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Immigration from Mexico and Local Fiscal Policy in the United States

By

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A dissertation submitted in partial satisfaction of the requirements for the Doctor of Philosophy

in

Political Science

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Jacob Citrin, Chair

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Abstract

Immigration from Mexico and Local Fiscal Policy in the U.S.

by

Morris Eli Levy

Doctor of Philosophy in Political Science

University of California, Berkeley

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Prominent social psychological and economic theories link ethnic diversity and low-skilled immigration to reduced provision of public goods. Both the level of ethnic diversity and the presence of low-skilled immigrants have increased dramatically in the United States since the 1960s. Immigration from Mexico has been the largest and most persistent driver of these demographic shifts. This dissertation theorizes and then explores empirically whether and how Mexican immigration has influenced local fiscal policy and related public preferences. Applying a new instrumental variables design, it finds little evidence that Mexican immigration has eroded local government spending on public goods or reduced tax receipts, though there is evidence that it has substantially increased the level of public debt. Subsequent chapters turn to explaining why Mexican immigration did not erode public goods spending as predicted. Leveraging the shock in the rate of naturalization among Mexican immigrants that followed the 1986 Immigration Reform and Control Act's legalization program, it argues that the acquisition of citizenship by Mexican immigrants helps explain non-negative effects of Mexican immigration on public goods provision and taxation. On the other hand, an analysis of 2006-2012 national survey data reveals that Mexican immigration does induce natives to express less support for public goods spending and taxation and less support for progressive taxation in particular. These findings suggest that while Mexican immigration does erode public support for the provision of public goods, these changes in public opinion do not in turn translate straightforwardly into the policy changes predicted in much of the literature on ethnic diversity and public goods. Finally, there is evidence that Mexican immigration increases mass polarization by heightening constraint between ideological identification, immigration policy preferences, and preferences over budgetary policy.

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CHAPTER 1: INTRODUCTION, THEORY, AND EMPIRICAL CHALLENGES

INTRODUCTION

Immigration from the developing world poses a variety of political dilemmas for democratic societies (Zolberg 1999; 2006; Tichenor 2002; Freeman 1995; Money 1999, Karapin 1999, Joppke 1998; Citrin, Reingold, and Green 1990; Citrin et al. 1997). Beyond intensifying debates over admissions policy, it raises questions about what rights and privileges the host society should accord immigrants already present. Official distinctions between legal and illegal immigrants and between citizens and non-citizens define the degree to which newcomers have attained the rights and privileges that come with full political membership. But subjective unofficial distinctions based on linguistic, cultural, and racial difference overlay designations of official status and can apply even to immigrants who have become citizens, separating images of co-national and stranger, and relegating some immigrants to the status of outsiders who are perceived to bear a lesser claim on the rights, privileges, and perquisites that natives enjoy (Citrin, Wong, and Duff 2001; Wong 2010; Sides and Citrin 2007; Wright 2011).

A critical political issue stemming from these status distinctions is to what extent immigrants merit access to the goods and services host governments provide. Controversies over immigrants' putatively illegitimate use of public benefits have unfolded at all levels of American politics. Federal and state governments have at times responded to public outcries by limiting non-citizens' or illegal immigrants' access to various forms of public assistance (Hero and Preuhs 2007). In other instances, such as Proposition 187 in California in 1994, state politicians have more aggressively attempted to capitalize on public flare-ups over the burden immigrants supposedly impose on the public coffers.

But such targeted exclusions can only partially address public discontent. Access to much of what governments provide is difficult or impossible to restrict. Public goods such as fire and police protection, roads, and parks, are by their nature non-excludable or extremely difficult to apportion only to some residents. In other cases, such as public education or emergency medical care, legal principles, political exigencies, or widely shared moral concerns make exclusion at present infeasible.¹ Even if non-citizens' use of these goods and services could be prevented, no democratic country has contemplated restricting their availability to naturalized citizens, many of whom fail to satisfy some natives' normative civic or ascriptive criteria for true membership in the national community (Wright 2011).

Absent recourse to targeted exclusion, might democracies respond to immigration by diminishing their overall investment in public goods and services? My dissertation seeks to shed light on this question by analyzing how Mexican immigration to the U.S. has influenced local patterns of spending, taxation, and debt and altered the U.S. public's preferences over taxation and spending in their states. Large and conflicted literatures assess the political and economic challenges that immigration poses to the welfare state (Lipset and Marks 2000; Alesina and Glaeser 2004; Banting et al. 2006; Banting and Kymlicka 2004). The question I examine is whether immigration hampers democratic governments' ability to provide a far more extensive universe of goods than those comprising the social safety net.

¹ Though not inconceivable. Texas' effort to ban illegal immigrant children from its public schools, overturned 5-4 by the Supreme Court in *Plyler v. Doe* (457 U.S. 202, 1982), was one serious such effort.

Though cutting back on the provision of public goods might seem a drastic response to immigration, there are four reasons to expect that democracies would react in this way. First, natives may value public goods less when they are shared with immigrants. Second, immigration may cause natives to care less about the public sphere and thus assign public goods lower value. Third, natives might devalue public goods whose form in part reflects immigrants' preferences and thus deviates from their own preferred form. And fourth, low-income immigration could increase natives' opposition to taxation because it increases natives' share of the tax burden relative to their share of the goods that taxes fund.

Each of these mechanisms has ample support from economic or social psychological theories. Social Identity Theory and group interest theories furnish the expectation that citizens' preferences over public spending are in part a function of the degree to which their own and other groups would benefit from it. To the extent that natives view immigrants as constituting a social group apart from the national community, immigration may cause a devaluation of public outlays by diluting the share of goods directed to one's own group and increasing the share that redound to other groups. Prejudice against immigrant groups may also cause natives to bristle at immigrants' access to public goods. Perceptions of immigrants as fiscal burdens and beliefs that migration decisions are influenced by a desire to reap the benefits of host society generosity would exacerbate these feelings. Robert Putnam's (2007) "Constrict Theory" suggests that the ethnocultural diversity immigration generates can lead people to withdraw psychologically and behaviorally from the public sphere and consequently prefer to invest less in public goods meant to enhance it. Immigration may also influence the material cost-benefit calculus associated with preferences over the level of public goods provision by decreasing the return on taxed income or introducing discord over the form public goods should take and rendering the compromise output less desirable.

The first and third explanations have been invoked to support the "widely accepted" (Habyarimana et al. 2007; p. 1) claim that racial and ethnic diversity causes governments to produce lower levels of public goods. Banerjee et al. (2005) assert that this is "one of the most powerful hypotheses in political economy" (p. 639, quoted on p. 1 of Habyarimana et al. 2007). It has been prominently applied to the case of the United States during the time period I study (Alesina, Baqir, and Easterly 1999; Alesina and La Ferrara 2005).

Despite its apparent relevance to immigration, however, the impact of immigrant-fueled ethnic diversity has not yet been considered separately from the impact of racial diversity among natives. And though research on the subject of ethnic diversity and public goods provision has proliferated in the past fifteen years, theoretical and empirical challenges in existing literature warrant a renewed look along the lines I pursue.

Relevant theories include a number of unverified assumptions and are subject to several counterarguments. One assumption is that people routinely think about immigrants, or are primed by politicians to do so, when developing opinions about the provision of public goods. This is perhaps doubtful when the public goods in question have no ostensible link to demographic change, and it is not clear whether even welfare policy is racialized with respect to Hispanics (Fox 2004) as it is with respect to blacks (Gilens 1995; 1999). The episodic salience of immigration to mass publics (Hopkins 2010; McGhee and Neiman 2010) also calls into question how enduring any such linkages would be. A second assumption is that immigration generates an intense and one-sided reaction. Especially in the case of models positing group preference mechanisms, distaste for immigrants' use of public goods would have to be strong enough to override one's own material interest in not cutting back on the amount produced. In

reality, the U.S. and other western publics evince a mix of favorable, unfavorable, and indifferent reactions to immigration (Newman 2013).

Even if the hypothesized impacts on public opinion are present, there are at least three reasons to question whether this will translate into the expected policy changes. For one, those most hostile to immigration may themselves have little political agency or involvement (Freeman 1995) compared to those with a more sanguine view of immigrants. Additionally, even if natives' preferences over the level of public goods respond to immigration as predicted, immigrants' preferences for more public goods may counteract this pressure and prevent policy change (Fox, Bloemraad, and Kesler 2013). Finally, fiscal federalism may also work against the impact of any change in public opinion on public policy because it entails competitive pressures that make slashing local developmentally oriented spending on public goods a particularly hazardous course of action (Peterson 1981).

On the empirical side, existing research yields no clear verdict on whether or under what circumstances ethnic diversity or immigration influence the provision of public goods. While the negative effect of ethno-linguistic diversity has been confirmed consistently in comparative studies in Africa (Easterly and Levine 1997; Miguel and Gugerty 2005; Habyarimana et al. 2007), research in the developed world is far more conflicted (Stichnoth and van der Straeten 2010; Alesina Baqir and Easterly 1999; Boustan et al. 2010; Hopkins 2011; Rugh and Trounstein 2011). And while research in Europe has focused on immigration as a source of ethnic diversity, few studies in the United States have considered the effects of immigration in particular, with more attention to the influence of the proximate black populations.

These mixed results emerge from a research literature fraught with conceptual and inferential problems. Conceptual problems relate to confusion over the multiple and heterogeneous demographic phenomena the independent variables researchers have chosen are in fact capturing. Commonly used measures of "diversity" or ethnic shares of the population (e.g. Alesina, Baqir, and Easterly 1999; Boustan et al. 2010; Hopkins 2011) conflate the effects of the presence of a variety racial and ethnic groups and arbitrarily assume a uniform effect of natives and non-natives from a given racial group. Research focused on the effects of immigration itself seldom differentiates between immigrants of different national origins (e.g. Hopkins 2010; Newman 2013), let alone contemplates which immigrant group would best serve as a case for assessing the theories that are invoked, though it is well documented that natives react differently to different immigrant streams (Ha 2010), that different immigrant groups differ substantially in their level of social, political, and economic integration and standing, and that the national origin composition of immigrant communities varies starkly both across countries and across regions within countries (Card 2001).

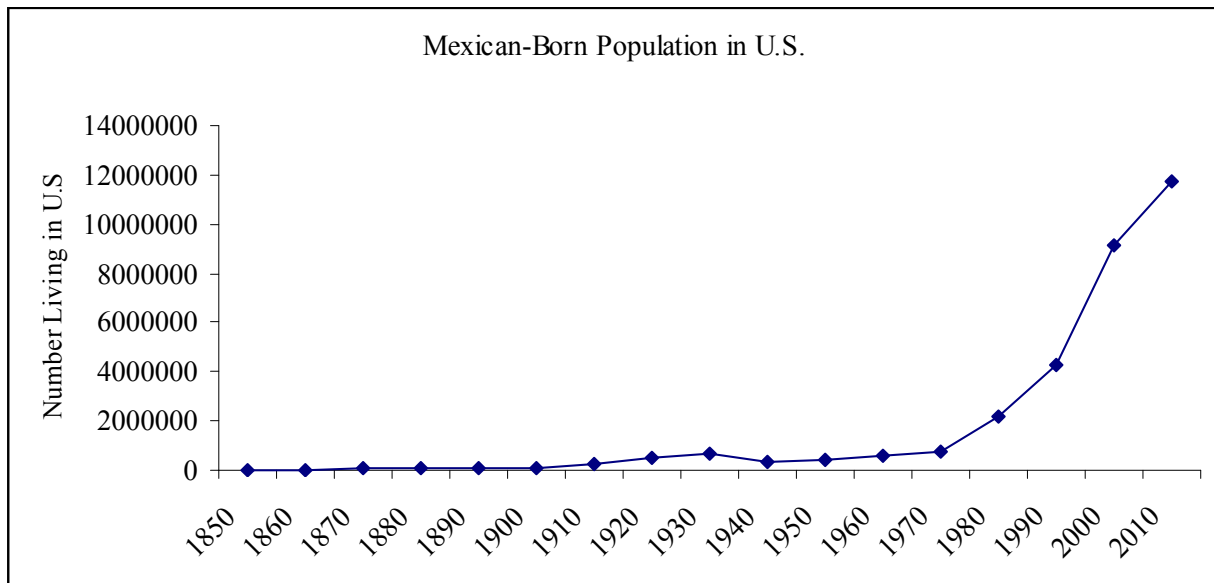
Causal inference is hampered by the fact that virtually the entire political science and sociology literatures exploring the effects of immigration or ethnic diversity on any outcome lack a convincing source of identifying variation in their key independent variables. This means that their often contradictory conclusions may spuriously reflect the causal impact of any number of factors associated with immigration or diversity in any particular case. For reasons I will elaborate in the next chapter, selection-on-observables designs commonly used in the literature are ill-equipped to remedy this problem.

My research addresses a number of the literature's conceptual shortcomings. Confining my focus to Mexican immigration homes in on a group whose attributes are theoretically congenial to the models developed above that link immigration to lower provision of public goods. Immigration from Mexico has been for a half century the largest source of increases in

the level of U.S. ethno-linguistic diversity and contributor to the growth of the illegal immigrant and low-income non-citizen populations. As I argue in detail in the next chapter, these attributes make Mexican immigration a “likely case” for at least three of the four models.

As demonstrated in Figure 1, the number of Mexican immigrants living in the United States skyrocketed in the late 1970s. Before that time, Mexican immigration to the U.S. was in large part a seasonal phenomenon and under auspices of the Bracero Program that lasted from 1942 to 1964. Workers cyclically sojourned for some months and then returned home. The rise in flows and settlement was triggered by the liberalization of immigration reforms and the prioritization of family reunification as a basis for admissions, which touched off a pattern of “chain migration” (Tichenor 2002). Critically for this dissertation’s empirical strategy, the supply of Mexican migrants rose precipitously over this period as well as a result of the Mexican baby boom that followed the Mexican Revolution and continued into the 1970s. Mexican immigrants are now the largest and most geographically dispersed immigrant group in the United States, accounting for approximately 30% of all foreign-born.

Figure 1



Source: Migration Policy Institute

To address key inferential challenges, I apply a research design first introduced by (Chalfin and Levy 2012) that leverages two well-established features of Mexican migration to the U.S. to generate an instrumental variable that isolates identifying variation in the timing and location of immigrant influxes. I argue that this approach helps rule out spurious correlations between immigration from Mexico and local fiscal outcomes in the U.S. produced by migrants’ self-selection into U.S. cities. The instrument has two components. Temporal variation in Mexican states’ mid-twentieth century baby booms predicts variation in those states’ emigration rates between 1980 and 2000 (Hanson and McIntosh 2010). Stable, historically-determined migration networks that I identify link emigration flows from each Mexican state to a distinct set of U.S. metro areas. I also import existing instrumental variables strategies that have been used in prominent studies examining immigration’s impact on host societies’ labor markets, where endogeneity has been a central concern (Card 1990; Altonji and Card 1991; Card 2001).

