(.091) | -.302<br>(.058) | -.330<br>(.061) |
| State of Residence Main Effects | No | Yes | No | Yes | No | Yes |
| | | | *First Stage for Schooling* | | | |
| CL 7 | .175<br>(.028) | .184<br>(.027) | | | .155<br>(.030) | .164<br>(.030) |
| CL 8 | .163<br>(.027) | .174<br>(.026) | | | .137<br>(.029) | .148<br>(.029) |
| CL 9 | .386<br>(.034) | .385<br>(.034) | | | .336<br>(.037) | .340<br>(.037) |
| CA 9 | | | .079<br>(.023) | .092<br>(.023) | .024<br>(.023) | .034<br>(.023) |
| CA 10 | | | .126<br>(.031) | .118<br>(.031) | .094<br>(.033) | .086<br>(.033) |
| CA 11 | | | .222<br>(.029) | .208<br>(.028) | .115<br>(.031) | .099<br>(.031) |

NOTE: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to women aged 40-49 in the Census Year. Standard errors corrected for state-year clustering are shown in parentheses. Entries are two-stage least squares estimates of the effect of years of schooling on the measure of Total Completed Fertility, namely the discrete choice variable 'Children Ever Born', using the excluded instruments indicated, i.e.: a set of dummies indicating state and year specific labor and school attendance laws that were in effect in the state of birth of the individual at age 14. All regressions contain Census year, year of birth and state of birth main effects. The sample size for all columns is 888,420.

**Table 2.8**
OLS and 2SLS Estimates of the Effect of High School Graduation on Total Completed Fertility

| | OLS (1) | OLS (2) | 2SLS (3) | 2SLS (4) | 2SLS (5) | 2SLS (6) | 2SLS (7) | 2SLS (8) |
|---|---|---|---|---|---|---|---|---|
| Instruments | | | CL | CL | CA | CA | CL & CA | CL & CA |
| High School Graduate (Completed 12+ years of schooling) | -.635<br>(.013) | -.624<br>(.012) | -1.002<br>(.352) | -1.163<br>(.370) | -.943<br>(.346) | -.994<br>(.362) | -.897<br>(.269) | -.900<br>(.286) |
| State of Residence Main Effects | No | Yes | No | Yes | No | Yes | No | Yes |
| R-squared | .074 | .078 | | | | | | |
| | | | *First Stage for High School Graduation* | | | | | |
| CL 7 | | | .017<br>(.005) | .015<br>(.005) | | | .010<br>(.005) | .007<br>(.005) |
| CL 8 | | | .027<br>(.005) | .026<br>(.005) | | | .018<br>(.005) | .016<br>(.005) |
| CL 9 | | | .056<br>(.006) | .057<br>(.006) | | | .041<br>(.006) | .040<br>(.006) |
| CA 9 | | | | | .018<br>(.004) | .019<br>(.004) | .012<br>(.004) | .013<br>(.004) |
| CA 10 | | | | | .035<br>(.006) | .039<br>(.006) | .033<br>(.006) | .035<br>(.006) |
| CA 11 | | | | | .049<br>(.005) | .051<br>(.005) | .037<br>(.006) | .038<br>(.005) |

**NOTE**: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to women aged 40-49 in the Census Year. Standard errors corrected for state-year clustering are shown in parentheses. Entries are estimates of the effect of high school graduation on of Total Completed Fertility, namely the discrete choice variable 'Children Ever Born'. High school graduation is defined as a binary variable that equals one if the individual completed 12 or more years of schooling, and zero otherwise. Entries in the 2SLS columns are two-stage least squares estimates using the excluded instruments indicated, i.e.: a set of dummies indicating state and year specific labor and school attendance laws that were in effect in the state of birth of the individual at age 14. All regressions contain Census year, year of birth and state of birth main effects. The sample size for all columns is 888,420.

**Table 2.9**
OLS and 2SLS Estimates of the Effect of Years of High School on Total Completed Fertility

| | OLS (1) | OLS (2) | 2SLS (3) | 2SLS (4) | 2SLS (5) | 2SLS (6) | 2SLS (7) | 2SLS (8) |
|---|---|---|---|---|---|---|---|---|
| Instruments | | | CL | CL | CA | CA | CL & CA | CL & CA |
| Years of High School | -.218 (.005) | -.214 (.004) | -.375 (.092) | -.387 (.093) | -.228 (.094) | -.212 (.097) | -.264 (.073) | -.265 (.075) |
| State of Residence Main Effects | No | Yes | No | Yes | No | Yes | No | Yes |
| R-squared | .068 | .072 | | | | | | |

**NOTE**: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to women aged 40-49 in the Census Year. Standard errors corrected for state-year clustering are shown in parentheses. Entries are estimates of the effect of completed years of high school on Total Completed Fertility, namely the discrete choice variable 'Children Ever Born'. Entries in the 2SLS columns are two-stage least squares estimates using the excluded instruments indicated, i.e.: a set of dummies indicating state and year specific labor and school attendance laws that were in effect in the state of birth of the individual at age 14. All regressions contain Census year, year of birth and state of birth main effects. The sample size for all columns is 888,420.

**Table 2.10**
OLS and 2SLS Estimates of the Effect of Schooling on Marital Status and Childlessness

| | OLS (1) | OLS (2) | 2SLS (3) | 2SLS (4) | 2SLS (5) | 2SLS (6) | 2SLS (7) | 2SLS (8) |
|---|---|---|---|---|---|---|---|---|
| | | | | *A. Probability of Marriage* | | | | |
| Instruments | | | CL | CL | CA | CA | CL & CA | CL & CA |
| Years of Schooling | -.0028 (.0002) | -.0028 (.0002) | .0058 (.0049) | .0059 (.0049) | .0011 (.0078) | -.0014 (.0079) | .0031 (.0046) | .0022 (.0047) |
| State of Residence Main Effects | No | Yes | No | Yes | No | Yes | No | Yes |
| R-squared | .009 | .011 | | | | | | |
| | | | | *B. Childlessness* | | | | |
| Instruments | | | CL | CL | CA | CA | CL & CA | CL & CA |
| Years of Schooling | .0091 (.0002) | .0088 (.0002) | .0061 (.0060) | .0058 (.0061) | .0178 (.0076) | .0194 (.0078) | .0087 (.0050) | .0086 (.0052) |
| State of Residence Main Effects | No | Yes | No | Yes | No | Yes | No | Yes |
| R-squared | .027 | .029 | | | | | | |

**NOTE**: The data are from the Census IPUMS for 1950 through 1990, with the sample restricted to women aged 40-49 in the Census Year. Standard errors corrected for state-year clustering are shown in parentheses. Entries in Panel A are estimates of the effect of years of schooling on the probability of marriage, constructed as a binary variable that equals one if the women was ever married, and zero otherwise. Entries in Panel B are estimates of the effect of years of schooling on childlessness, defined as a binary variable that equals one if the women never had any children, and zero otherwise. Entries in the 2SLS columns are two-stage least squares estimates using the excluded instruments indicated, i.e.: a set of dummies indicating state and year specific labor and school attendance laws that were in effect in the state of birth of the individual at age 14. All regressions contain Census year, year of birth and state of birth main effects. The sample size for all columns is 888,420.

