ESSAYS ON IMMIGRATION ECONOMICS

By

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Dedicated with inexpressible gratitude
to my loving parents,
Vincenzo and María Eugenia,
and to my beautiful wife
Indiana.

In memoriam of my aunt Nelly Davies.

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ABSTRACT

      The three questions I study in this dissertation relate to many topics in the area of immigration economics but they can be categorized into two main areas: illegal immigration and immigrant assimilation. In the first chapter I use a regulatory change to investigate the effect of unauthorized immigrants' driving behavior on road safety indicators. The last two chapters tackle two different questions relevant to the issue of immigrant assimilation, the first from an empirical point of view and the second from a theoretical perspective.

      The first paper studies the policy followed by most states in the United States of barring unauthorized immigrants from getting driver's licenses. Before 2001, many states did license all immigrants (legal and illegal). After the attacks of 9/11 most states introduced stricter standards and procedures for issuing driver's licenses. I take advantage of the likely exogeneity of these changes in licensing regulation to identify the effect of this policy on the number of fatal traffic accidents and hit-and-run wrecks over the period between 2000 and 2009. I find that, contrary to what is commonly assumed in policy discussions, restricting the issuance of licenses to undocumented immigrants reduces slightly the number of fatal crashes in states with low immigrant populations, but increases the number of hit-and-run crashes.

      In the second chapter I investigate a question related to the experience of children of immigrants living in two American cities with high immigrant density. High school employment has been shown to impact favorably early labor market outcomes for native youths. Since economic theory suggests that children of immigrants may stand to benefit more than natives from human capital obtained from

working while in school, I test whether this same pattern is observable in the case of children of immigrants. Results indicate that high school employment has a small effect only on wages and it is very heterogeneous. In particular, the wage benefit is restricted to male students and American children of immigrants.

The third chapter keeps the focus on immigrant assimilation, but now from an intergenerational perspective. Recent research in economics has identified a fall in the rate of economic assimilation among recent cohorts of immigrants to the United States. The slower assimilation seems to occur both within and across generations. One proposed explanation to explain this fact centers on immigrant settlement in ethnic enclaves and the resultant lower incentives to acquire skills and knowledge relevant to the U.S. labor market, particularly the English language. However, it has long been observed by linguists that immigrant groups tend to lose their first language fairly rapidly across generations, often as early as the third generation. This chapter proposes to investigate the link between ethnic enclaves and language assimilation in the context of a model of intergenerational language assimilation where ethnic concentration is possible.

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CHAPTER ONE

1 Does Denying Driver Licenses to Unauthorized Immigrants Affect Road Safety?

"For the most part, it's not a big risk because, if you think about it, the people that drive without a license are more careful."

California insurance broker about selling policies to unauthorized immigrants.

"My enforcement people said it's a matter of public safety. What we need is public safety, a reduction in traffic fatalities."

Gov. Bill Richardson on supporting giving driver licenses to unauthorized immigrants.

1.1 Introduction

As immigrants move from main gateway cities to communities all over the U.S. and begin to exert a noticeable influence on the economies of those communities, new challenges arise for local and regional policy makers. One such challenge refers to the definition of driving licensing standards and whether driver's licenses should be made available to unauthorized immigrants. The dispersion of the recent post-1995 unauthorized immigration wave to small cities with poor or nonexistent transit systems means that many unauthorized immigrants drive without a license. Though scholars have studied the travel behavior of immigrants (Liu and Painter 2011, Blumenberg 2009), little has been said about the traveling patterns of unauthorized immigrants or its effects on outcomes of the transport infrastructure.

The object of this paper is to measure the effect of excluding unauthorized immigrants from access to driver licenses on the road safety conditions of states as measured by the number of fatal crashes and the number of fatal hit-and-run accidents. Traffic accidents remain one of the most important causes of death in the U.S., accounting for between 30,000 and 40,000 yearly fatalities in the last decade. The potential repercussions of license restrictions on road traffic safety is one of the most discussed aspects of this policy. Opponents to the adoption of restrictions argue that denying licenses to undocumented immigrants will not prevent them from driving; worse, it will deny them the skill and knowledge to become better drivers. As a consequence such a policy may increase the number of traffic accidents, the percentage of uninsured drivers, and the frequency of hit and run accidents. Consistent with this expectation, previous research has shown that unlicensed drivers are more prone to cause or be involved in fatal accidents (DeYoung et al., 1997). However, denying licenses may also act through other channels that have so far received little or zero consideration. In particular, immigrants' driving behavior will likely change in reaction to the impossibility of getting a driver's license. Since the risk of deportation is higher when driving without a license, immigrants will face a higher cost of engaging in accident-prone driving behaviors under a restrictive policy regime. As a consequence, it is possible in principle that the aggregate number of accidents may fall after such a policy is implemented. This reasoning is analogous to the theory of risk compensation proposed in the setting of vehicle safety regulation by Peltzman (1975). This theory posits that the introduction of safety measures like seat belts may produce little change or even an increment in the number of accidents. The reason is that the risk of an accident is endogenous to incentives as perceived by the driver, and if safety regulations reduce this perceived risk, more reckless driving may result.

In the context of pending immigration reform, licensing standards have regained policy relevance because of the approval in 2012 of new rules by the Department of Homeland Security. The Deferred Action for Childhood Arrivals (DACA) program grants special status and deferral of deportation actions to individuals who immigrated illegally as children. This regulation turns thousands who previously were banned from obtaining a license into eligible for one. As of November 2013, at least 37 states have accepted the new DACA status as sufficient for meeting the requirement of lawful presence in the United States.

To test for the existence of risk compensation effects I take advantage of changes in state licensing

rules motivated by national security concerns after the 9/11 terrorist attacks. The rationale for studying this period is the likely exogenous character of these changes to the evolution of state traffic safety conditions. In the period between 2000 and 2009 the majority of states that issued driver licenses to unauthorized immigrants changed their policy. Congress passed the Real ID Act in 2005 establishing a set of national standards for the issuance of driver licenses and ID cards by states. Though state compliance with the new federal standards is required by the law, the majority of states are currently not in compliance with Real ID. However, since 2001 most states have adopted regulations and procedures similar to those mandated by Read ID, in particular, the establishment of a legal presence requirement to be satisfied by any foreign born individual applying for a driver's license or state ID card.

I merge the licensing data mentioned above with a panel of state data on traffic accidents, population characteristics, economic conditions and the presence of unauthorized immigrants, and use it to estimate a linear panel model of fatal traffic accidents and fatal hit-and-run accidents. The results show that not licensing unauthorized immigrants reduces the number of fatal accidents, but only in states with a low percentage of unauthorized immigrants in the general population. In states with an unauthorized immigrant share of less than 2%, the adoption of a restrictive policy lowers the number of fatal accidents. But the effect on the number of hit-and-run accidents is positive, and not a function of the share of unauthorized immigrants. Both strict licensing standards and the percentage of unauthorized immigrants increase the number of fatal hit-and-run accidents. Denying driver's licenses raises the number of hit-and-runs by 17% while each percentage point increase in the percentage of unauthorized immigrants increases the number of hit-and-runs by 9%.

1.2 Background

Whether to grant driver licenses to undocumented immigrants has been a recurrent issue in American public policy debates, especially so during the period of increased unauthorized immigrant arrivals since the mid-1990s (Passel, 2005). For various reasons this issue illustrates very well the diversity of conflicting and changing interests inherent to the definition of public policy. First, it is important to understand the unique character of driver's licenses in the United States. Besides the obvious safety implications associated with the definition of minimum competency and

physical requirements for vehicle operation, the issuance of driver's licenses in the United States involves considerations beyond the mere bestowal of driving privileges. This is so because there is no federal agency in charge of issuing standardized identity cards to all American citizens. Instead, driver's licenses are issued autonomously by each individual state, and for most practical purposes, function as the main source of identification. Each state sets its own eligibility criteria in terms of required documentation to obtain a driver's license. As the predominant form of identification, licenses provide ready access to many basic services, such as opening a bank account, cashing a check, or boarding a plane. A consequence of the special status of driver's licenses in American society is that, for many citizens, the concept of making them available to unauthorized immigrants has an element of implicit legitimization of impunity from federal immigration law (Mortensen, 2009). On the other side of the debate, immigrants' advocates recognize the need of revamping the immigration system, but disagree about using license restrictions as an instrument of immigration policy. They urge instead that immigration policy should be set at the federal level and focus on the public safety implications of having a growing population of state motorists without certified knowledge of the rules of the road. Some states like New Mexico and Washington agree with this perspective and have decided to license unauthorized immigrants in the belief that doing so will make them safer drivers. Another perceived benefit of licensing immigrants is that it provides the opportunity to state agencies of controlling and keeping track of a population inherently difficult to identify and measure.

1.2.1 9/11 and restrictions on driver's licenses

After the terrorist attacks of 2001 the issue of licensing standards became associated with national security concerns and acquired renewed prominence in the national policy debate. The attack revealed a critical weakness of the system. Most of the 19 terrorists that executed the attack used driver's licenses (instead of passports) to board the planes they hijacked. Lax standards for issuing driver's licenses in some states and the lack of integration of states' procedures with national security databases permitted terrorists to obtain valid driver's licenses, in some cases, such as Virginia, through fraudulent means. Seven terrorists obtained legitimate Virginia driver licenses by presenting forged notarized vouchers to satisfy identification and state residency requirements. This fact attracted intense scrutiny of procedures regarding the issuance of driver's licenses and

turned a routine bureaucratic transaction into a potential source of new threats to the national security. Simultaneously, the fast growth of identity theft crime also brought increasing attention to potential loopholes in licensing procedures because driver's licenses are sometimes used as a "feeder" document that facilitates identity theft and other criminal activities (Mortensen, 2009).

The aftermath of 9/11 provided a strong stimulus to base discussion of licensing procedures on national security repercussions and the need to ensure that a similar attack would not repeat in the future. This shift made it easier to reach an actionable consensus as all parties to the debate recognized the need to promptly address security vulnerabilities. Congress and President Bush created *The National Commission on Terrorist Attacks Upon the United States* (popularly known as the 9/11 commission) on November of 2002. The report released by the commission two years later explicitly recommended that the federal government set standards for the issuance of birth certificates and sources of identification, including driver's licenses. In 2005 Congress passed the Real ID Act which indirectly established national standards regulating states' issuance of driver's licenses and personal identification cards. Indeed, although the Real ID Act does not mandate the adoption of minimum standards on the part of states, the law authorizes federal agencies to reject state issued documents necessary for "official purposes" if such documents fail to comply with the recommendations established in the Real ID Act. As defined by the law, the phrase "official purposes" includes "accessing Federal facilities, boarding federally regulated commercial aircraft, entering nuclear power plants, and any other purpose that the Secretary shall determine". The Real ID Act specifically establishes verification of legal immigration status of applicants as one of the necessary requirements for deeming identity documents as acceptable for official purposes.

Although initially the law was scheduled to be partially implemented and enforced by 2008, it encountered strong opposition from the states as well as from interest groups in all sectors of the political spectrum. Enforcement of the Act has been postponed at least three times and the great majority of states are not yet in compliance with its stipulations. The main points of contention raised by states against the new law are, first, the perception that the new regime amounts to a virtual implementation of a national identification system threatening to citizens' individual privacy, and second, the significant burden on states' taxpayers of implementing the new requirements established by the law.

Though most states have refused to adopt Real ID standards for issuing driver's licenses, which

include the verification of legal presence in the country, many state legislatures have on their own adopted a legal presence requirement for obtaining a license. At the start of 2001, most state jurisdictions in the contiguous United States plus the District of Columbia already denied driver's licenses to unauthorized immigrants. However, after 2001 most of the states without license restrictions changed their policy by either including an explicit prohibition in their statutes or by revising their regulations at the administrative level. By 2009 only three states continued to license unauthorized immigrants–New Mexico, Utah, and Washington (with New Mexico actually starting to license unauthorized immigrants in 2003). Evidence for the importance of the 9/11 events in causing this policy shift can be appreciated in Figure 1, which refers to the contiguous United States and the District of Columbia. Before the attacks, some states were actually considering to loosen their standards. In effect, Tennessee made licenses available to all state residents irrespective of immigration status in 2001 (prior to the terrorist attacks).[1]

News archives from the period show that, in all cases where access to driver's licenses was withdrawn from undocumented immigrants, the most important consideration in public discussions of the measure was to limit the risk of terrorists using licenses to perpetrate another attack like 9/11. This is important because it lends credibility to the thesis that the regulatory changes were exogenous to states' traffic safety conditions. In other words, the adoption of a restrictive policy regarding driver's licenses is not caused by a perceived need to respond to traffic safety developments. Therefore, the decade between 2000 and 2010 is useful to study the effect on traffic safety of denying driver's licenses to illegal immigrants.

1.2.2 Driver's licenses as a safety regulation

The issuance of driver's licenses has been traditionally a right reserved to states, which have autonomously determined procedures for issuance and enforcement.[2] When the first automobiles were introduced, motorists did not need to obtain a license in order to drive their vehicles (Watner, 2004). However, as the number of cars on the road began to increase and accidents became more frequent, the need to protect public safety moved states to regulate the right to drive a car. Though initially not part of the licensing procedures, testing of drivers' knowledge of traffic laws

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[1]Then in 2004 it started restricting licenses again but introduced a "certificate for driving" aimed at applicants who did not meet immigration status requirements. Finally, in 2006 it also suspended issuance of the driving certificates.

[2]However, the federal government retains control over the issuance of commercial driver's licenses.

and practical competence with a vehicle were soon incorporated as part of licensing procedures. The first state to pass a driver's license law was Rhode Island in 1908 and by 1954 all remaining states had similar laws (Harberson and Doherty, 2002).

Though safety considerations were important for the adoption of driving licensing, the question of whether licensing indeed reduces risk of accident seems not to have been studied empirically. This is unsurprising given that answering such a question would necessitate randomly assigning licensing status to a sample of drivers and comparing their accident rates. Since holding a driver's license is mandatory for vehicle operation in every state, such experiment is not feasible. However, it does seem reasonable a priori to expect that going through the process of obtaining a license would diminish accident risk at least marginally.

