Human Capital Spillovers in Families: Do Parents Learn from or Lean on Their Children?

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I model how children's acquisition of a given form of human capital incentivizes adults in their household to either learn from them (if children can teach the skill to adults, adults' cost of learning falls) or lean on them (if children's human capital substitutes for that of adults in household production, adults' benefit from learning falls). Using variation in compliance with an English-immersion mandate in California schools, I find that English instruction improved immigrant children's English proficiency but discouraged adults living with them from acquiring the language. Whether family members "learn" or "lean" affects the externalities associated with education policies.

I. Introduction

Parents are often a child's first teachers, and economic models have long recognized the role parents play in passing on human capital to their children (see Becker and Tomes 1979; Becker and Tomes 1986; also see the response by Goldberger [1989]). In contrast, these models generally assume

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that children's human capital has little contemporaneous effect on parents and other adults in their household; it generally does not enter into the household production function and is not transferred to adults by peer effects or some other form of learning.[1] 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, e.g., Behrman and Rosenzweig 2002; Sacerdote 2002; Plug 2004; Black, Devereux, and Salvanesal 2005; Oreopoulos, Page, and Stevens 2006).

This article is, to the best of my knowledge, the first to model as well as empirically analyze the transfer of human capital from children to adults. In contrast to the classic models of intergenerational human capital transmission, which typically find that, all else equal, an increase in parents' human capital leads to an increase in that of their children, the model I develop shows that children's human capital investment can either increase or decrease that of the adult members of their household. The sign of the effect depends on the household production function and the learning technology.

Suppose a child exogenously acquires a new skill. On the one hand, an adult can learn from the child, as the cost to adults of learning the skill will fall if their children can teach it to them. This "learning effect" suggests positive human capital spillovers from children to adults. On the other hand, an adult can lean on the child, as the benefit to adults of acquiring the skill will fall if children's human capital can substitute for that of adults in the household production function. This "leaning effect" suggests negative spillovers. Moreover, the model I present offers a framework for predicting when adults are more likely to lean or learn. For example, if the skill is something that can be passed on by even children's inexpert tutoring, then adults are more likely to learn. The more children's human capital can directly increase adults' consumption, the more adults will lean on their children.

The empirical work focuses on an example where children received a plausibly exogenous shock to their human capital and estimates its effect on the human capital investment of the adults living with them. In 1998, California voters passed Proposition 227, which replaced bilingual education with English immersion in public schools. When classes ended for the summer in 1998, 29% of English learners received core academic instruction in their native languages; when classes resumed 3 months later, only 11% did. Using geographic variation in compliance with Proposition 227 across California and individual-level Census IPUMS (Integrated Public Use Microdata Series) data, I find areas highly compliant with the re-

[1] Ehrlich and Lui (1991) assume children's human capital affects parents in their old age and thus parents invest in their children's human capital because they will one day depend on their children's income. But the direction of the investment in this model is still from parents to children.

form saw greater gains in children's English proficiency between 1990 and 2000, which is consistent with English immersion promoting English acquisition. However, these same areas saw a decrease in the English proficiency of adults living with children. These results are driven by adults living with school-age children—suggesting that children's human capital acquisition, and not some omitted variable, is driving the effect on adults—and are robust to a number of specification checks. On net, adults in California appear to have leaned on their children's English skills.

The results in this article may interest a variety of researchers and policy makers. First, the model I present highlights the possibility of "negative" human capital spillovers, which has received little attention among economists studying peer effects. Of course, economists have studied free-riding in the context of public goods games and team work effort, but, in the context of human capital, they have generally assumed individuals learn from their peers.

Second, as most educational policies target children, determining the extent of human capital spillovers to older members of the household would allow policy makers to better compare the marginal social benefits and costs of these policies. In the case of English acquisition, my results suggest that gains to children may be tempered by negative spillovers on adults. If the goal is to assimilate entire immigrant families, then policy makers may wish to gather information on how a particular program affects adults in addition to how it affects the targeted children, and they may need to create separate programs to give incentive to adults to learn. In contrast, other types of human capital could trigger the "learning" instead of "leaning" effect, and in such cases, program evaluations that consider only the effects on a policy's prime targets may systematically underestimate its social benefits, as Miguel and Kremer (2004) demonstrate.

Finally, the results in this article relate to the literature on how different teaching philosophies affect language acquisition, an increasingly important question for US education policy. Between 1979 and 2006, the number of K–12 students speaking a foreign language at home tripled, and states have so far taken a variety of approaches toward these students (US Department of Education 2008). Although Proposition 227 remains controversial in California, Massachusetts and Arizona have since passed similar initiatives (*Economist* 2008). Taking the opposite approach, districts in Georgia and Utah have hired teachers from Mexico to conduct classes in Spanish to their growing population of Hispanic students (Thompson 2009). As both Presidents Bush and Obama have stressed English proficiency requirements in their comprehensive immigration reform proposals, the question of how best to promote English acquisition among children and adults is likely to remain an important public policy question.

This article is organized as follows. Section II presents a simple model to illustrate the interactions between children's and adults' human capital investments. Section III reviews the literature (mostly outside of economics) on language acquisition and family dynamics in immigrant households and provides background on Proposition 227 as well as the bilingual-versus-English-immersion debate. Section IV presents the data and empirical strategy, and Section V the results and robustness checks. Section VII concludes and offers directions for further research.

II. Model

A. Overview

This section provides a simple model of how adults' optimal level of human capital investment depends on the human capital of their children. As in the standard model of human capital investment (e.g., Becker 1964; Ben Porath 1967), adults weigh the benefit of the investment (the increase in consumption) against its price (the time, opportunity, or psychic cost).

Children change the standard model in two ways. On the one hand, children can decrease the cost of human capital investment for their relatives. For example, suppose that in order to learn English immigrant parents can either study at home with their proficient child or attend an English as a Second Language (ESL) class. Not only can they save money and time if their child acts as their private tutor, they may also "save face" as they can avoid making potentially embarrassing mistakes in front of strangers. This decrease in the price of investment leads parents to invest more in human capital acquisition. I call this phenomenon the "learning effect."

On the other hand, if children's human capital can substitute for that of adults in household production, then proficient children provide many of the benefits adults would enjoy from acquiring the human capital themselves. For example, a literate or English-proficient children can read contracts, bills, or coupons, and they can confer with landlords, doctors, and teachers on behalf of their family members; children's human capital may even assist adults in finding better jobs.[2] The ability of children's human capital to directly increase adults' consumption decreases adults' incentive to invest in human capital themselves. I call this phenomenon the "leaning effect."

B. Mechanics

I modify the classic returns-to-education model with the above ideas in mind. Adults maximize a separable utility function positive and concave in consumption and negative and convex in the cost of investment. Adults' consumption y is a positive and concave function of both their own human capital k and their children's human capital c, so $y = y(k, c)$. Adults'

[2] Basu, Narayan, and Ravallion (2001) use data from Bangladesh to show that having a literate member of the household is associated with higher wages for nonliterate members.

human capital k is a positive and concave function of their investment in human capital, which I denote by e, as one can think of investment in this context as "effort" or "education."

The cost of investment λ is increasing and convex in e. Importantly, there is a complementarity between adults' investment e and their children's level of human capital c, so that $\lambda_{ec} < 0$. As described above, having a proficient child can reduce the per unit psychic or monetary cost of investment e.

With the above assumptions in mind, I specify adults' utility as

$$\psi(y(k(e), c)) - \lambda(e, c). \tag{1}$$

As described above, y_k, y_c, k_e, λ_e, and λ_{ee} are positive and λ_{ec} is negative. As utility is a positive, concave function of consumption, $\psi' > 0$ and $\psi'' < 0$.

Adults choose e^* so as to satisfy the following first-order condition:

$$\psi' y_k k_e = \lambda_e. \tag{2}$$

Equation (2) yields the standard result that individuals set e^* so that the utility gain due to the increase in consumption associated with a marginal increase in e (the left-hand side of the equation) equals the increase in disutility associated with higher investment costs (the right-hand side).

The main comparative static addressed in the empirical work is the effect of children's human capital on the human capital of adult household members, or $\partial k(e^*)/\partial c$. On the one hand, e^*, and thus $k(e^*)$, will increase with c because of the "learning effect." Having a proficient child serve as a tutor decreases parents' per-unit cost of investment (more formally, recall that $\lambda_{ec} < 0$). As the right-hand side of the equation falls with an increase in c, individuals must increase e to satisfy the first-order condition.