**Table 2.11**
Implied changes in fertility for selected EU countries, 1960-90

| Country | Year | **Avge. Years of Schooling** | **Average Fertility Rate** | $yschool_{90}-yschool_{60}$ | $fertility_{90}-fertility_{60}$ | $\beta$*$\Delta fertility_{60-90}$ | | **% explained** | |
|---|---|---|---|---|---|---|---|---|---|
| | | | | | | $\beta$=-.264 | $\beta$=-.353 | $\beta$=-.264 | $\beta$=-.353 |
| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Spain | 1960 | **4.16** | **2.9** | | | | | | |
| Spain | 1970 | **4.31** | **2.9** | | | | | | |
| Spain | 1980 | **4.75** | **2.2** | | | | | | |
| Spain | 1990 | **6.05** | **1.3** | 1.74 | -1.6 | -0.459 | -0.614 | **28.71** | **38.39** |
| Portugal | 1960 | **1.53** | **3.2** | | | | | | |
| Portugal | 1970 | **1.92** | **3.0** | | | | | | |
| Portugal | 1980 | **2.84** | **2.2** | | | | | | |
| Portugal | 1990 | **3.26** | **1.6** | 1.34 | -1.4 | -0.354 | -0.473 | **25.27** | **33.79** |
| Ireland | 1960 | **6.67** | **3.8** | | | | | | |
| Ireland | 1970 | **6.61** | **3.9** | | | | | | |
| Ireland | 1980 | **7.65** | **3.3** | | | | | | |
| Ireland | 1990 | **8.13** | **2.1** | 1.52 | -1.8 | -0.401 | -0.537 | **22.29** | **29.81** |
| Greece | 1960 | **3.51** | **2.3** | | | | | | |
| Greece | 1970 | **4.43** | **2.4** | | | | | | |
| Greece | 1980 | **5.79** | **2.2** | | | | | | |
| Greece | 1990 | **6.36** | **1.4** | 1.93 | -1.0 | -0.510 | -0.681 | **50.95** | **68.13** |
| Germany | 1960 | **7.76** | **2.4** | | | | | | |
| Germany | 1970 | **8.03** | **2.0** | | | | | | |
| Germany | 1980 | **8.28** | **1.6** | | | | | | |
| Germany | 1990 | **8.45** | **1.5** | 0.42 | -0.5 | -0.111 | -0.148 | **22.18** | **29.65** |
| Italy | 1960 | **4.2** | **2.4** | | | | | | |
| Italy | 1970 | **4.79** | **2.4** | | | | | | |
| Italy | 1980 | **4.77** | **1.6** | | | | | | |
| Italy | 1990 | **5.67** | **1.3** | 0.88 | -1.1 | -0.232 | -0.311 | **21.12** | **28.24** |

**NOTE**: The fertility data come from Eurostat, as reported in E. Phillip Davis 'Population Aging and Retirement Income Provision in the European Union' (1998). The education data come from the Barro-Lee dataset ['see Barro, Robert and J.W. Lee, "International Measures of Schooling Years and Schooling Quality, AER, Papers and Proceedings, 86(2), pp. 218-223] which includes estimates of average schooling years in the female population aged 25+ for 126 countries in the world.

# 3. Family Allowances and Female Labor Force Participation

## 3.1. Introduction

In recent years, public policies aimed at facilitating the reconciliation of women's working life and family life have been implemented by many governments in OECD countries as part of a broader agenda to further all aspects of equal opportunities. In some cases, particularly in Europe, these measures also address a growing concern for low fertility rates and the subsequent threat they pose to the sustainability of public pension systems. Family allowances and child care subsidies are two such policies. They both aim at reducing the costs of child rearing to the family, and they both should, if implemented, induce an increase in the number of women who decide to have children. Their effects on the current size of the labor market, however, will theoretically differ.

The theory section of this paper uses a simple model of household production of child care to highlight the distinct nature of these transfers and their different effects on the labor market. While child care subsidies are awarded to working mothers, and therefore can only have a non-negative effect on labor force participation, receipt of family allowances is not conditional on employment status. If households pool all sources of income together, then increasing family allowances should raise unearned income and hence encourage women to reduce their labor supply and even to drop out of the labor force.[45] Such a claim is often made, however, without any evidence to support it. Empirical estimates of these effects are therefore necessary to inform any choice between these two different policy instruments.

The empirical literature has largely focused on the US, where universal family allowances do not exist, and mostly overlooked other countries. Partly as a result of that, the role of child allowances on labor market outcomes has not been investigated yet. Several studies have used US and Canada data to analyze the effect of child care costs on labor market outcomes of married women.[46] Blau and Robins (1988), Connelly (1992),

[45] There is evidence, however, of some kind of intrahousehold 'flypaper effect' (see Jacobi 1997). In other words, if households tend to use family allowances to purchase goods and services associated with raising children (i.e.: inputs in the production of child quality), then these allowances will not have a negative income effect on labor supply --they might even increase it. Therefore, allowances should be treated separately from other sources of income in the household model.

[46] See Blau (2000) for a detailed recent review. He argues that the most reliable estimates (corresponding to those studies which include both paid and informal child care, and which do not assume that paid care is always the best option) point to effects that are fairly small.

Blau (1995), Ribar (1995), Blau and Hagy (1998), and Lemke et al. (2000), among others, provide evidence suggesting that a reduction in the price of child care will increase the probability of maternal employment and labor force participation, even though the range of estimates is quite large. Moreover, this type of studies does not directly measure subsidies and their impact, but rather make an inference from the estimated price effects. More direct evidence on the employment effects of child care subsidies is provided by evaluations of actual child care subsidy programs. These include Berger and Black (1992), who use data on two Kentucky programs passed in 1989 to estimate that maternal employment rises by 8 to 25 percentage points as a result of the introduction of a $46 weekly subsidy; and Gelbach (1999), who uses quarter-of-birth as an instrument in the 1980 US Census to estimate that access to free public kindergarten school increased employment of single mothers by four to five percentage points. Meyers, Heintze and Wolf (2000) also found, using data on Californian welfare recipients in 1992-95, a positive relationship between receipt of child care subsidies and employment. Unlike child care subsidies, however, the effects of family allowances on employment have not been studied in the literature.

The empirical analysis in this paper attempts to address that question by looking at the labor force status of women receiving different levels of family allowances before and after a reform of such program in the United Kingdom. The estimation uses data from the British Household Panel for 1998-2000. These data are useful for the purpose of this paper because the BHP includes comprehensive information on employment status and receipt of all kinds of benefits for thousands of women sampled before and after the policy changes. The data show no statistically significant effects of family allowances on labor force participation of women. Even if the results were to be taken at face value, they would imply the effects are negative but quantitatively very small.

The remainder of the paper is organized as follows. Section II begins by developing a simple model of the household choice of labor supply, fertility and home production of child care, emphasizing the predicted effects on labor market outcomes of different policies aimed at increasing fertility. Section III describes the data sources and the identification strategy used in the empirical analysis. Section IV presents and interprets the estimation results. Finally, Section V concludes by summarizing the main findings and suggesting directions for future research.