The dissertation proceeds as follows. In the remainder of this opening chapter I provide a critical elaboration of relevant theoretical underpinnings, discuss pertinent research findings, and identify the empirical challenges in the topical literature. Chapter 2 is devoted to developing the main empirical strategy, which is similar to that put forward in Chalfin and Levy (2012). It provides heuristic and formal derivations and defenses of the use of the approach to answer the dissertation's motivating questions.

I then turn to the analysis itself. Chapter 3 applies the dissertation's research design to a study across U.S. metro areas of how increases in the percentage of a city's population that is Mexican foreign-born influences changes in public spending on non-excludable goods and services, the extent and progressivity of taxation, and fiscal balance. Measures of these dependent variables are constructed relative to other objects of spending and sources of revenue, to need (overall population and likely prime beneficiary population), and to capacity as captured by per capita personal income.

Chapters 4 and 5 are dedicated primarily to explaining Chapter 3's essentially null finding. Chapter 4 explores whether it is an artifact of the 1986 Immigration Reform and Control Act, which gave approximately three million illegal immigrants, most of whom were of Mexican origin, legal resident status and a chance to become citizens. The political empowerment of Mexican migrants may blunt otherwise negative effects of immigration on the provision of public goods. Chapter 5 then homes in on whether immigration from Mexico fosters fiscal conservatism in American public opinion. Its key dependent variables are questions repeated in the 2006, 2008, and 2010 Cooperative Congressional Studies about whether respondents believe their state budget deficit should be addressed using spending cuts or tax increases and subsequently about whether respondents prefer increases in the sales tax or property tax, sources of revenue that vary in their progressivity. The goal is to ascertain whether Chapter 3's mostly null result emerges because the hypothesized effects of immigration on public opinion do not materialize or whether such effects are observable but fail to significantly sway public policy. A brief conclusion summarizes the dissertation's contributions and points to avenues for future research.

THEORY

In this section I elaborate the theoretical support for the hypothesis that immigration from Mexico erodes the local provision of non-excludable goods and services in the United States. Each of the four theories I invoke posits a distinct antecedent of the utility people derive from the production of non-excludable public goods. The first asserts that the utility people derive from a public good is a function of which social groups they perceive to benefit from it. The second argues that it is a function of their attachment to and participation in the public sphere generally. The third and fourth depend on individual material self-interest as a motivation for supporting the production of a public good.

The four models that emerge from these assumptions are all political economic in the sense that they define citizens' utility functions over alternative fiscal policies as a function of the way resources are distributed and presume that government policy in democracies responds to changes in the median voter's public preferences. They also all presume that immigration will affect public attitudes and that those changes in public attitudes will be reflected in policy changes. Responsiveness may occur as a result of democratic responsiveness to electoral pressures or as a result of concerns about the "exit" (Hirschman 1970) of residents who pay taxes above the local average (cf. Peterson 1981).

However, the theories highlight different causal agents associated with increased immigration. Two are tied to immigration's role in increasing social, ethnic, and cultural diversity and in generating distinctions between co-nationals and those perceived as not fully part of the nation. The third and fourth are blind to social identities and ethnic diversity and derive purely from predictions about how immigration's impact on the income distribution, the range of preferences over public goods in society, and the prevalence of non-citizenship in turn influences the costs and benefits individuals derive from taxation and spending on public goods. They are not mutually exclusive, and the effects they predict immigration to have on the public provision of non-excludable goods and services are potentially complementary.²

For the moment I refrain from describing relevant research findings in detail or developing a methodological critique, reserving that for the chapter's next subsection. Instead, I proceed by explaining how each existing theory bolsters the proposition that immigration from Mexico reduces the provision of public goods in U.S. localities. I then briefly summarize the most relevant research findings. Finally, I develop several potential qualifications to these theories and, where possible, derive testable implications that complicate the dissertation's principal causal question.

Theories Supporting a Link Between Mexican Immigration and Local Public Goods Provision

Social Identities and Inter-Group Attitudes In claiming that ethnically diverse locales in the United States furnish lower levels of public goods, Alesina, Baqir, and Easterly (1999) speculate that "each ethnic group's utility [from a public good] is reduced if other groups also use it" (p. 1244). More broadly, citizens' preferences over public spending are alleged to be a function of their attitudes toward the social groups perceived to benefit from it, with spending benefiting members of one's own group valued above spending benefiting members of other groups (Vigdor 2002).

One explanation for this preference is selective altruism toward members of one's own group (Boheim and Mayr 2005). Another is in-group favoritism and a desire to maintain positive distinctiveness. A central claim of Social Identity Theory (Tajfel and Turner 1986) is that people are motivated to achieve the positive distinctiveness of their social identities, often at some personal cost. In the well-known "minimal group" experiments, subjects randomly designated as members of groups distinguished only by their arbitrary names frequently supported allocations of resources that maximized the degree to which their own group's payoffs were higher than the outgroup's, even forgoing the maximization of their own personal payoffs to do so. One

² The literature on the relationship between ethno-linguistic diversity and public goods provision in the developing world has also proposed that collective action suffers in such settings because social sanctions are more difficult to impose than in homogeneous societies (e.g. Miguel and Gugerty 2005). It is not clear how relevant such theories are in cases in developed settings where governments have ample enforcement and sanctioning capabilities to bring the production of public goods to fruition even where members of diverse groups cannot sanction one another. An intriguing alternative explanation is that shared ethnicity fosters a set of strong norms of reciprocity that lead to greater cooperation among co-ethnics (Habyarimana et al. 2007; 2009). It is not clear, however, that the sorts of experimental trust games Habyarimana et al. use in their study of African subjects would be relevant to citizens' willingness to support government production of non-excludable goods in developed settings since governments' enforcement potential obviates the need for trusting that other residents will not defect.

explanation roots these effects in a desire to maintain personal self-esteem by preserving or raising the status of the groups that are salient to an individual's identity (Rubin and Hewstone 2004).

Alesina and La Ferrara (2005) invoke an early version of Social Identity Theory to support the "preferences" link between ethnic diversity and public goods provision – "individuals may attribute positive utility to the well being of members of their own group and negative utility to that of members of other groups (Tajfel, Billig, Bundy, and Flament 1971)" (p. 765). In the case of immigration, social identities are defined not only by ethnic difference but by differences in language, shared history, cultural practice and values, and a sense of membership in the "imagined" national community. National identities in particular are a pervasively strong element of western publics' self-concepts (Citrin and Sears 2014; Schildkraut 2011; Theiss-Morse 2009). They are infused with emotional attachment to country and are accompanied by personal pride in the nation's achievements (Citrin and Sears 2014).

Taken together, these distinctions mean that social boundaries between immigrants and natives are readily visible and politicized. Thus it is reasonable to expect that the fiscal preferences of natives who regard many immigrants as outside the national community would exhibit in-group favoritism in the form of valuing goods more to the extent they preserve or increase the status of the national group.

In all but the most regressive tax systems conceivable, this may be especially true when an immigrant group is heavily situated in the lower echelons of the income distribution. Immigrants' consumption of public goods may then be viewed as burdening local budgets and consummating an undesired transfer of wealth from natives to immigrants who do not deserve the state's largesse. Alesina and Giuliano (2009) summarize this logic: "To the extent that new immigrants are near the bottom of the income ladder, their arrival should decrease the desired level of redistribution for the locals" (p. 18).

In brief, first, if natives feel that co-nationals are uniquely entitled to the goods government produces, immigrants' access to these goods may be taken as a threat to group boundaries and derogation of the status associated with national membership. Second, if natives perceive that immigrants are paying less than natives for the same enjoyment of a good or paying the same for greater enjoyment, then greater production of the good would result in the reduction of the resource differentials between these groups. These conditions are satisfied if natives pay more in taxes on average than immigrants do or if the goods in question are rivalrous, so that the use of the good by immigrants diminishes the amount available to natives. In either formulation, immigrants' access to these goods adds a psychological cost to providing the good, and Social Identity Theory suggests that this cost might be great enough to reduce the amount of the good natives wish to produce, even if their personal material interests would dictate producing a higher level of the good.

Several aspects of the Mexican immigrant population make it a congenial case for this theory. Mexican immigrants are to a large degree linguistically separated from the U.S. mainstream, with 29% of the population aged five and older speaking English proficiently (Gonzalez-Barrera et al. 2013). Comparing this figure to the nearly 90% of native-born people aged five and older of Mexican origin who speak English proficiently is a compelling reason to focus attention on the foreign-born population and not on the Mexican origin or Hispanic population as a whole. English ability is cited almost universally by native-born Americans as a criterion for being "truly American" (Wright, Citrin and Wand 2012). It is also clear that the U.S. public harbors views of Mexican immigrants as distinctly low in education and as poor

(Ramakrishnan, Esterling, and Neblo. n.d.), perceptions that parallel reality and are distinctly acute in the foreign-born population, as distinguished from the larger U.S. native-born Mexican origin population (Gonzalez-Barrera et al. 2013). Thus the widespread belief that immigrants are a net burden on public budgets is likely felt especially strongly in the case of this group.

Animosity toward certain immigrant out-groups in the form of racial prejudice, ethnocentrism, nativism, or negative stereotyping may cause some natives to derive disutility from immigrants' use of public goods even if they perceive no associated threat to the positive distinctiveness of the national group. The link between in-group favoritism and out-group derogation is empirically tenuous (Tajfel and Turner 1986), but a sense of economic cultural threat may heighten antipathy toward immigrants and compound hostility to their use of publicly provided goods.

There are several reasons to expect that Americans' views of Mexican immigrants would reflect such antipathy. One is simply that immigration flows appear to arouse anxiety. For decades, a far larger share of whites has favored reducing the overall level of immigration than increasing it, both in the U.S. and in Western Europe (Freeman 1995). This alone is not evidence of hostility to immigrants or Latinos per se, but there is a tight relationship, possibly primed by media coverage, between negative perceptions of Latinos and restrictionist sentiment in the U.S. (Valentino, Brader and Jardina 2013). Ha (2010) and Brader, Valentino, and Suhay (2008) have also demonstrated that Latino immigrants evoke particularly negative emotional reactions and stereotypes. Exposure to Spanish language also appears to trigger a sense of threat (Newman, Hartman, and Taber 2012), and there is clearly widespread concern that Latinos are not assimilating and retaining primary national loyalties to their countries of origin (Huntington 2004; Wright and Citrin 2010). In short, both ethnic and cultural difference may contribute to feelings of animosity toward immigrants of Mexican origin.

Under certain conditions, such as status equality and the salience of a set of common goals, one might expect "contact" between groups to foster reductions in prejudice (Allport 1954; Alvarez and Butterfield 2000; Pettigrew 1971; Rocha and Espino 2009). Yet these conditions seldom hold in contexts where Mexican immigrants live in proximity to U.S. natives, so "threat" is a more likely to prevail in response to an ethnically diverse context (Key 1949; Blalock 1967).

Debate persists over whether the threat hypothesis originally developed to explain white prejudice toward and discrimination against blacks extends to Latinos and immigrants. Some studies find evidence of it (e.g. Dixon and Rosenbaum 2004) while others do not (e.g. Citrin, Reingold, and Green 1990), and some even support contact theory (e.g. O'Neil and Tienda 2010). Yet mounting evidence suggests that immigration from Latin America at least conditionally increases anti-immigrant sentiment among whites. Where earlier levels of immigration were low (Newman 2013) or when immigration is nationally salient (Hopkins 2010), rapid growth in immigrant populations leads to greater expression of hostility to immigration among U.S. natives. Recent studies find evidence that even in longstanding immigrant-receiving areas, the size of the Latino population share is positively associated with concern about immigration among whites (Rocha et al. 2011).

These findings do not pertain only to whites. A separate line of research finds increased inter-minority hostility where Latino and black populations are substantial and black-Latino socio-economic inequality is high (Gay 2006). Thus in some areas blacks may also resent Latinos who appear to be superseding blacks economically. On the other hand, McClain (1996) and McClain et al. (2006) illustrate how realistic competition between blacks and Latino

immigrants, perhaps more likely in areas with more socioeconomic similarity, fosters antipathy. Recent research has demonstrated the complexity and contingency of black-Latino relations (e.g. Jones-Correa 2011; Morin, Sanchez, and Barreto 2011; Kaufmann 2003; Oliver and Wong 2003; Vaca 2004; Hero and Preuhs 2013). Clearly recognition of commonality often co-exists with a sense of intergroup linked fate as well as feelings of competition. The point here is that resentment of new immigrants' use of public goods and services may be present among blacks and not only among whites.

Exaggerated perceptions of the size and composition of the immigrant population may amplify the sense of threat associated with immigration and feed concerns about immigrants' burden on public budgets. Sides and Citrin (2007a; 2007b) find that European and American publics significantly overestimate the foreign-born share of their countries' populations. In the United States, Wong (2007) identifies similarly exaggerated public beliefs about the national size of non-white populations and documents that estimates are highly sensitive to the demographic composition of their immediate surroundings. The correlation between local minority population sizes and estimates of the national size (Wong 2007) leaves intact the tie between actual local demographic change and perceptions of it while still magnifying the perceived degree of these changes, though it is also likely that exaggerated views of the size of the immigrant and illegal populations reflect as well as foster concerns about the integrity of the nation and immigrants' access to public goods.

In addition to ethnic and racial prejudice and threat, natives may feel Mexican immigrants do not deserve access to public goods and services because of their legal status. The high percentage of the Mexican foreign-born population in the United States illegally is especially critical in this regard. In 2010, Mexicans were estimated to be 58% of the illegal immigrant population (Passel and Cohn 2011), and whereas approximately 28% of the entire foreign-born population is illegal (Passel and Cohn 2011), the figure for the Mexican foreign-born population was well over 50% (Terrazas 2010). The American public's average estimate of the share of the population that is living in the U.S. illegally vastly overstates reality (Sides and Citrin 2007b). Moreover, Ramakrishnan, Esterling, and Neblo (n.d.) find that Americans frequently associate illegality with Mexican origin, perhaps leading to an especial overestimate in this group, and have especially negative views of illegal immigrants who are Mexican (cf. Masuoka and Junn 2013). Many debates over immigrants' use of public services and rights have focused on the illegal population, potentially increasing its public salience in these regards (Ono and Sloop 2002; Espino and Jimeno 2012), and many Americans reject the extension of public benefits to immigrants. A recent (fall of 2013) national survey conducted by Matthew Wright, Jack Citrin, and Morris Levy found that while 85% of Americans support providing emergency room care to naturalized citizens, only 51% support it for legal immigrants who have been in the country at least five years, 46% for more recent legal arrivals, and only 27% for illegal immigrants. Differences between support for naturalized citizens' and non-citizens' access to other public benefits such as welfare and food stamps were even wider.

