Human Capital Spillovers in Families:
Do Immigrants Learn from or Lean on their English-Speaking Children?

Ilyana Kuziemko*

September 2007

Abstract: This paper examines how children's English skills affect their parents' incentives to learn English themselves in immigrant families. Parents can *learn* from their English-proficient children, as having someone in the household who speaks English should lower the cost of acquiring the language, and higher English skills lead to higher market wages. However, parents can instead *lean* on their children if children's and parents' English skills are substitutes in household production. I exploit a 1998 policy change in California—the introduction of English immersion in public schools—that significantly increased English proficiency among immigrant children. Using variation between California and control states as well as within-state variation due to local differences in compliance, I find that adults are less likely to learn English when there are English-speaking children in the household. Households with older children, whose inputs are more plausible substitutes for those of adults in household production, drive the result.

* I wish to thank David Cutler, Leemore Dafny, Claudia Goldin, Caroline Hoxby, Larry Katz, Steve Levitt, and Shanna Rose, as well as participants at Harvard's Labor and Public Finance Workshop. I am indebted to Caroline Hoxby and Nora Gordon for providing their data on California's Proposition 227. Contact information: Princeton University, 361 Wallace Hall, Princeton, NJ 08544; kuziemko@princeton.edu

Introduction

Models of household decision making generally focus on human capital investments and division of labor between husbands and wives (Becker 1981). Especially in developed countries, children's human capital is assumed to play little if any role: it generally does not enter into the household production function and is not transferred to parents by peer effects or some other form of learning. The empirical treatment of intergenerational transmission of human capital has followed the theoretical literature in focusing chiefly on the transmission from parents to children (see, for example, Sacerdote 2002, Solon 1999, and Borjas 1995).

However, parents' stock of human capital is not fixed once they have children and any transmission in the reverse direction could have important implications for models of household bargaining as well as policy towards educational interventions targeted at children. This paper examines a case where such transmission is plausible and estimates its effect on parents' human capital investments.

In immigrant households, children are often the first to become English proficient, due to their exposure to public schooling as well as the greater ability of the young to learn new languages. On the one hand, adults may choose to *learn* from their English-proficient children and enjoy the benefits of English acquisition such as higher wages and increased enjoyment of amenities. On the other hand, they may choose to *lean* on their English-speaking children, who can read mail, answer the phone, talk to the landlord, and do other tasks requiring English proficiency. If children's English skills can substitute for those of their parents in the household production function, parents may find it optimal to expend less effort learning English than they would if they did not have an English-proficient child. The first section of the paper offers a model to formalize this trade-off and to motivate the empirical work.

In the model as well as the above example, children's human capital accumulation is taken as exogenous. But language skills are generally not randomly determined and thus finding quasi-experimental variation in children's English proficiency is the key challenge addressed in the empirical work. I exploit a 1998 policy change in the California public school system that replaced bilingual education with English immersion and led to a significant increase in English proficiency among immigrant children. Using variation between California and control states as well as within-state variation due to differences in compliance among California MSAs, I find that adults are less likely to be English proficient when there are English proficient children in the household. Thus, it appears that, on net, adults *lean* on children rather than learn from them.

Besides exploring the connection between children and parents in household production, this study may be relevant to two other literatures. First, English-acquisition appears to be a rare example of negative spillovers from one's peers. If we think of a family as a type of peer group, my results indicate that outcomes among peers can be negatively correlated. This contrasts with the typical "like-begets-like" finding in this large literature (see, e.g., Case and Katz 1991).

Second, the effect of children's human-capital investment on such investments by their parents complicates the process of assimilation in immigrant families. The labor-market and educational outcomes of the children of immigrants is often viewed as a measure of assimilation (Card 2005). However, my results suggest that assimilation (in the form of English proficiency) by the children of immigrants has a negative feedback on the assimilation of their parents. If assimilation of all immigrants, not just children, is a policy goal, then separate programs and incentives may need to be developed for adults who have access to English-speaking relatives or friends.

The rest of the paper is organized as follows. Section 1 presents a simple model to illustrate the interactions between children's and adults' human capital investments. Section 2 provides background on California's Proposition 227 and discusses the data and empirical strategy. Section 3 presents the results. Section 4 concludes and offers directions for further research.

Section 1: Modeling human capital spillovers within families

Consider a utility function positive in both market income and household production.[1] Individuals (adults) divide their time endowment T between time spent learning English t and time devoted to household production $T-t$ (i.e., labor supply is fixed). Income y is a positive function of language skills l, and household production is the product of time devoted to household production and some production technology, expressed as a rate g.[2] Children enter into the model in two ways. First, individuals' language skill is a positive and concave function of both time devoted to learning English t and their children's English skills k. Second, the household production rate g is a positive function of both the individual's English skills l and children's English skills k.

[1] The unitary view of the family assumes that this utility function includes the consumption and leisure of all family members. A less altruistic view of parents suggests that this utility function is theirs alone and that their children's utility does not enter it. In this section, I will generally think of parents as maximizing their own utility.
[2] See Bleakley and Chin (2007) for evidence on the positive connection between English fluency and wages. Using plausibly exogenous variation based on the age of arrival in the US, they find that an increase of one level on the US Census designation of English fluency (e.g., from "Speaks English well" to "Speaks English very well") is associated with a 33% increase in wages.