On the other hand, e^* will decrease with c because of the leaning effect. An increase in children's human capital directly increases adults' consumption by y_c, thus lowering adults' marginal utility of consumption ψ'. Therefore, adults will decrease investment so as to equalize the marginal utility of consumption and the marginal disutility of investments costs in equation (2). All else equal, if adults can rely on children's human capital to increase household consumption, they will invest less in human capital themselves.

The idea of competing incentives is expressed more formally below:

Proposition 1. The effect of children's human capital on that of adults in the household, $\partial k(e^*)/\partial c$, can be positive or negative. It is a positive function of $(-\lambda_{ec})$. This term represents the extent to which learning from proficient children can lower the per-unit cost of adults' human capital investment (the "learning effect"). It is a negative function of y_c, the direct contribution of children's human capital to adults' consumption (the "leaning effect").

Proof. See the appendix.

While the model does not specify the sign of the effect, it does suggest when the sign is likely to be positive or negative. The learning effect is especially strong for adults with attributes that would be complements to children's tutoring. For example, in the immigrant context examined in this study, adults with some basic educational background themselves might find learning English from their children especially easy, whereas adults without any educational background might require more expert tutoring to achieve basic proficiency.

Conversely, the leaning effect is likely to be especially important if the ψ term of utility were a function only of simple items such as food or clothing, as their consumption value should be independent of an individual's human capital. However, the marginal utility of other consumption items may depend on one's own human capital stock. For example, the consumption value of most American movies or newspapers depends on having not only the resources to purchase the ticket or paper but also English proficiency. As such, if immigrants live in an area that provides a large array of consumption items and experiences in their native language, then consumption value would be independent of their English proficiency, leading to a larger leaning effect.

C. Discussion

The model obviously makes many simplifying assumptions and is meant mostly for illustrative purposes. For example, I make no real distinction between household production and adults' consumption and implicitly assume that parents' consumption increases even when the increase in household production is due entirely to their children's efforts. Instead, children may refuse to contribute to household production if they want their parents to learn the skill themselves. Similarly, children's human capital acquisition may change the bargaining power within the household. These effects would act to dampen any "leaning" incentive.

Moreover, the model assumes that children's human capital is determined outside the model. Instead, children may simply refuse to invest in human capital if they know their parents will free-ride off of them, thus making children's human capital endogenous to parents' expected behavior. If children only learn when they believe their parents will learn as well, then the leaning mechanism is effectively shut off.

Obviously, identifying plausibly exogenous sources of variation is essential for estimating the key comparative statics in the model, and this is the focus of the remainder of this article. The variation I exploit arises from an educational intervention in the state of California. Children exposed to the intervention achieve greater English proficiency, and I use this variation to estimate the effects on adults' English acquisition. The rest of this article provides more information on language acquisition and the specific reform I examine and then turns to the data, empirical strategy, and results.

III. Background on Language Acquisition and Proposition 227

This section begins by briefly reviewing the large literature on language acquisition, both by economists and by other scholars. I then review the much smaller literature, all outside economics, documenting the ways adults in immigrant families rely on younger members of the household to perform English-intensive tasks. Finally, I provide background on Proposition 227, the California English-immersion reform, and review existing empirical work examining the policy.

A. Past Work on Language Acquisition and Family Spillovers

In US immigrant households, children are often the first to become English proficient. This tendency is likely due to their exposure to public schooling as well as the greater ability of the young to learn new languages, especially during the so-called "critical period" (after infancy but before puberty) when for neural or behavioral reasons humans seem much more adept at language acquisition.[3]

Immigrant adults have many incentives to learn English, from their children or otherwise. There is a large economics literature linking immigrants' wages to their English skills, with almost all papers finding a strong, positive relationship.[4] For example, instrumenting for an immigrant's English proficiency with whether he/she arrived in the United States during his/her "critical period," Bleakley and Chin (2004) find that speaking English "well" as opposed to "poorly" (according to Census classifications) earns a 33% wage premium.

However, there is also much sociological and ethnographic work on how immigrant children can reduce adults' need to learn English. Sociologists have created the term "language brokering" for the practice of children in immigrant families negotiating the English-speaking world for their older relatives. Orellana et al. (2003, 505) provides a description from a daughter of Mexican immigrants:

> As a kid I translated phone calls, TV shows, bills, letters from the welfare department, visits to the doctor, visits with social workers, interviews; and I filled out applications for health care, welfare, and social security benefits. I did this because I was the only one who

[3] See Newport (2002) for a review of research on the "critical" or "sensitive" period hypothesis first developed by Lennenberg (1967). Functional magnetic resonance imaging evidence even suggests that adults and children use different parts of the brain when acquiring a new language.

[4] While a chief concern is omitted-variables bias (e.g., a standard "ability bias" scenario would likely lead to a positively biased coefficient on English skills), Chiswick and Miller (1995), Angrist and Lavy (1997), and Dustmann and van Soest (2002) all attempt to address this endogeneity problem. Bleakley and Chin (2004) review these and other papers.

> could do it. I was the only one in my family who could communicate in both English and Spanish. I became the key to accessing the resources my family needed.

To the best of my knowledge, there has been no attempt to systematically gauge how widespread this practice is across the United States, but sociologists have conducted small surveys in a variety of localities that include questions on language brokering. In a survey of 64 students from a "major metropolitan high school" who were born primarily in China and Vietnam, Tse (1996) finds that 59 students report translating for their parents (and four of the five who report not doing so indicate that they have older siblings who do). Orellana et al. (2003) report that "almost all" of the 236 Spanish-speaking students they survey in a Chicago elementary school act as language brokers, and they specifically report that 73% have brokered for their mothers.[5] They suggest that this share is remarkably high given that a significant share of the children's parents had been living in the United States for much of their lives. Finally, some evidence suggests that children are highly effective translators: in a small study of 16 Puerto Rican elementary school students from an "extremely low socioeconomic" neighborhood in New Haven, Connecticut, Malakoff and Hakuta (1991) find that children make very few errors when translating, though they display slightly higher accuracy when translating from Spanish to English than vice versa.

Thus, there appears to be strong incentive for immigrant adults in both the "leaning" and "learning" directions. As I will discuss in greater detail, an ordinary least squares (OLS) estimate of adults' English skills on those of their children is likely to be positively biased via any number of endogeneity scenarios and thus largely unhelpful in determining which incentive dominates. Being unable to randomly assign adults to households with or without English-speaking children, I turn instead to quasi-experimental variation generated by an abrupt policy shift in California.

B. Proposition 227

In June 1998, by a margin of 61% to 39%, California voters passed Proposition 227, which mandated that "all public school instruction be conducted in English." Although some exceptions were allowed and a year of "bridge" programs was offered to some students, the overall effect of the policy was a sudden shift from traditional "bilingual" education (in which students are taught subjects such as math and science in their native language and further development of the native language is often an explicit goal) to English immersion. When classes finished in June of 1998, 29% of limited-English-proficiency (LEP) students were being taught at least two

[5] The data are not presented in a disaggregated manner, so I cannot calculate what share have language brokered for family members in general.

core academic subjects in their native language; 3 months later, only 11% were.

Proposition 227 contains 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 presented waivers, that school could provide bilingual education in that language. However, these waivers had to be certified by the 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.

Past research has explored heterogeneity in compliance with Proposition 227. Garcia and Curry-Rodriguez (2000) find that schools that had a large share of limited-English-proficient students in bilingual education programs in the pre-227 period were more likely to certify waivers and thus retain bilingual education programs. Compliance also seems to depend on institution size: Bali (2003) finds that larger districts were more likely to notify parents of their right to petition for a waiver.

Evaluations of Proposition 227, like most research related to the bilingual-versus-immersion debate, have not focused on how it affected students' English proficiency, but instead on its effect on students' academic achievement. Overall, the evidence has been inconclusive. Using compliance with Proposition 227 as a source of quasi-experimental variation, Hoxby and Gordon (2004) find that bilingual education improves test scores in several subjects among students in early grades. Most papers, however, do not directly address potential endogeneity issues. Amselle and Allison (2000) highlight large post-227 gains on the state's Stanford 9 achievement exam for LEP students, while Butler et al. (2000) point out that non-LEP students enjoyed the same gains. The state's own evaluation of Proposition 227 found insignificant effects of bilingual education on math and reading scores (Parrish et al. 2006), though the authors acknowledge that their hierarchical model may not control for nonrandom selection into bilingual versus English-immersion classrooms. The lack of consensus regarding Proposition 227's effect on academic achievement mirrors that of the bilingual-versus-immersion debate more generally. Both Matsudaira (2005) and Jepsen (2010) provide excellent reviews.