Constrict Theory Observing a robust negative association between the level of neighborhood ethnic diversity and individuals' interpersonal trust, Robert Putnam famously claims that "diversity, at least in the short run, seems to bring out the turtle in all of us" (2007, page 151). Putnam's core finding is that living in an ethnically diverse area erodes trust not only in members of out-groups but in members of one's own group as well. Putnam terms this alternative to "threat" and "contact" theories, which suggest corrosive and salutary effects

respectively of exposure to diverse ethnic groups under different sets of circumstances “constrict theory.”

Putnam does not speculate about the psychological foundations underlying this effect, though others (van der Meer and Tolsma 2011) have provided a theoretical basis for supposing that ethnic diversity erodes generalized trust rather than trust in out-groups alone. They propose that generalized distrust emerges as a consequence of a retreat from social life fostered in the first place by a sense of threat induced by the presence of distrusted minority populations. As an alternative channel, they suggest that diverse settings give rise to anomie, “induc[ing] feelings of anxiety about the social structure of the neighborhood and uncertainty about the dominant societal norms and values” and making people “hesitant to meet and mingle with others in their neighborhood, regardless of the ethnicity of their fellow neighborhood residents” (p. 9). Alesina and La Ferrara (2000) propose a different mechanism: if people avoid social mingling that involves interactions with members of different groups, they will also miss out on social mingling with members of their own group and so their overall level of interpersonal trust will decline.

Whatever the mechanism behind it, a consequence of this attitudinal retreat, Putnam argues, is that people in ethnically diverse areas engage less in collective action and participation in politics and civil society. Though myriad cross-national studies in Europe (Soroka, Banting, and Johnston 2006; Alesina and La Ferrara 2002; Putnam 2007; see Harrell and Stolle 2010 for a review) have failed to generate consensus over the validity of Putnam’s thesis there, there is solid support for the hypothesis in the United States.

Alesina and La Ferrara (2000) show a strong tendency of residents of ethnically diverse U.S. metro areas to report lower rates of membership in a wide range of groups. Costa and Kahn (2003) find that ethnically diverse areas in the U.S. manifest lower rates of electoral turnout, volunteering, and membership in organizations, and trust. It is not altogether clear in either of these studies whether the results affirm constrict theory or are simply a function of existing inter-group antipathy, as emphasized in Alesina and La Ferrara’s (2000) model. Moreover, when Putnam (2007) disaggregates ethnic diversity and tests the effects of the black and Latino population shares in U.S. neighborhoods, he finds that both have independent negative effects on trust, suggesting that diversity fueled by immigration from Latin America, and not only the size of black populations, contributes to the relationship. Levy (n.d.) corroborates Putnam’s finding with respect to immigration from Mexico in particular, using an instrumental variables design and a dynamic state-level measure of social capital to find a causal relationship between increases in states’ Mexican population shares between 1986 and 2004 and declines in social capital.

This “hunkering down,” in turn, further weakens the bonds residents feel toward one another and toward the public sphere. It limits the degree to which social interactions take place and the strength of civil society, which hampers collective action (Alesina and La Ferrara 2000) and potentially a willingness to redistribute wealth (Putnam 2000). The impetus to support investment in communal projects is then a casualty of these declines in social cohesion, solidarity, and perceived obligation toward fellow residents. Ethnic diversity and immigration may also weaken class solidarity (Lipset and Marks 2000). In other words, rather than asserting that people derive utility from goods directed toward their group or disutility from goods directed toward other groups, this model implies that immigration causes a more general psychological retreat from public endeavors and a consequently diminished interest in the provision of public goods. The logic behind this link is twofold. One point is that people’s support for taxation and

public spending in part stems from a sense of attachment to those around them. A second is that people are less interested in investing in a public sphere from which they derive little use: if I stay home more often I will be less likely to care about the condition of roads, libraries, and parks. Researchers have debated and tested variants of this hypothesis (e.g. Anderson, Mellor, and Milyo 2004, 2008).

Moreover, immigration may impact not only interpersonal trust but trust in government. Citrin, Levy, and Wright (forthcoming) and McLaren (2012) link dissatisfaction with the changing political community in Europe (immigration and multiculturalism policies) with low levels of satisfaction with regime authorities and institutions. This may be true in the United States as well. Newman and Johnston (2012) find that trust in state government declines with hostility to immigration. Whether a simple reaction to dissatisfaction with expansionist policy or a more fundamental spillover from support for the political community to support for its political stewards, the result could be less willingness to support expansive government provision of public goods.

Non-citizenship Among Low-Income Residents and Redistribution A third model derives from standard median voter models of inequality and redistribution. Influxes of non-citizens at the low end of the income distribution dilute the return that the median income citizen receives on each dollar forfeited in taxes. This weakens his incentive to support spending that is in effect redistributive. Once again, since even regressive forms of local and state taxation leave higher-income residents paying more than lower-income residents in absolute terms, the production of most public goods involves at least some degree of redistribution and is therefore rendered less desirable.

This model emphasizes self-interest rather than group-identification and focuses on the distribution of income rather than ethnic diversity, but it also predicts that immigration from Latin America lowers pressure for redistribution and public goods provision. McCarty, Poole and Rosenthal (2006) suggest that a high rate of non-citizenship in the lower tiers of the income distribution have thwarted pressure for redistribution in the United States that would have arisen as a consequence of rising income inequality (Romer 1975; Meltzer and Richard 1981). The arrival of low-wage immigrants leaves the median *citizen's* income intact but lowers the overall per capita income. Consequently, immigration makes redistribution less beneficial and more costly to the median income voter despite concomitant rises in inequality between all resident family incomes. This again leads to the conclusion that immigration – or at least immigration of low-wage workers who do not rapidly attain U.S. citizenship – reduces redistribution and other forms of public spending.

Mexican immigration is apt case for this model, since it is composed primarily of low-skilled migrants who disproportionately occupy the very low end of their destination cities' income distributions and, especially when it comes to the large portion of Latin American immigrants who are from Mexico and Central America, exhibit relatively low naturalization rates. Whereas the naturalization rate among non-Mexican immigrants in the United States hovered between 58% and 68% between 2000 and 2011, for Mexican immigrants it rose slightly from 31% to 36% (Gonzalez Barrera et al. 2013).

Preference Heterogeneity A fourth model proposes that diversity brings discord over the form public goods take. Increasingly heterogeneous public preferences over how government funds should be spent force compromises that push the products of public spending further from the median voter's preferred form. This diminishes the value of the goods produced and lessens citizens' willingness to pay taxes to fund them (Alesina, Baqir, and Easterly 1999). Here ethnic

diversity is again epiphenomenal. It would not have an effect except by virtue of its presumably being a marker of diversity of preferences over the form public goods should take.

The evidence for ethnic diversity-based preference heterogeneity is largely anecdotal. Alesina, Baqir, and Easterly (1999) illustrate the argument with reference to debates over bilingual education and the location of public thoroughfares. While it is clear that there are significant differences in support for bilingual education between immigrants and natives in the United States, there is no evidence about whether these debates spill over into more general dispositions toward spending on education. There is also no direct evidence that public attitudes toward taxation take into account projections over what form public goods will take.

Zwane and Sunding (2006) claim that their design, which studies the effect of post-IRCA increases in the mostly Mexican migrant share of agriculture-heavy California counties, rules out preference heterogeneity as a mechanism because the migrants in question have not yet had a chance to achieve citizenship. This inference is not entirely warranted, however, because public services might well cater to migrants' preferences even if they cannot vote. For example, employers might prefer that a road be built closer to their employer base, and schools might introduce bilingual education as a practical measure even if there is no political pressure for it from voters. Moreover, given that during the period of their study millions of illegal immigrants had been granted legal permanent status with a chance to become citizens, public officials might have anticipated the subsequent spike in naturalization and begun courting immigrant voters.

Theoretical Counter-considerations

Several considerations call into question whether immigration from Mexico will actually influence public attitudes in the manner each of the four theories suggests. Immigration is salient only some of the time to mass publics (Hopkins 2010; McGhee and Neiman 2010). It is unclear whether it is habitually connected in the public mind with the fiscal policies that sustain the provision of public goods or welfare. While there is consistent evidence that the American public associates blacks with welfare (Gilens 1999), it is not clear that such "racialization" applies straightforwardly to ethnic diversity comprised of immigrant groups (e.g. Fox 2004; Hero and Tolbert 1996) or to other policy domains. There are well-known cases in which politicians have sought to capitalize on anxiety about immigration and hostility to non-citizens' – and especially illegal immigrants' – access to public benefits. However, these campaigns have targeted the exclusion of immigrants and not been part of a larger broadside against the provision of public goods generally.

Residents of longstanding immigrant destinations may also have grown accustomed to and even supportive of the presence of immigrants, rendering them unsusceptible to the reactions the first two models entail (cf. Newman 2013). Newman finds that residents of areas that have only begun to receive immigrants experience cultural threat in response to recent rapid increases but that these increases have the opposite effect in traditional receiving areas. The net effect may be null or even positive even if immigration is indeed having the hypothesized effect in newer destinations.

Even if immigration affects public opinion in the hypothesized manner, several considerations call into question the putative link between altered opinion and policy change. For one, those most incensed by immigrants' use of public benefits may be those whose opinions least often find expression in the policy-making process (cf. Freeman 1995). That is, hostility toward immigrants, like other forms of prejudice, is strongly correlated with education (Hainmueller and Hiscox 2007; Hainmueller and Hopkins 2013). Education is a marker of

political resources and efficacy and is associated with lower turnout (e.g. Wolfinger and Rosenstone 1980; Verba, Scholzman and Brady 2005). This may mean that the irritation such residents feel toward immigrants' use of public services is unlikely to find expression in public policy.

For another, democratic governments cut back on public investment at their peril. National governments would be held to account for the negative economic impact such a move would generate, and in federalist systems subnational governments that eschew productive public spending risk losing out as well in the competition for a robust tax base. Thus decisions over the levying and allocation of local and state public funds are to a large degree insulated from the changes in public opinion that the theories linking immigration to lower redistribution and public goods provision implicate. Peterson (1981), for example, shows that the exigency of preventing capital flight constrains city fiscal policy and ensures a focus on developmental spending. Redistributive spending varies according to fiscal capacity – how much income is available – rather than according to need or the demands of citizens or interest groups. In a finding that mostly corroborates Peterson's argument, Gerber and Hopkins (2011) use a regression discontinuity design to show that mayoral partisanship in U.S. cities has a very limited impact on tax policy and social policy.

Here I acknowledge that the choice of local government policy in a federalist system as dependent variable thus may pose a difficult case for the motivating theories linking immigration to public goods provision. On the other hand, subnational governments may incur the greatest fiscal burden from Mexican immigration (Zolberg 1999) because illegal immigrants often make extensive use of local public services such as schools while failing to qualify for federal benefits and because they often have federal income taxes withheld from their pay checks but more rarely pay local and state taxes other than those on sales. Thus subnational governments could have greater incentives than national governments to reduce the overall provision of goods and services in response to immigration that they are powerless to deflect (though see Light 2006; 2007). Moreover, perhaps the best known and most widely cited research linking ethnic diversity to public goods (Alesina, Baqir, and Easterly 1999) uses subnational government policy in the United States as its dependent variable.

Finally, whatever political power immigrants themselves wield, either through becoming citizens and attaining the franchise or other forms of participation, may prompt governments to provide more extensive amounts of public goods and services. Latino immigrants are known to support an energetic state that engages in liberal spending on social policy (Bowler and Segura 2012), and Mexican immigrants are no exception. Though their naturalization and participation rates are low relative to other immigrant groups, Mexican immigrants who do attain and use the franchise may pressure governments to produce public goods and services more expansively. Fox, Bloemraad, and Kesler (2013) point to protest or the threat of it as a way that even those without the right to vote can influence public policy – in their case, spending on welfare benefits.

EMPIRICAL CHALLENGES IN EXISTING RESEARCH

A large and sharply divided literature in political science and sociology considers how the racial identity of perceived beneficiaries of redistribution influences public support for it. Scholars have focused on welfare benefits and have used both measures of public attitudes and public policies as dependent variables. A recent review In the United States, for example, blacks are often regarded as undeserving recipients of welfare, and their share of the recipient pool is widely overestimated (Gilens 1996, 1999; Soss et al. 2011; Peffley Hurwitz, and Sniderman

1997; Luttmer 2001). In Europe, the focus has been on the effect of immigration-fueled diversity on support for welfare state institutions (Quillian 1995; Banting et al. 2004; Bay and Pedersen 2006; van Oorschott 2008). Though findings with respect to the impact of black populations have been reasonably consistent, research in Europe and on the impact of other groups in the United States is mixed (Stichnoth and van der Straeten 2010).

In the United States, scholarship on how immigration and the presence of heavily immigrant ethnic groups influences support for welfare and other social policy has been more limited (Hero 2010). Results have not consistently indicated a straightforward negative effect of immigrant or Hispanic population share (Fox, Bloemraad, and Kesler 2013) on preferences over redistribution and redistributive policy. Soss et al. (2001) find that Temporary Aid to Needy Families (TANF) rules were stricter and benefits lower in states with higher Latino populations (see also Fellowes and Rowe 2004; Fox 2012). Preuhs (2007) finds that increases in the number of Latino public officials can counteract threat and produce a curvilinear relationship between states' Latino population shares and their welfare spending. Hero and Preuhs (2007) conclude that whereas the immigrant share of states' populations were not associated with whether they included non-citizens in their TANF programs, states that did include non-citizens experienced an "erosion" of overall benefits levels. This is consistent with the idea that when exclusion was not an option, the effects of immigration spilled over to affect benefits levels overall. Brown (2013) finds evidence that both legality frames and anti-Hispanic frames play into social policy-making.

A smaller but widely known body of research focuses on how ethnic diversity and immigration influence public goods provision. As noted earlier, much of the logic that undergirds research on diversity's effects on redistribution can be applied also to the production of public goods more generally. Effects might be weaker if transfer from in-group to out-group or to "undeserving" beneficiaries is less salient. However, in either case natives might bristle at "fiscal leakage" (Razin, Sadka, and Swagel 2002) to immigrants and increase their resistance to taxation (cf. Coen-Pirani (2009)'s counterfactual exercises suggesting a large negative impact of immigration on education spending in California by way of making education more expensive to the median voter). Moreover, the effects of diversity or immigration on non-excludable goods production might also be stronger than the effect on welfare because welfare is a targeted transfer that non-citizens can be – and have been to a large degree in the United States – excluded from without resorting to producing less of the good overall.

Alesina, Baqir and Easterly's (1999) seminal article demonstrates that a commonly used measure of ethnic diversity correlates negatively across U.S. localities with spending on public goods in a variety of categories, including education, public welfare, health, police and fire, and roads and highways and positively with annual deficits and accrued debt. Several studies have corroborated this finding in a limited set of policy domains in the United States and in Europe, though the effects are sometimes small (Speciale 2012). However, Alesina, Baqir, and Easterly's findings clash directly with some subsequent analyses on the U.S. case that use the same or similar data (e.g. Boustan et al. 2010; Hopkins 2011).