A related topic that has received more attention is the traffic safety risk posed by drivers who choose to drive without or with a suspended/revoked license.[3] The economic significance of this phenomenon warrants its consideration. According to the American Automobile Association (2011), in the period between 2007 and 2009, drivers without a license or invalidly licensed were involved in 18.2% of fatal crashes, which resulted in the death of 21,049 people. In the case of strictly unlicensed drivers, the involvement rate in fatal accidents was of 5% over the same period. Inferring from these statistics whether unlicensed involvement rates are higher or lower than for validly licensed drivers would require us to know the prevalence of unlicensed and validly licensed drivers on the roads. This information is inherently difficult to get as unlicensed drivers are by definition missing from states' driver information records and their presence on highways goes undetected until they crash or are pulled over for a traffic violation. DeYoung et al. (1997) address this problem by applying a sub-sampling method to fatal accident data. In particular, they select two-vehicle fatal crashes where only one of the drivers is cited by law enforcement. This strategy hinges on the assumption that the set of innocent drivers (i.e., those not cited by law enforcement) represents a random sample of the general population of drivers. The percentage of innocent drivers in each group (licensed or unlicensed) constitutes then a measure of the group's exposure or prevalence on the roads. If the percentage of at-fault drivers in the group is greater (less) than the exposure percentage, then the group is over-involved (under-involved) in

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[3]Note this is not completely relevant to the previous question as drivers that choose to drive without or with a suspended/revoked license are not a representative sample of the overall population of drivers.

fatal car accidents. Using California data for the period 1987-1992 DeYoung et al. (1997) find that unlicensed drivers are significantly over-involved in fatal accidents compared with licensed and invalidly licensed drivers. They estimate involvement ratios (percentage at fault divided by the percentage innocent) of 0.73 for licensed drivers and 3.58 for unlicensed drivers. This suggests that, according to these data, unlicensed drivers are almost five times more likely to cause a fatal accident than licensed drivers. Obviously, other driver characteristics besides licensing status (age, sex, attitude toward risk) likely explain the majority of this difference in accident risk between both driving groups; however, it is not unreasonable to suppose that not being tested on driving skills and knowledge also plays a moderate role.

If licensing does reduce the risk of accident for drivers, then denying driver's licenses to unauthorized immigrants would increase their accident risk. In localities where immigrants are unable to substitute away from driving (by, for example, using public transportation or carpooling), it is likely that immigrants will continue driving even without a license in order to get to their jobs or carry out their everyday activities. This increased presence of presumably higher-risk drivers could then impact highway safety. This remains the main justification presented by immigrant groups' advocates against restricting licenses.

Other dimensions of traffic safety, such as the proportion of uninsured drivers, could also be affected by denying driver's licenses to unauthorized immigrants since purchasing car insurance usually requires submitting a driver's license number. Immigrants without a license are more likely to drive without car insurance also, resulting in a higher number of uninsured drivers on state highways. The consequence for fatality and collision counts is uncertain. Cohen and Dehejia (2004) report that traffic fatalities increase by 2% in response to a fall of one percentage point in uninsured drivers, suggesting uninsured motorists represent a lower accident risk on the roads. Query and Kumazawa (2011) by contrast find a positive association between the number of uninsured drivers and car fatalities. A negative externality on licensed drivers of a large presence of uninsured drivers is the rise of insurance premiums paid by licensed drivers. Finally, opponents to the licensing of illegal immigrants claim that immigrants have few assets to protect and therefore not much of an incentive to buy car insurance even if they are licensed. But providing licenses to immigrants could help them gain access to other public benefits.

Less disagreement exists on the potential consequences of having a large number of unlicensed

immigrant drivers on the frequency of hit and run traffic incidents. Unauthorized immigrants face a significant risk of being detained and deported if involved in a collision. As a consequence, they have at least as strong of an incentive as other drivers engaging in unlawful behavior (drunken driving, driving with a suspended or revoked license) to flee the scene of a traffic accident.

1.2.3 Conceptual framework

Assuming that driver's licenses do work as a traffic safety regulation effective in reducing accident risk, then it is possible to frame the discussion of the effects of license restrictions in terms of the literature examining the relation between safety regulation and driver behavior. This literature was initiated by Peltzman (1975) who studied the effects of government-mandated safety devices in vehicles, such as seat belts and padded dashboards. Peltzman posited that the potentially beneficial effects of mandatory safety features could be offset by the tendency of drivers to react by adopting more risky behavior. This hypothesized mechanism is referred to as the Peltzman compensation effect in the safety regulation literature. The basic message of Peltzman's analysis is the need to consider the changes on driver behavior generated by exogenous changes in safety regulation, i.e., to factor in the adjustments in the private demand for safety that result from regulation-induced changes in the price of risky behavior.

In the context of driver's licenses and unauthorized immigrants there is also a possibility of ignoring the immigrant drivers' response to different incentives brought about by regulatory change. Besides potentially increasing the accident risk of unauthorized immigrant drivers as discussed previously and as pointed out by immigrant advocates, restricting driver's licenses also increases the risk of deportation in the event of an accident. In effect, without a license an immigrant's unauthorized condition is more easily recognized by law enforcement. In consequence, withdrawing access to driver licenses affects the price of driving intensity[4] in terms of deportation risk. The immigrant driver's response in this case may be to lower the driving intensity by enough so that the rise in accident risk resulting from the lack of licensing testing is completely offset. Hence, although many advocates of driver's licenses for undocumented immigrants point out that not granting licenses increases immigrants' risk as motorists, the potential existence of a compensation effect gives reason to believe that the net effect on accident risk may be ambiguous. This is

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[4]This term, introduced by Peltzman, refers to any type of driving behavior that increases the probability of an accident, like driving faster, intoxicated, or in a more careless manner.

analogous to Peltzman's setting in the case of removing instead of implementing a safety regulation. Drivers in that case will offset the increased safety hazard by driving more carefully.

The immigrant driver's response can also occur along the extensive margin, i.e., regarding the decision on whether to drive or not in the first place. The compensation effect may induce some unauthorized immigrants to stop driving altogether, especially if they live in places with good public transportation or extensive immigrant networks. In such places immigrants can easily substitute toward commuting by using public transportation or carpooling.[5] Adjustments on the extensive margin might be important to road safety if they cause significant changes in the demographic and ethnic composition of highway drivers and if these characteristics influence drivers' accident risk.

1.3 Literature

The effect of regulation on drivers' behavior has been studied extensively by economists after the seminal work by Peltzman (1975). A large segment of the literature spawned by Peltzman's contribution has focused on testing empirically the existence of his compensation effect. The evidence so far has not been conclusive. In his paper Peltzman compares pre-regulation forecasts of traffic fatalities with post-regulation actual fatalities. He finds evidence of fewer occupant fatalities but more pedestrian deaths and nonfatal accidents, a pattern he interprets as confirmation of drivers' optimal response to regulation. Graham and Garber (1984) show that Peltzman's evidence of compensation via more non-occupant fatalities is not robust to reasonable changes in functional form specification and conclude that safety standards mandated by the federal government in 1968 did save lives. Crandall and Graham (1984) try to detect offsetting behavior by devising two proxy variables to measure the prevalence of crashworthiness standards. Their results show some evidence of offsetting behavior but not at the high levels reported by Pelztman.

Focusing solely on safety-belt laws Evans and Graham (1991), estimate a fixed-effect model of accident fatalities at the state level and report some evidence of risk compensation. Seat-belt regulations do increase the number of non-occupant fatalities, but not by enough to prevent a reduction of overall fatalities of between 40 and 50 percent. Cohen and Einav (2003) study the same question but decompose the law effect into two components: the effect of the law on seat-belt usage rates

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[5]Conversely, in more car-dependent communities unlicensed immigrant drivers will only be able to adjust by changing their driving intensity.

and the effect of usage rates on traffic fatalities. Using this instrumental variable strategy they conclude that seat-belt usage unambiguously reduces traffic fatalities of vehicle occupants and has no significant effect on fatalities among non-occupants. Contrary to the existence of the Peltzman effect then, they conclude that increases of seat-belt usage caused by regulation do not have any significant effect on driving behavior.

More recently, Sobel and Nesbit (2007) rely on individual driver data from NASCAR races instead of aggregate accident data to test the existence of a Peltzman effect. This approach represents an improvement over the use of aggregate data which by its nature includes the influence of many factors like compliance or enforcement that cannot be isolated and controlled for. The authors find evidence of offsetting behavior in NASCAR, which, however, does not translate into more injuries because the offsetting effect is not complete.

The literature on the transportation impact of immigrants has naturally paid little attention to the case of unauthorized immigrants due to obvious data limitations. Regarding immigrants in general, scholars have found that compared with natives, recent immigrants are more likely to rely on alternative modes of transportation such as public transit, carpooling, or walking. Immigrants with more time in the U.S. gradually assimilate to more automobile driving. Using data from the 2006 American Community Survey, Blumenberg (2009) estimates that immigrants are 1.8 times more likely to commute by carpool and 2.8 times more likely to commute by transit compared with native-born individuals. This pattern is more prevalent for immigrants living in ethnic enclaves or working in ethnic jobs (Liu and Painter, 2011).

A paper studying a question related to the focus of the present one is Query and Kumazawa (2011). It studies the relationship between the fraction of unauthorized immigrants and the percentage of uninsured drivers. The authors estimate a simultaneous equations model using state cross-section data for 2007 and report that an increment of the illegal immigrant population in states with loose licensing standards is associated with more uninsured drivers. It is not clear if this result represents a refutation of the premise that licensing unauthorized immigrants helps in reducing the uninsured motorist population. Their model does not measure a similar statistically significant effect of unauthorized immigrants on the uninsured percentage in states that deny driver licenses. Additionally, the coefficient on the dummy variable for lax licensing requirements (i.e., representing states that license unauthorized immigrants) in the uninsured percentage

equation is negative (though not significant).

1.4 Data

I use a panel of annual data for 41 states plus the District of Columbia over the period between 2000 and 2009. In total, the number of state-year combinations available is 420. After excluding Alaska and Hawaii, the rest of states are selected based on the estimates of unauthorized populations at the state level produced by Passel and Cohn (2011) for the years 2000, 2005, 2007, and 2010. These figures are based on the residual method,[6] which is the most commonly used one to estimate the size of unauthorized populations. No state is included for which Passel and Cohn (2011) do not provide an estimate of the unauthorized population in all of the years they consider. The rationale for this decision is that the unauthorized population in those states is likely sufficiently small that it would not generate variation in the number of accidents due to changes in immigrants' driving behavior. The complete list of excluded states is: Alaska, Hawaii, Maine, Montana, North Dakota, South Dakota, Vermont, West Virginia, and Wyoming. Data for the years in which Passel and Cohn (2011) do not produce a number are obtained by interpolating from the available years. A quadratic trend equation fitted using linear regression is used to complete the series for each state.

The National Highway Traffic Safety Administration (NHTSA) gathers data on the number of fatal vehicle accidents in each state. This agency prepares the Fatality Analysis Reporting System (FARS) which is a nationwide census of all vehicle accidents resulting in at least one fatality. These data were merged with data produced by the Census Bureau on state overall population, population of 15 years of age or less, population of 65 years of age or more, Black population, Hispanic population. Also from the Census Bureau is the median household income by state for each year in the sample. State unemployment rates come from the Bureau of Labor Statistics. The Office of Highway Policy Information (OHPI) produces data on the number Vehicle Miles Traveled (VMT) by state. VMT is an estimate constructed by a sampling of representative roads in each state.

The data on each state's regime regarding issuance of driver licenses come from different sources, mainly two policy reports: The "State Driver's License Requirements" prepared by the National Immigration Law Center (NILC) and "Summary of State Laws on the Issuance of Driver's Li-

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[6]Essentially, the residual method compares the number of legal immigrants as reflected in immigration administrative records with estimates of the total foreign born population obtained from the Census or a statistical survey like the Current Population Survey or the American Community Survey.

censes" by the Congressional Research Service (CRS). Information on two types of requirements are obtained from these reports to identify a state as one that provides unimpeded access to driver licenses: First, not requiring applicants to prove their identity by presenting a Social Security Number (SSN); second, not having a formal requirement for evidence of lawful presence in the country (such as a visa, green card, work permit, etc). However, in some cases, even though the statutes do not stipulate an explicit lawful presence requirement and/or allow exceptions to the SSN requirement, the state may still implement specific administrative regulations at the agency level that translate into the non-eligibility of unauthorized immigrants for a driver's license. Unfortunately, no record of these practices by states' transportation agencies is available. Because of this, the data obtained from the aforementioned sources had to be verified and complemented by systematic search queries of the news archives LexisAcademic and NewsBank. This makes it possible to contrast actual state practice, as commented on in the news record, with the summaries of statutes and written regulations compiled by the NILC and the CRS. To give an example, driver's license regulations in Missouri, Nebraska, and Nevada do not include provisions regarding lawful presence in the country and allow for exceptions to the SSN requirement. However, the set of acceptable documents defined by motor vehicle administrations in those states, which include federal immigration certifications in foreign passports, in practice exclude illegal immigrants from eligibility.

1.5 Empirical strategy

To measure the effect of a restrictive policy regarding driver licenses and unauthorized immigrants I use a linear panel data model

$$Y_{it} = \beta_0 + \beta_1 \, NoDL_{it} + \mathbf{X}_{it}\mathbf{\Delta} + S_i + T_t + \epsilon_{it}.$$

The dependent variable (either the logarithm of the number of vehicle accidents or hit-and-runs accidents that result in at least one fatality) refers to state i and year t. The model includes fixed effects for both individual states and years. The matrix $\mathbf{X}_{it}$ includes state characteristics that vary over time and influence the dependent variable. These controls measure demographic characteristics, presence of unauthorized immigration, and state economic conditions. The variable $NoDL_{it}$

is a dummy variable indicating if state i during year t had a policy of denying driver licenses to applicants unable to present sufficient evidence of lawful presence in the country. Clearly, β_1 is the coefficient of interest. Controlling for the unauthorized population allows me to measure policy effects that result from changes in driving or travel behavior, and not from movements of immigrants across state borders.