Perhaps because there is a strong a priori assumption that English immersion would be superior to bilingual education with respect to the specific goal of improving English proficiency, research has not focused on this outcome.[6] An important exception is Jepsen (2010). Using data from

[6] It is difficult to extrapolate from the academic achievement literature to English proficiency. While English proficiency no doubt helps students perform well

the California English Language Development Test (CELDT) in 2003 and 2004, he finds that, relative to English immersion, bilingual education lowers students' English proficiency. His results are consistent across OLS, propensity-score matching, and IV (instrumental variable) estimates.[7]

Jepsen's estimates are especially large for English learners with relatively low baseline English skills. As I focus on newly arrived immigrant students, his results suggest that the gains to English proficiency associated with English immersion should be especially pronounced among the students in my sample. The next section describes the individuals in this and related samples in greater detail, as well as the data sources from which they are drawn.

IV. Data and Empirical Strategy

A. Data Sources

1. Individual-Level Census Data

I use IPUMS census data from 1990 and 2000 to examine the English skills of immigrant household members before and after the passage of Proposition 227. Every person 5 years old and older is asked whether he/she can speak English, and, if so, if he/she 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, and Kominski (1989) finds it to be reliable, at least in earlier censuses.

I make several sampling restrictions. First, I only include immigrants from non-English-speaking countries. Second, to ensure that children in the treatment period would have spent most of their years in the United States under the English-immersion regime, I include only those who arrived in the United States within 3 years of being observed in the US Cen-

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on a general achievement test, the skill sets are distinct in many ways. Butler et al. (2000) argue that in particular the state's Stanford 9 exam is unable to measure English proficiency.

[7] Jepson cannot look before and after Proposition 227 as the CELDT was only established in 2001, though at one point he uses the change between the 1997 and 1998 school years in the probability of English instruction as an instrument. As described in Sec. IV, I use this change to predict changes in English proficiency between 1990 and 2000. While not as germane to the current study, other economists have also used quasi-experimental variation in studies on language instruction outside of the Proposition 227 context. Matsudaira (2005) uses a regression-discontinuity design and finds that assignment into an English-immersion classroom does not improve math or reading test scores. Unfortunately, he cannot measure changes in English proficiency per se, as those assigned to English-immersion classes never retake the English proficiency exam. Angrist, Chin, and Godoy (2006) find that exposure to English instruction in Puerto Rican public schools did not have lasting effects on English proficiency 30–40 years later. Of course, their example differs from the Proposition 227 setting in that in Puerto Rico English was not the official language.

Table 1
Summary Statistics, 1990 and 2000 Census Data

	Children Ages 5–18	Adults Ages 25–60	Adults Living with Children
Speaks English	.819	.748	.695
	(.385)	(.434)	(.460)
Speaks very well	.280	.269	.220
	(.449)	(.444)	(.414)
Speaks primarily English at home	.0719	.0601	.0499
	(.258)	(.238)	(.218)
Male	.537	.495	.468
	(.499)	(.500)	(.499)
Hispanic	.642	.468	.549
	(.479)	(.499)	(.498)
Age	12.09	35.29	35.85
	(4.377)	(9.127)	(8.928)
After Proposition 227	.471	.506	.489
	(.499)	(.500)	(.500)
Child in household		.642	
		(.479)	
Child age 7–15 in household		.368	.574
		(.482)	(.495)
Observations	27,760	44,105	28,508

SOURCE.—All data taken from the 1990 and 2000 IPUMS, weighted by IPUMS person-level sample weights.
NOTE.—To be included in the sample, respondents must be born in a non-English-speaking country, be living in California at the time they were observed in the Census, and have arrived in the United States no earlier than 3 years before the time of the Census. Standard errors are in parentheses.

sus.[8] Finally, when I analyze outcomes for children, I sample respondents ages 5–18; when I analyze outcomes for adults, I sample ages 25–60.

Summary statistics appear in table 1, separately for children, all adults, and adults living with children, as this final category is used in much of the empirical work. Immigrant children are more likely to speak English than adults, consistent with the research cited earlier, and this difference is even greater when children are compared to adults who themselves live with children. Very few individuals in any category speak English as the primary language at home.

While I will often focus on the distinction between not speaking English well and not speaking English at all, I will also make use of the more detailed information in the IPUMS regarding levels of English proficiency. Figure 1 shows that the majority of children in the sample fall into the two most proficient categories, whereas nearly two-thirds of adults living with children fall into the two least proficient categories.

[8] The Census places each immigrant in categories indicating the year they immigrated (e.g., "1987–90"), so I chose the most recent category consistent across both census years, which happens to be "arrived within 3 years."

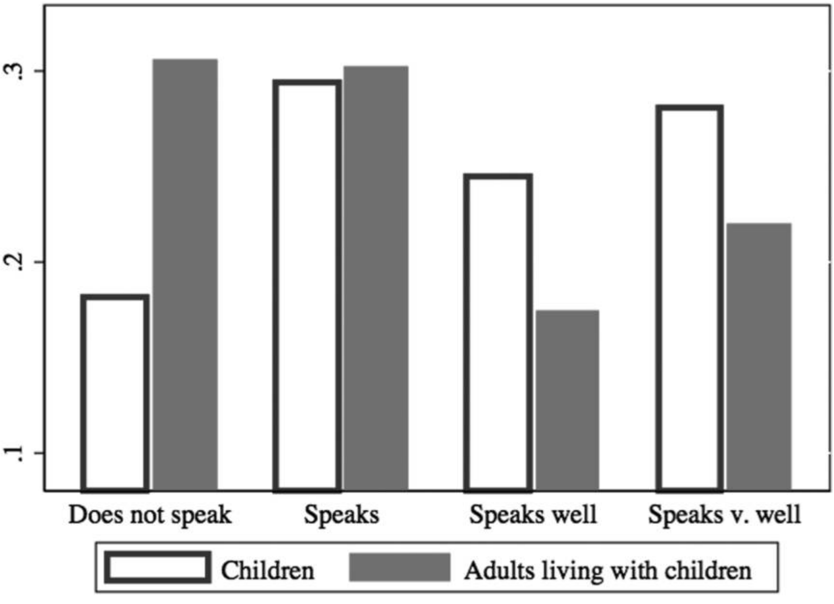

FIG. 1.—Distribution of English proficiency levels in the 1990–2000 IPUMS. Data are from IPUMS data files, 1990 and 2000. All individuals sampled emigrated from non-English-speaking countries within 3 years of the survey year. Children are between the ages of 5 and 18 and adults are between the ages of 25 and 60. A color version of this figure is available online.

2. *School-Level Proposition 227 Compliance Data*

I complement the IPUMS data with annual school-level data from the California Department of Education (CDE) Language Census data files.[9] These data provide the total number of "English learners" as well as the educational programs in which they are enrolled.

The variable I generally use from these data is the number of students in what the CDE terms "English Language Development and Academic Subjects through the Primary Language (L1)."[10] Students in this program receive at least two core academic subjects in their native language. For each school, I calculate the percentage-point change between 2000 and 1998 in the share of English learners enrolled in this program. As mentioned earlier, this share falls from 29% during the 1997–98 school year to 11% during the 1998–99 school year, where it remains during the 1999–2000 school year. For convenience, I generally refer to this percentage-point change as the "compliance" rate. However, it is simply the percentage-point change in the probability that an English learner will experience traditional, primary-

[9] These data can be accessed via the California Department of Education (CDE) at the following url: http://www.cde.ca.gov/ds/sd/sd/fslc01p234.asp.
[10] This variable is called ELD_{L1} in the CDE data set from the url given in note 9.

language bilingual education. Thus, for schools that had no primary-language instruction before and after 1998, the "compliance" measure will be zero even though they were perfectly compliant both before and after Proposition 227 was passed.[11]

I weight this compliance measure by the total English learner attendance in that school and take the weighted average for each Public Use Microdata Area (PUMA) in the Census data. While I would ideally like to match each student in the Census data with the school he or she attends, such geographic precision is not available in the IPUMS, and the PUMA is the most disaggregated level at which I can match students to compliance rates.[12]

Fortunately, as I document graphically in a later subsection, there is great variation in compliance across PUMAs. The typical student lives in a PUMA that saw the share of English learners in L1 instruction fall by 13 percentage points, whereas the most "compliant" PUMA saw its share fall by 26 percentage points and the least compliant increased this share by 3 percentage points.