Thus, individuals choose t so as to maximize the following expression

(1) $$U(y(l(t,k)),(T-t)g(l(t,k),k)),$$

giving the following first-order condition

(2) $$U_y y_l l_t + (T-t)U_g g_l l_t = gU_g .$$

The left-hand side of (2) denotes the utility gain to the marginal hour devoted to English acquisition: the utility gain via higher market income y and the utility gain via improved household production technology g. These gains are equated to the cost—spending an additionally hour on English acquisition (thus one fewer hour on household production) decreases utility by production rate g and marginal utility U_g.

I make the following simplifications: income y is a linear function of English skills and utility is the sum of market income and household production. Applying these assumptions and rearranging (2) gives the optimal time devoted to English acquisition t^*:

(3) $$t^* = T + \frac{\lambda l_t - g}{g_l l_t}.$$

Not surprisingly, individuals devote more time to English study when the market return to English skills λ and the productivity of time devoted to English acquisition l_t rise, and more time to household production when the household production rate g increases.

Proposition: The effect of children's English skills on that of parents, $\frac{\partial l(t^*,k)}{\partial k}$, can be positive or negative. The derivative is a positive function of g_{lk}, the complementarity between children's and parents' English skills in household production, and l_{tk}, the complementarity between children's language English skills and time devoted to study (the "learning" effect"). It is a negative function of g_k, the contribution of children's English skills to household production (the "leaning effect"). See Appendix for proof.

The main intuition can be shown with simple functional forms. Assume that $l(t,k) = \alpha tk$, $g(l,k) = \beta k$ and that parents have time endowment T = 1 and devote all their time to either English acquisition or household production. Then, parents will "learn" (i.e., supply t = 1) if any only if

$$\beta \le \lambda\alpha \tag{4}$$

and will "lean" otherwise. The left-hand-side of the equation is the "leaning effect" – as β grows, children's ability to contribute to household production rises. The right-hand side represents the incentives to "learn" – the ability of children to teach their parents English, α, and the market return to English skills, λ.

The model obviously makes many simplifying assumptions and is meant mostly for illustrative purposes. The most important assumption relevant to the empirical work is that children's English skills are determined outside the model. Thus, identifying plausibly exogenous variation will be essential in estimating the key derivatives. It is to this challenge that I now turn.

Section 2: Data and Policy Experiment

The ideal experiment for determining the casual effect of children's English skills on parents' English skills would be to randomly assign some immigrants to households with English-speaking children and assign the rest to households without English-speaking children and then determine which group is more likely to have learned English by some given date in the future. This randomization would correct for the fact that parents' and children's language skills are jointly determined. A naïve OLS regression of parents' English skills on children's English skills will almost certainly lead to a positively biased coefficient on children's English skills via any number of omitted-variables scenarios (e.g., if an inherent facility for learning foreign languages were both unobserved and genetically-linked).

Lacking a controlled experiment at the individual level, I turn instead to quasi-experimental variation generated by an abrupt policy shift in California. In 1998, Californians passed Proposition 227, which declared that "all children be placed in English classrooms." Though some exceptions were allowed and a year of "bridge" programs provided to some students, the overall effect of the policy was a drastic shift from traditional "bilingual" education (in which students are taught subjects such as math and science in their mother tongue) to English immersion. In 1997, 35 percent of limited-English proficiency students were in bilingual education programs. By the fall of 1998, only 11 percent were.

I exploit this reform in two ways. First, I compare California to other states in the same region and investigate how both adults' and children's English skills changed in these areas after the policy was introduced. Second, I use MSA-level variation in compliance with the proposition to generate variation in the intensity with which California students were exposed to the new English-immersion curriculum.

I use IPUMS census data from 1990 and 2000 to examine the English skills of immigrant household members before and after Proposition 227 was passed. Every person five years and older is asked to report whether he can speak English, and, if so, if he speaks well or very well. A more objective measure of English skills would be preferable, but this self-report is the best measure available in the data.

The Census also places each immigrant in categories indicating the year they immigrated (e.g., "1987-1990"). To ensure that children in the treatment period would have spent most of their years in the U.S. under the English-emersion regime, I include only those who arrived in the U.S. within three years of being observed in the census.

Summary statistics appear in Table 1. Not surprisingly, recently arrived immigrant children speak English at a much higher rate than recently arrived adults. California residents comprise more than half the observations, highlighting the states' role as the chief destination for immigrants during the 1980s and 1990s.

California vs. Control States

My first approach in measuring the effects of the policy on children and adults is to compare the change in language skills of immigrants between 1990 and 2000 in California to that change in near-by states that did not experience such a policy shock. [3] First, I determine whether the policy had the intended effect of raising students' English skills by estimating the following differences-in-differences equation:

---

[3] I use Arizona, Oregon, Texas, Nevada and New Mexico as my set of control states. In 1999 Arizona adopted a similar English-immersion program to Proposition 227, compromising its status as a control state. All results are robust to its exclusion.

(5) $English_{ist} = \beta_0 + \beta_1 Aft_t + \beta_2 Calif_s{*}Aft_t + \beta_3 X_{ist} + W_s + e_{ist}$

where $English_{ist}$ is a dummy variable coded as one if student *i* in state *s* in period *t* speaks English, Aft_t is a dummy variable coded as one if the individual is being observed after the 1998 policy (i.e., is in the 2000 Census data) and zero otherwise (i.e., is in the 1990 Census data), X_{ist} is a vector of individual-level characteristics such as age, gender, region of birth, and ethnic background, and W_s is a vector of state-level fixed effects. The variable of interest is the $Calif_s{*}Aft_t$ interaction term: the coefficient on this variable represents the effect on the probability a student speaks English associated with living in California and being observed after the policy change has occurred

In order to determine the possible effect on adults via children, I compare the change in English facility over this period for adults with children in their household and adults without children in their household. Note that I am not estimating equation (5) and simply changing the sample from children to adults. It is possible that California's passing Proposition 227 was part of a larger movement to encourage English among all of its citizens, so that Californians may be more likely to speak English in 2000 than in 1990, regardless of the English skills of their children. Hence, merely looking at whether adults' English skills changed more in California than in other states and attributing that difference-in-differences estimate to the effect of children's English skills is likely to yield biased estimates.