I take advantage of the adoption of stricter policies motivated by the security concerns raised by 9/11. After 2001, 14 states adopted a stricter regime for the issuance of driver licenses, typically ending their availability for unauthorized immigrants. The changes usually involved requiring that an applicant present proof of Lawful Presence or a Social Security Number. During this period Washington, Utah and New Mexico continued to furnish driver licenses to unauthorized immigrants.

Since the change in policies during this period is very likely not provoked by statistics on fatality crashes, this regime change can be treated as an exogenous shock useful to identify the effect of denying driver licenses to unauthorized immigrants on the number of fatal vehicle accidents.

1.6 Results

Table 1.1 presents estimation results for alternative specifications of the empirical model for annual fatal crashes. Specifications differ based on whether state economic conditions and the interaction between the policy and unauthorized immigrant population are included. Total population and demographic characteristics are included in all specifications. Demographic information on age and ethnic composition control for factors such as driving experience and skills, attitudes toward risky driving, and other driving-related patterns that vary with population characteristics. Finally, the percentage of unauthorized immigrants in the population is included as an independent variable. This helps to interpret the estimated policy effect as resulting from changes in the driving behavior by unauthorized immigrants in the state. In effect, if immigrants respond to the adoption of stricter licensing standards by fleeing or avoiding the state, then omitting a measure of the number of unauthorized immigrants may confound the hypothesized effect on immigrants' driving behavior.

Columns (1) and (2) report estimates from specifications excluding controls for economic conditions. The results in column (1) show a negative and statistically significant effect of restricting

driver licenses. States that do not license unauthorized immigrants see 3.4% fewer fatal accidents. The percentage of unauthorized immigrants, total population, and Hispanic population, all have positive and significant effects on the fatal accident count. Adding the interaction term of the effect of the policy times the percentage of unauthorized immigrants in the state increases the magnitude of the policy effect. However, the sign on the interaction term is positive which means that the policy effect lessens when the percentage of undocumented immigrants in the population is bigger. The mean value of the percentage of unauthorized immigrants (calculated over all states and years) is 2.97%. At that level, the impact of the law is of only -1%.

Models (3) and (4) include controls for economic conditions. Consistent with Ruhm (2000), fatal car accidents seem to be procyclical, i.e., the number of accidents is positively correlated with state economic conditions. The variables included to account for the state of the regional economy are real median household income and the unemployment rate. The unemployment rate is a highly significant predictor while the coefficient on household incomes is only marginally significant. Controlling for economic conditions makes sense in this setting because the sample period includes the first years of the economic recession (2007-2009). The recession may have caused a reduction in the prevalence of risky driving behavior such as drunk driving or speeding as well as a fall in overall driving (see Figure 2). This would lead to fewer vehicle accidents. Since the recession years are also those in which the majority of states have already stopped granting driver licenses to undocumented immigrants, failing to include measures of economic conditions may overestimate the effect of the policy. This seems to be confirmed by the estimates presented in the last two columns of Table 1.1. In effect, after including economic controls the policy effect falls by approximately one third in the specification without an interaction term. A similar decline is also verified when including the interaction of policy and unauthorized population.

The pattern observed in column (2) reappears in column (4). The coefficient on the policy variable increases its magnitude and the interaction variable has a positive coefficient. This suggests the policy may only be effective in reducing the number of accidents when implemented in states with low unauthorized populations. According to these estimates, fatal accidents decline by at least 1% when the percentage of unauthorized immigrants is at most 2.73%.[7] Over the period

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[7]Let p_{ui} be the percentage of unauthorized immigrants, then the effect is less than -1% when $-.0851 + 0.0275\,p_{ui} < -0.01$, which implies that $p_{ui} < 2.73$.

under study, the number of unauthorized immigrants as a fraction of the total population falls below this cut-off level for approximately half the state-year observations in the sample. The right-skewed distribution of state averages (calculated over the ten-year sample period) is summarized in Figure 3. States on the long tail of the distribution have bigger populations and higher urbanization rates. Immigrants in these states may find it easier to substitute away from driving in response to the restraining of driver licenses by using public transportation or other mobility alternatives. This could explain the pattern observed in columns (2) and (4) of Table 1.1 when the interaction term is included. Table 1.2 presents state specific effects calculated using specification (4) of Table 1.1.[8] Unsurprisingly, low immigration states where the policy seems to produce a negative impact exhibit estimated effects which are significantly larger than the coefficients reported in columns (1) and (3) of Table 1.1. This strengthens the case for the economic importance of the induced compensation effects on the driving behavior of unauthorized immigrants, at least in the case of states with low unauthorized immigrant populations.

1.6.1 Some robustness checks

The robustness of results presented so far can be tested by changing various aspects of the preferred specification presented in column (4) of Table 1.1. Though the preferred specification includes the dependent variable in logarithms and controls for population size, it may be argued that normalizing variables by population or driving intensity is more appropriate since population and accident data are generally trending. In the first two rows of Table 1.3 I explore two possibilities based on this criticism. The logarithm of two different normalizations of the accident count is used instead of the logarithm of the raw variable. Fatal crashes per 100,000 people is used in row (1) and fatal crashes per one billion vehicle-miles traveled in row (2). Additionally, in row (1) the logarithm of total vehicle-miles traveled per capita is included as an independent variables instead of the logarithm of total population. The results obtained in both cases are consistent with estimates presented previously.

In row (3) a proxy for the unauthorized population is used instead of the estimations based on Passel and Cohn (2011). Estimates of the number of non-citizen Hispanics in each state are

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[8]To calculate state-specific effects, the average percentage of unauthorized immigrants for each state is calculated using yearly values for the period 2000-2009. This value is then used in the expression $-0.0851 + 0.0275\,\overline{p_{ui,s}}$, formed using the first and second coefficient estimates in colum (4) of Table 1.1, and where $\overline{p_{ui,s}}$ represents the average unauthorized immigrant share of the population in state s.

constructed using micro data from the American Communities Surveys (2001-2009) and the 2000 Census. The choice of this proxy variable is motivated by the fact that individuals from Mexico and other Latin America nations make up the majority of the unauthorized immigrant population living in the U.S. (Passel and Cohn, 2011). Clearly, this is a less than perfect solution because a large proportion of non-citizen foreign-born Hispanics are not illegally living in the country (as this category includes legal permanent or temporary residents). However, for the purposes of testing robustness I use this as a proxy for the unauthorized population. The estimated policy effect is weakened considerably by doing this, although the direction of the effect remains unchanged. The estimated effect of the policy is still reducing accidents in states with a low presence of undocumented immigrants.

In rows (4) and (5) the recession years are excluded from the sample. As shown in Figure 2, during these years an important decline occurs in vehicle-miles traveled and fatal accidents that may confound the estimated effects. Row (4) drops years 2008 and 2009, while row (5) drops also year 2007. In both cases the results are comparable to those presented in Table 1.1.

Finally, in row (6) new variables are included to control for the influence of other regulatory changes at the state level that are intended to lower the number of fatal accidents. Over the period under study, a few states adopted more stringent safety measures to curb accidents. Simultaneously, the prevalence of seat belts usage increased consistently across all states. To partially correct for this confounding influence, I add three new variables to the preferred specification: the logarithm of seat belt usage rates (for all states excluding New Hampshire), a dummy variable for the state having primary enforcement of seat belt regulations (which means that police officers can stop drivers not wearing seat belts, even if no other traffic offense is committed), and a dummy for whether the state has a maximum Blood Alcohol Content (BAC) of 0.08 (in 2001 federal law allowed withholding of states' highway construction funds in the case of states that did not lower the maximum BAC from 0.10 to 0.08 by 2003). The results obtained after doing this are indistinguishable from the other more basic estimates.

1.6.2 Hit-and-run accidents

As discussed before, another dimension of road safety that could be impacted by denying driver licenses to unauthorized immigrants is the number of hit-and-run accidents. Besides potential

arrest, illegal immigrants without a license also risk being deported in the event of an accident. Hence, an unintended consequence of adopting stricter requirements for issuing a driver's license is that illegal immigrants that choose to drive will be more likely to flee the scene of a traffic accident. Anecdotal evidence for the existence of this relationship is often presented in the general press when discussing traffic law enforcement in states with a large presence of undocumented immigrants (Cabanatuan and McCormick, 2003).

To test this claim it would be ideal to include measures of the total number of state hit-and-run accidents in the empirical model. However, this is not available. The data in the FARS refer only to accidents involving at least one fatality. Hence, the hit-and-run accidents included in this analysis represent a subset of the total number of such accidents.

Table 1.4 presents estimation results using the same specifications as Table 1.1. Although the fit of these models is lower than in the case of total fatal accidents, the restriction of driver licenses does show a positive and statistically significant impact in Models (1) and (3). Also in these columns, the percentage of unauthorized immigrants increases the number of accidents with a hit and run as hypothesized before. Turning to columns (2) and (4), it appears that the positive effect of restricting licenses does not depend on the level of the unauthorized population as was the case with overall fatal accidents.

It is interesting to notice that the number of fatal hit-and-run accidents rises with license restrictions even though this policy causes the number of total fatal accidents to fall. This means that though the policy generates fewer fatal accidents, a higher proportion of those accidents involve a hit and run.

1.7 Conclusions

In this paper I have used state data for the period 2000-2009 to estimate the effect of restricting access to driver's licenses from unauthorized immigrants on the number of fatal accidents and fatal hit-and-runs. Contrary to the presumption typically made by policy advocates and analysts, restricting licenses seems to reduce the number of fatal accidents instead of increasing it, at least in states where the percentage unauthorized is less than 2%. The dependence of this relationship on immigrant density may be related to the greater availability of public transit and immigrant networks in states with a large presence of unauthorized immigrants which allows immigrants to

avoid driving cars. The result is robust to changes in variable definitions, period of analysis, and inclusion of controls for other driving regulations impacting the number of fatal crashes. The relevance of Peltzman's framework to study immigrants' response to regulations affecting deportation risk seems to be confirmed in this case. In the case of hit-and-runs, a restrictive policy increases the number of incidents as assumed in policy discussions, and this effect is independent of the unauthorized immigrant presence in the state.