Compliance is not randomly distributed across PUMAs. For example, because compliance is measured as the change in the probability a school instructs an English learner in English, a school that in the preperiod instructed almost all their English learners in English would likely have a low value for the compliance variable even if, in the postperiod, they still instruct a larger share of English learners in English than do most other schools. Empirically, the larger the Hispanic share of immigrants in the PUMA, the greater the predicted compliance rate. In contrast, the average age, adult educational attainment, or household income of the PUMA, its racial (as opposed to ethnic) composition, or its urban share do not predict PUMA compliance.[13] While Proposition 227 provides a shock to children's English instruction, it hardly provides a randomized, controlled trial, and

[11] This measure of compliance is also used by Hoxby and Gordon (2004) and Jepsen (2010).

[12] I actually use the *CONSPUMA* variable ("consistent PUMA") in the IPUMS, which are areas that are defined consistently for the 1980, 1990, and 2000 Censuses, as well as the 2005 and onward American Community Surveys. There are 33 CONSPUMAs in California.

[13] I define the urban share of the PUMA by whether a respondent lived in a city large enough to be designated by the IPUMS. Readers may recall from Sec. III that past work had found that large districts and districts with large preperiod bilingual programs were the most likely to petition for waivers to continue bilingual education. In contrast, I find no effect of a PUMA's urban share on compliance and a positive effect of the share Hispanic, which might seem inconsistent with past results. The main difference is that I define compliance as the change in the probability that a limited-English-proficient child is instructed in English, whereas most studies outside of the economics literature looked only at the level of instruction in 2000 (as I noted in footnote 11, the two economics papers on Proposition 227, Hoxby and Gordon 2004 and Jepsen 2010, use the same definition of compliance that I use here).

the next subsection details how I attempt to isolate its effect on children and the adults with whom they live.

B. Empirical Strategy

Simply regressing parents' English skills on those of their children would almost surely yield a positively biased coefficient on the latter variable. Any number of omitted-variables or reverse-causality scenarios exist. An inherent facility for learning foreign languages may "run in the family," an adult expressing embarrassment due to their inability to speak English may render his/her children timid in their efforts to learn and practice the language, and of course parents fluent in English can teach their children, as in Bleakley and Chin (2008). Indeed, regressing whether an adult speaks English on whether he/she lives with a child who also does yields a coefficient of 0.39 on the latter variable—taken literally, living with an English-speaking child increases the probability that an adult will speak English by 39 percentage points.

Instead of directly regressing parents' English skills on those of their children, I exploit variation generated by the uneven compliance with Proposition 227. I first examine whether immigrant students in areas that saw greater increases in English instruction experienced greater gains in English proficiency between 1990 and 2000, by estimating the following equation:

$$\text{Speak}_{ipt} = \beta(\text{Compliance}_p \times \text{After}_t) + \lambda_p + \varphi\text{After}_t + \theta X_i + \varepsilon_{ipt}, \qquad (3)$$

where i denotes the individual, p the PUMA, and t the year; Speak is an indicator variable for whether the individual reports being able to speak English (though other measures of proficiency will also be used); Compliance_p is, as described earlier, the percentage-point change between 1990 and 2000 in the probability a student in that PUMA would be instructed in English; After_t indicates that the individual is being observed after the imposition of Proposition 227 (i.e., in the 2000 Census as opposed to the 1990 Census); λ_p is a vector of PUMA dummies; and X_i are individual-level covariates. If English immersion promotes English acquisition, then the estimate for β should be positive.

I then turn to the effect on adults. The treatment effect I seek to estimate is having an English-proficient child in one's household. Assuming Proposition 227 compliance increases children's English proficiency (a claim I support in the next subsection), the treatment effect can be estimated by the following differences-in-differences-in-differences equation:

$$\begin{aligned}\text{Speak}_{ipt} = {} & \gamma(\text{Child-in-house}_i \times \text{Compliance}_p \times \text{After}_t) \\ & + \lambda_p + \mu_1\text{Child-in-house}_i + \mu_2\text{After}_t \\ & + \mu_3(\text{Child-in-house}_i \times \text{After}_t) \\ & + \mu_4(\text{Compliance}_p \times \text{Child-in-house}_i) \\ & + \mu_5(\text{Compliance}_p \times \text{After}_t) + \eta X_i + \varepsilon_{ipt},\end{aligned} \qquad (4)$$

where Child-in-house$_i$ is a dummy for whether adult i lives with a child and all other notation follows from equation (3).

If adults lean on (learn from) their children, then γ should be less (greater) than zero. The treatment effect represents the differential effect living in a Proposition 227–compliant PUMA has on adults who live with children relative to those who do not. Using variation across time should control for unobserved heterogeneity at the PUMA level; using the control group of adults without children should control for unobserved heterogeneity at the PUMA-year level, such as migration patterns or changes in industry composition, which might correlate with adults' English acquisition. The identifying assumption is that this unobserved heterogeneity has the same effect on adults with and without children.

While equation (4) illustrates the spirit of the estimation, my preferred specification makes two modifications. First, my preferred specification actually compares adults living with children of a "useful" age to other adults living with children. Children in this age range would be old enough to be able to teach adults some basic English or to take on some responsibility for language brokering—that is, they are old enough for their parents to either learn from or lean on them. But they would also be young enough to be required to attend school and to have arrived in the United States not long beyond their own "critical period" for learning English. As such, I define children between the ages of 7 and 15 as being in this "useful" age range, though I show later that changing the age range slightly does not affect the results. Moreover, this specification allows the comparison of a treatment group and a control group who both live with children and are thus likely to be similar along other dimensions.

Second, in order to be less parametric, instead of including Compliance$_p$ $\times$ After$_t$ and Compliance$_p$ $\times$ Child$-$in$-$house$_i$ (or, depending on the specification, Compliance$_p$ $\times$ Child-in-house-age-7–15$_i$), I actually interact After$_t$ with a full set of PUMA dummy variables and Child$-$in$-$house$_i$ (or Child-in-house-age-7–15$_i$) with a full set of PUMA dummy variables, though in practice the results are very similar.

V. Results

A. Basic Trends

Before turning to regression results, I graph the basic relationships between a PUMA's level of English proficiency and its compliance with Proposition 227. Specifically, for each PUMA, I plot the percentage-point change between 1990 and 2000 in the share of immigrant children who speak English "very well" against the percentage-point change in the probability they were taught in their primary language, that is, the "compliance rate."[14] As described in the figure note, some outlier PUMAs are dropped from the figure so that

[14] Results plotting the share who speak English at all are very similar and are available upon request.

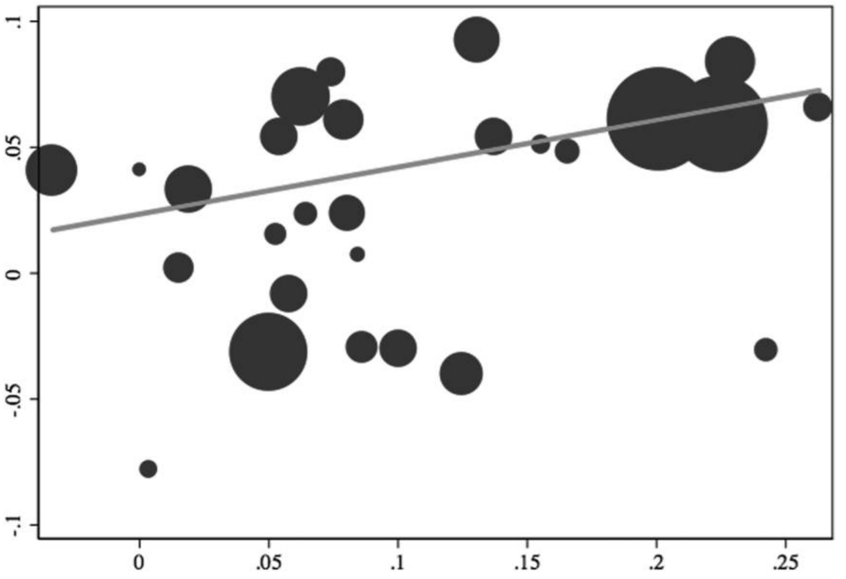

FIG. 2.—Change in share of children who speak English "very well," 1990–2000. Data are from IPUMS data files, 1990 and 2000, and school-level California Department of Education data. A school's compliance rate is defined as the percent-point decrease in the share of English learners receiving core academic instruction in their primary language. This measure is weighted by total English learner enrollment and averaged for all schools in a PUMA. In order to avoid having the area where the majority of the data lie from being overly compressed, the scatter plot drops outliers (those observations with *y*-axis variables greater than 0.1 or less than −0.1). The fitted lines, however, include these outliers. A color version of this figure is available online.

the scale is not compressed, but the fitted line reflects all observations. If, as in Jepsen (2010), English immersion is associated with greater English proficiency, then the change in the share of immigrant children who speak English should be most positive in the areas most compliant with Proposition 227. Figure 2 shows that the change in the share of children who speak English very well is indeed a positive function of compliance.