Instead, I separate any change affecting all adults in California from that affecting only those adults with children in their household in the following differences-in-differences-in-differences (DDD) equation:

$$(6)\quad English_{ist} = \gamma_1 + \gamma_2 Aft_t + \gamma_3 Calif_s{*}Aft_t + \gamma_4 Child\_in\_household_i + \gamma_5 Child\_in\_householdt_i{*}Aft_t + \gamma_6 Child\_in\_household_i{*}Calif_s + \gamma_7 Calif_s{*}Child\_in\_householdt_i{*}Aft_t + \gamma_8 X_{ist} + W_s + e_{ist}$$

$Child\_in\_household_i$ is a dummy variable coded as one if individual *i* shares a household with at least one child, the new interaction terms are coded naturally, and the rest of the notation is identical to that in equation (5).[4] In this equation, the key coefficient of interest is on the triple interaction term: this is the differential impact of the passage of the law on adults with children in their household compared to adults without children.

Variation in compliance among California MSAs

Comparing California to control states is a straightforward way to proceed, but it would be preferable to compare treatment and control groups that were as similar as possible. As two Californians are more likely to experience the same shocks and trends between 1990 and 2000 than are a Californian and a resident of a different state, I now focus on those living in California. Although all Californians were subject to Proposition 227, some school districts implemented the policy very aggressively and others actively opposed it.

[4] A child is defined here as someone less than 19 years of age without children of his own.

Proposition 227 generally contained strong language with few grounds for exceptions, but some schools found ways to limit and at times avoid the implementation of the policy. The law allowed parents to petition for waivers to keep their children in bilingual programs, and if more than 20 students speaking a given foreign language in a school received waivers, that school could provide bilingual education in that language. However, these waivers had to be certified by the student's local schools, so students who attended a school whose administrators were in favor of Proposition 227 were less likely to have their waivers certified than those who attended an anti-Proposition-227 school. Hoxby and Gordon (2004) find that schools that had a large share of their limited-English-proficient students in bilingual education programs before Proposition 227 were more likely to rely on waivers to retain these students in bilingual education programs after the law was passed. These schools were presumably more pedagogically committed to the idea of bilingual education. Hoxby and Gordon also find that larger schools were more likely to retain their bilingual education program, as a smaller share of parents would have to petition in order to reach the 20-waiver requirement.

This paper does not attempt to predict which schools would retain bilingual education and which schools would aggressively pursue English immersion. I merely exploit the fact that some California MSAs had schools that aggressively followed the spirit of Proposition 227 while some MSAs had many schools that attempted to retain bilingual education by issuing waivers. These MSAs will certainly differ from each other along unobservable dimensions. The identifying assumption, however, is that the unobserved differences between these MSAs did not change between 1990 and 2000 in a pattern that is correlated with their compliance with Proposition 227. (A sufficient condition would be that the unobserved differences across MSAs are merely in levels.)

Using school-level data from California on bilingual education enrollment throughout the 1990s, I can detect which schools complied fully with Proposition 227 and which schools did not. I use Hoxby and Gordon's definition of compliance as the percentage drop between 1997 and 2000 in the number of students in bilingual education in a given school. I weight this compliance measure by the total attendance in that school and take a weighted average of schools across MSAs. This weighted average represents the percentage fall in bilingual education enrollment experienced by the average student in the MSA. While I would ideally like to match each student in the Census data with the school he attended, such geographic precision is not available in the IPUMS and the MSA is the most detailed level I can attain.

Fortunately, there is great variation in compliance across MSAs. The typical student lives in an MSA that saw bilingual education enrollment fall 50 percent after the law was passed. San Francisco, known for its commitment to bilingual education, actually increased bilingual education enrollment by 14 percent during this period while Los Angeles, one of the more compliant MSAs, decreased its bilingual education enrollment by about 60 percent.

In order to determine the effect of the policy on the probability that California children speak English, I estimate a differences-in-differences equation similar to (5):

(7) $$English_{imt} = \beta_0 + \beta_1 Aft_t + \beta_2 Comply_m * Aft_t + \beta_3 X_{imt} + W_s + e_{imt}$$

where $Comply_m$ is the percentage fall in bilingual education enrollment in MSA m between 1997 and 2000. The variable of interest is the interaction term between *Comply* and *After*.

Similarly, I employ a parallel estimation to that embodied in equation (6) to measure the effect of the policy on adults:

$$(8) \quad English_{imt} = \gamma_1 + \gamma_2 Aft_t + \gamma_3 Comply_m{*}Aft_t + \gamma_4 Child\_in\_household_i + Child\_in\_household_i{*}Aft_t + \gamma_6 Child\_in\_household_i{*}Comply_m + \gamma_7 Comply_m{*}Child\_in\_household_i{*}Aft_t + \gamma_8 X_{imt} + W_s + e_{imt}$$

Again, the reduced-form estimate of the effect of the policy change unique to adults living with children vis-à-vis other adults is the triple interaction term between *Comply*, *After*, and *Child_in_household*.