In most instances researchers proffer a subset of the theories elaborated here to explain why such a link would be expected. The specific mechanisms advanced are seldom isolated and alternative explanations are rarely tested against one another (exceptions are Miguel and Gugerty 2005's and Habyarimana et al.'s (2007) studies of ethno-linguistic diversity's effects on cooperation in Africa). However, the divisions in the literature and its empirical weaknesses

make rigorous tests of the causal link itself a first-order concern. If no causal relationship is discovered, the endeavor to explain one is obviously fruitless.

Weaknesses The empirical evidence bearing on these hypotheses is sparse and conflicted. Little research has directly examined the relationship between immigration per se and public spending. McCarty, Poole and Rosenthal (2006), for example, present no direct test of immigration's impact on redistribution or redistributive preferences. The few direct analyses of immigration's effects on public spending in the U.S. have focused on potentially idiosyncratic cases (Hopkins 2010; Zwane and Sunding 2006). Moreover, researchers have yet to convincingly document a relationship between ethnic diversity or immigration and public support for redistribution or public goods provision. Cross-locality analyses of the American case are few and conflicted, as is cross-national research on Europe (Stichnoth and van der Straeten 2010; van Oorschott 2008; Alesina Baqir and Easterly 1999; Hopkins 2011; Boustan et al. 2010).

More to the point, there are serious empirical challenges to drawing inferences about the effect of immigration on local fiscal policy, a point made in reviews of the topical literature but addressed only sporadically (Stichnoth and van der Straeten 2010). Selection-on-observables designs that control for a range of factors known to be associated with diversity or immigration and fiscal outcomes (Hopkins 2010, 2011; Alesina Baqir and Easterly 1997, 1999; Rugh and Trounstein 2011; Boustan et al. 2010) are subject to bias in several respects. For one, the full universe of confounds is virtually impossible to address, let alone adequately measure. As an example, no analysis controls for cities' industry compositions. To the extent that service sector-heavy economies require less investment than manufacturing-intensive ones in infrastructure spending, immigrants' concentration in service-heavy "segmented labor markets" (Piore 1979) produces a spurious relationship between developmental spending and ethnic diversity.

The potential for post-treatment bias compounds the difficulty of trying to address endogeneity bias through adding statistical controls. Immigration is known to be caused by and likely to be a cause of a wide variety of social, political, and economic transformations that also may influence local public finances. In attempting to control for factors that influence immigrant flows and local fiscal policy, the researcher may also control for factors that stem from immigration, biasing the causal estimate. For example, some analyses (e.g. Putnam 2007; Alesina, Baqir and Easterly 1997) control for income inequality, but income inequality may in part arise as a result of immigration (Borjas 2003) and may be correlated with other impacts of immigration that, in turn, affect local public finances as well.

That the research literature is divided on how income inequality influences public finances (Alesina and Glaeser 2004; cf. Meltzer and Richard 1981; Boustan et al. 2010) makes the direction of the bias once again uncertain. Another example is attitudes toward immigrants. Immigrants may be deterred from hostile localities (although the lure of jobs may overwhelm this deterrent), and hostility toward immigrants may be associated with a wide range of other political attitudes that influence spending and taxation. Yet immigration, at least under some circumstances (Newman 2013; Hopkins 2010), may itself influence attitudes toward immigrants, which may in turn influence public finances.

Reverse causality looms as an additional threat to inference. Depending on whether immigrants are drawn to localities with more generous redistribution (e.g. Borjas 1999) or de-emphasize public goods (Zavodny 1999), residential selection could again introduce bias in either direction. This possibility applies not only to redistribution but to public taxation and

spending generally, as immigrants may systematically sort into localities with distinctive “bundles” of public goods (Tiebout 1956).

Research on the impact of diversity on local public goods provision in the United States illustrates the importance of these concerns. Alesina, Baqir, and Easterly’s (1999) seminal article measures the cross-sectional relationship between cities’, counties’, and metro areas’ 1990 level of ethnic diversity, calculated as a Herfindahl Index over the 5 racial categories the recognized in the U.S. Census, and the share of spending dedicated to health, education, police, fire, roads, welfare, and sewerage and trash, as well as on fiscal aggregates (debt, spending, and taxation per capita) as reported in 1992 U.S. Census of Governments data. They find a strong negative correlation between diversity and the share spent on roads, sewerage and trash, and welfare, though a positive correlation with the amount spent on police and with spending overall (the latter finding they suggest may be attributable to increased patronage). There is also evidence that taxes do not keep pace with spending, leading to greater debt. Alesina, Baqir, and Easterly also attempt to test the robustness of their results to analysis over time.

Their work has been criticized by Boustan et al. (2010) and Hopkins (2011), who use similar data to arrive at quite different results. Both point out the possibility of spurious correlation in Alesina, Baqir, and Easterly’s cross-sectional analysis, and Hopkins also points to an interpretive challenge: the correlations between present diversity and local fiscal outcomes may be the residue of earlier impacts that are no longer operative. Boustan et al. use data from 1970-2000, controlling for city fixed-effects, to corroborate that spending on roads declines as a share of all expenditures while police especially but also fire and hospitals increase but find no per capita decline in spending on roads and an increase in own-source per capita revenue. Since the effects of ethnic diversity are only a small part of their research, most of which concerns the effects of income heterogeneity, they do not elaborate on this finding but appear to take it more as a rejection than a confirmation of Alesina, Baqir, and Easterly’s main contention.

Hopkins (2011) using multi-level models to analyze the effect of levels and changes in cities’ ethnic diversity, and of black and Latino population shares individually, on the share of spending devoted to the same set of categories explored by Alesina, Baqir, and Easterly (1999) but with the exception of education. He analyzes a more extensive time period: 1952-2002. He reports null results for the effects of changes in ethnic diversity on changes in spending on productive public goods, though corroborates a positive impact on criminal justice spending. Hopkins’ argument about the interpretive challenges of cross-sectional analyses of diversity’s impact are compelling. And his argument that effects may vary over time as a consequence of which types of spending political rhetoric induces masses to associate with diversity is a novel application of his “politicized places” hypothesis to the provision of public goods. However, the combination of levels and changes of ethnic diversity in the statistical models makes it difficult to interpret why prior levels of ethnic diversity influence subsequent changes in fiscal outcomes but changes in ethnic diversity for the most part do not. More importantly, they do not furnish a test that converts Alesina, Baqir, and Easterly’s cross-sectional model into its longitudinal equivalent. Compounding the lack of comparability, Hopkins uses a more expansive set of controls than Alesina, Baqir, and Easterly do, so it is possible that this is the main reason his results deviate from theirs, possibly by eliminating confounds but possibly by introducing post-treatment bias.

Boustan’s and Hopkins’ main critique of Alesina, Baqir, and Easterly (1999) is the likelihood of confounding in their cross-sectional analysis. This critique is important. However, Alesina, Baqir, and Easterly recognize this issue and actually do test the robustness of their

county-level results to fixed-effects analyses between 1960 and 1990. They decide not to use cities as the units of analysis in this test, as Boustan and Hopkins do, because they find that cities' boundaries change too dramatically over time (see p. 1267). They find that their basic results are robust to this analysis when all other controls are excluded but vanish to insignificance when population size, age composition, median household income are controlled in the statistical models. Thus beyond the problematic use of cities as units of analysis in Hopkins' and Boustan's longitudinal analyses, it is also unclear whether Alesina, Baqir, and Easterly's cross-sectional findings are spurious, as Hopkins (2011) and Boustan et al. (2010) appear to suggest, or whether they are valid but vanish when post-treatment bias from these controls attenuates the estimate of the causal effect.³

Another potential issue in all three longitudinal analyses is random measurement error. Differencing or de-meaning measures that already contain error can severely compound the error. This is especially true where measures are serially correlated (Griliches and Hausman 1986). Measurement error in the differences between fiscal outcomes over time or, similarly, in the fixed-effects models may have biased estimates in these analysis toward zero, again lending more credibility to the cross-sectional than the longitudinal analyses.

While it is uncertain which side of this debate is correct, the conflicting results demonstrate the empirical ambiguities that can result in selection-on-observable approaches. If we control for few variables, we leave open the strong possibility of spurious correlation. If we control for an extensive set of covariates, we are likely to introduce post-treatment bias. A better approach to this question involves isolating a convincingly exogenous source of variation in localities' levels of or changes in diversity and observing whether this source of variation is associated with variation in public spending patterns and levels. Hopkins (2011) acknowledges the limitations of approaches that are unable to do this: "To be clear, without an exogenous instrumental variable to draw on, the goal here is to use conditional probabilities from observational data to assess the relative plausibility of several hypotheses" (p. 357).

A handful of papers have used such approaches to date, including in the case of immigration. Zwane and Sunding (2006) claim that legalizations of illegal immigrants through IRCA's Special Agricultural Workers program generated as-good-as-random immigrant influxes into agricultural counties in California in the late 1980s. They use measures of land use and agricultural employment as instruments. They find large negative effects on public goods provision. Idiosyncrasies of the local causal effect related to the time and type of immigration IRCA initially brought aside, it is questionable whether the instrument satisfies the exclusion restriction (i.e. that it is associated with the change in the dependent variable only as a result of its impact on immigration). Agricultural counties in California may have experienced greater

³ Alesina, Baqir, and Easterly address the discrepancy in their results with and without controls by pointing to loss of statistical power: "The fixed effects estimator is very costly in degrees of freedom in a sample with a large cross section but only two time periods. We do not think that the strong cross-section results should be disregarded because of the insignificance of ETHNIC in the fixed effects regression with all controls," (p. 1269). While the statistical reasoning is potentially valid, the argument is likely a red-herring since it is the few additional time-varying covariates that eliminate the significance of their key relationships, not the hundreds of dummy variables that extinguish many degrees of freedom but leave the core result intact. A more likely explanation is that adding covariates admits post-treatment bias into the differenced equation or reduced endogeneity bias.

declines in public goods provision than other counties did during this period for reasons other than immigration. Cascio and Lewis (2010) use an instrumental variables method developed by Altonji and Card (1991) and discussed at length in the next chapter to find that immigration from Latin America caused “native flight” in California school districts between 1970 and 2000.⁴

Studies outside the United States have also begun to adopt methods focused on isolating exogenous flows of immigration. Speciale (2012) uses exogenous influxes of immigration to EU countries following the 1990s Balkans wars, a design pioneered by Angrist and Kugler (2003), to estimate a small but significant negative effect of immigration on public spending on education. In another case of borrowing from the labor economics literature, McQuoid (2011) uses a technique devised by Friedberg (2001) to study regional variation in the influx of post-Soviet migrants into Israel and its apparently null effect on public goods expenditures there.

The approach taken here is related to McQuoid’s and Speciale’s research in particular that it seeks to isolate a portion of the immigrant flow into destinations that is untainted by endogeneity by leveraging an immigration “push” in the source region and linking it to destinations through pre-existing networks. As I will discuss, this increases the plausibility of the exclusion restriction, since the instrument is situated in the source region and is thus less likely to be related to the outcome measures taken in the destination except through immigrant flows. However, unlike their research, which draws on unique and discrete historical episodes to generate inferences, the design I advance and implement draws on a push factor that accounts for a high share of the overall volume of Mexico-US migration flow. It is therefore much more likely to yield conclusions that are broadly generalizable. The next chapter develops the design. It presents a formal derivation of the core strategy surrounded by a discussion aimed at providing a heuristic understanding of the approach.

⁴ Related research by Alesina et al. (2004) and Boustan (2010) uses large-scale black emigration from the South after World War I as an exogenous source of increased ethnic diversity in particular regions of the north and west on changes in school jurisdictions and white flight.

CHAPTER 2: EMPIRICAL STRATEGY

The previous chapter demonstrated how four social psychological and economic theories lead to the expectation that immigration from Mexico has reduced the local provision of public goods in the United States. It also, however, pointed to several potential counterarguments to these theories, heightening the need for a convincing empirical test. However, the related literature on ethnic diversity and local public goods in the United States is divided and subject to a number of methodological concerns, most notably the likelihood of endogeneity bias and measurement error and the introduction of post-treatment bias through the addition of statistical controls that are themselves impacted by increases in immigration. It argued that selection-on-observables designs used in most of the literature are ill-equipped to deal with these problems and that a better solution would be to isolate a portion of the immigrant flow that is exogenous with respect to the dependent variables of interest. A valid instrumental variables approach would accomplish this purpose, but there are only a few recent studies in that vein, and they are subject to concerns over generalizability. This is because the exogenous immigration flows that they isolate account for only small and idiosyncratic portions of the total immigration flow.

This chapter develops and defend an instrumental variables approach that I will use in the next chapter to estimate the causal effect of U.S. localities' Mexican immigrant population shares on a variety of subnational government tax and expenditure categories. Before proceeding to a mathematical derivation of the instrumental variables strategies I use to accomplish this aim, it is helpful to begin with a summary of purpose and a description of how this purpose is achieved.

The instrument I construct is very similar to one developed by Chalfin and Levy (2012) to estimate the impact of Mexican immigration on U.S. natives' employment outcomes. It combines a supply push in the level of emigration from each Mexican state with longstanding migration linkages between Mexican states and U.S. metro areas that channel emigrants from each Mexican state to distinct constellations of U.S. locales to isolate an exogenous portion of variation in the amount of immigration U.S. metro areas received between 1980 and 2000. Variation in the sizes of Mexican birth cohorts 18 to 50 years before the period of study furnish the supply push, and longstanding networks from source to destination channel this potential migrant supply in different measures to different U.S. metro areas.

Crucially, different Mexican states reached the heights of their 20th Century baby booms at different points (Hanson and McIntosh 2010). Thus they also had different supplies of eligible migrants at different points, corresponding to the time at which members of large birth cohorts had reached prime labor market age. Moreover, larger Mexican state birth cohorts tend to experience larger rates of emigration (Hanson and McIntosh 2010), likely as a result of wage pressures in Mexico. A well-known feature of Mexico-U.S. migration is that migrants tend to follow historically determined networks linking their source region and the destination. The "paisano" relations that undergird these networks confer information and other social and economic benefits to prospective migrants (Massey 1999). Although some migrants from Mexico will opportunistically travel to a destination city in the U.S. that holds economic promise or some other benefit, irrespective of these ties, many will predictably follow the paths that previous migrants from the same area traveled. In the aggregate, though there is obviously a good deal of overlap, each Mexican state has historically sent migrants to a different array of U.S. cities. As a result, we can predict larger-than-normal increases in the migrant share of U.S.

cities linked by migration networks to Mexican states that had larger-than-normal birth cohort sizes eighteen to fifty years prior.