Figure 3 displays this relationship for adults living with children ages 7–15, though it uses "speaks at all" as the outcome of interest. In contrast to the figure for children, there is a noisy but negative relationship between compliance and the relative increase in English proficiency among this group of adults. By contrast, there is, if anything, a slightly positive though essentially flat relationship between compliance and English skills among adults living with children who do not fall in the "useful" age group (not shown).[15]

[15] In regression results, the sign of the effect of compliance on the control group of adults living with children outside of the useful-age group is dependent on the exact

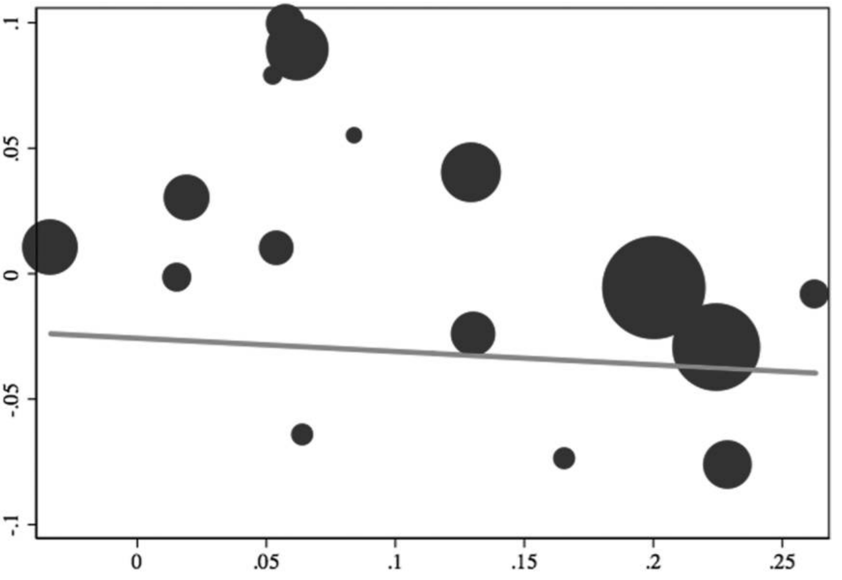

FIG. 3.—Change in English-speaking share of adults who live with children ages 7–15, 1990–2000. See fig. 2 for information on data sources and definitions. Except for the sample and outcome variable ("speaks at all" in this figure instead of "speaks very well" in fig. 2), the analysis in this figure is parallel to that in the previous figure. In order to avoid having the area where the majority of the data lie from being overly compressed, the scatter plot drops outliers (those observations with y-axis variables greater than 0.1 or less than -0.1). The fitted lines, however, include these outliers. A color version of this figure is available online.

To summarize, between 1990 and 2000, children's English proficiency improved in areas that saw the largest shift away from bilingual education. However, this shift appears to have the opposite effect on the adults these school children live with, especially in comparison to adults living with children outside the "useful" age range I specify. Thus, figures 2 and 3 provide graphical evidence consistent with adults "leaning" on their children's improved English skills.

B. Regression Results on the Effect of Compliance on Children's English Skills

The first three columns of table 2 show the results from estimating variants of equation (3). All regressions in these and other columns include

set of controls used, though in general, it appears slightly positive, as in fig. 3. Such a relationship is perhaps not surprising, given that one would expect that areas that have become "tougher" on immigrant children's ability to use their native language at school might also act in other ways to make it more difficult to get by without English skills, increasing the incentives for adults to learn English as well.

Table 2
The Effect of Compliance with Proposition 227 on Children's English Proficiency

	Dependent Variables: Measures of English Proficiency							
	Speaks (1)	Proficiency (2)	Speaks Very Well (3)	Speaks Very Well (4)	Speaks (5)	Speaks Very Well (6)	Speaks Very Well (7)	Speaks Very Well (8)
Compliance rate × After Proposition 227	.112**	.357*	.246***	.148	.137*	.294***	.257**	.0294
	(.0519)	(.194)	(.0855)	(.0993)	(.0786)	(.0899)	(.0976)	(.0720)
After Proposition 227	.00812	.117***	.0231	−.0232	.0197	.0679***	.0279	−.00648
	(.00868)	(.0305)	(.0170)	(.0245)	(.0226)	(.0246)	(.0196)	(.0152)
Male	−.0164***	−.0634***	−.0231***	−.0230***	−.0164***	−.0241***	−.00738	−.00999*
	(.00573)	(.0150)	(.00539)	(.00538)	(.00573)	(.00581)	(.00769)	(.00539)
Hispanic	−.0990***	−.527***	−.214***	−.214***	−.0990***	−.191***	−.178***	−.170***
	(.0123)	(.0450)	(.0224)	(.0224)	(.0123)	(.0347)	(.0394)	(.0256)
Ages in sample	5–18	5–18	5–18	5–18	5–18	5–18	7–15	5–18
Added PUMA controls?	No	No	No	Yes	Yes	No	No	No
Added individual-level controls?	No	No	No	No	No	Yes	No	No
Placebo (1980–90)?	No	No	No	No	No	No	No	Yes
Observations	27,760	27,760	27,760	27,760	27,760	27,760	14,891	29,573

SOURCES.—All data are taken from the 1990 and 2000 IPUMS, except for the last column, which uses the 1980 and 1990 IPUMS.

NOTE.—All regressions are weighted by IPUMS person-level sample weights. See table 1 for sampling rules. Proficiency is a 0–3 categorial variable corresponding to Does Not Speak English, Speaks English but Not Well, Speaks Well, and Speaks Very Well. Speaks is an indicator variable for having a proficiency level greater than zero, and Speaks Very Well is an indicator variable for having a proficiency level of 3. All regressions include fixed effects for PUMA of residence, age in years, and region of country of birth. Additional PUMA controls refer to Hispanic immigrant share × After, as a PUMA's Hispanic share of immigrants is predictive of Compliance. Added individual controls" refers to individual-level measures (Hispanic dummy variable, age, and household income) interacted with After and Compliance. Placebo (1980–90) uses data from 1980 and 1990, with 1990 now acting as the post-227 treatment period. Standard errors are in parentheses.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

dummy variables for age in years, gender, race, Hispanic origin, and the region of the country of origin.[16] Results without these controls are very similar (they are essentially depicted already in fig. 2).

The first column suggests that a child in a highly compliant PUMA after the passage of the proposition is more likely to speak English: the point estimate suggests that going from the most compliant PUMA (which has a compliance value of 0.26) to the least compliant (which has a value of −0.03) would increase the probability an immigrant student speaks English by $0.112 \times 0.29 = 3.2$ percentage points, or 4.0% given a baseline probability of 0.819. The results also indicate that on average boys and Hispanics in the sample are less likely to speak English.

The second column shows that this result holds when instead of a dummy variable for whether the child reports speaking English, the categorical variable from zero to three indicating proficiency level is used.[17] In fact, the reform seems to have a large effect on improving children's English at the high end of this spectrum. Column 3 suggests that going from the least to the most compliant PUMA increases the probability that a child speaks "very well" by 7.1 percentage points, or 25%.

The remainder of the table subjects these results to robustness checks. As noted earlier, a strong predictor of PUMA compliance is the PUMA's Hispanic share of its immigrants. I thus test whether this PUMA characteristic, and not the PUMA's Proposition 227 compliance rate, actually explains the positive coefficient on Compliance × After. The result in column 4 shows that when "speaks very well" is the outcome, controlling for Hispanic immigrant share × After decreases the magnitude of the coefficient of interest relative to that in column 3, though it remains positive, with a p-value just above 0.1. In contrast, when "speaks at all" is the outcome, including the control increases the coefficient of interest relative to that in column 1. Taken together, it does not seem that Hispanic share × After better explains the evolution in children's English skills than does Compliance × After.