Section 3: Results

State-level variation within the U.S. Southwest

Table 2 displays the results from linear-probability estimations of equations (5) and (6). The first two columns focus on the effect of the policy on students' English skills. I limit the sample to those individuals between the ages of 5 and 18, who emigrated from non-English speaking counties within three years of being observed in the Census. The results suggest that immigrant students in the U.S. Southwest were more likely to speak English in 2000 than in 1990. However, those students in California experienced an especially large increase relative to their counterparts in other states. The coefficient on the interaction term suggests that the policy increased the probability that students speak English by 3.1 percentage points (or, roughly, that children exposed to the policy were 3.8 percent more likely to speak English, given a baseline rate of 82 percent). Note that the other covariates in the regression have coefficients of reasonable signs and magnitudes: a negative effect for Hispanic origin and a negligible effect for gender.

The last two columns focus on the change in English skills among adults, as specified in equation (6). The same sample restrictions are employed in these regressions, though the age restrictions change so as to include only those immigrants between the ages of 25 and 50. The positive coefficient on the *California**After* interaction term indicates that the state saw an increase in English skills among adults generally, suggesting that Proposition 227 was part of a larger movement during the 1990s to encourage the use of English among immigrants. However, for adults living with children, that trend is muted: while childless adults in California were 2.45 percentage points more likely to speak English in 2000 than in 1990, adults living with children saw only a 1.8 percentage point increase. The coefficient on the triple-interaction term suggests that the effect of the policy on adults via children was a 0.63 percentage-point fall in the probability of speaking English.

Finally, if the effect on adults is being driven by their increased reliance on English-speaking children in the household, then this effect should be strongest when children are at an age where they can take on long-term responsibility for the household. In Col. (4), I compare the treatment effect when there are children in the household between the ages of 6 and 16. Children younger than six are unlikely to be trusted with important tasks, and children older than 16 may not stay much longer in the household (and also have had a smaller English-immersion share of their total education). The results suggest that the negative effect is being driven almost entirely by household with children in this “useful” age range.

MSA-level variation within California

Before turning to the results from estimation of equations (7) and (8), I present graphical evidence of the effect of compliance on students and parents. Figures 1-3 have MSA compliance on the x-axis. Figure 1 shows that the growth between 1990 and 2000 in the share of students who speak English was faster in MSAs that were more compliant with the law. Figure 2 shows that adults also increased their English proficiency in the same manner, suggesting a general increase in English proficiency in compliant MSAs, but the increase is muted for adults with children in their household. Finally, Figure 3 shows that the proficiency of adults living with children in the “useful” age range fell in highly compliant MSAs, while the proficiency for other adults living with children rose slightly. Taken together, the figures depict the same patterns as do the earlier cross-state regressions: children’s English skills appear to increase with exposure to the policy, but the proficiency of adults who have contact with children, especially children who might be especially helpful in navigating English-speaking society, decreases.

The results in Table 3 are not surprising, given the trends depicted in Figures 1-3. In Col. (1), the interaction term between *Comply* and *After* is positive and significant, suggesting that the probability that a child speaks English increased the most in MSAs that complied with the policy. The point estimates suggest that a child in a school that fully complied was 4.2 percentage points (or just over 5%, on a 82% base) more likely to speak English than a similar child in a school that did not comply at all. Note that the coefficient on the *After* dummy variable is also large, positive and significant, suggesting that all California students improved their English during this period, which is not surprising given that the passage of Proposition 227 was probably a sign that voters wanted all immigrant children in the state to increase their English fluency. However, Table 3 also shows that there was a differential effect across MSAs, with those that pursued the policy more aggressively seeing the largest results.

The next two columns of Table 3 affirm the results in the previous section, that although English immersion seems to improve children's language skills it appears to discourage adults with children in their households from learning the language themselves. The triple interaction term in Col. (2) is negative and significant, indicating that between 1990 and 2000 adults living with children in highly compliant MSAs saw their English proficiency fall relative to other adults. Finally, Col. (3) indicates that the result is driven by households with children in the "useful" age range.

Robustness tests and differential treatment effects

The estimates in Table 3 assume that selective immigration patterns into MSAs did not change between 1990 and 2000. If, for example, adult migrants especially motivated to learn English selected (avoided) MSAs with stringent English-emersion policies, then the estimate on the triple-interaction term would be positively (negatively) biased. As checking for differential selection along all possible observed and unobserved dimensions is impossible, I instead check how stable the coefficients of interest in Table

3 are when other covariates ***W*** and their corresponding interactions ***W****after*, ***W****Comply*, and ***W****Comply**After* are added.

I test for differential selection based on household income, age, and Hispanic origin. Although many of these variables have statistically significant coefficients, their addition to the regression in equation (8) does not affect the estimates of the coefficients of interest. In fact, in all cases the addition of additional controls increases the magnitude of these coefficients (the results are reported in the notes to Table 3). Thus, the results appear robust to controlling in a very flexible manner for possible differential selection into the treatment group.

A related question is whether the treatment effect is stronger for certain groups. As reported in the final columns of Table 3, the effect is much stronger for those adults with less than a high-school education than those with at least twelve years of schooling.[5] Perhaps the return to learning English is greater for more educated immigrants, and so a strong "learning" effect competes with the "leaning" effect. The effect is stronger for men than for women and slightly stronger for Hispanic immigrants than non-Hispanic immigrants (though I do not report this last result in the interests of space).