These shocks in the Mexican-born shares of cities populations are plausibly exogenous because they are the product of fertility decisions and infant mortality rates *in Mexico* that took place long in the past and because the network channels in question were established prior to the period of study. Therefore the correlation between the immigration shocks I isolate and the taxation and spending dependent variables measured in U.S. localities cannot be the product of reverse causation and is unlikely to be confounded by any factor that gave rise simultaneously to both lagged fertility shocks and later changes in U.S. cities' fiscal policies. Moreover, the dual mechanisms of population surplus and migration networks jointly constitute one of the most significant sources of Mexican migration to the U.S. during the period in question, so there is reason to believe that whatever results emerge are not idiosyncratic.

I turn now to a mathematical elaboration of this instrumental variables strategy. Earlier research has pursued the related question of how ethnic diversity influences such outcomes using a regression equation similar to (1) but with a measure of ethnic diversity instead of the Mexican immigrant population share variable shown here:

$$y_{ct} = \alpha + \beta m_{ct} + \gamma X_{ct} + T_t + \varepsilon_t \quad (1)$$

In Equation 1, y_{ct} is some public finance outcome for U.S. geographic unit c in year t , α is a constant intercept term, and m_{ct} is the Mexican immigrant population share in c in year t . For convenience, we will assume that the geographic unit we are interested in studying is the metro area, though in the next chapters I will also consider states, and counties as units of analysis. X_{ct} is a vector of metro area characteristics (controls) measured in year t . β is a coefficient of variation denoting the impact of a one percentage point increase in the Mexican immigrant population share on y , and γ is a vector of coefficients of variation representing the impact of a one unit increase in each of the controls on the outcome. T_t is a vector of year fixed-effects, dummy values that take a value of one for observations in year t and zero otherwise and allow y to have a different intercept in each year. Where only one year of data is used, such as in Alesina, Baqir and Easterly's (1999) core analysis, this term is omitted. Their purpose is to account for any trends in the dependent variable common to the geographic units during the period in question that might also be related to a trend in the error term. This error term, ε_t , captures variation in y that is unaccounted for by the other covariates and the constant term.

Critically, ε_t is assumed to be uncorrelated with the covariates, an assumption violated when pre-treatment variables related to both m and y are omitted from the right-hand side of the equation. The key parameter for this analysis, β is therefore biased whenever a variable that influences both a city's Mexican population share and the outcome y is unmeasured or omitted from the equation. As argued in Chapter 1, it is unlikely that all omitted correlates of m and y could be identified, let alone adequately measured. Worse, it is not at all clear in which direction the net bias would run. We discussed the balance of service and industrial industry concentration as one example of a critical control that has been omitted from earlier analyses of the effect of ethnic diversity on public finances (e.g. Alesina, Baqir and Easterly 1999; Hopkins 2009) and of similar inquiries into the effect of immigration on group attitudes and social cohesion (Hopkins 2010; Putnam 2007). Yet it is not difficult to identify other potential confounds, such as the general orientation of a local regime toward the private and public sectors. Governments espousing economic conservatism may seek to keep local taxes low and to rein in regulation of

housing and employment that can deflect immigrants (see, e.g., Light 2006), another route to a spuriously negative association between immigration and public spending. Or more directly, immigrants may select into or systematically de-emphasize a localities' level of public goods spending when choosing where to migrate, which could introduce negative or positive bias.

Moreover, as a measure of the causal effect of m on y , β is also biased if y influences m , that is, if the causal arrow is reversed or if there is a reciprocal causal relationship between the variables. This, as we noted, could result from Tiebout sorting of immigrants into geographical areas. Thus even if reliable measures of immigration attitudes at the level of metro area existed, controlling for them might bias the estimate of β by absorbing an important channel through which immigration affects the extent and distribution of taxation and government spending.

In equation 1, β is tapping not only co-variation between changes in m and y within MSAs over time but also covariance between m and y across MSAs. One way to limit the potential for endogeneity bias is to limit the analysis to variation over time within MSAs by taking long-differences. With Δ indicating the change in a variable between t and $t-10$ to mimic inter-Censal intervals, we arrive at the following by differencing Equation 1:

$$\Delta y_{ct} = \alpha + \beta \Delta m_{ct} + \gamma \Delta X_{ct} + T_t + \Delta \varepsilon_t \quad (2)$$

Here we are regressing changes in public finance outcome y on changes in m . Thus stable differences between MSAs can no longer confound the estimate of β . In other words, we must assume only that unaccounted for variation across cities in Δy (represented by the differenced error term $\Delta \varepsilon$) is unrelated to variation in cities' Δm . For simplicity's sake, the notation for the constant term, α , and for the year fixed-effects are preserved. This general approach is pursued by Alesina, Baqir, and Easterly (1999), who use county fixed-effects and data from 1960 and 1990, and by Boustan et al. (2010), and Hopkins (2011), who use long-differences and data from each Census between 1970 and 2000.

While estimates using fixed-effects or first differences may be less subject to omitted variable bias than those derived from cross-sectional models, it is clear that they do not wholly resolve the fundamental challenges to causal inference in the literature on diversity or immigration and fiscal policy. This has been the motivation for more recent literature exploiting natural experiments to study the effect of plausibly exogenous immigration shocks on public goods provision in receiving areas (McQuoid 2011; Speciale 2012; Zwane and Sunding 2006).

The problem of causal inference is especially critical in research concerning the local effects of immigration. Immigrants' strategic behavior has long been recognized in the literature on immigration and employment in the U.S. and, more recently, in research on how immigration affects U.S. crime rates (see, e.g., Chalfin forthcoming). The labor economics literature has been particularly attuned to this problem since immigrants select into cities experiencing wage growth, which generates positively biased correlations between immigrant influxes and changes in natives' local labor market outcomes (see, e.g., Card 1990; Card and Altonji 1991; Borjas, Freeman and Katz 1997; Borjas 2003; Card 2001). The direction of bias is less clear in the association between immigration and crime (Chalfin forthcoming), and, as I have argued and others have noted (Speciale 2012), virtually impossible to know a priori in the present case, with plausible arguments on both sides. In effect, it is a challenge that must be addressed empirically.

The Network Instrument Labor economists have dealt with the endogeneity of immigration in two ways.⁵ The first is via natural experiments. For example, in his well known paper on the effect of the Mariel Boatlift on Miami’s labor market, David Card exploits an exogenously-driven migration push that dramatically increased the foreign-born population share in Miami but not in other, arguably comparable, cities. Miami natives’ employment outcomes did not fare substantially worse in the aftermath of the Mariel Boatlift than natives’ employment outcomes in a set of comparison cities, suggesting that immigration has minimal effects on low-skilled natives’ labor market outcomes (Card 1990). While such natural experiments get around the endogeneity problem and can present powerful evidence concerning immigration’s effects, they are also rare and potentially idiosyncratic. The Marielitos, for example, were refugees pushed to a city where a large politically well situated Cuban community already existed. Thus the Miami labor market might have been better equipped to incorporate them than normally occurs with large-scale influxes. Still, much rides also on the choice of comparison cases and whether they constitute truly valid controls.

The second approach is to use an instrumental variable that taps a portion of the variation in immigration to a city that is as-though-randomly assigned while discarding variation that is potentially endogenous. The archetype in this literature is the “network instrument” first proposed by Altonji and Card (1991). Leveraging the persistence of settlement patterns among immigrants from each country of origin (Massey 1999; Massey, Durand, and Malone 2002; Woodruff and Zenteno 2007), the network instrument assigns cities the same share of the total immigrant flow into the U.S. from a given country as their share of all immigrants from that country living in the U.S. in some earlier year. Summing across immigrants from all countries of origin gives the predicted immigrant influx over the period in question. Since only the total national flows of immigrants into the U.S. and predetermined settlement patterns are involved in generating this predicted value, it is presumably purged of variation induced by destination factors.

To see how the network instrument is derived formally, observe that the total influx of immigration from Mexico into city c over some time period ending in year t can be written as the sum of the influxes to that city from each of the 31 Mexican states and the Federal District. Typically the Network instrument has been computed as a sum across sending countries, but the logic is the same, and since the main instrument used in this paper (discussed below) relies on exogenous variation in the emigration levels from each Mexican state, it is useful to retain the Mexican state as the sending unit in this discussion.

$$\Delta m_{ct} = \sum_i MIG_{ict} \quad (3)$$

⁵ The literature on immigration and native wages and employment remains sharply divided. The two methods discussed here have tended to indicate minimal effects. A third method, known as the structural or factor-proportions approach, is omitted from this discussion because it does not address the issue of endogeneity. Instead, it solves a national factor productions model in which immigration shares within skill-experience groups are an input and assumes that, due to the selection of immigrants into healthy labor markets, whatever estimates are generated can be viewed as lower-bounds of the magnitude of immigration’s negative effects on natives’ labor market outcomes (see Borjas 2003). Absent any clear notion of whether correlations between immigration and public spending reflect positively or negatively biased estimates of immigration’s causal effect, no such argument can be made and the endogeneity problem must be addressed empirically.

Here i indexes the immigrant source states in Mexico and MIG_{ict} represents the total flow of migrants from Mexican state i into city c in the time interval ending in year t . MIG_{ict} is quite likely related not only to push factors affecting i but also to destination conditions in c that may well also influence the outcome variables y . Altonji and Card's insight is that this term can be decomposed into a portion that is explained by push factors in i alone and a portion that is explained by factors potentially related to destination conditions in c . This can be shown as follows:

$$\Delta m_{ct} = \sum_i MIG_{it} \times \rho_{ic} + (MIG_{ict} - MIG_{it} \times \rho_{ic}) \quad (4)$$

The term MIG_{it} is the total U.S.-bound emigration flow from country i for the period ending in year t . Only a portion of this total volume is assigned to c , however. This portion, ρ_{ic} , is the fraction of all U.S.-bound immigrants from i living in c in some year prior to the period defined by t . If the total outflow of migrants from i heading to the U.S. is for all intents and purposes exogenous to conditions in c , and if the tendency of a consistent fraction of them, ρ_{ic} , to settle in c is based on the benefits of settling where earlier migrants from one's country of origin have gone, then this first part of the summation taps a portion of the variation in immigration to c that is exogenous to conditions in c . The remaining portion of MIG_{ict} is potentially contaminated by conditions in c and is therefore purged, leaving the network instrument for immigration from Latin America:

$$\Delta m_{ct}^{net} = \sum_i MIG_{it} \times \rho_{ic} \quad (5)$$

Understood heuristically, the network instrument seeks to remove variation in each U.S. metro area's immigrant population share that is driven by conditions in the metro area itself. It seeks to capture only variation in the immigrant share that is driven by conditions in the source state or country and then depends on the tendency of immigrants to follow longstanding source-destination networks to channel that exogenous variation in the emigration flow into different U.S. metro areas. Since the networks are computed based on data prior to the period of analysis, it is argued that they cannot themselves be a product of differential conditions in the source region.

For the purpose of developing the network instrument in a way that demonstrates as clearly as possible its relation to the Birth Cohorts Instrument discussed below I have used Mexican states as the relevant sending units. In practice, however, the network instrument has been constructed at the sending country level, and data on the stock of migrants from each Mexican state into each U.S. city are not available for the period of study. Thus I use a single-country, Mexico-specific version of the network instrument in the analyses to follow. Its form is

$$\Delta m_{ct}^{net} = MIG_{Mt} \times \rho_{Mc} \quad (6)$$

Where MIG_{Mt} is the net flow of Mexican migrants to the United States in year t and ρ_{Mc} is a vector denoting the pre-existing distribution of Mexican migrants across U.S. locales.

Birth Cohorts Instrument The network instrument has recently come under methodological attack. If persistent characteristics of destination cities that initially drew migrants from a given country continue to attract migration from the same country, the network instrument is still potentially endogenous to U.S. city conditions, a point Card himself acknowledges (see Card 2001, footnote 23 on pages 43-44). For example, if migrants from Mexico in t follow the settlement patterns of earlier migrants from Mexico in part because

changes in conditions in those destinations have been consistently favorable economically, socially, or politically, then both the total outflow from Mexico in t and their settlement patterns are endogenous to conditions in U.S. cities that could also be linked to local fiscal policies. Differential wage growth, for instance, or expansion of demand for low-skilled service industry labor in traditional gateway cities for Mexican migrants consistently outpaces wage growth or demand for such labor in other cities, the Card instrument will in part tap variation in immigration that is endogenous to changes in U.S. city conditions. These changes may, in turn, influence the level or distribution of local taxation or spending.

As Pugatch and Yang (2011) point out, solving this problem requires isolating a portion of the variation in MIG_{it} that is clearly exogenous to conditions in U.S. destination cities. Their solution is to restrict the analysis to immigration from Mexico and use rainfall shocks in Mexican states and a pre-determined set of migration network weights linking Mexican states to U.S. states to predict yearly Mexican migration to each U.S. state. High rainfall reduces emigration from Mexican states because it improves local agriculture-heavy economies and employment prospects at home. Since Mexican migrants from each state have followed distinctive and persistent patterns of settlement across U.S. states, and since those patterns were forged far earlier than the period of study⁶, rainfall variation in each Mexican state can serve as an exogenous predictor of variation in the migrant share of each U.S. state's population. This approach has since been adapted by Chalfin (forthcoming) to study the effect of immigration from Mexico on crime in U.S. cities and subsequently by Levy (2013) to study the effect of immigration from Mexico on U.S. states' social capital.

Chalfin and Levy (2012) propose a different solution, arguably better suited to the present analysis. Historical birth cohort sizes in Mexican states determine the size of the eligible migrant pool once their constituents have reached prime migration age. Moreover, as Hanson and McIntosh (2010) have shown, members of larger Mexican state birth cohorts exhibit a higher propensity to emigrate than members of smaller birth cohorts do. Their explanation is that members of larger birth cohorts experience a higher degree of labor market competition in Mexico and thus have an added incentive to migrate to the U.S. Mexican birth cohort sizes in all states rose dramatically from the post-Revolution period until the present, due in large part to development-driven rises in fertility that have only in recent decades declined.

Yet, crucially, different Mexican states reached the pinnacles of their baby booms at different times. Again because different Mexican states send migrants to different constellations of U.S. cities, this variation in birth cohort sizes across Mexican states and over time predicts variation in U.S. Mexican migrant populations across U.S. cities. Chalfin and Levy (2012) demonstrate both temporal variation in birth cohort size shocks across Mexican states and variation in the Mexican migrant source states associated with each U.S. city as well as the instrument's predictive power. These are replicated for the present sample below.

First, however, the birth cohorts instrument is derived formally. As Chalfin and Levy (2012) show, this can be done by starting with a Mexican state-specific network instrument and demonstrating how it preserves "good" variation in migrant shares while discarding variation

⁶ In many cases, persistent migration linkages between U.S. and Mexican states were originally formed due to rail routes that early 20th Century labor recruiters from the U.S. followed in bringing seasonal agricultural workers from Mexico to particular destinations (Woodruff and Zenteno 2007). In fact, Pugatch and Yang create their weights in part on the basis of historic railroad routes leading from each border crossing point to a different set of U.S. destinations.

potentially contaminated by pull factors in U.S. cities that might be associated with those cities' public finances.