## 3.2. Background

Family allowances, also commonly referred to as child benefits, are a regular (usually monthly or weekly) government cash payment given to parents depending on the presence, number, and sometimes the age and ordinal position of children in the family. These benefits are commonly modest (on average, less than 10 percent of mean wages), but sometimes contribute a significant component of family income, especially for large or low-income families. Coverage is generally extended to children from the time of birth to the age of majority or completion of formal education, provided other eligibility criteria are met. Currently 88 countries in the world provide family allowances, in some cases also supplemented by birth grants, school grants, special supplements for single parents, or supplements for disabled children.[47]

Family allowance programs were initially motivated mostly by pro-natalist objectives. They originated in the private sector late in the 19th century, partly in response to proposals to relate workers' wages to their family responsibilities. Therefore, they were originally available as an employment-based benefit only, and limited to families with at least one wage earner. Nowadays, these programs mainly aim at equalizing the financial burden of those families with and those without children (horizontal equity), and at redistributing resources from the well off to the poor and thus indirectly reducing the rate of child poverty (vertical equity). Coverage is universal in many countries, regardless of parental income (Austria, Denmark, Finland, France, Germany, Ireland, Luxembourg, the Netherlands, Norway, Sweden, United Kingdom); however, some have means-tested benefits (Australia, Canada, Greece, Iceland, Italy, Japan, New Zealand, Portugal and Spain), and few still restrict benefits to families with at least one employed parent (Belgium, Greece). In some cases, although there is a universal level of benefits, a supplement is available to low-income families (Austria), or a means-tested premium is provided to single-parent households (Ireland, France).

[47] See full list with details in the Spring issue brief of The Clearinghouse on International Developments in Child, Youth and Family Policies (2002).

Benefit levels, not unlike coverage and eligibility rules, also vary greatly across nations. Moreover, the rate per child is in some cases uniform, regardless of the total number of children in the family (Australia, Spain, Norway, Sweden), while in other cases the rate is larger for later children or even increasing for each additional child (Italy, Belgium, Germany, Luxembourg, France).[48] Southern European countries (Spain, Portugal, Greece, Italy) have particularly low levels of benefits, even relative to other countries. In the case of Spain, however, current talk of reforms suggests that both the coverage and the level of generosity of the system are likely to increase in the near future. This is in line with the Presidency Conclusions of the Lisbon European Council in March 2000, which emphasized the reduction of poverty and social exclusion as one of the key common social objectives within the European Union, and identified children as one of the specific target groups for priority action. Many Member States are now paying particular attention to the position of children living in poor households and have been considering the implementation of more generous family allowance plans. An analysis of the effects of this type of programs should help inform such decisions.

Before moving on to the description of the British case, it is important to underline that the United States has no family allowance program. That probably explains why there has been no attention directed at evaluating such programs, whereas the literature on child care subsidy programs is already considerable.[49]

[48] In France, moreover, a family becomes eligible for an allowance only after the second child is born.

[49] For a full picture of how countries provide additional income to families with children, however, both tax and cash benefits should be considered. Historically, many countries have also allowed tax deductions (which are regressive, since they are subtracted from the taxable income base before calculating tax liability, and hence the value of the benefit depends on the marginal tax rate) or tax credits (which are a reduction in tax liability, but also benefit more affluent families unless the credits are refundable) to families with children. In recent years, there has been an increased use of the tax system, with some countries supplementing their allowances by targeted child or family tax benefits. Some nations have even substituted specially targeted tax benefits for family allowances. In the United States, for example, a combination of non-refundable and partially refundable tax credits exist, but the country does not yet have a universal benefit for all families with children. Only Canada and Israel (and also the UK, after the period studied in this paper) provide a refundable tax credit that effectively works as a cash payment to low-income families with children –in that case, the administration of family allowances has shifted from their social welfare system to their tax ministry, but the tax benefit continues to be a cash benefit.

***Family allowances in the United Kingdom***

The Child Benefit[50] program in the United Kingdom is a universal cash benefit paid to families with children. It is weighted towards the first child, with the rate for subsequent children being lower than the main rate. Neither of these rates depends on the age of the child. Child Benefit is normally paid for children up to the age of 16[51]. The payments are made weekly to the mother or to the parent the child is living with.

A policy reform was introduced starting in 1999, aiming at improving the financial position of most families with children. That year saw a large increase in child benefit for the eldest child. As shown in Table 3.1, weekly family allowances were increased for the first or only child from £11.45 in April 1998 to £14.40 in April 1999 (an increase of £2.50 plus indexation, or 20% in real terms) to £15.00 in April 2000 (up 3% in real terms), while the increases in benefits for subsequent children were modest, only to keep up with price inflation (£9.30 to £9.60 to £10 in the same period). Therefore, households faced different absolute and relative increases in family allowances according to the number of children they had, which provides some degree of cross-sectional as well as time variation in the level of family benefits.

In addition, a lone-parent premium of £5.65 was being withdrawn for new claims since July 6th, 1998 –thus effectively reducing child benefits for this group. Thus, single parents who had been previous recipients of child benefits still received a high £17.10 allowance for the first child in 1998, while "new" single parents received the same as two-parent families (£11.45).

As part of the set of measures to help families with children, the 1999 Budget also included a tax credit available to all families with dependent children, the Children Tax Credit. This program, which replaced the less generous married couple's allowance and additional personal allowance programs, was only introduced in April 2001. Therefore, an analysis limited to the years 1998 to 2000 of the effects on female labor force participation of the Child Benefit program will not be confounded by the simultaneous

[50] Child Benefit replaced an old program, Family Allowance, in March 1977. Family Allowance was payable only to the second and subsequent children. Also, an old program called One Parent Benefit was incorporated (as a lone-parent premium) into Child Benefit as of March 1997.

[51] If a child, over 16, is in full-time non-advanced education (i.e.: up to A-level or NVQ level 3 standard) at a recognized educational establishment, benefit may be paid for them until they reach age 19. Exceptionally, Child Benefit can also be paid for a short period for 16 or 17 year olds who have just left school and are registered for work or work based training for young people.

presence of a tax credit program, while still being able to exploit the change in rules and benefit levels for different groups as an identification strategy.

## 3.3. Theoretical Model

The decision to enter, reenter or remain in the labor market for a woman with children is strongly linked with participating in the market for child care services. Hence, the cost of child care must be an important determinant of women's labor force participation and labor supply decisions. A simple static model of labor supply, fertility and the home production of child care will show that, other things equal, a higher cost of market child care will result in a lower probability of employment for the household member that specializes in child care –typically the wife. Its impact on the hours of labor supplied in the market is theoretically ambiguous and will depend on the relative strength of the income and substitution effects.

Policies aimed at reducing the family's costs of child care should encourage labor force participation of non-working women and possibly induce an increase in the labor supply of female workers too. If these policies are successful in decreasing the private cost (shadow price) of having children, they should also result in an increase in the number of women who decide to have children. In countries with relatively low levels of female attachment to the labor force, and where fertility rates are very low and in some cases still decreasing, policymakers are naturally considering the adoption or strengthening of such policies. Their success will have an impact, to some extent, on the current and future size of the labor force, which in turn will determine the sustainability of publicly funded, Pay-As-You-Go Social Security systems.

In order to discuss the effects on labor market outcomes (and on fertility) of policy instruments such as family allowances and childcare subsidies, I will develop a simple framework (loosely) based on the Gronau (1977) model of home production, where market goods and household time are viewed, much in the spirit of Becker (1965), as mere inputs in the production of those activities (or 'commodities', in his terminology) that provide some (dis)utility to the agents.