Another potential worry is that compositional changes across counties might be in part driving the result. In column 6 of table 2, I interact the Hispanic dummy variable as well as Household income and Age—three variables highly predictive of English proficiency—with the Compliance and After variables and include them, as well as the main effect of income, in the regression in column 3.[18] The coefficient on Compliance × After remains positive and significant and actually grows slightly in magnitude rel-

[16] The regions are Central American and the Caribbean, South America, Western Europe, Central, Eastern and Southern Europe, East Asia, South Asia, the Middle East, and Africa.

[17] Ordered logit regression coefficients have the same sign and statistical significance as OLS results and are available upon request.

[18] Variables Hispanic and Age main effects were already included in the baseline specification.

ative to the point estimate in column 3. Although not shown, the coefficient of interest also grows in magnitude relative to that in column 1 when "speaks" is the outcome.

As noted earlier, I will often focus on children between the ages of 7 and 15, as such children would have arrived in the United States not too long after their "critical period" for language acquisition but are also old enough to language broker. As column 7 shows, the main result holds for this subsample as well.

The final column of table 2 presents results from a placebo test using 1980 and 1990 IPUMS data. The results in this column are from a specification identical to that in column 3, but while Compliance is still based on changes between 1990 and 2000, now the 1990 IPUMS acts as the postperiod and the 1980 IPUMS as the preperiod. As such, Compliance × After picks up the effect of changes in compliance between 1990 and 2000 on changes in children's English proficiency between 1980 and 1990. A positive coefficient on this variable would suggest that trends predating Proposition 227 might be responsible for the results so far in this article. A large negative coefficient might be suspect as well, as then the results between 1990 and 2000 might reflect some mean reversion. In fact, the coefficient on this variable is essentially zero, and the hypothesis that it is equal to the coefficient in column 3 can be rejected with $p < .010$.

Because children ages 7–15 are critical for the analysis in the next subsection, appendix table A1 performs each of the robustness checks in table 2 on this smaller sample. While standard errors tend to be larger due to the smaller sample, the points estimates are very similar, and the results using this sample appear equally robust to those using the larger sample in table 2. Note that we can reject with $p = .013$ that the coefficient using the 1980–90 placebo sample is equal to that when the 1990–2000 sample is used.

C. Regression Results on the Effect of Compliance on Adult's English Skills

I now examine how compliance with Proposition 227 affected adults by estimating variants of equation (4). The first column of table 3 compares adults who live with children (of any age) to adults who do not live with children. As noted earlier, living with a child is a very crude measure of the effect I seek to estimate living with an English proficient child capable of either helping an adult learn the language or performing household tasks that require English fluency. For example, a parent living with their infant child would be considered part of the "treatment group" in this regression, even though such a child has no capacity for language, much less an ability to teach or help. For these reasons, it is not surprising that the coefficient on the interaction term, while negative, is not statistically significant. To conserve space, I do not show other measures of English proficiency, but the coefficients in those specifications are also negative but not significant.

Table 3
The Effect of Compliance with Proposition 227 on Adults' English Proficiency

	Dependent Variables: Measures of English Proficiency							
	Speaks (1)	Speaks (2)	Proficiency (3)	Speaks Very Well (4)	Speaks (5)	Speaks (6)	Speaks (7)	Speaks (8)
Compliance × Treatment × After	−.0816	−.217**	−.255	.0265	−.319**	−.250**	−.185*	.00586
	(.0841)	(.104)	(.226)	(.105)	(.133)	(.118)	(.0931)	(.0894)
Male	.0428***	.0641***	.124***	.0184**	.0641***	.0644***	.0642***	.0740***
	(.00333)	(.00395)	(.0138)	(.00696)	(.00394)	(.00400)	(.00398)	(.00380)
Hispanic	−.143***	−.167***	−.506***	−.157***	−.167***	−.124***	−.167***	−.105***
	(.0141)	(.0185)	(.0586)	(.0286)	(.0185)	(.0224)	(.0186)	(.0200)
Sample	All adults	Adults with children	Adults with children	Adults with children	Adults with children	Adults with children	Adults with children	Adults with children
Treatment, lives with children aged . . .	0–18	7–15	7–15	7–15	7–15	7–15	6–16	7–15
Added PUMA controls?	No	No	No	No	Yes	No	No	No
Added individual-level controls?	No	No	No	No	No	Yes	No	No
Placebo (1980–90)?	No	No	No	No	No	No	No	Yes
Observations	44,105	28,508	28,508	28,508	28,508	28,508	28,508	33,642

Sources.—All data are taken from the 1990 and 2000 IPUMS, except for the last column, which uses the 1980 and 1990 IPUMS.

Note.—All regressions are weighted by IPUMS person-level sample weights. See table 1 for sampling rules. Proficiency is a 0–3 categorial variable corresponding to Does Not Speak English, Speaks English but Not Well, Speaks well, and Speaks Very Well. Speaks is an indicator variable for having a proficiency level greater than zero, and Speaks Very Well is an indicator variable for having a proficiency level of 3. All regressions include fixed effects for PUMA of residence, age in years, and region of country of birth. In addition, they also include After interacted with each PUMA fixed effect, as well as Treatment interacted with each PUMA fixed effect. Added PUMA controls refer to Hispanic immigrant share × Treatment × After (as well as all lower-order terms of this interaction not already absorbed by the previously noted controls) as a PUMA's Hispanic share of immigrants is predictive of Compliance. Added individual-level controls refer to individual-level measures (Hispanic dummy variable, age, and household income) interacted with all lower-order terms of Compliance × Treatment After. Placebo (1980–90) uses data from 1980 and 1990, with 1990 now acting as the post-227 treatment period.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

The rest of the analysis compares adults living with children between the ages of 7 and 15 to other adults living with children. Column 2 shows that this specification yields a negative and significant coefficient on the interaction term. Relative to other adults with children, adults living with children in this "useful" age group are 6.3 percentage points (9.1%) less likely to speak English when they live in the most compliant versus least compliant PUMA.

Columns 3 and 4 suggest that while the effect on adults is indeed large at the bottom of the proficiency spectrum—that is, moving adults from the "does not speak" to "speaks" category—it fades for higher proficiencies. In column 3, the effect using the four-category proficiency measure as the outcome variable is smaller in magnitude than the corresponding effect for children and not statistically significant. And the point estimate in column 4 suggests that exposure to the policy had no effect of moving adults from the "speaks well" to the "speaks very well" category (in fact a positive point-estimate, with $p > .8$). As noted earlier, relatively few adults with children report having these higher proficiency levels, so it is not surprising that the effect would be concentrated at the lowest levels of proficiency. Overall, it appears that exposure to Proposition 227 increases children's proficiency throughout the proficiency distribution, whereas having an English-proficient child seems to mostly discourage adults from acquiring basic English proficiency.

The rest of the table subjects the result in column 2 to robustness checks. Column 5 tests whether the Hispanic immigrant share of the PUMA can better explain adult English patterns than the compliance rate, by including Hispanic immigrant share × Lives with child age 7–15 × After and all lower-order terms not already absorbed by the baseline controls. Including these controls only increases the magnitude of the coefficient on the variable of interest. Following table 2, column 6 interacts additional individual-level controls (a Hispanic indicator variable, Age, and Household income) with all lower-order terms of the triple interaction term Compliance × Treatment × After. Again, the magnitude of the coefficient on the triple interaction term increases slightly.

Column 7 verifies that column 2 is not a knife's-edge result due to fortuitously choosing the 7–15 age range. Allowing children to be considered of a "useful age" when they are between age 6 and 16 leads to very similar results, as does ages 6–15 or 7–16 (not shown). Column 8 performs the same 1980–90 placebo test as for the child sample, and again the coefficient on the interaction term is essentially zero.

D. Comparing the Effects on Children versus Adults

In principle, the results in table 2 documenting the effect of the policy on children and the results in table 3 documenting the effect on the adults they live with could be used to estimate the effect of children's English skills on that of adults. However, scaling the results in table 3 by the "first-stage" results in table 2 should be done with caution. For several important reasons, such an exercise is likely to produce an overestimated effect of children's language skills on that of adults in the household.