Recall that Equation 5 above shows the network instrument with Mexican states as source units and i indexing the 31 Mexican states and the Distrito Federal. I assure by construction that ρ_{ic} is predetermined with respect to the period identified by t by limiting the data used in estimating migration linkages to reports of migration trips that occurred prior to the period of study. The potentially endogenous term, however, is MIG_{it} since variation in emigration from Mexican state i may not only be the result of push factors in m but also a product of conditions in each c linked to i .

To address this problem, MIG_{it} can be decomposed into the product of each Mexican state's pre-determined eligible migrant pool at time t (the supply of potential migrants) and the probability of migrating to the U.S. in t , conditional on being in that pool. The pre-determined supply of migrants at time t is simply the average size of birth cohorts that have reached prime migration age, that is, the sum of the sizes of all birth cohorts *lagged between eighteen and fifty years*, represented by the term $BIRTHS_{it}$ in Equation 7. The term $Pr(MIG_{it}|In-Cohort)$ is the conditional probability of emigrating given that an individual was in Mexican state i 's birth cohorts 18-50 years ago.⁷

$$\Delta M_{ct}^{net} = \sum_i BIRTHS_{it} \times Pr(MIG_{it} | In - Cohort) \times \rho_{ic} \quad (7)$$

This decomposition makes it clear that the conditional probability component is the problematic source of variation in MIG_{it} since it may be influenced by conditions in networked linked c 's as well as exogenous push factors. The size of birth cohorts lagged eighteen to fifty years, by contrast, is a source of variation in the emigration pool from each Mexican state that is plausibly exogenous to conditions in cities linked to m through the factor ρ_{ic} .⁸ Unless fertility-related decisions in m are affected by prescient expectations as to conditions in networked link U.S. cities nearly two decades in the future, $BIRTHS_{it}$ constitutes a source of variation in m 's network-linked U.S. cities that is independent of contemporaneous conditions in those cities. Purged of the network instrument's potentially contaminated conditional probability term, the birth cohorts instrument may be written heuristically as

$$\Delta M_{ct}^b = \sum_i BIRTHS_{it} \times \rho_{ic} \quad (8)$$

⁷ Note that this operationalization is slightly different from the one presented in Chalfin and Levy (2012). Chalfin and Levy use the size of birth cohorts lagged 17-52 years to predict the share of migrants in 1980, 1990, and 2000 and then take ten-year differences to compute an instrument for the change in migrants. The present formulation instruments directly for the decadal influx using the average supply of those eligible to migrate, a closer analog to the network instrument. However, all results here were re-estimated using the Chalfin and Levy formulation and using fertility instead of births, and no substantial differences emerged.

⁸ In fact birth cohort sizes do influence the conditional probability of migration (Hanson and McIntosh 2010), but they do so only as a source-push factor. Equation 7 could separate out variation in conditional probability due to large birth cohort sizes from other, potentially contaminated, variation in the conditional probability of migration, but for simplicity this is not shown.

That is, to compute the value of the instrument for the Mexican migrant influx to a given U.S. metro area in a given year, I compute the average size of each of the 32 Mexican states' birth cohorts between 18 and 50 years ago (each Mexican state's eligible migrant supply), multiply each of these birth cohort sizes by the pre-1980 probability that a migrant from that Mexican state traveled to the U.S. city (ascertaining how many of each Mexican state's Mexican migrants would be expected based on pre-1980 networks to travel to the U.S. city in question), and then sum these predicted immigrant pools from each Mexican state to arrive at the total predicted influx of Mexican migrants to the city.

In practice, I compute the average size of a U.S. city's eligible migrant pool over each decade of the analysis (1980-90 and 1990-00). The average eligible migrant pool is the sum of the 18-50 year lagged birth cohort sizes in each of the city's network-linked Mexican state in each year of the decade in question, divided by ten. As an example, to predict the change in the Mexican migrant share of Tuscon between 1980 and 1981, the birth cohorts instrument would be the sum of the birth cohort sizes in each Mexican state linked with Tuscon between 1930 and 1963. Each of those linked Mexican state birth cohort components of the sum would be weighted by the pre-1980 propensity of migrants born in each of the Mexican states to have reported going to Tuscon. The 1981-1982 change would then be predicted by the same formula, only from 1931-1964. Each of these 32 year eligible migrant pool sizes is summed and then divided by ten to compute an average over the decade 1980-1990. The formula is as follows:

$$\Delta M_{c[t-10,t]}^b = \sum_i [(1/10) \sum_{j=t-10}^{j=t} BIRTHS_{i[j-18,j-50]} \times \rho_{ic}] \quad (9)$$

This figure is then scaled by the population size of the U.S. city in 1980 for 1980-1990 changes and in 1990 for 1990-2000 changes.

Advantages and Validation For the present analysis, using the birth cohorts instrument confers at least two advantages over rainfall as used by Levy (2013), Chalfin (forthcoming) and Pugatch and Yang (2011). First, since according to Hanson and McIntosh (2010) variation in Mexican birth cohort sizes accounts for 40% of variation in the level of immigration to the U.S. (a figure that reflects only the higher propensity of members of larger birth cohorts to migrate and not the additional fact that more births mean more potential migrants overall), the local average treatment effect identified by using births should much more closely approximate the overall average treatment effect of immigration than the local average treatment effect of low rainfall-driven migration would. Second, rainfall is a better predictor of year-to-year variation in migration than it is of cumulative changes in migrant shares over a ten year period. Over a long period, aggregate rainfall is likely to approximate its long-run mean and a great many unrelated factors driving migration to drown out its measurable effect. The opposite is true of births. Since the sizes of eligible migrant pools in consecutive years are nearly identical, births are a poor choice to predict short-run variation in migration. However, the sizes of births-driven eligible migrant pools do vary substantially by decade, so lagged births should be expected to predict changes in migration over longer periods. Decadal changes in migration are likely to be cleaner indications than year-to-year fluctuations of how migration influences public policy since policies take time to update and would be expected to respond to substantial and stable shifts.

To be valid, an instrument must satisfy two major conditions. It must be a strong predictor of the endogenous regressor it replaces – in this case variation across cities in changes in Latin American migrant share over the decades 1980-1990 and 1990-2000. We establish the predictive power of both the network and birth cohorts instrument after discussing the sources of

data through which they – and the other variables in the analyses – are constructed. It must also satisfy what is known as the exclusion restriction. This means that the instrument may only be correlated with the outcomes of interest, in this case decadal changes in U.S. cities' public finances, by way of its impact on the endogenous regressor.

We have seen that the network instrument may violate this condition if emigration flows from a country are in part determined by persistent shocks in its linked U.S. destinations that also influence destination city public finances. The birth cohorts instrument remedies this problem by isolating a portion of variation in emigration that is due to lagged births – a push factor. Unless variation in the sizes of Mexican state birth cohorts influences changes in the public finances of their particular destination-linked U.S. cities eighteen to fifty years later through some channel other than immigration, the birth cohorts instrument satisfies the exclusion restriction.

Potential Threats to Validity, Birth Cohorts Instrument

Correlation birth cohort sizes in a Mexican state and among migrants from that state living in network-linked U.S. cities. One channel other than immigration through which lagged Mexican state birth cohort sizes may be correlated with changes in linked U.S. cities' public finances: lagged birth cohort sizes in Mexican states are associated with lagged birth cohort sizes among Mexican mothers who have already migrated to linked U.S. cities. Corresponding changes in the second generation Mexican population share may therefore be associated with changes in the Mexican migrant population share and influences apparently attributable to immigration may in fact be a result of changes in the size of the native-born Mexican population. While clearly an important question in its own right, the impact of growth of the native-born Mexican population is beyond the capability of the present empirical strategies to address.⁹ Consequently, I control in all analyses, as much as available data permit (see the next section for details of the procedure), for the sizes of lagged U.S.-born birth cohorts of Mexican descent.

Anticipation. Anticipation could introduce endogeneity into estimates derived from the birth cohorts instrument in two ways. One is that networks could have formed in part because migrants anticipated future changes in U.S. cities' public goods spending or some other factor that influences public goods spending and selected into those cities in response to their anticipation. Subsequent immigrants might have followed this path, leading to a spurious correlation between immigration and public goods spending. In a simple example, if Mexican migrants in, say, 1970, were attracted to a city because they accurately predicted enduring growth in its provision of public goods, the correlation between immigration and public goods provision would be positive even if immigration itself had no effect.

Such selection based upon anticipation seems unlikely in part because it appears to rest on implausible prescience among a large group of migrants. Studies have indicated that

⁹ Theories we have invoked to connect Mexican immigrant population shares to public goods provision are not as easily transferrable to U.S. native population of Mexican ancestry as one might intuitively expect. Theories grounded in high rates of non-citizenship among low income voters would for one predict sharply different effects. U.S. natives of Mexican descent are highly assimilated (Citrin et al. 2007) and are of course all citizens, meaning that for many Americans they might be viewed as entitled to publicly provided goods as anyone else. On the other side, racial antipathy may still lead whites and blacks to disdain sharing these goods with Hispanics, and Alesina, Baqir, and Easterly's (1999) theory grounded in between-group preference heterogeneity might still hold.

migration networks often convey inaccurate information about *current* economic conditions (McKenzie, Gibson, and Stillman 2007) in the destination let alone conditions substantially into the future. It is also inconsistent with the nature of migration patterns prior to the period of study (Massey et al. 2002). The great majority of migrants during this period sojourned in the U.S. rather than settled there, so even if they could have anticipated future changes in economic conditions it is unlikely that they would have based their decisions about where to locate on these predictions. It is unclear why migrants from different states would have developed such diverse expectations about which U.S. metro areas were the best bet for future growth.

Persistence of Conditions Drawing Migrants Even without accurate anticipation of future economic trends in destination cities, migrants may behave as though they had such insight. This could occur if migrants are drawn to growing locales and growth is correlated over time. Such a possibility is the heart of Pugatch and Yang's (2011) critique of Altonji and Card's (1991) network instrument. However, because the birth cohorts instrument uses the birth-determined eligible supply of potential migrants rather than the actual size of the emigrant pool, such anticipation would, if anything, pose a threat to external but not internal validity. Even if migration networks are a function of economic conditions, there is no endogeneity in the causal estimates derived from the birth cohorts instrument unless the size of the eligible migrant pool (determined by the number of births in each Mexican state 18-50 years before) is also a function of current economic conditions. This would entail on average accurate anticipation by Mexican mothers of economic conditions in network-linked U.S. destinations decades into the future and tailoring of their fertility-related and childcare-related decisions to these expectations.

Such anticipation seems very unlikely, but I acknowledge at least one way it could have come about. If migrants traveled prior to the period of study to U.S. destinations experiencing economic growth, and if migrants from these locales set higher levels of remittances that encouraged women in Mexico at the time to bear more children or enabled them to care effectively for their children, and if economic conditions in these cities persisted over time and were correlated positively with public goods provision, the birth cohorts instrument would produce a spuriously positive estimate of the effect of immigration from Mexico on public goods provision in the U.S. This could potentially be addressed using remittances data from before the period of study, but I have not yet gathered these data.

It is also important to acknowledge that the birth cohorts instrument, like all instruments, estimates a local average treatment effect, not the average treatment effect of immigration from Mexico generally. The common tradeoff between internal and external validity in social science research applies. Given that the mechanism tapped by the birth cohorts instrument applies to a large share of the total Mexico-U.S. migration flow during the period of study, this concern is arguably less worrisome here than in other work using instrumental variables methods. However, if migrant networks developed as a result of migrants' response to current conditions in U.S. destinations, and if those conditions persisted over time, we would be estimating the causal effect of a portion of the immigration from Mexico that traveled to destinations in the U.S. that were experiencing higher than average economic growth. This possibility cannot be ruled out and would limit the generalizability of the instrumental variables estimates, though it would still provide an informative estimate of what impact a large portion of migration from Mexico has had on local public goods provision in the U.S.

Construction of Instruments and First Stage

The network instrument was constructed entirely from Census microdata made public by the Minnesota Population Center's Integrated Public Use Microdata Series (IPUMS). Recall that it assumes that migrants from each country exhibit stable patterns of settlement across U.S. cities; that is, migrants travel to the same set of U.S. destinations that earlier compatriots did. The share of migrants from Mexico residing in each MSA was computed for 1980 and 1990 using a variable that identifies birth country, and from this I derive the net numeric influx of migrants from Mexico during the period 1980-1990 and 1990-2000. Multiplying the net influx from 1980-1990 (1990-2000) from Mexico by the share of migrants from Mexico residing in a given MSA in 1980 (1990) predicts the total migrant flow from Mexico to the MSA from 1980 to 1990 (1990 to 2000) in way that is presumably purged of variation induced by changing destination MSA conditions. Dividing by the total MSA population in 1980 (1990) scales the instrument so that it can be interpreted as the predicted increase in the Mexican foreign-born population share in the MSA.

Data used in the creation of migration network weights linking Mexican states to U.S. metro areas come from the Mexican Migration Project (MMP), an ongoing research endeavor of the Office of Population Research at Princeton University that interviews residents of major Mexican migrant sending communities about their own and their family members' sojourns in the United States. Surveys date back to 1982, and while self-reported trips range as far back as the early years of the Twentieth Century the bulk of reported trips were – reflecting broader immigration patterns – from 1960 onward. Though, as Hanson (2006) has pointed out, the MMP is not a representative sample of U.S.-bound Mexican migrants, it is the largest available source of data linking Mexican source regions to U.S. destinations.¹⁰ The MMP reports migrants' Mexican state of birth as well as the metro area to which they journeyed on each trip. Approximately 77% of the MMP sample traveled to an identifiable U.S. MSA. After dropping migrants who did not travel to an MSA, I created a matrix of migration linkages (weights) in which each entry reflects the share of all migrants born in a particular Mexican state who traveled to a given U.S. MSA. Critically, Table 2.1 shows that different U.S. metro areas have drawn migrants from different sets of Mexican states, meaning that fertility shocks in each state can be predicted to influence growth in the migrant share in different U.S. cities.

¹⁰ The 2006 Latino National Survey samples over 3,000 foreign-born Mexicans across a sample of metro areas comprising over 80% of the U.S. Latino population and asks respondents' Mexican state of origin. While a useful robustness check on the MMP weights, the late date of the LNS data precludes creation of migration network weights that are pre-determined with respect to the period of study, which would raise questions as to their exogeneity to contemporaneous conditions in U.S. cities. The Mexican Government's Encuesta Sobre Migracion en la Frontera Norte de Mexico also asks migrants their Mexican state of origin and U.S. destination, but while U.S. states are identified in their data, few metro areas are. Mexican-U.S. regional migration linkages can also be estimated from data the Mexican government has collected on hundreds of thousands of applicants for Matricula Consular identification cards in the U.S. Since offices exist in most of the largest U.S. Mexican migrant receiving cities and there are data on migrants' states of origin, very reliable linkages can be inferred from these data. However, the data date back only to 2006, again too late for the present study.