For simplicity, let us consider a full-certainty, one-period model of a one-person household.[52] This household is assumed to have preferences defined over market work ($Z_W$), child services ($Z_C$) and leisure activities ($Z_L$). Each one of these commodities (activities) is 'produced' using a combination of goods ($X_1$,..., $X_k$) and time inputs ($T_1$,..., $T_m$):

$$Z_i = f_i(X_1,\ldots,X_k,T_1,\ldots,T_m) \ , \qquad i = \{W, L, C\} \tag{3-1}$$

I will assume that market work ($Z_W$) requires some time spent working or commuting to the workplace ($T_W$), the purchase of child care services in the market ($X_1$), and the purchase of other goods and services associated with work in the market, yet unrelated to having children or enjoying leisure, such as transportation ($X_3$). Child services ($Z_C$) are a combination of market child care services ($X_1$), time spent in home production of child care ($T_C$) and other goods and services associated with having children, but not with market work or leisure, such as diapers or child clothes ($X_2$). Finally, leisure ($Z_L$) is the result of time spent in leisure activities ($T_L$) and the goods and services consumed during that time ($X_4$), which for simplicity are assumed to be different from those involved in the production of either $Z_C$ or $Z_W$. Formally, this means the production technology equations in (1) can be rewritten as follows:

$$Z_W = f_W(X_1,X_3,T_W) \ , \tag{3-1-W}$$

$$Z_C = f_C(X_1,X_2,T_C) \ \text{, and} \tag{3-1-C}$$

$$Z_L = f_L(X_4,T_L) \quad . \tag{3-1-L}$$

Note that I am assuming that no good $X_i$ or time slot $T_i$ is used as an input in the production of more than one commodity or activity $Z_i$, except for market child care $X_1$. The case of $X_1$ is a departure from the the usual Becker approach in that the same amount

[52] I will derive the implications of this model under the assumption that the wife is the one member of the household who specializes in home production of child care services. Therefore, when the model predicts any effect of a given policy change on labor force participation and labor supply, it will be implicitly discussing female labor force participation and female labor supply.

of the good is simultaneously used as an input in the production of both $Z_C$ and $Z_W$, and hence its cost cannot be uniquely allocated between those two commodities.[53]

The household maximizes its welfare,

$$U = U(Z_W, Z_C, Z_L) \tag{3-2}$$

subject to two constraints: the budget constraint,

$$\sum_i^4 p_i X_i \leq wZ_W + y_0 \quad , \tag{3-3}$$

and the time constraint,

$$T_C + T_W + T_L \leq T \, , \tag{3-4}$$

where $w$ denotes the wage rate, $T$ is the total time available and $y_0$ stands for non-labor sources of income.[54]

The maximization of (2) subject to these constraints, given the production technology (1-$W$), (1-$C$) and (1-$L$), yields the necessary conditions for an optimum:

$$u_C = \lambda[p_1 \cdot (\partial X_1/\partial Z_C) + p_2 \cdot (\partial X_2/\partial Z_C) + \hat{w} \cdot (\partial T_C/\partial Z_C)] \equiv \lambda\hat{\pi}_C \quad , \tag{3-5}$$

$$u_W = \lambda[p_1 \cdot (\partial X_1/\partial Z_W) + p_3 \cdot (\partial X_3/\partial Z_W) + \hat{w} \cdot (\partial T_W/\partial Z_W) - w] \equiv \lambda\hat{\pi}_W, \text{ and} \tag{3-6}$$

$$u_L = \lambda[p_4 \cdot (\partial X_4/\partial Z_L) + \hat{w} \cdot (\partial T_L/\partial Z_L)] \equiv \lambda\hat{\pi}_L \quad , \tag{3-7}$$

[53] This is closer in spirit to the characteristics approach to consumer theory first developed in Lancaster (1966). This view regards goods as 'public inputs', in the sense that the marginal contribution of one good in the production of any given commodity ('characteristic', in his terminology) is completely independent of its being an input in the production of a different characteristic. Becker's approach, while being arguably less realistic, derives most of its power as an analytical tool precisely from ruling out this independence in production. The model presented here does not require that, however, since all inputs other than $X_1$ have been assumed to contribute to the production of only one commodity.

[54] If the model describes the wife's choice between work in the market and home production of child care, and if the husband's labor supply is assumed to be exogenous, then this term will include his labor earnings.

where $u_i$ $(=\partial U/\partial Z_i)$ denotes the marginal utility of commodity $Z_i$, $\lambda$ is the marginal utility of income, and $\hat{\pi}_i$ is the shadow price of commodity $Z_i$, which depends on the marginal costs of the goods and time inputs involved in the production of $Z_i$ (this is, on the marginal inputs valued at their respective prices –which are market prices $p_i$ in the case of market goods, and the shadow price of time $\hat{w} \equiv \mu/\lambda$, where $\mu$ is the marginal utility of time, in the case of time inputs).[55] The marginal rate of substitution in consumption between commodities $Z_i$ and $Z_j$ equals their shadow price ratio $(u_i/u_j = \hat{\pi}_i/\hat{\pi}_j)$.

Note that from (6), and normalizing $Z_W$ so that work is measured in time units (and hence $\partial T_W/\partial Z_W = 1$), we have that the shadow price of time $\hat{w}$ equals

$$\hat{w} = w - [p_1 \cdot (\partial X_1/\partial Z_W) + p_3 \cdot (\partial X_3/\partial Z_W)] + (u_W/\lambda). \qquad (3\text{-}8)$$

If an individual is observed working for a wage $w$ when, in order to work, she has to incur the cost of purchasing market goods $X_1$ and $X_3$ at prices $p_1$ and $p_3$ and she obtains a marginal disutility from work equal to $u_W$ (negative), then it must be that the value she places on her time is in fact lower than $w$ (this is, she would still be willing to work for a lower wage if she could do without $X_1$ and $X_3$). Alternatively, some women whose shadow price of time is equal to, or somewhat below, market wage $w$ will not be observed working because of the existence of child care costs and transportation costs, which make work in the market not worth their while. This is true even if market work does not yield any disutility to the individual.

In this approach to household consumption, where market goods are not the direct source of utility, the demand for these goods is a derived demand. It is determined by the demand for each activity or commodity and by the technology that transforms those goods into these activities. Deriving the optimal combination of inputs in the production of $Z_i$ requires solving for the cost minimization problem for each $Z_i$. In this case, however, the problem for $Z_W$ and $Z_C$ must be posed jointly, since one input is used

[55] The optimum conditions (5)-(7) are obtained by maximizing the Lagrangian

$$L = U(Z_C, Z_W, Z_L) + \lambda\left(wZ_W + y_0 - \sum_i^4 p_i X_i\right) + \mu(T - T_C - T_W - T_L)$$

with respect to $Z_W$, $Z_C$ and $Z_L$, given the production technology described by (1-*W*), (1-*C*) and (1-*L*).

simultaneously in the production of both activities and its cost cannot be allocated uniquely between them.[56]

The solution to these cost minimization problems are the familiar conditions that the marginal rate of substitution in production equals the the input price ratio:

$$\frac{\partial Z_L/\partial T_L}{\partial Z_L/\partial X_4}=\frac{\hat{w}}{p_4}, \tag{3-9}$$

$$\frac{\partial Z_C/\partial T_C}{\partial Z_C/\partial X_2}=\frac{\hat{w}}{p_2}, \text{ and} \tag{3-10}$$

$$\frac{\partial Z_W/\partial T_W}{\partial Z_W/\partial X_3}=\frac{\hat{w}}{p_3}, \tag{3-11}$$

while the solution to the problem for $Z_W$ and $Z_C$ includes a slightly adapted version of the above:

$$\frac{\partial Z_C/\partial X_1}{\partial Z_C/\partial T_C}+\frac{\partial Z_W/\partial X_1}{\partial Z_W/\partial T_W}=\frac{p_1}{\hat{w}}. \tag{3-12}$$

The demand for each good will thus depend on its price, this is, on its marginal cost of production, a key element of which is the value of time.