First, it would seem likely that in answering the Census question, immigrant children and adults at least in part compare their English to that of their peers, and because immigrant children acquire new languages far better than adults, a 1-unit increase in the English proficiency variable for an adult might translate into a far smaller improvement in absolute English skills than a 1-unit increase for children. Second, this estimate does not account for the fact that some adults will have multiple children in the household who are capable of performing English-intensive tasks, which would tend to increase the leaning effect but is not accounted for in the first-stage denominator. Finally, leaning instead of learning might be more attractive for an immigrant adult in California than it would be in any other state. In the 2000 Census, 26.8% of the state's population was born outside the United States (the next highest is New York, at 21.3%), many newspapers and radio and television stations are in languages other than English, and employers have long depended on immigrant labor (documented or otherwise).

With these caveats in mind, I do perform a rough calculation. Taking the estimate in column 7 of table 2 and that in column 2 of table 3, it would appear that the effect of increasing by 1 percentage point the probability of having a "useful"-age child who speaks English very well is to reduce the probability that an adult living with her speaks English at all by $0.217 \div 0.257 = 0.84$ percentage points. Thus, even after accounting for the above limitations, it appears that the leaning effect may still be economically meaningful.

E. Heterogeneity in "Leaning" versus "Learning"

Table 4 examines how the propensity to lean on or learn from proficient children varies for specific subgroups of adults, using the baseline specification in column 2 of table 3. I choose the specific cuts of the sample to try to explore some of the implications of the model in Section II, namely, that the tendency to learn or lean should reflect the underlying costs and benefits to adults of learning English. As I will discuss, these divisions of the data may be correlated with unobserved factors that make adults more or less likely to lean, so some caution should be used in interpreting these results too strongly.

One of the model's implications is that when the quality of adults' consumption is independent of English skills, then there is less incentive to learn and more incentive to lean. For example, adults who live in areas with large social networks of individuals from their native country or stores or

Table 4
Heterogeneous Effects of Proposition 227 Compliance on Adults' English Proficiency

	Dependent Variable: Speaks English					
	(1)	(2)	(3)	(4)	(5)	(6)
Compliance × With child 7–15 × After	−.260*	−.0299	−.401***	−.0110	−.586**	.131
	(.148)	(.133)	(.133)	(.130)	(.251)	(.163)
Sample	Hispanic	Non-Hispanic	High language similarity	Low language similarity	No high school education	High school education
H0: Coefficient equal across samples:	p = .183		p = .015		p = .031	
Observations	15,733	12,775	14,333	14,175	12,574	15,934

NOTE.—Data are taken from the 1990 and 2000 IPUMS and include adults living with at least one child. See table 1 for additional sampling rules and table 3 for variable definitions. Except for the different sampling criteria, these regressions replicate the specification in col. 2 of table 3. The first two columns split the sample by Hispanic origin. Columns 3 and 4 split the sample based on "language similarity"—the share of residents in an individual's PUMA in 1990 who report speaking the same primary language as the individual. "High" and "low" language similarity is based on whether an individual is above or below the median value for this variable. Columns 5 and 6 divide the sample based on reported education level. The table also reports p-values for two columns being compared (e.g., Hispanic and Non-Hispanic): these statistics refer to the hypothesis test that the coefficient on the triple interaction term is the same for the sample in the second of the columns and the sample in the first column. In this test, all other coefficients are allowed to vary across samples. Standard errors clustered at the PUMA level a re reported in brackets. IPUMS person-level sample weights are used in all regressions.
* $P < .10$.
** $P < .05$.
*** $P < .01$.

other services that operate in their native language will have a greater incentive to lean.[19] Given the large Hispanic communities in California, I first test whether Hispanic immigrants are more likely to lean. Columns 1 and 2 provide suggestive evidence of this claim. Compared to the baseline specification in column 2 of table 3, the coefficient when only Hispanics are sampled is 25% larger in magnitude, and when only non-Hispanics are sampled is essentially zero, though the hypothesis that the two coefficients are equal cannot quite be rejected at conventional levels.[20] It is also impor-

[19] Indeed, Lazear (1999) finds that the probability that an immigrant learns English is falling in the share of his/her locality that speaks his/her language. Similarly, Lewis (2011) finds that the return to speaking English is very low in heavily Hispanic localities, as a "Spanish-speaking" labor market emerges.

[20] For all cross-sample tests, I run separate regressions for each sample and then test the equality of coefficients using seemingly-unrelated regressions analysis. That is, I do not require that any coefficient be equal across the two samples. When I instead interact the triple interaction term and all lower-order terms with a dummy variable for being in one sample versus the other (and then examine the t-statistic from the quadruple interaction term), I am generally more likely to reject the hy-

tant to note that among children of the "useful" 7–15 age range, Hispanics appear somewhat more likely to learn as a consequence of Proposition 227, as can be seen in appendix table A2. While this difference is not close to statistical significance, it is still difficult to conclude whether the lower levels of English proficiency among Hispanic adults in compliant PUMAS after 1990 is due to their greater propensity to lean or because Proposition 227 had a slightly larger effect on their children than on other immigrant children.

The next two columns of the table present more direct analyses of this local language hypothesis. For each respondent, I calculate the share of all individuals (not just immigrants) in his/her PUMA in 1990 whose primary language is also the respondent's primary language.[21] I separate the sample based on being above or below the median value of this measure (16.9%), and in columns 3 and 4, I present the results from estimating the baseline specification on each of these subsamples. As predicted, adults living in areas where a larger share of residents speak their language are more likely to lean, and the difference is statistically significant. Appendix table A2 shows that this difference does not exist among children (if anything, Proposition 227 has a slightly larger effect for children who live in PUMAs where fewer people speak their native language).

The final pair of columns explores heterogeneity in the cost of acquiring English, using educational background as a proxy. For example, if the cost of learning a new language decreases in educational attainment, then those with limited education will only learn the language if absolutely necessary (i.e., if there are no children of suitable age to translate). Similarly, children's relatively inexpert tutoring may be sufficient for someone with a basic education to pick up English but not enough for someone less educated. Indeed, columns 5 and 6 show that adults who report having fewer than 12 years of schooling are far more likely to lean than other adults, and again this difference is statistically significant. In contrast, the effect of Proposition 227 for children living with an adult with at least 12 years of schooling is es-

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pothesis of equal coefficients on the triple interaction term, so I take the more conservative approach in table 3.

[21] Note that this variable varies by individual and MSA, but not by time. I calculate the MSA language variables for 1990 so that they do not reflect any effects of Proposition 227, though results that pool 1990 and 2000 to estimate the share of individuals speaking a certain language in each MSA are very similar. "Primary language" is based on the Census question: "What language do you speak at home?" That is, I calculate the share of individuals in his/her PUMA who speak at home the language that the respondent speaks at home. As noted earlier, very few adults in the regression samples speak English at home, so this variable should be highly correlated with the share of individuals in a PUMA who speak at home the respondent's native language. Also, if I calculate this measure instead using the share of immigrants in his/her PUMA that speaks an individual's language, the results are very similar.

sentially identical to that for other children (cols. 5 and 6 of table A2). Of course, English proficiency may make educational attainment easier, and thus some reverse causality might be at play if many adults were obtaining education in the United States. However, given the ages and recent arrivals of the adults in the regression sample, the vast majority would have likely finished their education in their home country, and the result holds when I exclude adults who are currently enrolled in school. As such, I take the pattern of coefficients in the last two columns as offering suggestive evidence that the tendency to lean or learn is related to the cost of acquiring English in a manner consistent with the model's predictions.

F. Discussion

The evidence in this section suggests that adults are less likely to learn English when they can rely on the English skills of children and that this effect may be economically significant. The increase in students' English proficiency between 1990 and 2000 was greatest in highly compliant PUMAs. In contrast, English proficiency of adults living with children who could serve as language brokers fell the most in the very areas where students gained the most.

While the heterogeneity results each have endogeneity concerns, taken together they offer some support for the framework described in Section II. Adults indeed appear most likely to "lean" when the need to learn English is limited or the cost of learning is high.

VI. Conclusion

In this article I model how children's acquisition of a given form of human capital can either encourage or discourage adult household members from acquiring it as well. On the one hand, children can teach the skill to adults, which, all else equal, will lower adults' marginal cost of learning the skill. I call this incentive the "learning effect." On the other hand, children's human capital can substitute for that of adults in household production, which will lower adults' marginal benefit of learning the skill. I call this incentive the "leaning effect."