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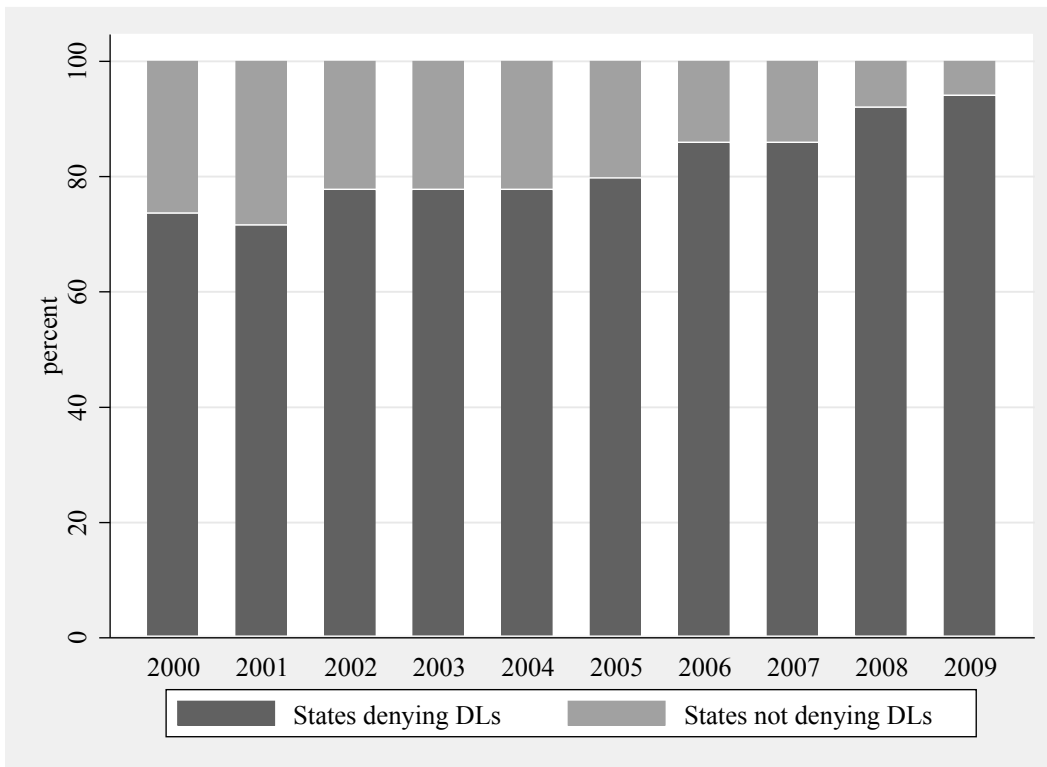
Figure 1: **Evolution of states' licensing of unauthorized immigrants**
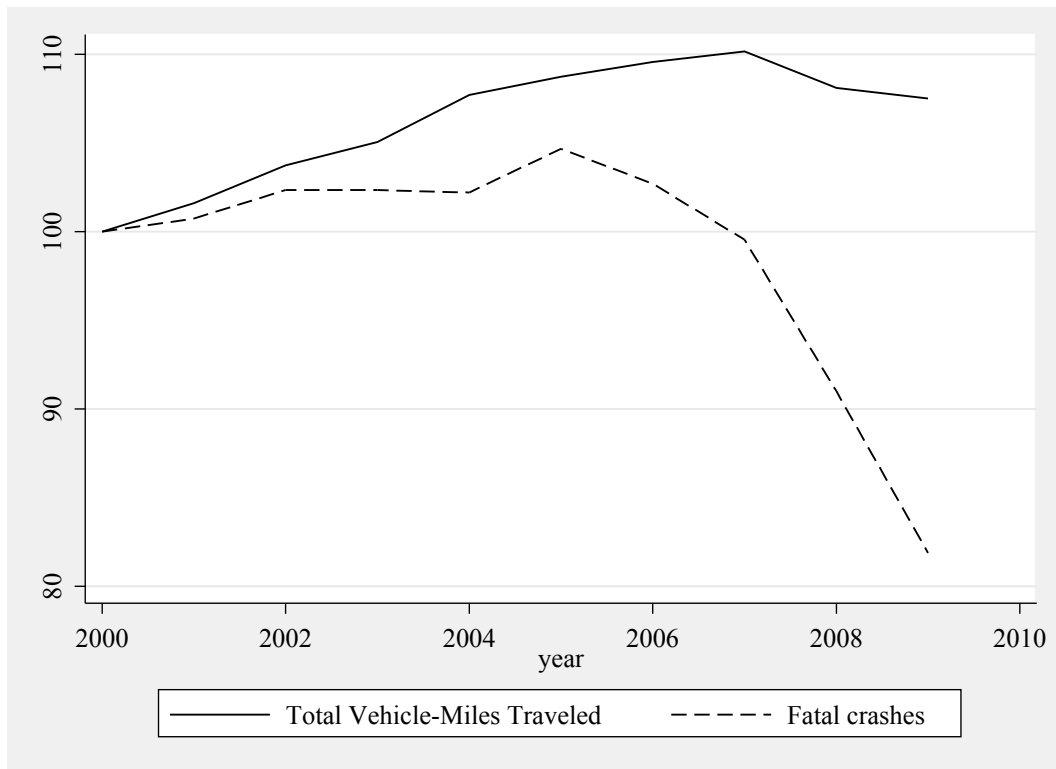
Figure 2: **National trends in VMT and fatal crashes (Year 2000=100)**
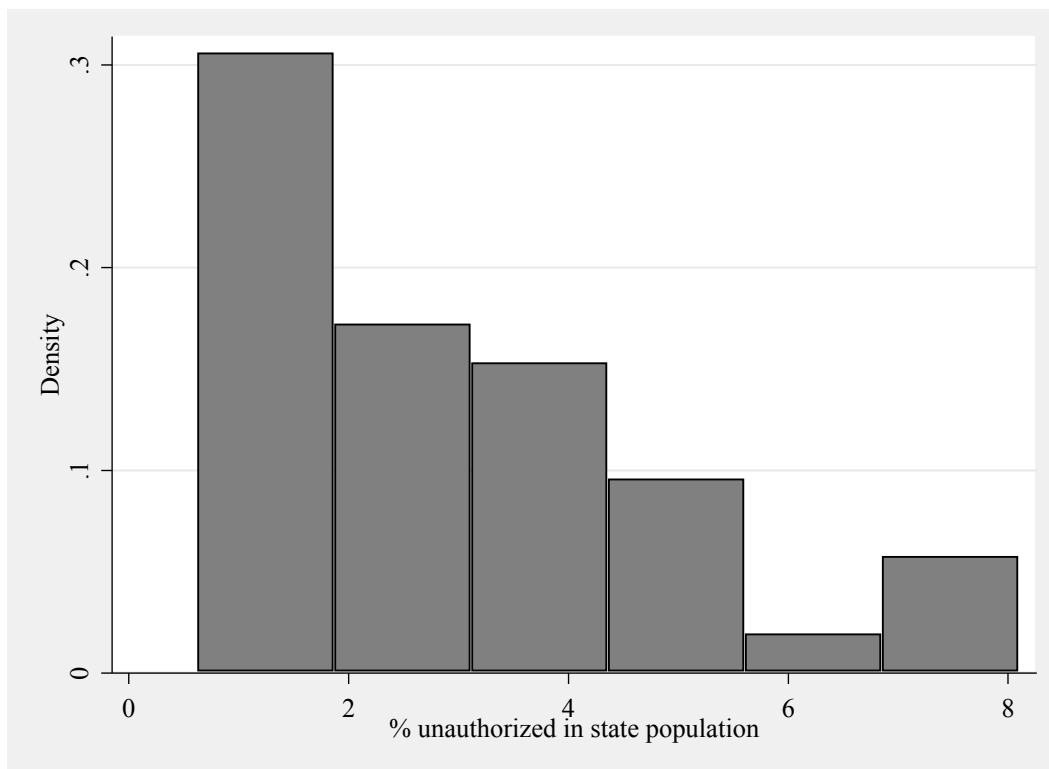
Figure 3: **Distribution of states' 2000-2009 average unauthorized populations**

Table 1.1: **Determinants of total number of fatal car accidents**

	(1)	(2)	(3)	(4)
State restricts driver licenses	-0.0335**	-0.1058***	-0.0228	-0.0851***
	(0.0160)	(0.0311)	(0.0139)	(0.0301)
$\times$ % unauthorized immigrants		0.0322***		0.0275**
		(0.0101)		(0.0102)
% unauthorized immigrants	0.0307**	0.0043	0.0204	-0.0014
	(0.0143)	(0.0139)	(0.0137)	(0.0143)
Log of population	0.7515***	0.6816***	0.6010***	0.5570***
	(0.2088)	(0.1986)	(0.1910)	(0.1836)
Log of % Blacks	-0.0173	0.0027	0.0137	0.0293
	(0.1670)	(0.1552)	(0.1584)	(0.1522)
Log of % Hispanics	0.4073***	0.3923***	0.4366***	0.4219***
	(0.1013)	(0.1020)	(0.1006)	(0.1011)
% younger than 25	-0.0034	-0.0127	0.0364	0.0266
	(0.0315)	(0.0281)	(0.0283)	(0.0253)
% older than 65	-0.0203	-0.0155	0.0152	0.0173
	(0.0450)	(0.0450)	(0.0450)	(0.0448)
Log of household median income			0.1537	0.1394
			(0.1015)	(0.1031)
Log of unemployment rate			-0.1312***	-0.1251***
			(0.0421)	(0.0403)
R^2	0.6157	0.6237	0.6347	0.6405

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors, shown in parenthesis, are clustered at the state level in all specifications. The dependent variable is the logarithm of total vehicle accidents in state that result in at least one fatality. State and year fixed effects included in all specifications. The 42 observational units over the 10-year sample period yield 420 observations.

Table 1.2: **State specific impact of restricting driver licenses on log fatal accidents**

	Avg % Unauthorized	Policy effect		Avg % Unauthorized	Policy effect
Alabama	1.49%	-0.044**	Mississippi	1.11%	-0.055**
Arizona	7.37%	0.117**	Missouri	0.68%	-0.066***
Arkansas	1.60%	-0.041**	Nebraska	2.42%	-0.019
California	7.27%	0.114**	Nevada	8.11%	0.137**
Colorado	4.68%	0.043*	New Hampshire	1.08%	-0.055**
Connecticut	2.62%	-0.013	New Jersey	5.55%	0.067**
Delaware	2.85%	-0.006	New Mexico	3.52%	0.012
DC	4.64%	0.042*	New York	3.84%	0.020
Florida	4.96%	0.051**	North Carolina	3.78%	0.018
Georgia	4.42%	0.036	Ohio	0.77%	-0.063***
Idaho	2.14%	-0.026*	Oklahoma	1.58%	-0.041**
Illinois	3.46%	0.009	Oregon	3.67%	0.016
Indiana	1.35%	-0.048**	Pennsylvania	1.04%	-0.057**
Iowa	1.60%	-0.041**	Rhode Island	2.58%	-0.014
Kansas	2.26%	-0.023	South Carolina	1.34%	-0.048**
Kentucky	0.99%	-0.058**	Tennessee	1.98%	-0.031**
Louisiana	0.63%	-0.068***	Texas	5.89%	0.077**
Maryland	4.04%	0.026	Utah	3.87%	0.021
Massachusetts	2.84%	-0.007	Virginia	3.41%	0.008
Michigan	1.45%	-0.045**	Washington	2.88%	-0.006
Minnesota	1.67%	-0.039**	Wisconsin	1.51%	-0.043**

Notes: *** p<0.01, ** p<0.05, * p<0.1. State specific impact calculated using specification (4) in Table 1.1.

Table 1.3: **Robustness checks**

		Policy dummy	Interaction term	N
(1)	Uses logarithm of fatal crashes per 100,000 population as dependent variable	-0.0689** (0.0297)	0.0240** (0.0098)	420
(2)	Uses logarithm of fatal crashes per 1 billion Vehicle-Miles Traveled as dependent variable	-0.0625* (0.0311)	0.0239** (0.0102)	420
(3)	Uses percentage of non-citizen Hispanics as proxy for unauthorized immigrants	-0.0478* (0.0259)	0.0152 (0.0105)	420
(4)	Restricts sample (year<2008)	-0.0770** (0.0307)	0.0262** (0.0126)	336
(5)	Restricts sample (year<2007)	-0.0753** (0.0355)	0.0223* (0.0132)	294
(6)	Controls for other driving regulations that impact total number of fatal accidents	-0.0830** (0.0333)	0.0270** (0.0110)	410

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, shown in parenthesis, are clustered at the state level in all specifications.

Table 1.4: **Determinants of total number of fatal hit and run accidents**

	(1)	(2)	(3)	(4)
State restricts driver licenses	0.1747**	0.1299	0.1737**	0.1264
	(0.0857)	(0.1266)	(0.0845)	(0.1316)
$\times$ % unauthorized immigrants		0.0199		0.0208
		(0.0589)		(0.0602)
% unauthorized immigrants	0.0904**	0.0741	0.0936*	0.0770
	(0.0452)	(0.0757)	(0.0485)	(0.0771)
Log of population	1.3086	1.2715	1.3888*	1.3555*
	(0.7897)	(0.8083)	(0.7709)	(0.7895)
Log of % Blacks	0.3266	0.3390	0.3078	0.3197
	(0.3977)	(0.4070)	(0.4001)	(0.4092)
Log of % Hispanics	0.9738***	0.9646***	0.9780***	0.9668***
	(0.3208)	(0.3061)	(0.3072)	(0.2907)
% younger than 25	-0.2885***	-0.2942***	-0.3060***	-0.3135***
	(0.0875)	(0.0855)	(0.1027)	(0.1015)
% older than 65	-0.3591***	-0.3561***	-0.3687***	-0.3671***
	(0.1271)	(0.1243)	(0.1333)	(0.1311)
Log of household median income			0.1413	0.1304
			(0.4341)	(0.4443)
Log of unemployment rate			0.0735	0.0781
			(0.1503)	(0.1498)
R^2	0.2207	0.2210	0.2212	0.2215

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, shown in parenthesis, are clustered at the state level in all specifications. The dependent variable is the logarithm of the total number of hit and run accidents. State and year fixed effects included in all specifications. The 42 observational units over the 10-year sample period yield 420 observations.

2 High School Employment and Early Labor Market Outcomes of Children of Immigrants

2.1 Introduction

Understanding the adaptation and long-term outcomes of children of immigrants is important because of this group's growing size. In large cities, second generation Americans account for 20% of the population. However, there is very little research on this group and much more effort has been devoted to investigating the experiences of adult immigrants. This is due in part to the limited sources of statistical data available. More importantly, the lack of longitudinal data sources that can provide a better understanding of these individuals' choices and experiences is a significant obstacle to understanding how the assimilation or lack thereof actually occurs.

From a policy perspective, the vital patterns followed by the so called new second generation of immigrants have the potential to influence substantially the social landscape in decades to come. For example, their successful economic assimilation could prevent or reduce excessive dependency on social programs (Borjas, 1999).

One area of interest regarding the life experiences of children of immigrants is the school to work transition. Education represents the primary mechanism through which these youths will acquire the skills necessary to integrate successfully in the labor market, but the potential benefits of work experience while in school has been increasingly recognized. In particular, in the United State the school to work transition has become less abrupt and clearly defined for the case of the typical non-immigrant youth (Mortimer 2003). Combining work with school during high school

and college years has become common among members of all social backgrounds, although less so among minority and disadvantaged youths.

A large literature has investigated the beneficial effects on young adults' labor market outcomes of working while in high school (Ruhm 1997; Light 1999). Very few papers have sought to establish this relationship for the case of second generation immigrants. Those that have investigated the question have not addressed the endogeneity of the decision to work while in school (Perreira et al 2006). This paper contributes to the literature about the labor market experiences of second generation immigrants by trying to identify the causal effect of school work experience of second generation immigrants on their wages in early adulthood using standard techniques to control for observed and unobserved heterogeneity. Additionally, this study makes use of a rich data set specifically designed to study multiple aspects of the experience of the second generation. These data were collected by the Children of Immigrants Longitudinal Study (CILS) which sampled students from schools in two high immigration cities in the United States in 1992 (Portes and Rumbaut, 2005). Participants were again interviewed in 1995 and in 2002. This design allows for a comprehensive look at the early life experiences of a key demographic group to understanding the future evolution of the American population.

My results suggest that high school employment has a modest positive effect on the wages of male students and U.S. born children of immigrants. OLS regressions with a wide array of variables to control for background characteristics yield a significant effect on the order of 5%, which is a third of what the literature reports for native youths. IV regressions detect a bigger effect, but this is not statistically significant. Surprisingly, high school employment does not appear to have any effect on other important labor outcomes associated with economic assimilation, such as the probability of graduating from college or the Treiman score for job prestige. Overall, the results suggest that high school employment is not very important as a mechanism of helping economic assimilation. From a policy perspective, this evidence may detract from concerns raised about limited employment opportunities among children of immigrants. Resources to help manage economic assimilation should be more focused on ensuring that opportunities for formal education are available to the second generation.