The solution to this model (i.e.: the equilibrium values of the twelve unknowns, namely the goods inputs $X_1$, $X_2$, $X_3$ and $X_4$, the time imputs $T_W$, $T_C$ and $T_L$, the 'commodities' $Z_W$, $Z_C$ and $Z_L$, the marginal utility of income $\lambda$ and the shadow price of time $\hat{w}$) will satisfy the utility maximization problem's first order conditions (5), (6) and (7), the cost minimization problems' first order conditions (9), (10), (11) and (12), the

[56] In this case, the cost minimization problems are: maximizing

$$-p_4X_4-\hat{w}T_L+\gamma_L\left[f_L(X_4,T_L)-Z_L^*\right]$$

with respect to $X_4$ and $T_L$, where $Z_L^*$ comes from the utility maximization program above; and maximizing:

$$-p_1X_1-p_2X_2-p_3X_3-\hat{w}T_C-\hat{w}T_W+\gamma_C\left[f_C(X_1,X_2,T_C)-Z_C^*\right]+\gamma_W\left[f_W(X_1,X_3,T_W)-Z_W^*\right]$$

with respect to $X_1$, $X_2$, $X_3$, $T_C$ and $T_W$, and where $Z_L^*$ and $Z_L^*$ also come from utility maximization.

budget constraint (3), the time constraint (4) and the production technology equations (1-*W*), (1-*C*) and (1-*L*) simultaneously.

The standard model of the theory of home production, consumption and labor supply would correspond to the particular case where the following simplifying assumptions are made: $u_W = 0$, so the household only cares about child services and leisure, and market work is simply a means to secure goods; $Z_W = T_W$ and $Z_L = T_L$, so no inputs but time are necessary in order to work in the market and in order to enjoy leisure, and $\partial Z_C / \partial X_2 = 0$ (usually $Z_C = X_1 + f(T_C)$). This extreme situation where the only possible use of resources is to buy child care services ($X_1$), the analysis of an increase in non-labor income $y_0$ is equivalent to that of a decrease in the price of child care $p_1$. Child care subsidies and family allowances will then be equivalent and have the same impact on the decision to participate in the labor market. In this simple case where $\partial Z_W / \partial X_1 = 0$, and hence work for pay is not associated with the purchase of child care services in the market, there is an effect of child care subsidies on labor force participation simply because, by reducing $p_1$ it is possible to buy more of $X_1$ with the same resources, and therefore home production of child care becomes relatively less productive. Since all the household can do with money is buy $X_1$, then the family can be better off by reducing their home production of child care and using some of that free time to work in the market and use the earnings to buy some child care services in the market.

On the other hand, when the assumption that $\partial Z_W / \partial X_1 = 0$ is relaxed, so child care services are an input in market work, then a reduction in the price of child care $p_1$ will close the gap between the shadow price of time $\hat{w}$ and the going market wage rate $w$, thus making work for pay more attractive for all women –not only a lower $p_1$ will allow her to buy more of $X_1$, but she will also get to keep more of what she can earn in the labor market, because now the costs associated with work will have been reduced. Hence, if, say, the assumption $\partial Z_L / \partial X_4 = 0$ is also relaxed, the additional earnings a woman will be able to keep after the price of child care goes down can be spent in $X_4$ (or any other input to any activity she might enjoy), which will indirectly raise her utility. To sum up, in the more general model it is not necessary to assume that the good whose price is reduced is a good that directly enters the woman's utility function, in order to obtain the result that such a price reduction will give her an incentive to work. (Actually, it is not even necessary to assume that such good is an input in the production of a 'commodity' that she enjoys –as long as it contributes to an activity, such as market work, that can allow her to purchase inputs to other activities she might enjoy, this result will hold). Therefore, the model presented here gives an interesting insight: variations in the price of child care services (such as those induced by child care subsidies) may have a sizable effect on labor supply, leisure and the probability of labor force participation, even if expenditures in child care only amount to a tiny fraction of household spending, due to the price effect of $X_1$ through $Z_W$. Variations in the sources of non-wage income (such as family allowances), on the other hand, will not have this effect of closing the gap between $\hat{w}$ and $w$, so their predicted effects in the generalized model will be the same as those in the simple case –they may provide incentives to work in the market to the extent that they turn home production of child care relatively less productive. The effects of family allowances and of child care subsidies on female labor force participation can, then, be of very different magnitudes according to this model.

## 3.4. Data and Econometric Framework

The British Household Panel Survey (BHPS) consists of some 5,500 households and 10,300 individuals drawn from 250 different areas of Great Britain and interviewed every year since 1991. Despite the relatively small sample size compared to other UK surveys available (such as the Labor Force Survey or the New Earnings Survey), the BHPS has the main advantage of featuring information on the nature and the level of government transfers, including Child Benefits, received monthly in the household. Other advantages include the panel nature of the data and the lack of proxy respondents.

The sample used in this study is drawn from the 1998, 1999 and 2000 waves of the BHPS, limited to women of working age (16 to 63 years old at the time of the 1998 survey) with one or more dependent children in at least one of the surveys. After excluding cases with missing data, the sample contains 5,070 observations, corresponding to 1,690 individuals interviewed in three consecutive years.

The main variables of interest are labor force status, family allowances, and some of the demographic determinants of the level of benefit awarded, such as marital status or the number and age of dependent children. All values for these variables refer to

September of the survey year, and all monetary values have been converted into real terms using the UK Retail Price Index series, and are expressed in constant September 2000 British pounds. Descriptive statistics, general and organized by year, are reported in Table 3.2. The great majority of women in the sample received family allowances in September of the survey year (over 85%, as reported in Table 3.2). This high take-up rate should come as no surprise given that the program does not involve the levels of complexity and the stigma usually associated with means-tested benefits.

As an econometric framework for studying the effect of family allowances on labor force participation of women, consider the following equations[57]:

$$flfp_{it} = \alpha \cdot allow_{it} + Z_{it}\beta_0 + X_{ait}\beta_a + \delta_t + u_i + \varepsilon_{it} \tag{3-15}$$

$$allow_{it} = Z_{it}\gamma_0 + X_{bit}\gamma_b + \phi_t + v_i + \eta_{it} \tag{3-16a}$$

where $flfp_{it}$ is a binary variable indicating whether female individual $i$ is actively participating in the labor market at time $t$, $allow_{it}$ is the level of family allowances (child benefit) received by $i$ at time $t$, $Z_{it}$ is a vector of demographic characteristics of the mother that determine the level of benefits she receives (such as marital status or the number of children), and the $X$'s are vectors of individual and household characteristics that might include both fixed variables (region, race, education) and variables that might change over time (age, household size, other welfare benefits, other non-wage income, etc.)[58]. Lastly, $u$ and $v$ represent unobservable individual-specific determinants of female labor force participation (such as distaste for work) and of family allowances (such as knowledge of the process necessary to claim the benefits) respectively, $\delta$ and $\phi$ are time effects, and $\varepsilon$ and $\eta$ are disturbances.