The empirical work focuses on an example in which children receive sudden shocks to their human capital levels and in which I estimate the effects on the adults in their household. In the summer of 1998, Californians passed Proposition 227, with the aim of replacing bilingual education with English immersion in public schools. I find that area compliance with Proposition 227 is associated with greater increases in children's English proficiency between 1990 and 2000. But in the very areas where children's English skills improve the most, adults living with children (and especially adults living with school-age children) have lower levels of English proficiency compared to other adults. Thus, in general, adult immigrants in California appear to lean on their English-speaking children. Moreover, the propensity to lean appears related to the costs and benefits of acquiring English in a manner consistent with the model's predictions.

A natural avenue for future work would be to explore other potential examples of child-to-adult spillovers. The transmission of human capital from children to adults likely plays an especially important role in developing countries. Unlike many developed countries, in which average educational attainment has plateaued, educational attainment in developing countries is still rising with each successive cohort, so children often have more total years of schooling than their parents and thus opportunities to teach them new information and skills. To my knowledge, few of the many studies examining randomized educational interventions aimed at children have investigated whether measures of parents' human capital also change. Programs that encouraged older adults to learn basic literacy from their children could potentially have large welfare gains and leverage the benefits of education expansions. Given the scarcity of resources of governments and nongovernmental organizations in developing countries, promoting investments with positive spillovers to adults and addressing situations with negative spillovers could lead to important welfare gains.

Even in settings where parents and children have the same level of formal education, children often invest more in learning how to use new technologies (e.g., computers). Marketing research suggests that one-half of US children have helped their parents use the Internet to shop at online stores, plan vacations, get driving directions, or download tax forms (Gardner 2007). Thus, child-to-adult spillovers may play an especially important role in settings with rapid technological growth and may help determine the rate at which groups adopt new technologies.

Regardless of the setting, future work may wish to examine the welfare effects of adults' decision to lean versus learn and in particular the effects on their children. While this article focused on empirically establishing child-to-adult spillovers, it is generally silent on how to evaluate the welfare implications. Being forced as a child to be a language or literacy broker might promote responsibility, maturity, and independence, which could improve future labor market outcomes. However, some sociological studies highlight the costs of parents' inability to speak English. Weisskirch and Alva (2002) find that children list report cards and letters from school as the items they most often translate for their parents, suggesting that English-deficient parents face challenges monitoring their children's educational progress. And while many factors likely inhibit health care access for immigrant children, immigrant parents perceive "language problems" as among the most important (Flores et al. 1998). To better understand the welfare consequences of educational externalities within families, future studies evaluating programs aimed at adults—whether ESL classes in the United States or adult literacy interventions in developing countries—might also include measures on their children's outcomes.

Appendix

Proof of Proposition 1

Proof. First, note that adult's human capital $k = k(e)$ is a positive function of investment e alone, so it is sufficient to show the above result for optimal investment e^*.

Recall the first-order condition that holds for optimal e^*: $\psi' y_k(k(e^*), c)k_e(e^*) = \lambda_e(e^*, c)$. Differentiating the first-order condition with respect to c yields

$$\psi'[(y_{kk}k_e e_c^* + y_{kc})k_e + k_{ee}e_c^* y_k] + \psi''(y_k k_e e_c^* + y_c)y_k k_e = \lambda_{ee}e_c^* + \lambda_{ec}. \quad \text{(A1)}$$

Gathering like terms gives an expression for e_c^*:

$$e_c^* = \frac{y_c y_k k_e \psi'' - \lambda_{ec} + y_{kc}k_e\psi'}{\lambda_{ee} - \psi' k_e^2 y_{kk} - k_{ee}y_k\psi' - \psi'' y_k^2 k_e^2}. \quad \text{(A2)}$$

By the assumptions regarding functional forms described in Section II, the denominator is always positive. Thus, the sign of e_c^* is equal to the sign of the numerator.

What is the effect of $-\lambda_{ec}$, the "learning effect?" It only enters the expression in the second term of the numerator, and as $-\lambda_{ec}$ increases, the expression increases as well. Thus, the sign on the "learning effect" is positive.

What is the effect of y_c, the "leaning effect?" It only enters inthe first term of the numerator, and it is multiplied by ψ'', which by the concavity assumption is negative. Thus, the sign on the "leaning effect" is negative.

Table A1
The Effect of Compliance with Proposition 227 on English Proficiency of Children Ages 7–15 (Additional Robustness Checks)

	Dependent Variables: Measures of English Proficiency						
	Speaks Very Well (1)	Proficient (2)	Speaks (3)	Speaks Very Well (4)	Speaks (5)	Speaks Very Well (6)	Speaks Very Well (7)
Compliance rate × After Proposition 227	.257**	.329**	.118*	.146	.121	.307***	.0509
	(.0976)	(.159)	(.0657)	(.135)	(.0716)	(.0978)	(.0790)
After Proposition 227	.0279	.148***	.0108	−.0212	.0123	.0953**	−.0330*
	(.0196)	(.0311)	(.0113)	(.0253)	(.0256)	(.0390)	(.0173)
Male	−.00738	−.0183	−.00286	−.00740	−.00285	−.00787	.000909
	(.00769)	(.0197)	(.00601)	(.00768)	(.00601)	(.00820)	(.00784)
Hispanic	−.178***	−.433***	−.0657***	−.178***	−.0657***	−.146***	−.130***
	(.0394)	(.0603)	(.0135)	(.0395)	(.0135)	(.0424)	(.0331)
Ages in sample	7–15	7–15	7–15	7–15	7–15	7–15	7–15
Additional PUMA controls?	No	No	No	Yes	Yes	No	No

Table A1 (*Continued*)

	Dependent Variables: Measures of English Proficiency						
	Speaks Very Well (1)	Proficient (2)	Speaks (3)	Speaks Very Well (4)	Speaks (5)	Speaks Very Well (6)	Speaks Very Well (7)
Additional individual-level controls?	No	No	No	No	No	Yes	No
Placebo (1980–90)?	No	No	No	No	No	No	Yes
Observations	14,891	14,891	14,891	14,891	14,891	14,891	16,696

SOURCES.—All data are taken from the 1990 and 2000 IPUMS, except for the last column, which uses the 1980 and 1990 IPUMS.

NOTE.—For the sake of comparison, the first column reproduces the result from col. 8 of table 2. All regressions are weighted by IPUMS person-level sample weights. See table 1 for sampling rules. Speaks is an indicator variable for having a 0–3 English proficiency level greater than 0, and Speaks Very Well is an indicator variable for having a proficiency level of 3. Additional PUMA controls refer to Hispanic immigrant share × After, as a PUMA's Hispanic share of immigrants is predictive of Compliance. Additional-level controls refers to individual-level measures (Hispanic dummy variable, Age, and Household income) interacted with After and Compliance. Placebo (1980–90) uses data from 1980 and 1990, with 1990 now acting as the post-227 treatment period. Note that in the final column, one can reject that the coefficient of interest equals that in the first column with $p = .013$. Standard errors are in parentheses.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

Table A2
Heterogeneous Effects of Proposition 227 Compliance on English Proficiency of Children Ages 7–15

	(1)	(2)	(3)	(4)	(5)	(6)
Compliance × After	.319**	.178*	.207	.346***	.213**	.201**
	(.124)	(.100)	(.141)	(.119)	(.104)	(.0911)
Sample	Hispanic	Non-Hispanic	High language similarity	Low language similarity	Highest education level < high school	Highest education level > high school
H0: Coefficients are equal across samples		$p = .367$		$p = .477$		$p = .914$
Observations	17,995	9,765	12,337	15,423	15,543	12,217

NOTE.—Dependent variable is Speaks English Very Well. Data are taken from the 1990 and 2000 IPUMS and include adults living with at least one child. See table 1 for additional sampling rules and table 3 for variable definitions. Except for the different sampling criteria, these regressions replicate the specification in col. 2 of table 3. The first two columns split the sample by Hispanic origin. Columns 3 and 4 split the sample-based language similarity—the share of residents in an individual's PUMA who report speaking the same primary language as the individual. "High" and "low" language similarity is based on whether an individual is above or below the median value for this variable. Columns 5 and 6 performs the same exercise but excludes Hispanics. Columns 7 and 8 divide the sample based on reported education level. The table also reports p-values in every even-numbered column: these statistics refer to the hypothesis test that the coefficient on the triple interaction term is the same for the sample in the column and the sample in the previous (odd-numbered) column. In this test, all other coefficients are allowed to vary across samples. Standard errors clustered at the PUMA level are reported in parentheses. IPUMS person-level sample weights are used in all regressions.

* $p < .10$.
** $P < .05$.
*** $P < .01$.

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