2.2 Literature

Various authors have studied the determinants and characteristics of the process of economic assimilation of immigrants. Beginning with the seminal paper by Chiswick (1978), a continuous stream of research has been produced trying to understand the characteristics of this adjustment process. Two interpretations seem to dominate the debate. The original thesis advanced by Chiswick (1978) is that immigrants exhibit a relatively fast pace of assimilation as measured by growth in earnings and the closing of the initial wage gap relative to natives. Chiswick measured this process of equalization and concluded that the average immigrant is able to level or even overtake the native population in just under 15 years after immigration. This can be easily understood in the context of the standard human capital model by Becker (1993). In effect, according to this view, immigrants face an initial wage disadvatange due to the depreciation incurred in transferring some of their skills to a new environment; the initial period in the new country provides immigrants the opportunity to acquire the necessary skills to adapt to their new social reality and become productive members of the economy. This interpretation of the data has been forcefully criticized by Borjas (1985) on the principle that the use of cross sectional data sets to study a dynamic process like economic assimilation is fundamentally flawed. Borjas points to the influence of important cohort effects in any evaluation of the immigrant's earnings adjustment process using cross sectional data. If immigrants from older cohorts are different, for example, more educated, healthy, or culturally close to the host country, then they cannot be used as a reference to predict the outcomes of more recent immigrants.

Another side to this debate has focused on mechanisms of intergenerational assimilation (Trejo 2003). That is, instead of focusing on the earnings trajectory of the individual, the focus is on the evolution of the family. According to this view, first generation immigrants will not make much progress, but provide their offspring with the conditions to be able to assimilate successfully.

However, very little progress has been made to understand the specific resources or mechanisms that immigrants use to affect the pace of their assimilation. In a sense, the discussion has centered on descriptive patterns of more or less assimilation, but the actual decisions made by immigrants are very poorly understood. This is of course a consequence of the limited sources of information available. In order to approach the subject in a more behaviorally conscious way it is necessary to

use more detailed sources of data that follow immigrants through the process of potential adaptation.

The availability of good panel data sources such as the National Longitudinal Survey of Youth (NLSY) has allowed economists to expand their understanding of the decisions made by individuals and their consequences. One clear case in point is the effect of high school employment. Various recent studies (Ruhm 1997, Light 1999, Light 2001, Hotz et al. 2002) use the NLSY to show that high school employment has positive benefits on post-school labor outcomes.

This paper contributes to the literature by adapting techniques employed to understand the long term economic effects of labor supply by in-school youths to investigate more closely the implications for the assimilation of second generation immigrants of a simple decision: whether to work or not while in high school.

2.3 Data

Due to the geographic agglomeration of immigrants in a few large population centers in the United States, national data sets with only modest immigrant representation are less attractive to study the effect of immigrant's decisions to advance their assimilation. The presence of more or fewer opportunities and obstacles for progress varies substantially with the social landscape that surrounds the immigrant. To account for these issues, the Children of Immigrants Longitudinal Study (CILS) focuses on the detailed experiences of a cohort of second generation immigrants in two cities with a large immigrant presence. Obviously, this approach limits the generalizability of the results since the national population of second generation immigrants is not perfectly represented by these data, but it allows evaluating the effectiveness of different mechanisms of assimilation under conditions that are relevant for a sizable segment of the immigrant population.

Additionally, studies using nationally representative data have found lower rates of youth employment for members of minority groups. However, Ruhm (1997) reports that, conditional on employment, work effort of non-whites is similar to that of whites. This suggests that the lower probability of employment by minority youths may be correlated with demand side constraints. By focusing on geographical areas with an important immigrant presence, the constraint on available opportunities for employment may be reduced somewhat, which furnishes our econometric

analysis with a sample featuring a high proportion of working youths that are still enrolled in school.

2.3.1 The CILS

The CILS is a panel data study based on a random group of second generation immigrants in 49 schools in San Diego and Miami. It followed the participants from early adolescence to early adulthood. The data employed in this paper come from the Interuniversity Consortium for Political and Social Research (ICPSR) website (data set 20520).

The CILS consists of three phases or waves that were implemented in 1992, 1995 and 2002. The number of students who participated in the first phase was of 5,262. To be eligible, students had to have at least one immigrant parent and be born in the US or have immigrated before the age of 10. The sample design was defined so as to include a proportional representation of all nationalities in each area and to reserve one fourth of the sample for smaller nationalities. Another feature of the sampling system was to ensure an even distribution by gender and US birth.

I employ data from all three waves of the study. The dependent variables used in the following analysis include: log hourly earnings, an indicator variable for college graduation, an indicator variable for working in an occupation where most other workers are of white ethnicity, and a widely used measure of job prestige, called Treiman score. I select observations for which employment information from the second and third waves is available. I consider only individuals who are working either full or part time by the time of the third survey.

The key regressors or variables of interest are two different measures of high school employment. One is an indicator variable for having worked in the week prior to the administration of the survey and the other is the actual number of hours worked if any.

My specifications include four sets of covariates selected with the objective of reducing as much as possible the potential for observed heterogeneity bias. In all models I include a baseline set of controls that reflect demographic and ethnic characteristics. These consist of indicator variables for being born in the US, coming from the Miami subsample and being a female; variables measuring marital status, number of children and a dummy for working part-time (all three as reported in 2002); dummy variables for national origin, which indicate whether the respondent is either Asian,

Filipino, Caribbean or of "other origin"[1] with the base category being Hispanic. Also included is a continuous measure of knowledge of English constructed from self-reported scores of the ability to read, understand, speak and write English obtained from the 1992 survey.

The second set of controls includes attributes regarding family and socioeconomic background: whether any parent is born in the US, completed years of education by the mother (or step-mother/female guardian), a vector of dummy variables reflecting family structure (both biological parents present in the household, only one biological parent and spouse, father only or mother only), household size, birth order, and the log of monthly family income. Monthly family income comes from the Parent survey which was given to only half of the sample. However, since the parents interviewed were selected at random and with attention to preserving the gender and national origin stratification design of the full sample, I save information by including a dummy variable for missing family income and assigning the sample mean to these observations.

An extended set of controls adds variables for school characteristics,[2] school and peer environment,[3] and study habits characteristics[4] from the 1992 survey. Finally, the group of variables capturing ability are from the 1992 survey and include: scores in both the English and mathematics components of the Abbreviated Stanford Achievement Test and school GPA obtained from school records.

Table 2.1 presents summary statistics for all the variables mentioned above.

2.3.2 Descriptive statistics

The second wave of the CILS includes a randomly selected sample of junior and senior high school students in the cities of Miami and San Diego. Extensive information is available on multiple areas such as family background, attitudes and values, English proficiency, expectations, perceptions of discrimination, and employment behavior. All students are asked if they are currently working and, if they are, how many hours they worked in the previous week (reference week). In Table 2.2

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[1] A small group that includes children with origins from Europe, Canada, Middle East and Africa.

[2] Whether minority students represent at least 60% of school population, the percent of students eligible for subsidized lunch at school, and if the school is located in the inner city.

[3] If many of a student's friends plan to get a full-time job after high school, if many of a student's friends plan to attend a 4-year college, if a student got into a physical fight in school at least once, and if someone offered to sell drugs to a student at school.

[4] If a student reported to spend more than 3 hours daily in school work and whether the person that most helped the student with homework was a parent

summarizes the employment behavior of the different national groups represented in the sample. Employment is common among all national groups, although Asians work less often and for fewer hours. Participation in the labor market is much greater for students in grade 12 than in grade 11. In general, employment rates are substantial for all national groups, which make this data set ideal to study the effects of high school employment among second generation immigrants.

Seven years after the second wave, in 2002, respondents were again interviewed and asked about aspects of their early adulthood adaptation such as education, employment, marital status, residence, etc. The survey includes detailed questions about type of employment, hours worked and personal earnings. In Table 2.3 compares hourly wages by national origin and grade between those who worked while in high school and those who did not. A positive relationship between working in high school and hourly wages in early adulthood is present, especially for certain ethnic groups. Also, for the overall sample there is a gain in average hourly wages for both cohorts. In-school job holding is associated with a 12% increase in hourly wages for individuals who were in twelfth grade and 5% for individuals in eleventh grade.

2.4 Empirical strategy

To identify the effect of high school employment I use a multiple regression model with a broad set of controls and for different outcome variables. The basic equation is:

$$y_i = \alpha_0 + \delta\, h_i + \mathbf{X}_i^{'}\beta + \epsilon_i,$$

where h_i represents either the number of hours worked in the reference week or a dummy variable for whether the person worked in the reference week. This labor supply information is taken from the second wave of the survey when interviewed persons are in the eleventh or twelfth grade of high school.

All the outcome variables are obtained from the third wave of the CILS which collects information seven years after the measurement of the key explanatory variable. By this time the effect of in-school work experience, if it exists, should be apparent in the data. Notice that measures of educational attainment and post-school work experience are absent from the model. This is because such attributes are plausibly influenced or caused by high school employment. If they

were included in the model, a "bad control" problem as defined by Angrist and Pischke (2009) would arise. This situation refers to the fact that the estimated coefficient on the variable of interest would not represent a true causal effect because of bias. Angrist and Pischke (p. 65) show that even if the treatment is assigned at random, when bad controls are included in the specification, coefficients are biased. In the context of my application, adding schooling for instance would mean that I would be trying to measure the effect of high school employment keeping schooling constant. However, I would not get this "partial" effect because in comparing outcomes of, say, college graduates who worked in high school relative to that of college graduates that did not, part of the difference will likely be due to the fact that those who worked in high school are different in ways that are correlated with the outcome (for instance, more focused, disciplined or motivated). In view of this problem, I adopt the approach followed by Ruhm (1997) and attempt the identification of the effect of high school employment by using two devices: first, assembling an exhaustive set of covariates to estimate the effect via OLS, and second, evaluate the robustness of this estimate by considering exogenous variation in the context of an instrumental variables model. After this, I will explore what part of the estimated effect is due to direct mechanisms and what part is due to indirect channels of influence via induced changes in, for instance, subsequent schooling decisions.

2.5 Results

To attempt to identify the effect of high school employment on measures of economic assimilation it is necessary to control for observed and unobserved heterogeneity. Since working while in school is a choice variable probably correlated with characteristics like socioeconomic background, motivation and ability that influence the extent of economic assimilation achieved by children of immigrants, it is necessary to adjust the results presented in section 3 for the confounding effect of potential endogeneity.

2.5.1 Effect on earnings

Table 2.4 shows the results obtained when estimating the basic model with students in the junior and senior years of high school. The sample includes individuals who are still enrolled in school

in either junior or senior year by the time of the first follow-up survey in 1995 according to school records and who report being employed in the second follow-up survey administered in 2002. By the time of the second follow-up survey, students who were juniors in 1995 are on average 23.7 years old, while students who were seniors in 1995 are on average 24.6 years old.

I estimate various versions of the basic model using two different specifications for the explanatory variable of interest: a quantitative measure of work intensity (number of hours worked during the reference week) and a measure of labor supply choice (indicator variable for whether the individual worked or not in the reference week). More control variables are progressively added when moving from left to right in Table 2.4. The panel nature of the dataset provides an extensive set of controls (described in detail in section 3) that permit to reduce a sizable part of the influence of individual heterogeneity associated with differences in ability, motivation, preferences and financial constraints.

According to Table 2.4, employment during the junior year of high school does not appear to have any effect on future earnings. This is unsurprising because the model does not control for the employment behavior of these students in the twelfth grade, which is when work effort and employment become more frequent. Since the dataset does not provide this information for the case of junior students, it is difficult to conclude anything from this first set of estimates presented in Table 2.4. However, in his study about high school employment of non-immigrant youths Ruhm (1997), using the more complete NLSY79 dataset, also finds no evidence of an effect of employment in the sophomore and junior years of high school. All subsequent analysis from here on will focus on the sample of senior high school students.

Table 2.4 shows that the model fit is better for the case of junior students than for senior students. This is not surprising because junior students are younger at the time of the second follow-up survey, which means that they exhibit less dispersion in earnings and the model can account for a larger fraction of the overall variation in earnings.

The effect of employment is clearly significant and positive in the case of senior students using both specifications of the variable of interest. For the first three models, the estimates are not very sensitive to the addition of covariates, which suggests that unobserved heterogeneity, to the extent that it is correlated with observed heterogeneity, is not driving the results. However, we do observe an important change between Models 4 and 4' when variables measuring ability are

added. The estimated coefficient in both cases increases moderately (about 20%). This suggests that the students who chose to work in the last year of high school are lower ability students as measured by their test scores and GPA taken from the first survey in 1992 (when these students were in the eighth and ninth grades). If lower ability is correlated with unobserved characteristics that impact negatively future incomes, then OLS could be underestimating the true effect of employment in high school. Since the bias works against finding an effect, and the estimates show a small but clearly significant effect of high school employment, this could be taken as evidence that high school employment does indeed impact positively the earnings in early adulthood of second generation immigrants. To investigate the robustness of this hypothesis I re-estimate the model with the full set of controls for different subsamples.

Table 2.5 presents the estimation results when the model is applied to four different groups obtained by splitting the full sample according to two criteria: gender and US birth.[5] The results show that the positive effect of in-school job holding is very heterogeneous across these groups. In particular, only males and US-born youths seem to benefit from high school employment. The other two groups also exhibit positive coefficients, but they are not statistically significant.

The estimates for males and youths born in the US are also sensitive to the inclusion of ability controls. That is, when the set of controls for ability are included and the model is estimated with males or US-born students, the effect of employment goes up by about 30% for each group. This does not occur in the case of females and child immigrants. This is illustrated in Table 2.6.

These results suggest that there may be negative selection into working while in high school for males and those born in the US. In the case of females and child immigrants it could be that negative selection is stronger since their coefficients, though small, are also positive.

2.5.2 Accounting for selection

Dealing with the bias of potential endogeneity created by negative selection into high school employment is difficult in this context because of the limitations imposed by the data set. Although the data set offers a wide array of variables to control for different background characteristics, by design it is limited to subjects from two different geographic locations. This makes it impossible

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[5]As explained in the data section, the study design provided for representative samples according to gender and birth in the United States. Children not born in the U.S. were brought to the U.S. before age 10.

to use geographic labor market characteristics as an instrument to induce exogenous variation in the variable of interest, which is the approach used by Ruhm (1997), Light (1999) and Light (2001).