The parameter of interest, $\alpha$ in equation (3-15), is the "causal effect" of family allowances on female labor force participation: it gives the expected increase in the probability of being active if a randomly selected member of the female population were

[57] In the very short run (such as one year, in this case), it is reasonably safe to assume that we can abstract from the effect of family allowances on the number of children, and thus treat the latter as an exogenous variable in equation (3-15).
[58] In fact, $X_b$ may well be empty (i.e.: no observable characteristic of the recipient affects the level of benefits), but that is completely irrelevant to the identification of equation (3-15).

to receive an additional monetary unit (£1) in monthly child benefits[59]. Estimation of equation (3-15) is complicated, however, by the fact that $\varepsilon$ and $\eta$ are likely to be correlated. A mother who is strongly motivated to work may also be relatively more aware of the existence of the benefit, and may have more knowledge of the process to claim it.

Note that equation (3-16a) is only valid as long as there is no change in policy. If there is a change in how benefits are determined, however, and this change affects how, say, marital status or the number of children contribute to the computation of the level of benefits received, then this equation should be as follows,

$$allow_{it} = Z_{it}\gamma_0 + Z_{it} * post_t\tilde{\gamma}_0 + X_{bit}\gamma_b + \phi_t + v_i + \eta_{it} \tag{3-16b}$$

where $post_t$ is an indicator of whether the change in the determination of benefits has taken place as of time $t$. In that case, $Z_{it}*post_t$ can be used as an instrument in equation (3-15), thus allowing for the identification of $\alpha$.

More precisely, the identifying instruments in this model are those factors that provided a mother with a different level of benefits before and after the policy change. Since the standard rate for the first child increased dramatically over this period, whereas the rate for subsequent children remained pretty much constant in real terms, one natural candidate for an instrument is an indicator for whether a woman has, after the policy change, a 'new' baby –this is, whether she has a child who is at most one year old. This builds on the reasonable assumption that parents are more likely to claim benefits soon after the birth of a new child (and will probably be more likely to do so if the benefits are higher), rather than several years after the birth of the last child. Also, since the lone parent premium was abolished and no longer available for new claims after 1998, another plausible instrument should be an indicator for whether a woman has become a new single mother after the policy change. In terms of the notation used above, $Z$ will consist of the variables '*new baby*' and '*single mother of new baby*', which have just been defined. Hence, in order to estimate $\alpha$ I will run regressions of labor force participation on family allowances and a set of controls, including time, '*new baby*' and '*single mother*

[59] A linear equation is specified, despite the binary nature of the dependent variable, for ease of interpretation and estimation.

*of new baby*' main effects, and I will use the interaction between '*new baby*' and *post* and '*single mother of new baby*' and *post* as instruments for the level of benefits.

The validity of the estimates obtained using these instruments will hinge on the assumption that there are no significant differences over time in how having a new baby, holding everything else constant, affects the probability of participating in the labor market, and in how having a new baby while being a single mother, holding everything else fixed, affects the decision to participate in the labor market. In other words, the '*new baby*' and '*single mother of new baby*' main effects are assumed to be constant over time. If this assumption were incorrect then the Two-Stage Least Squares (2SLS) estimates would be biased. It is hard, however, to think of a plausible reason why unobserved determinants of the decision to participate in the labor market would be different for mothers of a six-month-year-old than for mothers of a three-year-old in a given year, but not in another previous period.

## 3.5. Results

Table 3.3 presents estimates of equation (3-15). Each column reports estimates of $\alpha$ from a different specification or obtained by a different method. The first two columns control for the number of dependent children separated by age group, whereas the third and fourth columns use dummies for the exclusive presence of children in each age group instead, a specification very similar to that used by Blau and Tekin (2002) in their study of the effects of child care subsidies on labor outcomes. Also, because of the panel nature of the data, I use fixed effects and random effects regression methods for each of those specifications. In all cases, increases in the level of benefits appear to be associated with significant increases in the likelihood of labor market activity. Taking receipt of family allowances as truly exogenous, we would conclude that the causal effect of child benefits on female labor force participation is positive. It has already been noted, however, that treating variation in family allowances as exogenous is not reasonable, since there is likely to be a correlation between unobserved, potentially time-varying determinants of labor market participation and unobserved factors that affect the receipt and hence the level of child benefits. Therefore, I refrain from giving a causal interpretation to the effects estimated in this table. It is useful, however, to note how different the fixed effects and random effects estimates are. In fact, a standard Hausman test of the difference in the coefficients rejects the null hypothesis that they are estimating the same vector of parameters.[60] In other words, there must be some unobserved individual-specific characteristics that are correlated with the observed explanatory variables in the equation, hence rendering the random effects estimator inconsistent. For this reason, the remainder of this empirical analysis will only use fixed effects models.

Table 3.4 presents fixed effects 2SLS estimates of the same two specifications as in Table 3.3, using the interaction between the time variable *post* and the demographic variables '*new baby*' and '*single mother of new baby*' as identifying instruments. The validity of this instruments is shown in Panel a of Table 3.4, which reports results from regressions of monthly family allowances on '*new baby*'**post* and '*single mother of new baby*'**post*, along with controls also included in previous regressions.[61] There is a large and statistically significant positive relationship between having recently had a baby and family allowances received, after the policy change. The effect is even larger than the pure mechanical effect of the increase in the standard rate for the first child. As discussed in Section 2, the standard weekly rate was raised some £3 in real terms, while the rate for subsequent children was only increased just enough to keep up with price inflation. Therefore, if all eligible families had been claiming benefits at all times, family allowances should have increased by a little over £12 a month in real terms. The fact that the average increase in observed benefits for that group exceeded that figure suggests that the probability of receipt also went up after the introduction of higher rates of benefits[62]. In other words, the results are consistent with increases in the generosity of the system affecting take up rates positively, and hence raising average observed benefits for those families more likely to be favored by the policy change. This seems to validate the identification strategy used in Table 3.4.

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[60] For the difference between the coefficients in columns (1) and (2), the Hausman test statistic is 135.51, while for columns (3) and (4) it is 133.03. In both cases, this is way beyond the critical value in the chi-square distribution with thirteen degrees of freedom, which equals 27.69 for a type-I error of .01. In fact, the associated p-value for the null hypotheses of no systematic difference is <.0001 in both cases.

[61] In terms of the notation used in the previous section, Table 4a presents estimates of equation (3-16b), where I am allowing for $X_b$ to include all the elements in $X_a$, this is, I am allowing for any observable characteristic of the individual or the household to affect the decision to claim family allowances, and hence the level of benefits received.

[62] Indeed, among recent mothers with a baby who is at most one year old at the time of the survey, the fraction receiving child benefits becomes higher every year. In September 2000, it is .954, some ten percentage points above the average for the whole sample, thus supporting the claim that the increase in benefits resulted in an increase in take-up rates increased among recent mothers of new babies.