Instrumental Variables Estimation

That Tables 2 and 3 are qualitatively similar is heartening as two separate sources of variation were used to generate the estimates. However, both tables report only reduced-form results, and do not directly connect children's and parents' English skills. Ideally, one would like to estimate an equation such as

[5] The vast majority of adults in the sample finished their high school education in their country of origin, alleviating the concern that their acquisition of English in the US directly affects their level of schooling.

$$(9) \qquad English_{imt} = \beta_0 + \beta_1 ChildEnglish_{imt} + \beta_2 Aft_t + \beta_3 W_m + \beta_4 X_i + \eta_{imt}$$

Where $ChildEnglish_i$ is a measure of the English skills of the children in adult *i*'s household. As discussed before, the difficulty with estimating equation (9) is that unless exogenous variation in children's English skills is isolated, endogeneity between parents' and children's language ability will cause severe positive bias.

I attempt to use Proposition 227 as an exogenous shift in the probability that someone's child speaks English. I first create a dummy variable, $ChildEnglish_i$, indicating whether any child in individual *i*'s household speaks English. This variable is defined only for households with a child over the age of four, as the Census does not record language proficiency for younger children.

I estimate the following first-stage equation:

$$(10) \qquad ChildEnglish_{imt} = \alpha_0 + \alpha_1 Aft_t + \alpha_2 Comply_m{*}Aft_t + \alpha_3 \mathbf{W}_m + \alpha_4 \mathbf{X}_i + \varepsilon_{imt},$$

where all variables are as previously defined. This equation is fundamentally similar to equation (7), which uses variation in the compliance of MSAs to estimate the effect of the policy on children's English skills. In this equation, the same variation is driving the estimates as in equation (7), though the sample has changed slightly (from all children to only those children in households with an adult between the ages of 25 and 50), and thus it should not be surprising that the first-stage estimation in Col. (1) in Table 4 is similar to that in Table 2.

I then use the predicted values of the probability that a child speaks English in the second-stage equation:

$$(11) \qquad English_{imt} = \beta_0 + \beta_1 \widehat{ChildEnglish}_{imt} + \beta_2 Aft_t + \beta_3 W_m + \beta_4 X_i + \eta_{imt}$$

where $\widehat{ChildEnglish}$ is the predicted value of *ChildEnglish* from the first-stage equation. The identifying assumption is that *Comply*Aft* does not affect adults' English skills except through the effect it has on children. Note, however, that if *Comply*Aft* were to have a positive effect on adults' English skills (which is likely if Proposition 227 were merely part of a larger effort to encourage immigrants in the state to assimilate), then the instrumental-variables estimate would be biased towards the OLS estimate – that is, biased against finding the "leaning on" effect suggested by the results in Tables 2 and 3.

Table 4 shows the results from estimating (11), the second-stage equation. For the sake of comparison, Col. (1) shows the naïve OLS regressions when I do not instrument for *ChildEnglish*. Not surprisingly, the coefficient on *ChildEnglish* is positive and significant. However, the coefficient changes sign in Col. (2), the instrumental-variables estimate. The coefficient suggests that living with a child who speaks English decreases the probability an immigrant speaks English by about 53 percentage-points.

One concern about the results in Col. (2) is that the magnitude of the coefficients is over-stated by the binary measure of an endogenous variable that is actually continuous (Angrist and Imbens 1995). Obviously, children's English skills vary continuously, and the probability that a parent speaks English could respond to the exact level of children's English skills. While I do not have a strictly continuous measure of children's English skills, I can make the endogenous variable more continuous by using all four of the Census classifications for language skill instead of merely the dummy variable, so that the variable now ranges from 0-3. I then average this measure across all children in the household, adding further continuity. As Col. (3) shows, however, the coefficient barely changes, other than being mechanically scaled by a third. The results suggest that an adult with a child who does not speak English is about 51 percentage points more likely to speak English than a parent with a child who speaks English very well.

Section 4: Conclusion

This paper has found that immigrant adults are less likely to learn English when they live with English-proficient children. This finding is potentially important in richer policy evaluation. Even if a policy intervention has the desired effect for the targeted group, the effect on peers must also be measured in order to gauge the policy's overall impact. While interventions may often have positive spillovers (e.g., vaccinations not only reduce disease prevalence among the treated, but also among their peers), when division of labor and public-goods issues arise, free-riding off of treated members may also occur.

This paper does not offer any evidence that such free-riding by parents is actually welfare-reducing. Perhaps being forced to act as "runners" to the outside world actually benefits children later in life, as they learn early on to be independent and savvy. As has been well noted in the literature (see, for example, Chiswick and DebBurman, 2004), the children of immigrants enjoy much success in the labor-market and invest greatly in human capital (going to school longer than both immigrants and children of natives). On the other hand, perhaps they would achieve even more if their parents had been able to move out of an ethnic neighborhood or find a higher-paying job, tasks that English fluency may aid.

Whether the effects on children should concern policy makers depends on one's view of the household. If one adopts the unitary view where households are headed by a benevolent decision-maker, then parents will adapt their human-capital investments to those of their children in such a way that household utility is maximized. If one believes, instead, that each member of the household maximizes his own utility and that distribution within the household is the outcome of a cooperative or non-cooperative game, then parents' human-capital decisions may not be optimal for their children. In the context of marriage, such conflicts between spouses can be mitigated by the possibility of divorce and other threat-points as well as by prenuptial contracts. As children do not have access to these mechanisms and are often taught to obey their parents, they may be unable to stop their parents from expropriating their own human-capital investments.

Appendix

Proposition: The effect of children's English skills on that of parents, $\frac{\partial l(t^*,k)}{\partial k}$, can be positive or negative. The derivative is a positive function of g_{lk}, the complementarity between children's and parents' English skills in household production, and l_{tk}, the complementarity between children's language English skills and time devoted to study (the "learning" effect"). It is a negative function of g_k, the contribution of children's English skills to household production (the "leaning effect").