The alternative I employ is modeled after the strategy proposed by Angrist and Krueger (1991). They use the constraints imposed by school age requirements combined with compulsory schooling laws to identify the effect of schooling on earnings. Their strategy works because season of birth determines that some students have to wait longer in order to be free from the obligation to attend school. In particular, students born earlier in the year are older when they start school and this allows them to reach the maximum compulsory age faster, ending up with slightly less education than otherwise comparable students that started school younger. The variation induced by this feature of the system allows them to identify the effect on adult earnings of a few more months of secondary education.

In my case, I intend to use the combined effect on youth labor supply decisions of school age policies and state-specific child labor laws as instruments for the labor supply choice of the youths in my sample. Although the federal Fair Labor Standards Act (FLSA) does not regulate labor supply of workers with 16 or 17 years of age,[6] exempting in this way many students in twelfth grade from employment restrictions, both California and Florida do have state laws that regulate the employment of 16 and 17 year olds. These two states regulate diverse aspects of the labor market for youths including the number of hours per day a minor can work both while school is in session and not in session, the maximum number of work hours that minors are allowed per week, the hours of day minors can work, the types of occupations they can pursue, etc.

The CILS data set does not contain information about quarter or month of birth. However, it is possible to use the age of the respondent in 1995 (which the first follow-up survey did ask) as a proxy for the time of the year the respondent was born. The age of the respondent at any one time during the school year, and relative to other students in the same cohort, corresponds perfectly with the age at which the respondent started school. Since both California and Florida had late school start cut-off dates at the time respondents in my sample began school (Colosanti 2007), the age of the respondents reflects the time of the year they were born. There are three possibilities: (i) that they were born not much earlier than the school start date and be not much older than 17 years of age by the time they start the twelfth grade, (ii) that they were born after the school

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[6] Other than banning these workers from engaging in occupations declared as hazardous by the Secretary of Labor.

start date but before the cut-off date and be 16 years old at the start of twelfth grade, or (iii) that they were born not much later than the cut-off date and be close to passing the age of the majority at the time when they initiate the twelfth grade. Hence, by virtue of institutional constraints on the age of school entry, some students in my sample are covered by restrictive rules regarding their employment while others are not. Since the time of year a student is born is unrelated to their ability, motivation or socioeconomic status, the variation in labor supply choices explained by state child labor laws could be considered exogenous and used to identify the effect of high school employment on earnings in early adulthood.

To use this strategy I need to make sure that most intra-cohort age variation is not correlated with any other factor affecting wages. For this reason, and to avoid the confounding effect of miscoding or outliers, I drop from my sample all observations that report an age by the time of the first follow-up survey below 16 or above 18. Additionally, the extensive set of controls I have in my model will likely pick up a sizable portion of any residual endogenous variation in age that is correlated with ability, home environment, cultural background, etc. I also perform various standard tests to assess the assumption of exogeneity of my instruments.

Various studies have analyzed, from the perspective of economic theory, the importance of family as an effective resource for finding employment (Montgomery 1991). From an empirical perspective and in the context of youth employment, Michael and Tuma (1984) and Rees and Gray (1982) point out, respectively, that the number and employment status of siblings is a good predictor of own employment. Since the number of siblings is correlated with household size, which attending to evidence by Black et al. (2005) and Hanushek (1992), I include in my structural equation, I use instead the number of same sex siblings living with the respondent as an instrument to capture the mechanism identified by Michael and Tuma (1984) and Rees and Gray (1982).[7] This is a reasonable choice because it is plausible that individuals are more able to leverage the social networks of siblings of the same sex. With each additional sibling of the same sex the potential opportunities for employment multiply for the youth in question.

In Table 2.7 I present descriptive evidence for the relevance of my instruments. For the overall sample, the story behind the choice of instruments seems to be confirmed. Older students have greater rates of employment participation, and students with more siblings of the same sex also

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[7]Notice that I am including the total number of siblings in my first-stage equation as well.

work more. Curiously, the employment participation rate of youths with no siblings of the same sex tends to be higher than that observed for individuals with more siblings of the same sex. This probably is a reflection of social differences between larger and smaller families in the sample. For instance, it may be that children from families of higher social status that tend to be also families of smaller size, are more motivated to work or find it easier to get a job.

When disaggregated by subsample, we see that the connection between the instruments and the endogenous variable no longer holds in every instance. In particular, it seems to hold well for males and US born youths, but less perfectly for females and child immigrants. For this reason, I will estimate the model both for the whole sample and for all indicated subsamples.

Table 2.8 presents the results of the IV estimation. None of the estimated effects are significant as expected because of the small size of the samples used. With all the models it is not possible to reject the null of Hansen's over-identification test which suggests that the instruments are indeed exogenous. Although not reported, the structural equation is also estimated for each subsample, including the instruments in the specification. In all the cases the coefficients are insignificant. Another piece of reassuring evidence is that the obtained coefficients are within the order of magnitude of the corresponding OLS estimates, which indicates that no significant bias is introduced by using these instruments. In fact, the set of estimated coefficients is so similar that in all cases a Hausman test fails to reject the null that the explanatory variable of interest is exogenous. This contradicts the hypothesis that the OLS estimates are biased by negative selection into high school employment.

With the IV estimates it is again observed that in-school work experience seems to benefit exclusively youths that are male or born in the US. Females and youths brought to the US at an early age do not benefit. The IV results suggest that they may instead be negatively impacted in the earnings they obtain in early adulthood. However, since these coefficients are small and very insignificant, the true impact may be zero. It is very intriguing why this pattern emerges repeatedly throughout the analysis. It is not obvious why females and youths not born in the US should benefit less from working while in high school. One possible reason is that the type of jobs taken by these groups is less conducive to the acquisition of valuable skills in the post-schooling labor market. Another hypothesis is that these groups are less inclined to invest heavily in acquiring human capital via on-the-job training.

For the case of males and American children of immigrants it is important to assess to what extent the measured effect of high school employment reflects the true effect of this activity and not the influence of later decisions that are caused by working in high school. One important channel through which high school employment can affect indirectly future earnings is continued education after high school. Maybe males that work in high school are more likely to continue their education, in which case the measured effect of high school employment is an overestimate because it includes the indirect effect via post-secondary education. Conversely, in-school employment may induce youths to rule out the possibility of continuing their education and entering the labor force earlier. In order to evaluate both of these hypotheses the next section presents more estimates using additional outcome variables.

2.5.3 Other measures of economic assimilation

In order to evaluate the autonomy of the estimated effects of high school employment on earnings we study now its effect on other outcome variables that are also correlated with earnings and that are routinely used as measures of the economic assimilation by second generation immigrants. These variables are the probability of being a college graduate, the respondent's job Treiman's occupational prestige score,[8] and the probability of working in an occupation where the majority of peers are of white ethnicity. This will complement the results obtained in the previous section.

First, I estimate a Probit model for the likelihood of being a college graduate at the time of the second follow-up survey in 2002. All the subsamples are investigated separately attending to the heterogeneous results obtained previously. The results are shown in Table 2.9. Although all the estimates are negative, they are also very small and insignificant. Working while in high school does not seem to have any effect on the probability of receiving a college degree. This result applies for the overall sample and for all the subsamples. It is interesting to notice that the same groups that do not obtain wage benefits from working while in high school are the ones with the largest predicted probabilities of graduating college. This is consistent with the hypothesis that these groups are less interested in acquiring human capital through on-the-job training.

Even less conclusive evidence is obtained from estimating a Probit model with the probability of

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[8]The prestige score is a measurement of the cachet or worth attributed to an occupation by the general social perception.

working with white coworkers as the outcome variable (Table 2.10). The results are insignificant and similar across all groups, except for the case of youths born in the US. For this last group there is an important effect on the outcome variable from working in the senior year of high school. Working 10 hours in the senior year of high school is correlated with an increase in the predicted probability from 33.2% to around 38%.

Finally, Table 2.11 presents the results from modeling the Treiman's job prestige score. This is a potentially interesting measure because it is useful to evaluate the nature and long-term impact of the effect on earnings from working in high school. If the prestige score is negatively affected by holding a job while in school, then any pecuniary gains reflected in higher earnings in early adulthood are only temporary and will not contribute to further improvements later in life. The results of Table 2.11 suggest that working while in high school does not materially impact the Treiman prestige score. The coefficient is negative in most cases, but small and statistically insignificant. Only for females does working in high school appear to have a sizable negative effect, equivalent to half a standard deviation, but again the estimate is not statistically significant.

These results point to the conclusion that working while in high school does not have a large impact on the economic assimilation outcomes of the children of immigrants. The greatest influence operates through earnings in early adulthood, but it does not seem to improve much the abilities of second generation immigrants to assimilate more or faster to American society.

2.6 Conclusions

There is indeed an effect on early adulthood earnings of working while in the senior year of high school, but it is much smaller than that observed in the case of non-immigrant youths: around a third of the return for non-immigrant youths according to the estimates of Ruhm (1997). Similarly, as in comparable studies with non-immigrant youths, the effect of in-school work experience on educational attainment is ambiguous. Although the coefficients are mostly negative, they are also insignificant. In fact, high school employment appears not to have any effect beyond inducing a small increase in hourly wages for male youths.

This casts some doubt on the hypothesis that part of the gap in economic outcomes of minorities could be explained by different patterns of in-school employment, as determined, for instance,

by cultural factors or differentiated opportunities for employment, as suggested by Ruhm (1997), Freeman and Wise (1982) and others. If this hypothesis is correct, it is sensible to expect this mechanism would also act in the case of second generation immigrants that in general exhibit not only the same economic and social handicaps as traditional minority groups do, but are also disadvantaged to the extent that their cultural background conflicts with the mores and practices of their adopted country (the use of another language being the most obvious example of this). The (small) effect of in-school job holding on earnings is concentrated among US born students, while non-US born youths don't exhibit any effect. This is opposite to what one would expect if this hypothesis is correct since students that are not born in the US would stand to benefit more from opportunities for early out-of-school acculturation conditional on ability and socioeconomic status.

These results show that policy makers in areas and cities with large immigrant populations should use with care estimates about the effect of school work experience that apply for nationally representative samples since they do not necessarily reflect the realities of second generation immigrants.

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Table 2.1
Descriptive Statistics

	Grade 11		Grade 12	
	Mean	SD	Mean	SD
Basic/demographic				
Born in the US	0.46	0.49	0.45	0.49
From Miami subsample	0.52	0.49	0.50	0.50
Knowledge of English (max=4)	3.76	0.42	3.77	0.41
Female	0.53	0.49	0.53	0.49
Number of children	0.19	0.50	0.25	0.61
Married	0.15	0.36	0.17	0.37
Asian	0.16	0.36	0.15	0.35
Filipino	0.17	0.38	0.22	0.41
Caribbean	0.07	0.26	0.08	0.28
Hispanic	0.57	0.49	0.51	0.50
Other nationality	0.01	0.11	0.03	0.16
Family/SES background				
One parent born in US	0.13	0.33	0.13	0.34
Mother education (years)	11.8	3.8	11.8	4.0
Two biological parents in hh	0.71	0.45	0.71	0.45
One biological parent and partner in household	0.11	0.31	0.10	0.30
Father only in household	0.01	0.11	0.01	0.11
Mother only in household	0.13	0.34	0.13	0.33
Household size	4.2	1.78	4.2	0.18
Birth order	2.6	1.9	2.6	1.9
Log family income	10.6	0.6	10.6	0.6
Extended controls				
Minority students represent at least 60% of school pop	0.45	0.49	0.38	0.48
Percent eligible students for subsidized lunch	0.52	0.24	0.36	0.20
School in inner city	0.34	0.47	0.31	0.46
Parents own home	0.68	0.46	0.65	0.48
Student spends more than 3 hours daily on homework	0.39	0.48	0.43	0.49
Parent helped with homework	0.31	0.46	0.26	0.43
Friends plan to get full time job	0.77	0.41	0.73	0.43
Friends plan to go to 4-year college	0.92	0.25	0.94	0.22
Fight in school	0.12	0.33	0.14	0.34
Offered drugs in school	0.28	0.45	0.23	0.42
Does not feel safe at school	0.26	0.44	0.23	0.42
Ability				
GPA in 1992	2.70	0.81	2.75	0.84
ASAT mathematics 1992	695	46	710	56
ASAT reading 1992	668	37	674	64

Table 2.2
Employment Behavior by National Origin

	Grade11		Grade 12	
	% Employed	Hours worked	% Employed	Hours worked
Cuban	38	20	51	22
Mexican	20	18	40	21
Filipino	25	16	38	19
Caribbean	33	20	48	22
South American	28	21	49	23
Central American	31	20	51	23
Southeast Asian	19	18	31	18
East and South Asia	27	14	37	18
Other	32	17	56	23

Note: Number of hours worked per week is conditional on being employed, not the average for the whole group. Caribbeans consist of: Dominicans, Haitians, Jamaicans and West Indies. South Americans: Argentineans, Colombians, Ecuadorians, Peruvians and Venezuelans. Central Americans: Costa Ricans, Guatemalans, Hondurans, Nicaraguans, Panamanians, and Salvadorians. Southeast Asians: Vietnamese, Laotians, Cambodians, and Hmongs. East and South Asia: Chinese, Taiwanese, Japanese, Koreans, Indians, Pakistanis. Other: Europe, Canada, Middle East and Africa.

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Table 2.3
Early Adulthood Hourly Wages by National Origin and Grade in Second Survey

National origin	Grade11		Grade 12	
	No HS work	HW work	No HS work	HS work
Cuban	12.3	12.9	13.2	15.7
Mexican	11.0	11.4	13.2	14.4
Filipino	10.4	13.3	13.1	12.0
Caribbean	13.9	10.0	9.9	15.3
South American	10.0	11.6	11.8	13.8
Central American	11.5	12.1	12.8	12.8
Southeast Asian	13.1	15.9	12.5	16.5
East and South Asia	19.7	10.4	13.9	12.9
Other	12.3	10.3	14.3	14.3
Overall	11.9	12.5	12.7	14.2

Note: HS = High School.