Table 2.1: Top 5 Mexican State Sources for Migrants, 15 Largest U.S. MSAs

MSA	State	Source #1	Source #2	Source #3	Source #4	Source #5
Atlanta	GA	Veracruz (16)	Durango (16)	Guanajuato (16)	Guerrero (12)	Nuevo Leon (12)
Chicago-Gary-Lake	IL	Guanajuato (21)	Jalisco (15)	Durango (14)	Mexico (11)	Michoacan (10)
Dallas-Fort Worth	TX	Guanajuato (28)	San Luis Potosi (21)	Durango (13)	Jalisco (11)	Chihuahua (6)
Denver-Boulder-Longmont	CO	Yucatan (30)	Chihuahua (23)	Jalisco (10)	Zacatecas (8)	Miscellaneous
Houston-Brazoria	TX	San Luis Potosi (35)	Guanajuato (29)	Zacatecas (5)	Durango (5)	Nuevo Leon (4)
Las Vegas	NV	Hidalgo (23)	Jalisco (23)	Michoacan (16)	Nayarit (9)	Durango/Zacatecas (6)
Los Angeles-Long Beach	CA	Jalisco (24)	Zacatecas (15)	Michoacan (13)	Guanajuato (11)	Oaxaca (4)
Minneapolis-St. Paul	MN	Morelos (100)	---	---	---	---
New York - Northeastern NJ	NY/NJ	Puebla (58)	Morelos (13)	Jalisco (7)	Tlaxcala (5)	Miscellaneous
Philadelphia	PA/NJ	Guanajuato (86)	Michoacan (9)	Miscellaneous	---	---
Phoenix	AZ	Chihuahua (25)	Guanajuato (12)	Jalisco (11)	Michoacan (8)	Durango (7)
Portland-Vancouver	OR/WA	Yucatan (93)	Miscellaneous	---	---	---
San Diego	CA	San Luis Potosi (28)	Jalisco (25)	Baja California Norte (15)	Michoacan (6)	Guanajuato (6)
San Francisco-Oakland	CA	Jalisco (27)	Yucatan (21)	Michoacan (21)	Guanajuato (8)	Zacatecas (6)
San Jose	CA	Jalisco (37)	Michoacan (13)	Veracruz (11)	Nayarit (10)	Guanajuato (7)

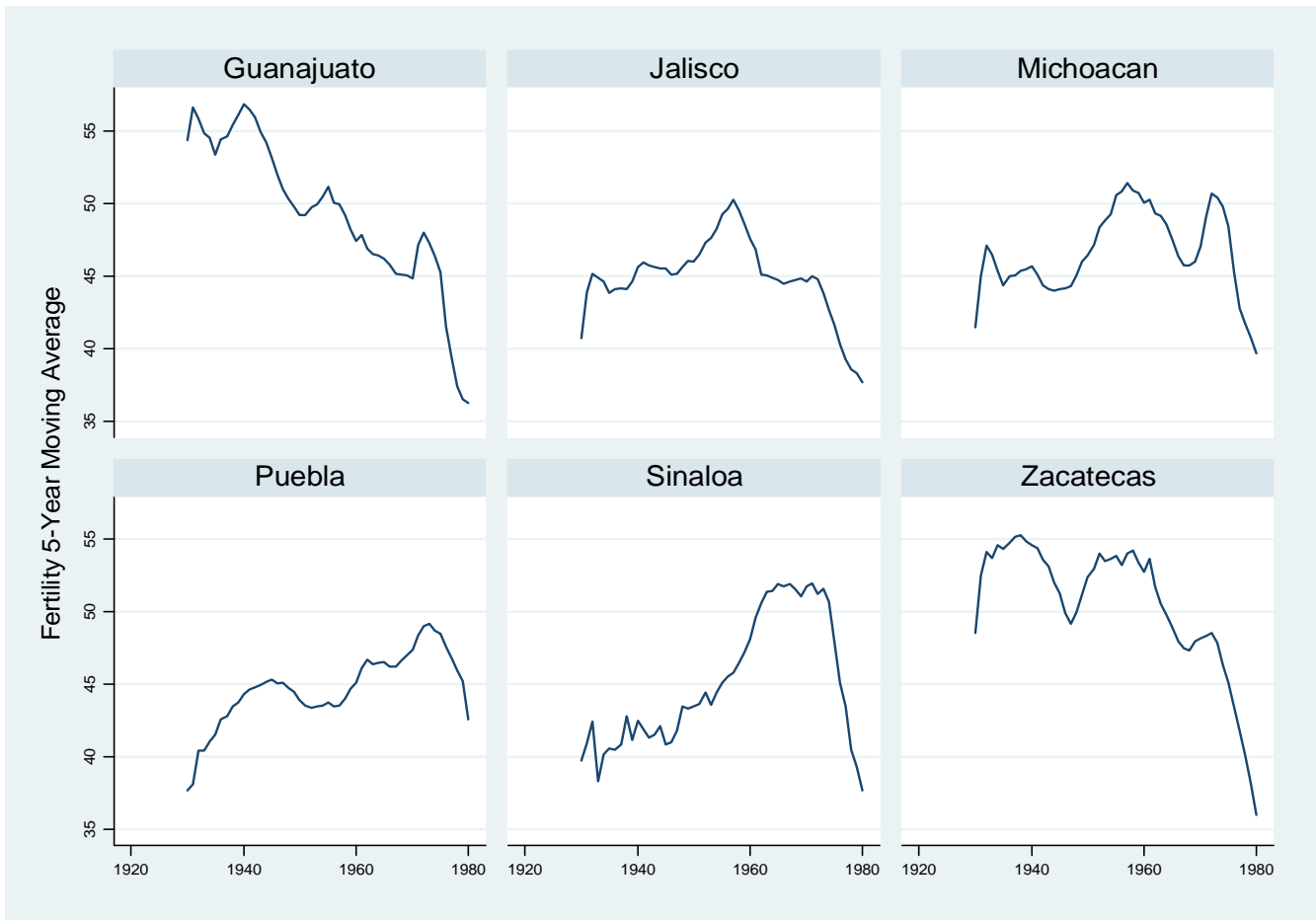
Note: Numbers in parentheses are the percentage of first-journey migrants to each MSA who report having been born in the specified Mexican state.

Source: Author's Compilation of Mexican Migration Project Data.

The sizes of historic Mexican state birth cohorts were extracted from scanned almanacs generated by Mexico's Instituto Nacional de Estadística y Geografía (INEGI), an agency of the Mexican federal government charged with collecting and maintaining a wide range of demographic and economic data. These almanacs, entitled "Anuario Estadístico de los Estados Unidos Mexicanos," report the number of registered births by sex and approximate age at registration in each recent year and in each of the thirty-one Mexican states and the Distrito Federal, were published at inconsistent intervals between 1936 and the present, but many report several years' worth of birth cohort sizes, so that data are available for each year since 1928.

Figure 2.1 demonstrates considerable variation, despite a common national signal, in when different Mexican states experienced the baby booms that propelled migration to historically linked cities in the U.S. The lines shown are five-year moving averages of fertility (births per thousand females) in each state. Each state's pattern is unique, but three basic variants emerge. Four of the states – Guanajuato, Zacatecas, Jalisco, and Michoacán – are among the largest traditional migrant sending states to the U.S. The first two reached the pinnacle of their fertility rates early, with a consistent drop-off thereafter. The second two were steadier but reached their fertility apexes later, in the 1950s and 1960s. The other two states – Puebla and Sinaloa – are not among the traditional migrant sending states but have more recently become active, with Puebla and surrounding states accounting for most of the recent influx of Mexicans into New York City (Smith 2006). Consistent with our framework, fertility in these states remained low into the early 1960s, after which it rose steadily for approximately a decade.

Figure 1: Fertility Trends in Selected Mexican States



Source: Author's compilation of data in INEGI annual statistical almanacs, 1936-1990.

Note: Each chart shows the five year moving average of the number of registered births in each Mexican state per 1,000 women in each year. The key observation is that fertility peaked in different states at different times, leading to variation in the timing of emigration shocks from these states.

The birth cohorts instrument was constructed from a combination of MMP and INEGI data. Since prime migration age has tended to be 18-50 years old (Hanson and McIntosh 2008), the potential emigrant pool from each Mexican state was computed as the average size of the sum of birth cohorts lagged eighteen to fifty years. For example, the size of the eligible emigrant pool in the Mexican state Zacatecas in 1980 is the sum of Zacatecan birth cohort sizes from 1930 to 1964, for 1981 the sum of birth cohort sizes between 1931 and 1965 and so on. The average Zacatecan potential emigrant pool over the decade 1980 to 1990 would be the sum of the potential emigrant pools in each year 1980-1989 divided by ten. Eligible migrants from each state are then assigned to U.S. cities according to the migration network weights derived from MMP data. In other words, the birth cohorts instrument assigns cities whatever share of Zacatecas' potential emigrant pool it had received of all Zacatecan U.S.-bound migrants prior to 1980.

To be valid, the network and birth cohorts instruments must be potent predictors of decadal changes in U.S. metro areas' Latino immigrant population shares. Moreover, they must be predictive even when time trends in the data are controlled with year fixed-effects. Table 2.2 shows first stage regression results for each instrument. The network instrument has been repeatedly shown in the economics literature to be a powerful predictor of immigration flows, but for completeness, I show the first-stage results for it as well. The critical metric is the F-Statistic on the excluded instrument, and, at 27.8 and 40.5 for the births and network instruments respectively, these easily exceed the Stock-Yogo (2005) threshold for a strong instrument (16.4).

Table 2.2: First-Stage Regressions Predicting MSA-Level Change in % Mexican Foreign-Born

	(1) Network Instrument	(2) Birth Cohorts Instrument
Network Instrument	0.0445 ^{***} (0.0070)	
Birth Cohorts Instrument		0.0271 ^{***} (0.0052)
Lagged U.S.-Born Mexican Births Cohorts	0.0052 (0.0111)	0.0426 ^{***} (0.0128)
Year 2000 Dummy	-0.5741 ^{***} (0.0996)	-0.7840 ^{***} (0.1218)
Constant	0.8433 ^{***} (0.0903)	1.0252 ^{***} (0.1043)
<i>N</i>	400	400
F-Statistic For Excluded Instrument	40.5	27.8
adj. <i>R</i> ²	0.557	0.380

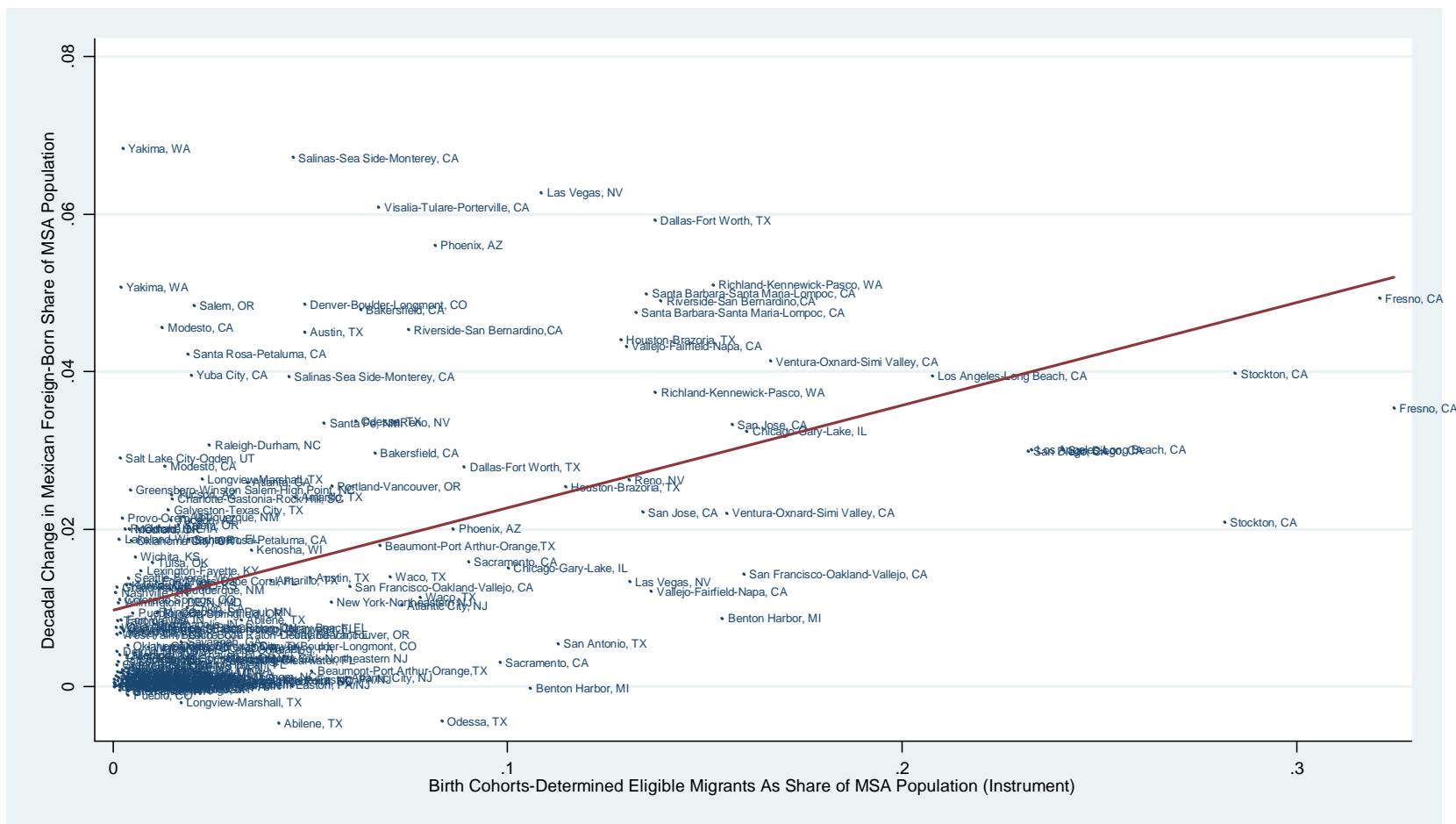
Note: Heteroskedasticity-corrected standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Sources: Author's compilation of IPUMS-USA microdata for Census 1980, 1990, and 2000; Mexican Migration Project survey data; INEGI Anuarios Estadísticos 1936-1990.