Adding *'single mother of new baby'*post* to the regression raises the previous coefficient a bit, while that variable itself enters with a fairly large and negative coefficient, although the standard error is too large to make it statistically significant. The sign and size of the effect, however, are consistent with the details of the policy change, by which new claimants would no longer be eligible for the lone parent premium. Hence, new single mothers will receive less than their veteran counterparts, and for that very same reason are less likely to claim the benefits in the first place, thus resulting in an even larger negative effect on observed benefits of being a single mother of a new baby after the policy change. While the sign and the magnitude of the estimated coefficient for *'single mother of new baby'*post* is perfectly reasonable, its low t-statistic suggests precaution is in order when interpreting any 2SLS results where it has been used as an instrument.

The standard errors in the FE 2SLS estimates are six to eight times larger than in the simple FE models, and hence inferences are less precise.[63] The estimated effect of family allowances on labor force participation of women in the sample is negative and ranges from -.00009 to -.00062, as is shown in panel b of Table 3.4. Even though the coefficient estimates are always less than their standard error, the point estimates are much lower than the simple fixed effect estimates that did not correct for the potential endogeneity problem, suggesting that the simple FE estimates are biased upward.

While the sign of the estimated effect is now in line with the prediction of the theory, its magnitude is small. Taken at face value, and abstracting from the fact that they are not statistically significant, the point estimates in column (1) imply an elasticity of female labor force participation with respect to family allowances of -.01, compared to an elasticity of -.04 for other benefits and -.08 for other non-wage income. Put another way, the estimates suggest that, while it would take a 12.5% increase in non-wage income or a 25% increase in other benefits in order to reduce female labor force participation of mothers by one percentage point, the equivalent increase in family allowances necessary to bring down the activity rate of mothers also by one point would be 100%. Even the

[63] As can be computed from the summary statistics presented in Table 2, out of the 1,690 women included in the sample only 156 were in the *'newbaby*post'* group, and only 27 were in the *'single mother of new baby *post'* group, which are relatively small numbers and account for the increase in the standard errors in the FE 2SLS estimates. A Hausman test for the equality of the FE 2SLS and the simple FE coefficients cannot reject the null hypothesis.

more negative estimates obtained in columns (3) or (4) would not imply an elasticity any bigger (in absolute value) than -.08, which would make the effect of family allowances roughly comparable to that of other sources of income. In any case, given the large standard errors of these estimates it seems reasonable to conclude that the effect is bound to be small. Table 3.4 certainly provides no evidence whatsoever of large negative effects of family allowances on female labor force participation.

## 3.6. Conclusion

Critics of family allowances routinely claim that these subsidies imply a strong disincentive to female labor force participation. While economic theory predicts that any increase in unearned income will indeed result in a few women withdrawing from the labor market, the empirical estimates in this study suggest that such effect is small and statistically insignificant.

A possible explanation for these results is that households devote increases in allowances mostly to childcare expenditures, and hence any changes in the generosity of the program will not significantly affect the demand for any other commodity, including market work. In order to test for this explanation, it would be interesting to explore whether (and to what extent) expenditures in childcare respond to changes in family allowances. Unfortunately, such data were not available for most observations in our sample.

Arguably, the size of the change in benefits induced by this program might have not been salient enough to trigger a measurable behavioral response. Admittedly, the policy change and the dataset used in this paper do not constitute the equivalent to an extremely powerful experiment involving random assignment, which would settle the discussion around the question posed. Nevertheless, this study is a first attempt at measuring the effect on labor force participation of a truly exogenous variation in family allowances.

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**Table 3.1**
Rates of Child Benefit, 1996-2002 (£ per week)

| Date | First Child | First Child (Lone Parent) | Subsequent Children |
|---|---|---|---|
| **08/04/1996** | £10.80 | £17.10 | £8.80 |
| **07/04/1997** | £11.05 | £17.10[a] | £9.00 |
| **06/04/1998** | £11.45 | £17.10[b] | £9.30 |
| **12/04/1999** | £14.40 | £17.10 | £9.60 |
| **10/04/2000** | £15.00 | £17.55 | £10.00 |
| **09/04/2001** | £15.50 | £17.55 | £10.35 |
| **11/04/2002** | £15.75 | £17.55 | £10.55 |

**Source:** Department of Social Security (Various years), *Social Security Statistics*, London: Government Statistical Service; Child Poverty Action Group (2000) *Welfare Benefits Handbook 2000/2001*, London: CPAG.

**Notes:**
**a** In April 1997, the premium for lone parents, One Parent Benefit, was abolished and incorporated into the main Child Benefit rates to give the new benefit Child Benefit (Lone Parent).
**b** On 06/07/98, new claims by lone parents could only be made at standard Child Benefit rates. However, existing lone parent claimants could continue to claim the higher rate Child Benefit (Lone Parent).

**Table 3.2**
Descriptive Statistics for the British Household Panel samples

| Variables | 1998-2000 | 1998 | 1999 | 2000 |
|---|---|---|---|---|
| | *Dependent Variable* | | | |
| Active | .590 | .556 | .596 | .617 |
| (Female Labor Force Participation) | (.492) | (.497) | (.491) | (.486) |
| | *Regressor* | | | |
| Monthly Family Allowance, if any | 100.09 | 98.50 | 99.32 | 102.47 |
| | (41.73) | (41.45) | (42.01) | (41.66) |
| Fraction Receiving Family Allowance | .851 | .849 | .856 | .849 |
| | (.356) | (.358) | (.351) | (.359) |
| | *Covariates* | | | |
| Other Benefits | 140.26 | 128.79 | 142.89 | 149.09 |
| | (275.40) | (263.10) | (280.31) | (282.16) |
| Other Non-Wage Income | 1,169.13 | 1,130.99 | 1,151.68 | 1,224.73 |
| | (1,350.94) | (1,330.96) | (1,316.35) | (1,402.93) |
| No Partner | .271 | .273 | .256 | .283 |
| | (.444) | (.446) | (.436) | (.451) |
| Household Size | 4.06 | 4.06 | 4.06 | 4.06 |
| | (1.18) | (1.20) | (1.17) | (1.18) |
| Has Kids 0-4 Years Old | .313 | .362 | .314 | .263 |
| | (.464) | (.481) | (.464) | (.440) |
| Has Kids 5-11 Years Old | .620 | .635 | .617 | .609 |
| | (.485) | (.482) | (.486) | (.488) |
| Has Kids 12-16 Years Old | .407 | .353 | .404 | .462 |
| | (.491) | (.478) | (.491) | (.499) |
| Age | 33.69 | 32.69 | 33.69 | 34.69 |
| | (8.94) | (8.90) | (8.90) | (8.90) |
| | *Instruments* | | | |
| New Baby | .150 | .199 | .158 | .092 |
| | (.357) | (.400) | (.365) | (.290) |
| Single Mother of New Baby | .023 | .026 | .027 | .016 |
| | (.150) | (.159) | (.161) | (.125) |
| N | 5,070 | 1,690 | 1,690 | 1,690 |

**Notes:** The data are from the British Household Panel for 1998 through 2000, with the sample restricted to women of working age (16-64) with children under 18 in the household in September of at least one of the three survey years. Standard deviations are in parentheses. All other entries are means. 'No Partner' is coded as one if the woman is neither married nor cohabiting with a partner. 'New Baby' is coded as one if a woman has a child who is at most one year old in September of the survey year, and zero otherwise. 'Single Mother of New Baby' is coded as one for single women who have had a child who is at most one year old in September of the survey year, and zero otherwise. All monetary values have been converted into real terms using the UK Retail Price Index series, and are expressed in constant September 2000 British pounds.