Proof

First consider the effect of k on t^*, the optimal amount of time to devote to English acquisition. The derivative can be signed by differentiating the first-order condition implied by (3) with respect to k.

$$\text{(A1)}\quad \frac{\partial}{\partial k}[\lambda l_t + (T-t)g_l l_t - g] = l_{tk}(\lambda + (T-t)g_l) + g_{lk}(T-t)l_t - g_k + [(T-t)l_t l_k g_{ll} - g_l l_k].$$

Note that the first term is positive, as both income and the household production rate are positive functions of English skill, so the expression is a positive function of l_{tk}. The second term is also positive, as language skills are a positive function of t, so the expression is a positive function of g_{lk}. Finally, note that g_k enters negatively into the expression.

Given t^*, the optimal level of English acquisition will be

$$\text{(A2)}\qquad \frac{\partial l(t^*,k)}{\partial k} = \frac{\partial l}{\partial t}\frac{\partial t^*}{\partial k} + \frac{\partial l}{\partial k}.$$

As the expression on the right-hand side is a positive and differentiable function of $\frac{\partial t^*}{\partial k}$, the properties proven for $\frac{\partial t^*}{\partial k}$ hold for $\frac{\partial l(t^*,k)}{\partial k}$ as well.

Sources

Angrist, Joshua and Guido Imbens. 1995. "Identification and Estimation of Local Average Treatment Effects," *Econometrica* (March): 467-75.

Becker, Gary. 1974. *Treatise on the Family*. Cambridge: Harvard University Press.

Bleakly, Hoyt and Aimee Chin. 2007. "English Proficiency and Social Assimilation Among Immigrants: An Instrumental Variables Approach." CCIS Working Paper 147 (March).

Borjas, George. 1995. "Ethnicity, Neighborhoods, and Human-Capital Externalities," *American Economic Review* (June): 365-390.

Case, Anne and Lawrence Katz. 1991. "The Company You Keep: The Effects of Family and Neighborhood on Disadvantaged Youths." NBER Working Paper 3705.

Card, David. 2005. "Is the New Immigration Really So Bad?" *Economic Journal* (November): F300-323.

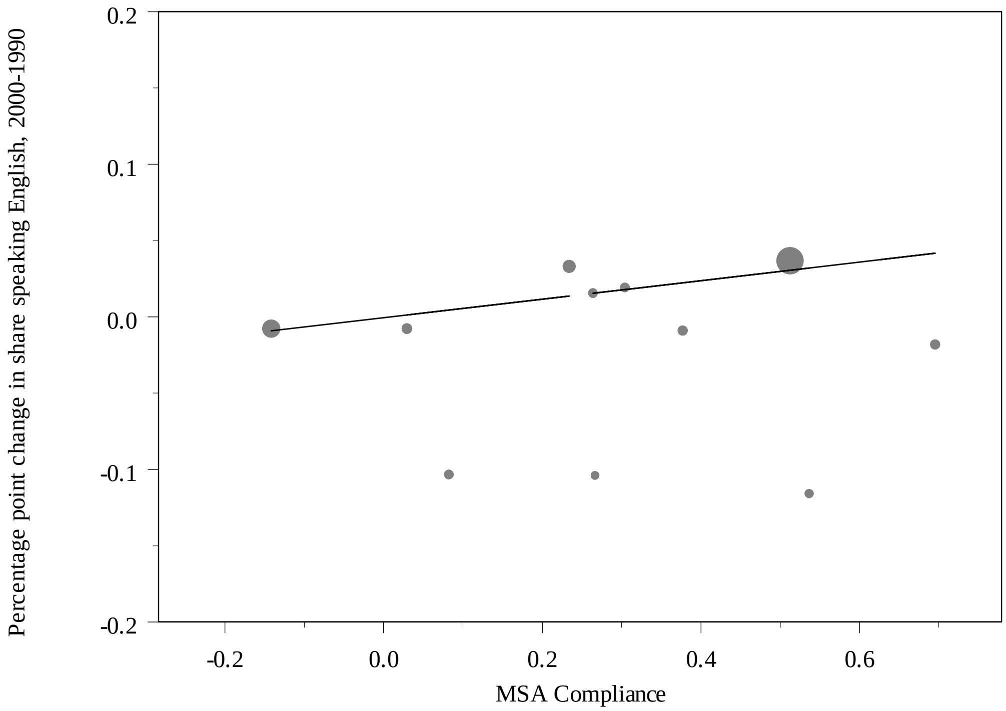
Chiswick, Barry and Noyna DebBurman. 2004. "Educational Attainment: Analysis by Immigrant Generation," *Economics of Education Review* (August): 361-379.

Hoxby, Caroline and Nora Gordon. 2004. "Achievement Effects of Bilingual Education vs. English Immersion: Evidence from California's Proposition 227," HIER Working Paper.

Sacerdote, Bruce. 2002. "The Nature and Nurture of Economic Outcomes," *American Economic Review* (May): 344-348.