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Table 2.4
Effect of High School Employment on Log Earnings (OLS)

	Model							
	(1)	(2)	(3)	(4)	(1')	(2')	(3')	(4')
Students in grade 11								
Number of hours worked in reference week	0.0001 (0.0016)	0.0000 (0.0017)	-0.0002 (0.0017)	-0.0003 (0.0017)	--	--	--	--
Respondent worked in reference week	--	--	--	--	0.0167 (0.0358)	0.0121 (0.0363)	0.0145 (0.0367)	0.0079 (0.0362)
R^2	0.153	0.170	0.194	0.211	0.152	0.169	0.194	0.211
Sample size	774	774	774	774	780	780	780	780
Students in grade 12								
Number of hours worked in reference week	0.0033** (0.0015)	0.0032** (0.0015)	0.0030** (0.0015)	0.0037** (0.0015)	--	--	--	--
Respondent worked in reference week	--	--	--	--	0.0652* (0.0369)	0.0642* (0.0369)	0.0542[+] (0.0366)	0.0643* (0.0368)
R^2	0.096	0.112	0.124	0.141	0.096	0.113	0.124	0.140
Sample size	762	762	762	762	766	766	766	766
Controls								
Basic	Y	Y	Y	Y	Y	Y	Y	Y
Family and SES background	N	Y	Y	Y	N	Y	Y	Y
Extended	N	N	Y	Y	N	N	Y	Y
Ability	N	N	N	Y	N	N	N	Y

Note: Robust standard errors are reported in parenthesis. Significance levels: ** P value < 0.05; * P value < 0.10; + P value < 0.15.

Table 2.5
Effect of Employment in Senior Year on Log Earnings for Different Sub-Samples (OLS)

			Sample		
	All	Males	Females	US born	Not-US born
Hours worked in reference week	0.0037** (0.0015)	0.0064** (0.0022)	0.0011 (0.0022)	0.0054** (0.0021)	0.0014 (0.0023)
R^2	0.141	0.263	0.132	0.213	0.146
Sample size	762	344	418	369	393
Respondent worked in reference week	0.0643* (0.0368)	0.0955* (0.0553)	0.0375 (0.0504)	0.1140** (0.0493)	0.0095 (0.0566)
R^2	0.140	0.255	0.133	0.216	0.144
Sample size	766	348	418	371	395

Note: Robust standard errors are reported in parenthesis. Significance levels: ** P value < 0.05; * P value < 0.10; [+] P value < 0.15.

Table 2.6
Effect of Employment on Log Earnings with interaction terms for gender and US birth (OLS)

	Model	
	No ability covariates	With ability covariates
Hours	0.000997 (0.002794)	0.000952 (0.002825)
Hours*male	0.003571 (0.003925)	0.004181 (0.003919)
Hours*US born	-0.000739 (0.003949)	0.000186 (0.003943)
Hours*male*US born	0.002852 (0.004702)	0.003139 (0.004614)
R^2	0.127	0.146
Sample size	762	762

Note: Significance levels: ** P value < 0.05; * P value < 0.10; [+] P value < 0.15.

Table 2.7
Percent of Subsample that Report Being Employed in Senior Year

		Sample		
	All	Males	Females	US born	Not US born
Age of respondent in 1995					
16	37	26	44	34	43
17	42	42	42	48	39
18	49	45	52	50	48
Number of same sex siblings					
0	48	45	50	53	42
1	40	39	41	44	37
2	49	46	51	40	56
3	50	44	56	50	50

Table 2.8
Effect of Hours Worked in Senior Year on Log Earnings (IV)

	Sample									
	All		Males		Females		US born		Not US born	
Structural equation										
Hours worked in reference week	0.0045	0.0016	0.0094	0.0084	-0.0024	-0.0057	0.0165	0.0166	-0.0037	0.0037
	(0.0149)	(0.0130)	(0.0214)	(0.0135)	(0.0147)	(0.0151)	(0.0222)	(0.0226)	(0.0146)	(0.0118)
Hausman Test (P value)	0.86	0.92	0.86	0.86	0.83	0.64	0.59	0.62	0.79	0.76
First stage										
Age 16	-4.1322**	-4.0678**	-6.2355**	-6.6290**	-2.7376	-2.3847	-5.1238**	-5.1921*	1.9085	2.6776
	(2.0433)	(2.0663)	(2.5822)	(2.6063)	(3.1450)	(3.1658)	(2.6292)	(2.6988)	(3.7995)	(3.5875)
Age 17	-2.2067**	-2.0611**	-1.0725	-1.1199	-3.5871**	-3.3217**	-0.2286	-0.1991	-4.1126**	-3.7653**
	(0.9031)	(0.9070)	(1.4978)	(1.5030)	(1.1922)	(1.2055)	(1.3406)	(1.3491)	(1.2991)	(1.2990)
Same sex siblings	--	0.7547	--	1.7047*	--	0.4188	--	0.0711	--	1.5210**
		(0.5847)		(0.8924)		(0.7789)		(0.9547)		(0.7459)
Hansen J Test (P value)	0.48	0.69	0.66	0.98	0.42	0.45	0.73	0.45	0.59	0.69

Note: Robust standard errors are reported in parenthesis. Significance levels: ** P value < 0.05; * P value < 0.10; + P value < 0.15.

Table 2.9

Effect of Employment in Senior Year on Probability of Graduating College (Marginal Effects)

	Sample				
	All	Males	Females	US born	Not US born
Hours worked in reference Week	-0.0010 (0.0017)	-0.0005 (0.0020)	-0.0018 (0.0026)	-0.0005 (0.0022)	-0.0024 (0.0026)
LL	-311.61	-109.12	-177	-137.18	-151.58
Sample size	749	338	394	361	369
Predicted Prob.	0.2479	0.1747	0.3284	0.2406	0.2734

Note: Significance levels: ** P value < 0.05; * P value < 0.10; [+] P value < 0.15.

Table 2.10
Effect of Employment in Senior Year on Probability of Having Mostly White Coworkers (Marginal Effects)

	Sample				
	All	Males	Females	US born	Not US born
Hours worked in reference week	0.0011 (0.0017)	0.0015 (0.0025)	0.0007 (0.0024)	0.0047** (0.0024)	0.0002 (0.0023)
LL	-436.15	-181.71	-232.49	-192.31	-219.62
Sample size	744	334	410	357	387
Predicted Prob.	0.3553	0.3504	0.3463	0.3322	0.3334

Note: Significance levels: ** P value < 0.05; * P value < 0.10; + P value < 0.15.

Table 2.11
Effect of Employment in Senior Year on Treaimn's Job Prestige Score (OLS)

	Sample				
	All	Males	Females	US born	Not-US born
Hours worked in reference week	-0.0224 (0.0333)	0.0011 (0.0490)	-0.0562 (0.0470)	-0.0239 (0.0495)	-0.0356 0.0474
R^2	0.194	0.244	0.199	0.245	0.218
Sample size	812	377	435	391	421

Note: Robust standard errors are reported in parenthesis. Significance levels: ** P value < 0.05; * P value < 0.10; + P value < 0.15.

3 Ethnic Enclaves and the Evolution of Immigrants' Language Assimilation

3.1 Introduction

Since the immigration reform of 1965 the United States adopted a liberal policy with regard to the selectivity of immigrant admission into the country. The previous legislation stipulated a system of national origin quotas that in practice limited the immigration of non-Europeans. The new system opened the doors to a new large wave of immigration comparable in magnitude to the one occured during the early 20th century. This change in policy determined a dramatic shift in the national origin mix of immigrants. Simulatenously with this change, a deceleration occurred in the rate of immigrant assimilation compared with previous immigration experiences. Attempts to explain this phenomenon have multiplied among social scientists.

Immigrant assimilation is considered a desirable outcome by most analysts. Although economic theory predicts that the greater the differences between immigrant and natives the larger the productivity benefits of immigration, the advisability of maintaining a minimum level of cultural and social evennes across ethnic groups is also recognized. Understanding the determinants of immigrant assimilation in the context of the change verified

in the last decades is important to formulate effective policies that support the capabilities of the economy to absorb and benefit from immigration.

The emergence of ethnic enclaves has become also a much more prevalent phenomenon during the recent decades. Sociologists have long recognized the potentially detrimental effect of ethnic enclaves for the integration of minority groups into mainstream society. In particular, ethnic enclaves hinder the process of language assimilation. This represents a hypothesis worthy of careful examination since immigrant's proficiency in the language of the host society improves labor market outcomes. This positive correlation has been repeatedly recognized in the economic literature (see, for instance, Bleakley and Chin, 2003). Though language proficiency has been proved to matter for immigrants' labor market outcomes, intergenerational language assimilation for immigrant groups seems to proceed rapidly (Petrovic, 1997). This suggests the question of whether ethnic enclaves may persist for long in the face of rapid intergenerational language assimilation.

Although the role of ethnic enclaves in retarding the economic assimilation of immigrants has been studied, the potential impact that slower asimilation may also have in contributing to the formation and persistence of ethnic enclaves has not received much attention. Similarly, the relevance of rapid intergenerational language assimilation has not been considered explicitly. I study this question in the context of a dynamic model of intergenerational language assimilation with ethnic enclaves. I present two main contributions. First, even if assuming very large rates of language assimilation, once an ethnic enclave reaches a certain size the dilution of that geographic concentration occurs fairly slowly. Second, even when allowing for high rates of intergenerational language assimilation, the free mobility of a larger proportion of immigrants with lower human capital endowments reinforces the formation and persistance of ethnic enclaves.

3.2 Background

Assimilation of immigrants into their host countries, understood to include not just so-cial integration but also the homogenization of distinctive attributes, has been extensively studied by economists and other social scientists. Although each discipline character-izes the process of assimilation differently, most definitions center on the importance of cultural and socioeconomic factors. From the point of view of cultural determinants, as-similation consists of the acquisition of cultural traits by individuals or groups immersed in a foreign cultural context. Multiple cultural characteristics have been studied, from po-litical beliefs and language to the prevalence of intermarriage among immigrant groups. Until the 1960s, the dominant view among American social scientists was described by the "melting pot" metaphor. According to this view, all immigrants to the United States ex-perience a time-dependent process of increasing Americanization, whereby they acquire typically American mores and values (Gordon, 1964). Immigrants with the longest time residing in the United States and their American-born offspring would exhibit the great-est similarities with the native population. This view was gradually replaced, however, by a conceptualization positing a multicultural social landscape as the more normal state of things. In this view, assimilation along some dimensions would occur simultaneously with differentiation in other characteristics (Glazer and Moynihan, 1963).

The economic literature has emphasized socioeconomic characteristics; particularly, par-ity of earnings between immigrants and natives. The seminal work by Chiswick (1978) studied the empirical post-migration wage convergence between immigrants and natives. Using cross-sectional census data, Chiswick estimates earnings equations with the num-ber of years since migration as an additional explanatory variable. He finds that foreign born men face an initial earnings disadvantage after migrating to the United States rela-tive to natives, but that this gap becomes smaller with time spent in the country. This pat-tern suggests that recent immigrants have fewer skills and less knowledge of U.S. markets compared to native workers; however, as they spend more time in the country, immigrants

are able to accumulate new human capital that helps them close that initial earnings gap.

Borjas (1985) challenges the interpretation of the evidence advanced by Chiswick (1978) by pointing to the existence of differences in earnings potential across immigrant cohorts. The findings by Chiswick (1978) could reflect the fact that earlier immigrant cohorts who have resided the longest in the country are intrinsically of higher "quality" than more recent ones. Borjas (1985) does find evidence of cohort effects using data from multiple censuses.

Immigrant socioeconomic assimilation has been studied also from an intergenerational perspective, which involves looking at the evolution of socioeconomic indicators among second- and third-generation immigrants.[1] Here, too, apparent differences between earlier waves of immigration to the U.S. and more recent ones is detected by some investigators (Borjas 1993, Grogger and Trejo 2002, Trejo 2003), while others interpret the evidence as not indicative of any real differences (Card et al. 2000, Smith 2003). Though there exists disagreement about the actual (intra- and inter-generational) assimilation performance of more recent immigrant groups compared wth previous ones, multiple theories have been proposed to explain the hypothetical economic assimilation lag. This broad array of explanations ranges from discrimination (Portes and Rumbaut, 2001), geographical proximity to originating countries and illegal status in the country (Huntington, 2004), lower propensity to learn English (Lazear, 1999), effect of ethnic externalities (Borjas, 1995), and attrition in ethnic self-identification (Duncan and Trejo, 2007).

In the economic literature, explanations based on language assimilation and concentrated settlements have received special attention from researchers. For example, recent work by Borjas (2013) identifies a negative cohort effect also in the 10-year growth rate of both earnings and language assimilation of recent immigrant groups, and correlates it with the pre-existing size of corresponding national origin groups. Immigrant groups

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[1]Due to data limitations only the first three generations are typically defined: the first generation is formed by immigrants; the second generation consists of U.S.-born individuals (and, sometimes, immigrant children) with at least one foreign-born parent; the third generation includes those born in the U.S. whose parents are also born in the U.S.

with a larger initial presence exhibit worse performance of more recent cohorts regarding both economic and language assimilation. Interestingly, the lower economic assimilation of recent cohorts is verified across all the largest national origin groups (Mexico, El Salvador, Guatemala, Cuba, Dominican Republic, China, Korea, Philippines, Vietnam, and India). The measure of pre-existing group size used by Borjas (2013) is the product of the raw group size and the Herfindahl concentration index by state of the national origin group. Controlling for the immigrant group size, the negative cohort effect of immigrants that entered in the late 1990s is reduced by 20% in the case of economic assimilation and by 40% for the rate of language assimilation.

3.2.1 Ethnic concentration and intergenerational assimilation

In the U.S., Mexican immigrants represent the group most concentrated in ethnic communities. According to the 2000 U.S. Census, among PUMAs[2] with presence of Mexican immigration the proportion of residents born in Mexico is 15%. The same number for non-Mexican immigrants is less than 3% (Lazear, 2007). Since Mexican immigrants also fare poorly in terms of their assimilation along multiple dimensions compared with other groups, it is natural to consider the influence on immigrant assimilation of ethnic concentration.