**Table 3.3**
Fixed- and Random-Effects Estimates of the Effect of Family Allowances on Labor Participation

| | Fixed Effects (1) | Random Effects (2) | Fixed Effects (3) | Random Effects (4) |
|---|---|---|---|---|
| Family Allowance | .00092 | .00044 | .00090 | .00027 |
| | (.00023) | (.00018) | (.00023) | (.00018) |
| Other Benefits | -.00017 | -.00032 | -.00017 | -.00032 |
| | (.00003) | (.00003) | (.00003) | (.00003) |
| Other Non-Wage Income | -.00004 | -.00003 | -.00004 | -.00003 |
| | (.00001) | (.00001) | (.00001) | (.00001) |
| No Partner | .0696 | .0321 | .0646 | .0284 |
| | (.0287) | (.0216) | (.0287) | (.0218) |
| Household Size | .0152 | -.0126 | .0060 | -.0328 |
| | (.0136) | (.0093) | (.0128) | (.0080) |
| Number Kids 0-4 Years Old | -.0390 | -.0747 | | |
| | (.0177) | (.0152) | | |
| Number Kids 5-11 Years Old | -.0034 | -.0372 | | |
| | (.0140) | (.0114) | | |
| Number Kids 12-16 Years Old | -.0085 | -.0220 | | |
| | (.0162) | (.0134) | | |
| Children 0-4 Only | | | -.0382 | -.0299 |
| | | | (.0272) | (.0221) |
| Children 5-11 Only | | | .0130 | .0107 |
| | | | (.0161) | (.0143) |
| Children 12-16 Only | | | -.0586 | -.0753 |
| | | | (.0556) | (.0494) |
| Age | .0791 | .0737 | .0780 | .0693 |
| | (.0205) | (.0066) | (.0205) | (.0065) |
| Age Squared | -.00058 | -.00094 | -.00057 | -.00086 |
| | (.00027) | (.00010) | (.00027) | (.00009) |
| New Baby | -.0297 | -.0408 | -.0363 | -.0587 |
| | (.0201) | (.0193) | (.0197) | (.0183) |
| Single Mother of New Baby | .0322 | .0928 | .0312 | .0841 |
| | (.0437) | (.0407) | (.0437) | (.0407) |

**Notes:** The data are from the British Household Panel for 1998 through 2000, with the sample restricted to women of working age (16-64) with children under 18 in the household in September of at least one of the three survey years. Standard errors are reported in parentheses. Entries are estimates from regressions of Female Labor Force Participation in September of the survey year (i.e.: the discrete choice variable 'Active', which is coded as one if the woman is in the labor force, and zero otherwise), on family allowances received that month. All regressions contain year main effects, and Random Effects regressions also contain a set of seven education dummies and twenty region dummies as controls. The sample size is 5,070.

**Table 3.4a**
Interactions of Demographics and Time as Instruments for Family Allowances:
First-Stage Estimates

| Variables | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| New Baby * Post | 17.85<br>(2.25) | 19.25<br>(2.46) | 18.49<br>(2.21) | 19.92<br>(2.43) |
| Single Mother of New Baby * Post | | -7.48<br>(5.28) | | -7.53<br>(5.29) |

**Notes:** The data are from the British Household Panel for 1998 through 2000, with the sample restricted to women of working age (16-64) with children under 18 in the household in September of at least one of the three survey years. Standard errors are shown in parentheses. Entries are OLS estimates of the effect of demographic variables interacted with time on family allowances. 'New Baby' is coded as one if a woman has a child who is at most one year old in September of the survey year, and zero otherwise. 'Single Mother of New Baby' is coded as one for single women who have had a child who is at most one year old in September of the survey year, and zero otherwise. All regressions contain the same controls as in Table 3.4b, so the specifications in columns (1) through (4) in this table correspond to the specifications in columns (1) through (4) in Table 3.4b. The sample size is 5,070.

**Table 3.4b**
Fixed-Effects IV Estimates of the Effect of Family Allowances on Labor Force Participation

| | FE 2SLS (1) | FE 2SLS (2) | FE 2SLS (3) | FE 2SLS (4) |
|---|---|---|---|---|
| Family Allowance | -.00009 | -.00014 | -.00058 | -.00062 |
| | (.00169) | (.00166) | (.00160) | (.00158) |
| Other Benefits | -.00015 | -.00015 | -.00014 | -.00013 |
| | (.00005) | (.00005) | (.00005) | (.00005) |
| Other Non-Wage Income | -.00004 | -.00004 | -.00003 | -.00003 |
| | (.00001) | (.00001) | (.00001) | (.00001) |
| No Partner | .0703 | .0703 | .0662 | .0662 |
| | (.0288) | (.0288) | (.0289) | (.0289) |
| Household Size | .0167 | .0167 | .0101 | .0102 |
| | (.0138) | (.0138) | (.0136) | (.0135) |
| No. Kids aged 0-4 | -.0350 | -.0348 | | |
| | (.0189) | (.0189) | | |
| No. Kids aged 5-11 | -.0009 | -.0008 | | |
| | (.0147) | (.0147) | | |
| No. Kids aged 12-16 | -.0060 | -.0059 | | |
| | (.0167) | (.0167) | | |
| Children 0-4 Only | | | -.0369 | -.0369 |
| | | | (.0274) | (.0274) |
| Children 5-11 Only | | | .0120 | .0120 |
| | | | (.0163) | (.0163) |
| Children 12-16 Only | | | -.0564 | -.0564 |
| | | | (.0560) | (.0560) |
| Age | .0830 | .0832 | .0832 | .0833 |
| | (.0215) | (.0215) | (.0214) | (.0214) |
| Age Squared | -.00060 | -.00063 | -.00062 | -.00062 |
| | (.00028) | (.00028) | (.00028) | (.00028) |
| Post | -.0212 | -.0211 | -.0210 | -.0210 |
| | (.0167) | (.0167) | (.0168) | (.0168) |
| New Baby | -.0282 | -.0281 | -.0332 | -.0331 |
| | (.0203) | (.0203) | (.0201) | (.0201) |
| Single Mother of New Baby | .0395 | .0399 | .0426 | .0429 |
| | (.0454) | (.0454) | (.0456) | (.0456) |

**Notes:** The data are from the British Household Panel for 1998 through 2000, with the sample restricted to women of working age (16-64) with children under 18 in the household in September of at least one of the three survey years. Standard errors are shown in parentheses. Entries are estimates of the effect of Family Allowances on Female Labor Force Participation, namely the discrete choice variable 'Active', which is coded as one if the woman is in the labor force, and zero otherwise. 'Post' equals one for observations after 1999, and zero otherwise. 'New Baby' is coded as one if a woman has a child who is at most one year old in September of the survey year, and zero otherwise. 'Single Mother of New Baby' is coded as one for single women who have had a child who is at most one year old in September of the survey year, and zero otherwise. The sample size is 5,070.