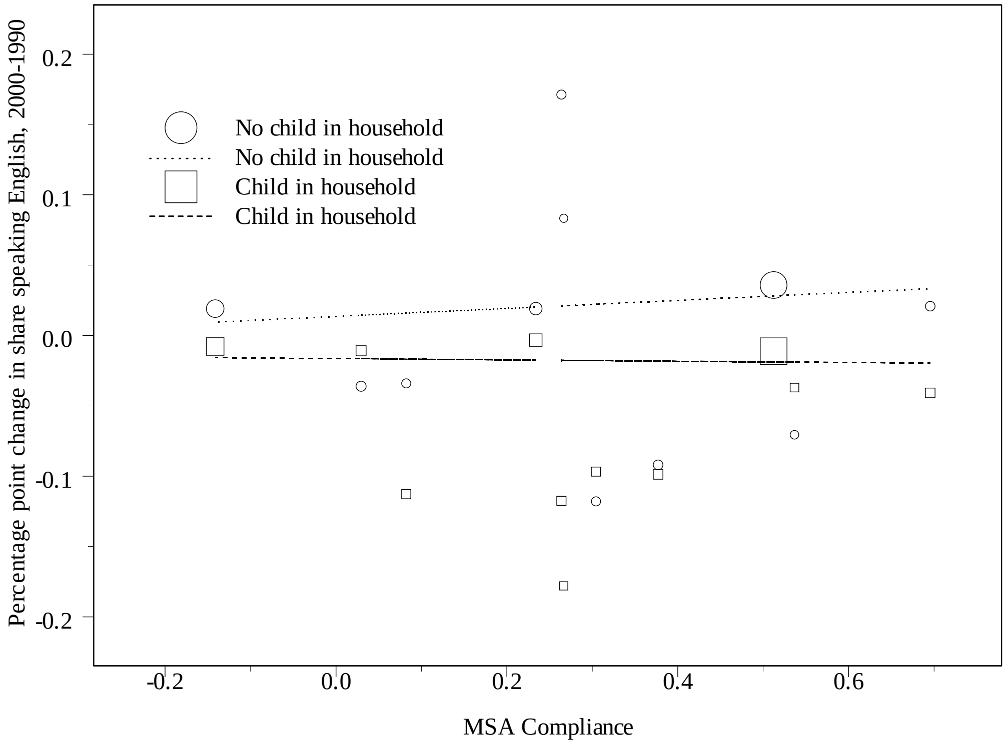
Solon, Gary. 1999. "Intergenerational Mobility in the Labor Market," in D. Card and O. Ashenfelter (eds.), *Handbook of Labor Economics*, Vol. 3A, North-Holland.

Figure 1: Change in English proficiency, California students



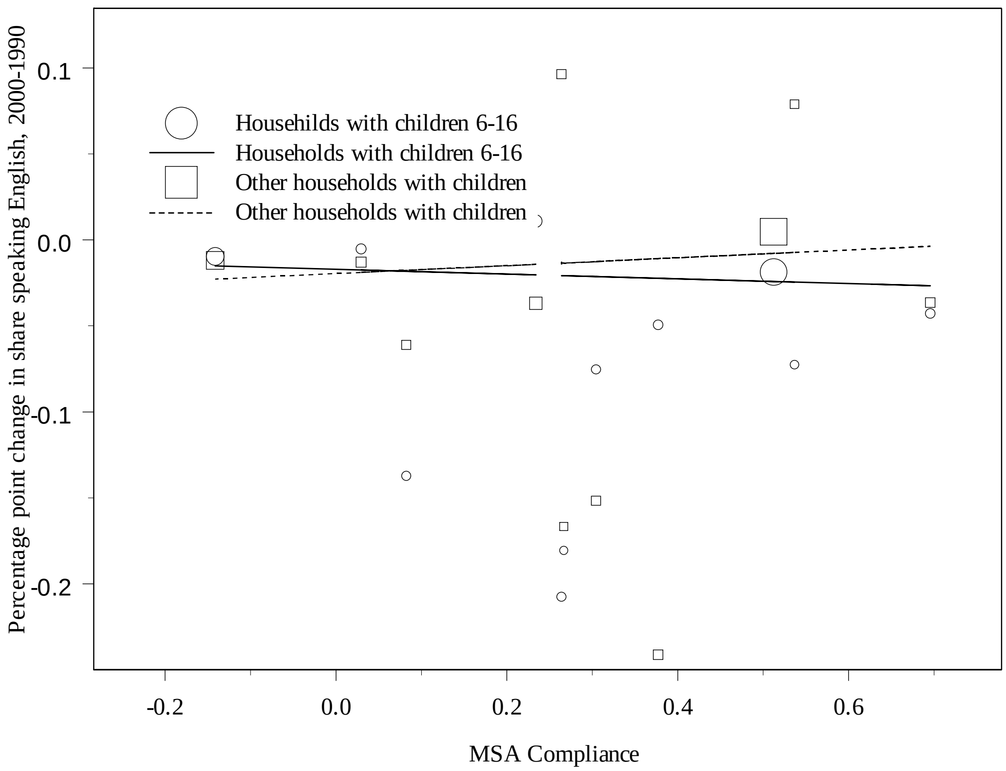
Notes: Data taken from the 1990 and 2000 IPUMS and Hoxby and Gordon (2004). The x-axis tracks the percent decrease in children attending bilingual-education classes between 1997 and 2000 (so full compliance would be recorded as one). The y-axis tracks the percentage-point change between 2000 and 1990 in the share of children in the MSA speaking English, as recorded in Census data.

Figure 2: Change in English proficiency, adults in households with and without children



Notes: Data taken from the 1990 and 2000 IPUMS and Hoxby and Gordon (2004). The x-axis tracks the percent decrease in children attending bilingual-education classes between 1997 and 2000 (so full compliance would be recorded as one). The y-axis tracks the percentage-point change between 2000 and 1990 in the share of adults in the MSA speaking English, as recorded in Census data.