In economics, there are at least two theoretical links between ethnic enclaves and immigrant assimilation. Borjas (1995) posits the existence of ethnic externalities that affect the process of human-capital accumulation. Concentration in ethnic neighborhoods creates an environment that influences the outcomes of the next generation as much as the investment made by the parent generation. The existence of ethnic externalities may retard the process of intergenerational assimilation. Lazear (1999) formalizes a simple insight:

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[2]Public Use Microdata Areas (PUMAs) are geographical zones defined by the U.S. Census that include upward of 100,000 people.

immigrants that settle in communities where a large proportion of conationals also live face fewer incentives to invest in learning the English language. Evidently, a large presence of individuals from the same country facilitates entering in economic transactions with other conationals without needing to use the English language. Provided the number of individuals from the same country is large enough, unassimilated immigrants will prefer to live inside their enclave, where they will most likely remain unassimilated.

While Borjas (1995) explicitly discusses the intergenerational implications of his mechanism, Lazear (1999)'s story focuses more strictly on the incentives for language assimilation affecting immigrants but not necessarily their offspring.[3] When considering the intergenerational transmission of language in this context it is important to consider other factors that affect the likelihood of language assimilation among immigrant descendants. The experience of immigrants and their first language use in a society with an unrelated dominant language has been extensively studied by linguists (Petrovic, 1997). For instance, second generation immigrants who often attend schools with a monolingual orientation are usually proficient in both their parents' language and their adopted country's language. However, as early as in the third generation complete loss of the first language is verified and a movement to monolinguism is realized. This phenomenon of *language shift* across generations has been observed in diverse immigrant populations, like the case of Turkish immigrants in The Netherlands or Italian immigrants in Australia.

Assuming that immigrants care about the wellbeing of their offspring so that a measure of their children's expected earnings is included in their utility function, then the decision to move to an enclave would involve a tradeoff. On the one hand, there is an immediate benefit from being able to trade with a larger number of people who share the same language; on the other hand, this decision would also entail a cost by immersing the offspring in an environment that reduces its chances to become language assimilated.

This discussion suggests that a formal framework for describing the transmission of

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[3]Actually, Lazear (1999) does discuss generational effects in the restricted context of a society where a minority group achieves a sufficient critical mass such that a shift in dominant culture may be induced.

cultural characteristics such as language may be useful for understanding the economic assimilation of immigrants. Although it seems clear that unassimilated immigrants will always prefer to live close to other individuals from their same country, they may also weight positively the likelihood of assimilation of their offspring. Additionally, unlike their parents, second generation immigrants are subject to a non-zero probability of assimilation even if living in an ethnic enclave due to the majority language being the dominant or high-status language.

3.3 Theoretical framework

To model the generational evolution of language assimilation a dynamic model can be used. My objective is to model a situation where there are two localities, one that qualifies as an ethnic enclave and the other which does not. In the enclave, the proportion of the immigrant population is higher. Each individual can be either an immigrant or a native based on whether the person is language assimilated. If the person speaks English, then that person is considered indistinguishable from a native. Both inside and outside the enclave each type of individual grows at a constant rate.

In all specifications natives are indifferent between living in the enclave or outside of it. This means that natives will choose to remain in whichever locality they happen to be. New immigrants and unassimilated immigrant descendants instead may choose to move to the enclave.

Consider locality i without specifying its type (enclave or mainstream). Let $n^i(t)$ denote the number of natives in locality i and $m^i(t)$ be the number of immigrants. Growth rates for each group are specific to that group and immigrants grow at a higher rate than natives. We use g_j to denote the growth rate of type j. Then, $g_m > g_n$. The growth rate of immigrants represents both natural population growth (births minus deaths) and the arrival of new immigrants. Consequently, in the case of natives the growth rate represents

only their natural increase.

The process of language assimilation in each locality is dependent on the prevalence of immigrants in that locality. Following (Lazear, 1999), the more immigrants that are present in the locality the less assimilation will occur because immigrants may be able to engage in trade with conationals and face a low opportunity cost of not learning English. The easiest way to model this idea is by specifying that the proportion of immigrants that assimilate each period is equal to the pre-existing proportion of natives already present in the locality. To understand better this idea suppose there is no assimilation at all. Then both populations in locality i would grow at a constant rate:

$$n^i(t+1) = (1+g_n)\, n^i(t),$$
$$m^i(t+1) = (1+g_m)\, m^i(t).$$

In this case, the number of new immigrants arising in period t and added to the population in period $t+1$ are the same, $g_m m^i(t)$. To model language assimilation, however, it is necessary that a proportion of those new immigrants become natives by time period $t+1$. Let $N^i(t) = n^i(t) + m^i(t)$ be the total population in locality i, then the proportion of new immigrants arising in period t that assimilate by period $t+1$ is

$$p_n^i(t) = \frac{n^i(t)}{N^i(t)}.$$

Clearly, $p_n^i(t)$ is the percent of natives in locality i at time period t. It is also clear that the percentage of natives in period t can be re-written as a function of the percentage of immigrants in the same period, $p_n^i(t) = 1 - p_m^i(t)$. In this way we can incorporate the prediction by Lazear's (1999) model that the proportion that assimilates each period is inversely proportional to the prevalence of immigrants: the greater the proportion of immigrants in i the less the number of new immigrants that will assimilate each period.

Including the effect of assimilation, the system describing population growth in locality i is

$$n^i(t+1) = (1+g_n)\, n^i(t) + \frac{n^i(t)}{N^i(t)}\, g_m\, m^i(t),$$

$$m^i(t+1) = m^i(t) + \frac{m^i(t)}{N^i(t)}\, g_m\, m^i(t).$$

This can be rewritten more clearly as

$$n^i(t+1) = (1+g_n)\, n^i(t) + [1 - p^i_m(t)]\, g_m\, m^i(t),$$

$$m^i(t+1) = m^i(t) + p^i_m(t)\, g_m\, m^i(t).$$

It is necessary now to model simultaneously the two localities (enclave and mainstream) and the movement of unassimilated immigrants between them. In both localities there will be assimilation as described in the previous example. Additionally, there will also be a proportion of new immigrants each period that choose to move from the mainstream location to the enclave. This group represents those immigrants that are not able to assimilate and need an ethnic environment to be able to find viable employment. Changes in the proportion of new immigrants that move to the enclave could represent changes in the "quality" of immigrant cohorts as interpreted by Borjas (1985). Lower quality cohorts will be more likely to prefer to concentrate in the ethnic enclave, while higher quality cohorts will invest in assimilation in order to profit from the better economic opportunities available in the mainstream locality. Cohorts with a greater sense of altruism for their children and wanting to improve the economic opportunities available to them will also prefer to remain in the mainstream locality so as to increase the probability of assimilation of their offspring.

The complete model of population growth with immigrant assimilation and ethnic concentration can be described then by the following equations (the superscript e denotes variables from the enclave locality)

$$n(t+1) = (1+g_n)\, n(t) + [1 - p_m(t)]\, g_m\, m(t),$$

$$m(t+1) = m(t) + (1-\delta)\, p_m(t)\, g_m\, m(t),$$

$$n^e(t+1) = (1+g_n)\, n^e(t) + [1 - p_m^e(t)]\, g_m\, m^e(t),$$

$$m^e(t+1) = m^e(t) + p_m^e(t)\, g_m\, m^e(t) + \delta\, p_m(t)\, g_m\, m(t).$$

As can be seen the parameter δ in the last equation determines the proportion of new immigrants that choose to relocate from the mainstream locality to the ethnic enclave. Obviously, the initial proportion of immigrants in the enclave locality is much higher than in the mainstream locality. Growth rates for immigrants and natives are constant over time and across both locations. Besides learning English, assimilated immigrants adopt the same fertility rates of natives.

3.4 Discussion

To explore the implications of the model presented in the previous section, I will simulate the system of equations presented. Consider first the case when there is only one locality. Based on estimates by the U.S. Census I use as a benchmark a yearly population growth rate of 0.8% for the native population and 2% for immigrants. The initial percentage of the immigrant population clearly depends on what type of locality is being modeled. For a rural or isolated community the initial percentage of immigrants will be small (less than 5%); for a gateway or large city with ample economic opportunities for every type of labor the immigrant presence could be significant.

The calculations in Table 3.1 assume that a generation corresponds to approximately 25 years. As can be seen, the presence of unassimilated immigrants tends to be diluted gradually. However, when the initial immigrant presence is above 20%, we see that even after 5 generations (125 years) there remains an important proportion of unassimilated immigrants. This result is significant since the model is imposing a very dramatic rate of assimilation with each new generation. With more realistic rates of assimilation we would see an even greater tendency of unassimilated populations to persist over time.

Table 3.1. Evolution of immigrant presence in one locality

Locality	Generation	% Immigrant		
Rural	0	3.00	5.00	8.00
	5	1.14	1.94	3.21
	10	0.42	0.73	1.23
Urban	0	20.00	30.00	40.00
	5	9.10	15.15	22.40
	10	3.70	6.56	10.46

The results shown in Table 3.1 are very robust to changes in the growth rate of the immigrant population. In particular, even with a growth rate comparable to that of the native population, results are practically the same. This suggests that beyond a certain level, the formation of a large minority within a locality tends to persist over long periods of time, even when very large rates of assimilation are assumed to operate. This is also consistent with the thesis of Lazear (1999) that the formation of large ethnic enclaves acts very strongly against economic and social assimilation. The case of Mexican immigration in the United State seems to confirm this view. It should be mentioned that the percentages used for an urban locality in Table 3.1 are not unrealistic. Data from the 2010 American Community Survey reveals that a fair number of American cities have comparable rates of immigrant presence. For instance, the foreign-born population reaches more than 35% in Miami, San Jose and Los Angeles; 28% in New York; more than 20% in Washington DC,

Houston, and San Diego; and more than 15% in Chicago, Sacramento, and Boston.

The more complex model with two localities is simulated in Table 3.2. Recall that in this case a proportion δ of new immigrants in the mainstream locality that remains unassimilated each period relocates to the enclave the next period. Hence, the immigrant population in the enclave increases for three reasons: natural population growth, new immigration from abroad, and influx of immigrants from the mainstream locality. In the case shown in Table 3.2, the proportion of unassimilated individuals in the mainstream locality that migrate to ethnic enclave is 50% ($\delta = 0.5$). The population size at the mainstream locality is also assumed to be five times that of the ethnic enclave. This is consistent with the assumption that economic opportunities are better in the mainstream location.

Table 3.2. Evolution of immigrant presence in two localities ($\delta = 0.5$)

Locality	Generation	% Immigrant		
Mainstream	0	5.00	5.00	5.00
	5	1.84	1.84	1.84
	10	0.06	0.06	0.06
Enclave	0	20.00	30.00	40.00
	5	9.57	15.58	22.79
	10	3.96	6.82	10.72

The calculations in Table 3.2 again assume a generation of 25 years and the same population growth rates as before. Comparing these results with those in Table 3.1, we see that they are very similar. The first noticeable difference is that when simulating two localities and allowing for flows from the mainstream location to the ethnic enclave, the degree of persistence over time of the ethnic enclave increases. Hence, the formation of ethnic enclaves is reinforced by the existence of other locations that also receive immigrants as long as some of the immigrants in those other locations decide to relocate to ethnic enclave. Though not shown in Table 3.2, this pattern is even stronger if the mainstream location has an immigrant population of at least 10% of its total population. This suggests that

even if a policy of promoting balanced settlement of immigrants is adopted, the potential for the emergence of an ethnic enclave is still present.

Another factor that contributes to the formation of ethnic enclaves is the parameter δ. As mentioned before, this parameter could either represent the quality of immigrant cohorts or their commitment to assimilate in the new culture. Lower quality immigrants will probably need to live in ethnic areas that facilitate their insertion into the economic system. Immigrants with a stronger attachment to their origin culture will also prefer to live in ethnic enclaves.

Table 3.3 simulates again the model from Table 3.2 but with a higher value of $\delta = 0.8$. If it is the case, as claimed by Borjas (1985), that immigrant quality has declined in the last decades, then this factor contributes to the formation and persistence of large ethnic cities.

Table 3.3. Evolution of immigrant presence in two localities ($\delta = 0.8$)

Locality	Generation	% Immigrant		
Mainstream	0	5.00	5.00	5.00
	5	1.78	1.78	1.78
	10	0.06	0.06	0.06
Enclave	0	20.00	30.00	40.00
	5	9.82	15.82	23.01
	10	4.11	6.96	10.87

If the economy receives large inflows of immigrants with less intrinsic capability to integrate, then the persistence of ethnic enclaves is reinforced. Notice that all the results occur over very long periods of time. It is not a rapid process. However, it is aided by multiple factors.

If, as argued by Borjas (1995) and Lazear (1999), ethnic concentration plays an important role in understanding the fall in the rate of assimilation observed among recent immigrants, it is a factor that can continue retarding assimilation for a long time. If the size of the immigrant presence in a locality reaches a critical mass (in my calculations it seems

to be around 20%), then the process of assimilation decelerates. As seen before, slower assimilation in turn feeds back into the processes that generate the formation of ethnic enclaves.

3.5 Conclusion

In this paper I have shown that, provided the immigrant density in ethnic enclaves is sufficiently large (around 20%), rapid intergenerational language assimilation does not eliminate ethnic enclaves very rapidly. If in addition to this the human capital endowment of immigrants is low, such that they can only attain financial stability by relocating to ethnic localities, then the persistence of these localities is increased.

These results call into question the view that it is not necessary to manage immigration flows because the intergenerational progress of immigrants' descendants works to alleviate temporary cultural and social disruptions. Though the model presented did not include measures of economic performance like earnings or wealth, it is very likely that poor language assimilation leads to poor economic outcomes as well. In addition to this, cultural or language segmentation introduces duplication inefficiencies, transaction costs, and a reduced market size for many economic activities.

The results in this paper suggest that it may be socially beneficial to prevent the formation of large ethnic centers. Especially if no policy is used to select admitted immigrants based on their characteristics.

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