Figure 3: Change in English proficiency, adults with children in household



Notes: Data taken from the 1990 and 2000 IPUMS and Hoxby and Gordon (2004). The x-axis tracks the percent decrease in children attending bilingual-education classes between 1997 and 2000 (so full compliance would be recorded as one). The y-axis tracks the percentage-point change between 2000 and 1990 in the share of adults in the MSA speaking English, as recorded in Census data.

Table 1: Summary statistics

	Children (ages 5-18)		*Adults (ages 25-50)*	
	Mean	Standard deviation	Mean	Standard deviation
Speaks English	0.819	0.384	0.744	0.436
Male	0.542	0.498	0.511	0.499
Hispanic	0.688	0.463	0.530	0.499
Age	12.07	4.378	33.81	6.37
California	0.674	0.468	0.678	0.467

Notes: The category "children" includes only those older than four years in this case because language skills are not recorded until this age. There are 41,223 children observations and 53,600 adult observations. For both categories, the sample is limited to those who were born in a non-English-speaking country and have arrived in the U.S. within three years of being observed in the Census. All data are from the 1990 and 2000 IPUMS.

Table 2: Effect of policy on English skills of children and adults in Southwestern states

	Children	*Treatment = child in household*	*Treatment = child in household age 6-16*
After	0.0167 (0.0015)	-0.012 (0.00088)	-0.0305 (0.00224)
Calif*After	0.033 (0.00062)	0.030 (0.0032)	0.0240 (0.00315)
Treatment	--	-0.0378 (0.00139)	-0.025 (0.0022)
Calif*Treatment	--	0.0068 (0.00096)	0.00948 (0.00125)
Treatment*After	--	0.0036 (0.00089)	0.0322 (0.00055)
Calif*Treatment *Aft	--	-0.0103 (0.00177)	-0.00533 (0.00228)
Male	-0.0167 (0.0096)	0.0394 (0.0060)	0.0640 (0.0065)
Hispanic	-0.097 (0.023)	-0.187 (0.0120)	-0.207 (0.0140)
Sample	Children (ages 5-18)	Adults (ages 25-50)	Adults with child in household
Observations	41,223	53,600	35,525
R-squared	0.111	0.216	0.0193

Notes: An adult is considered to have a child in his household if anyone less than 19 years of age without her own child is recorded as living in the same household. All regressions are estimated using a linear probability model, include age and region-of-origin fixed effects, and cluster standard errors at the MSA-year level. All data are taken from 1990 and 2000 IPUMS residents in California, Texas, New Mexico, Arizona, Nevada and Oregon.

Table 3: Effect of policy on English skills of children and adults across California MSAs

	Children	*Treatment = child in household*	*Treatment= child in HH age 6-16*	*Treatment= child in HH age 6-16*	*Treatment= child in HH age 6-16*
After	0.0360 (0.00532)	0.0161 (0.00383)	-0.00776 (0.0105)	-0.0499 (0.0218)	-0.0298 (0.0162)
Comply*After	0.0424 (0.0120)	0.00642 (0.00659)	0.0109 (0.00954)	0.0276 (0.0341)	0.0437 (0.0277)
Treatment	--	-0.0207 (0.00468)	-0.0177 (0.0105)	-0.0537 (0.0180)	-0.00225 (0.0154)
Comply* Treatment	--	-0.0441 (0.00865)	0.0233 (0.0184)	0.163 (0.0339)	0.0294 (0.0268)
Treatment*After	--	-0.00077 (0.00381)	0.0385 (0.0083)	0.0801 (0.0229)	0.0475 (0.0156)
Comply* Treatment*Aft	--	-0.0245 (0.00536)	-0.0492 (0.0137)	-0.129 (0.0280)	-0.107 (0.0284)
Male	-0.0169 (0.00543)	0.0383 (0.00493)	0.0616 (0.00528)	0.0993 (0.00713)	--
Hispanic	-0.109 (0.0069)	-0.169 (0.0199)	-0.198 (0.0224)	-0.211 (0.0338)	-0.198 (0.0115)
Sample	Children (ages 5-18)	Adults (ages 25-50)	Adults with child in household	Adults with child in HH, <12 yrs school	Adults with child in HH, male
Observations	24,831	32,737	20,986	8465	9830
R-squared	0.111	0.228	0.207	0.0780	0.226

Notes: An adult is considered to have a child in his household if anyone less than 19 years of age without her own child is recorded as living in the same household. All regressions are estimated using a linear probability model, include age and region-of-origin fixed effects, and cluster standard errors at the MSA-year level. The *Comply* variable is defined as the percentage fall in the share of immigrant students in bilingual education classes in an MSA between 1990 and 2000. All other data are taken from 1990 and 2000 IPUMS residents in California. The coefficient (standard error) on the interaction term of interest from Cols. (1)-(3) are 0.0466 (0.0276), -0.0162 (0.00861), and -0.0596 (0.0166).

Table 4: The effect of children's English skills on adults in the same household

	Naïve OLS regression	*First stage*	*Second stage (zero/one endog. var.)*	*Second stage (continuous endog. var.)*
English-speaking child in house	0.320 (0.00706)	--	-0.538 (0.0215)	--
Comply*Aft	--	0.0512 (0.0144)	--	--
Avg. English skills of children in house	--	--	--	-0.179 (0.0848)
Observations	16,641	16,298	16,298	16,298
R-squared	0.227	0.0355	--	--

Notes: All regressions are estimated using a linear probability model, include age and region-of-origin fixed effects, and cluster standard errors at the MSA-year level. The *Comply* variable is defined as the percentage-point fall in the share of immigrant students in bilingual education classes in an MSA between 1990 and 2000. All other data are taken from 1990 and 2000 IPUMS residents in California.