Because in some analyses I control for measures of intergovernmental revenue per capita and per million dollars of personal income, it is fortunate also that both instruments remain predictive even with this control ($F=27.1$ and $F=39.4$, again for the births and network instruments respectively). Further analysis (not shown) corroborates the assertion that variation in the timing of fertility shocks in Mexican migrant-sending states underlies the validity of the birth cohorts instrument. Substituting the mean 18-50 years lagged fertility rate in Mexican states for the mean 18-50 years lagged birth cohort size generates predictive power that is statistically indistinguishable from the first-stage regression shown in Model 2. Finally, as a graphical illustration of the first-stage for the birth cohorts instrument (Figure 2.2), the simple bivariate relationship of the weighted average size of lagged birth cohorts in each U.S. MSA's network-linked Mexican states and the decadal change in each MSA's Mexican immigrant population share assuages concerns that outliers are producing an idiosyncratically significant result.

Figure 2: Illustration of First Stage – Birth Cohorts Instrument: Eligible Migrants vs. Actual Migration



Source: Author's compilation of U.S. Census Bureau IPUMS Microdata, Mexican Migration Project Data, and data in INEGI annual statistical almanacs, 1936-1990.

CHAPTER 3: THE EFFECT OF IMMIGRATION FROM MEXICO ON LOCAL PUBLIC PROVISION OF GOODS AND SERVICES IN THE UNITED STATES, 1980-2000

This chapter implements the research designs developed in chapter 2 to estimate the effect of immigration from Mexico on local government expenditure overall and on public goods, taxation, and debt in the United States between 1980 and 2000. During this period the Mexican immigrant population of the U.S. increased from 2.2 million to 9.2 million. The primary question this chapter addresses is whether this massive increase caused declines in local governments' provision of non-excludable goods and services. Its approach is to look across metro areas, counties, and states at the association between exogenous local influxes of Mexican immigration and measures of fiscal policy. I do this in two ways. First, I adopt the cross-sectional approach emphasized by Alesina, Baqir, and Easterly, beginning by replicating and updating their results for 1990 by including 1980 and 2000 Census data. I then disaggregate their measure of ethnic diversity to observe the distinct effects of the size of localities' black populations, native-born Hispanic populations, Mexican-born populations, respectively. Second, I test the effect of Mexican-born population share on the outcomes in question longitudinally using ten-year differenced measures and applying the instrumental variables described in Chapter 2 for the change in the Mexican population share.

The chapter proceeds as follows: (1) I begin by briefly articulating the main hypotheses I test. (2) I then describe in detail the measures of independent and dependent variables that I use. (3) A discussion of issues relating to unit of analysis follows. (4) I present the findings, robustness checks, and (5) provide a concluding summary.

HYPOTHESES

The four theories I have invoked to link immigration from Mexico to public goods provision in the United States converge on the following two predictions: (1) Immigration brings about lower levels of taxation – and of progressive taxation especially. (2) Influxes of immigrants reduce local government spending on public goods and services. (I will describe below which spending categories are regarded as public goods and which as targeted spending.) Some research also suggests the recent unexpected growth rather than the level of immigration is a key source of threat (Hopkins 2010; Newman 2013). People in longstanding immigrant destinations may get accustomed to large numbers of immigrants living in their midst, but those living in areas that have not previously confronted large-scale immigrant influxes may be attuned to a sudden appearance of immigrants, even if the immigrant share of the population is small. Thus I also test whether hypotheses 1 and 2 are valid when one substitutes recent growth in the Mexican immigrant population share for its level.

I have noted, however, that the theories supporting these hypotheses make several inferential leaps. One leap pertains to the assumption that immigration will influence public opinion in the manner that hypotheses 1 and 2 require. If the link between immigration and public goods provision is insufficiently salient, possibly because public goods spending may not evoke considerations of immigration in the way that welfare spending tends to evoke race, hypotheses 1 and 2 might not be strongly confirmed. People also may not routinely link immigrants' use of public goods with their own tax burdens. These possibilities work against the expectation that Mexican immigration will have a strong and one-sided impact on natives'

preferences over the provision of public goods and taxation and might lead us to retain the null hypothesis: (3) No negative effect of immigration on taxation and public goods spending.

Other leaps call into question the presumption that effects on public attitudes toward local fiscal policy will translate into policy change. Null results or even results opposite the predictions of hypotheses 1 and 2 may also emerge if immigrants themselves prefer extensive provision of public goods and increases in tax revenues and if local governments heed those preferences. This would counteract pressure from natives for retrenchment. Since those most opposed to immigration tend to be less educated and politically involved, it is possible as well that immigration's impact on public attitudes toward government spending and taxation is concentrated among those whose attitudes are seldom politically influential. Finally, localities may be unable to yield to public pressure for reductions in public goods spending because spending less on education and infrastructure would put them at a disadvantage relative to other cities in the competition to recruit and retain a strong employer base.

If immigration does not lower public spending but renders it more difficult to meet expenditure needs by levying taxes, localities might opt instead to take on more debt. Rugh and Trounstein (2011) find that politicians can structure multi-item public bond votes in a way that maintains funding levels while also passing political muster – fewer votes on bonds of higher amounts circumvent an otherwise corrosive effect of ethnic diversity on public goods provision. This leads to the hypothesis that immigration will have (4) a positive effect on accrued public debt. This is consistent with Alesina, Baqir, and Easterly's finding that debt is higher in diverse locales, a result they attribute to fiscal stress induced by disagreement over the sources of public funds and rejection of taxes. To what extent increases in debt are actually a sign of fiscal stress can be gauged in part by examining whether immigration influences the effective interest rate on debt. Higher interest rates would suggest that localities are borrowing because they cannot meet funding needs even if it requires taking on expensive debt. No effect on interest rates would suggest that debt is an economically expedient recourse.

To summarize, the null hypothesis is that immigration from Mexico has no impact on local fiscal outcomes. The alternative hypotheses are that it has decreased spending on public goods, decreased taxes – and especially progressive taxes, and increased public debt. The first two emerge directly from the four theories described in chapter 1 linking immigration from Mexico to fiscal outcomes. The expectation with respect to debt emerges from the possibility that politicians will strategically substitute debt for taxes as a way of funding the provision of public goods when anti-tax sentiment increases among their constituents.

DATA AND MEASURES

All data used in the construction of the independent variables comes from 1980, 1990, and 2000 U.S. Census microdata provided by IPUMS-USA. All measures were computed from these microdata by the author. In all analyses, the key independent variable is the percentage of a locality's population that is Mexican foreign-born. The Mexican immigrant population share of each MSA (*Mexicanfb*) is constructed using the Birthplace variable provided in IPUMS data and is simply the size of the population reporting Mexican birth divided by the total population size. I instrument for this endogenous regressor using the techniques developed in Chapter 2.

To compare these results to Alesina, Baqir and Easterly's, I also replicated their construction of a variable called *Diversity*, which is $1 - \sum_i RACE_i^2$, where i indexes five racial categories (white, black, Asian and Pacific Islander, American Indian and Alaskan Native, and Other) and $RACE_i$ is the share of the MSA population that identifies as each race. As in Alesina,

Baqir, and Easterly's analysis, since there are five racial groups, *Diversity* has a theoretical maximum of 0.8 and a minimum of 0, but I rescale it to run from 0 to 100 for comparability with the other demographic independent variables, most of which are expressed as percentage shares.¹¹

Mexicanfb and *Diversity* are substantially correlated across city-year observations, at .46. However, the correlation of diversity with the city percent black, *Black*, is significantly higher, .74. Intuitively, a higher minority share of both types leads to greater diversity overall. It is also not surprising that the black population would be more strongly correlated with the diversity measure because it is larger and more widely dispersed across cities than the Mexican foreign-born population, meaning that a higher level of *Diversity* will more often be explained by a higher black population share rather than a higher Mexican-born population share.

Interestingly, *Mexicanfb* and *Black* are significantly but weakly negatively correlated across observations at -.11, meaning that areas with a higher black population share have a lower Mexican foreign-born population share. Attempting to explain this correlation is beyond the scope of the dissertation, but the weak negative correlation means that the diversity measure Alesina, Baqir, and Easterly (1999) and others have used conflates quite different types of ethnic diversity. Some areas' diversity stems from Mexican immigration, others' from a large black population. This motivates the question of whether the cross-sectional relationship Alesina, Baqir, and Easterly find between diversity and public spending is due to immigrant shares, black population shares, or both.

When we look at the correlation between changes in *Diversity* and changes in the black and Mexican-born population shares, however, a different picture emerges. Changes in *Mexicanfb* are highly correlated with changes in *Diversity*, at .62, while changes in *Black* are less strongly related to changes in *Diversity*, at .29. This essentially means that much more of localities' increases in diversity over time is accounted for by growth in their Mexican-born populations than by changes in the size of their black populations.

The difference from the cross-sectional correlations of *Black* and *Mexicanfb* with *Diversity* is notable: cross-sectional variation is more closely tied to variation in the black population share than in the Mexican-born population share whereas longitudinal variation is more closely tied to the Mexican-born population share than the black population share. This may help explain generally weaker effects of *Diversity* in longitudinal than in cross-sectional analyses. The strong cross-sectional relationships Alesina, Baqir, and Easterly find may come closer to tapping the effects – contemporary or historical – of large black populations. Weaker or null findings in their own longitudinal analysis and that of Boustan et al. (2010) and Hopkins (2011) may be partly attributable to the different composition of levels and changes in ethnic diversity. Descriptive statistics for all independent variables used in the analyses are displayed in Table 3.1.

¹¹ Though their claims refer to “ethnic diversity” throughout their article, Alesina, Baqir, and Easterly rely only on race classifications in constructing their diversity variable, ETHNIC. They note a high correlation between the racial category “other” and Hispanic identification, though only about half of Hispanics identified in that category. I have preserved their operationalization for comparability.

Table 3.1
Descriptive Statistics for Independent Variables

	1980		1990		2000	
	Mean	SD	Mean	SD	Mean	SD
Population (Thousands)	2,548	2,743	2,496	2,659	2,834	2,900
Mean Income (2012 \$)	63,478	6,353	74,756	11,103	84,186	13,471
Median Income (2012 \$)	56,585	6,822	63,229	10,275	64,649	10,828
Mexican Foreign Born	0.01	0.02	0.02	0.04	0.04	0.05
Foreign-Born	0.08	0.06	0.11	0.09	0.14	0.10
Black Native-Born	0.13	0.08	0.13	0.09	0.12	0.09
Hispanic Native-Born	0.05	0.05	0.06	0.06	0.08	0.07
Asian Native-Born	0.01	0.03	0.01	0.04	0.01	0.03
Unemployment Rate	0.06	0.02	0.06	0.02	0.06	0.02
Employment Rate	0.70	0.04	0.74	0.04	0.73	0.04
Poverty Rate	0.13	0.03	0.14	0.04	0.14	0.04
School Age Population Share	0.23	0.02	0.19	0.02	0.20	0.02
Percentage Adults >25 W/Coll Degrees	0.12	0.03	0.15	0.04	0.18	0.04
White	0.83	0.09	0.77	0.12	0.72	0.13
Black Native-Born	0.13	0.09	0.14	0.09	0.14	0.09
Native American	0.00	0.01	0.01	0.01	0.01	0.01
Asian	0.02	0.05	0.04	0.06	0.05	0.06
Other Race	0.00	0.00	0.05	0.06	0.09	0.08
Diversity	33.3	14.7	44.1	18.6	52.1	18.1

Note: All computations are based on a population-weighted sample of Minimum 200 Metropolitan Statistical Areas identifiable in IPUMS U.S. Census microdata in each year.

Source: Author's calculations from IPUMS U.S. Census microdata.

Dependent Variables Data on government finances, which constitute the dependent variables in the analysis, come from the 1982, 1992, and 2002 U.S. Census of Governments, which collects a variety of fiscal data from over 80,000 recognized subnational governments in the U.S. each five years. All measures were computed by the author from the Census of Governments microdata, downloaded from the Census website. Government types include counties, municipalities and townships (general purpose governments), as well as school districts and special district governments (special purpose governments). The responsibilities and scope of these types vary greatly across states and even within states, so it is necessary to aggregate taxes, spending, and debt from all governments operating within a county. Very few governments cross county lines, though there are several that covered a multi-county metropolitan region. Census of Governments data does not identify MSAs, so these were assigned (based on 1980 definitions) using a crosswalk provided by the Census Bureau.

Key dependent variables are total own-source revenue (receipts), total direct expenditures, and total debt. I also look separately at effects on sales tax receipts, property tax receipts, and income tax receipts, as well as residual categories consisting of various types of fees. Income taxes are the most progressive of these forms of tax, though only a few of the localities I analyze have them. Property taxes are generally more progressive than sales taxes since taxes on consumption are shared by residents at all income levels whereas property taxes fall on the wealthier set of homeowners.

When it comes to spending, it is essential to separate out categories reflecting use for the production of public goods and those from which immigrants can be excluded or are essentially private goods allocated on a fee basis. Examples of the latter are welfare spending, especially after the Clinton-era reforms, a variety of potential patronage categories, and utilities. Expenditure categories I classify as public goods are education, health and hospitals, police, corrections, judicial costs, fire, roads, transit and highways, parks, police, inspection, sewers, waste management, libraries, and parking. However, I also create a smaller measure of “core public goods” that excludes spending on corrections, police, judicial costs, and inspection. In practice these are a small percentage of total public goods spending anyway, but they are distinct because they could be components of an adverse reaction to immigration that emphasizes stricter law and order. Accordingly, Alesina, Baqir, and Easterly (1999) and Hopkins (2011) find positive effects of diversity on police expenditures. I also use a second core public goods measure that excludes health and hospital costs, since these goods may be privately rather than publicly allocated, and since immigrants can be excluded in the United States from health services other than emergency care. Examining each of these expenditure categories separately helps ensure that the results are not unique to a single category or highly divergent across public goods categories. Within the category of debt, I explore both long-term debt and short-term debt.

I analyze all revenue categories per capita and as a share of the total personal income. Direct spending measures are constructed per capita (though in the case of education spending, per school-aged child), as a share of total personal income, and as a percentage of total expenditures. Previous research has examined the impact of diversity on these categories measured as the share of total expenditures (Alesina, Baqir, and Easterly 1999) and expenditures per capita (Boustan et al. 2010 and also Alesina, Baqir, and Easterly 1999). In fact all three constructions of the dependent variables are potentially relevant indicators of the effects of immigration on redistribution and public goods provision. A category’s share of total spending

gives a sense of its budgetary priority relative to other budget items. The amount spent on a category relative to a locality's total income may be understood as a measure of "effort" relative to fiscal means or capacity. However, prioritization may simply reflect local need fostered by immigration-fueled population or labor market growth. In the case of immigration increases the size of school age populations, spending on education may rise mechanically as a share of total spending but nevertheless fail to keep pace with increased need. Descriptive statistics for aggregate fiscal measures per capita are shown in Table 3.2.

Table 3.2
Aggregated Outcome Fiscal Measures Per Capita

	1980		1990		2000	
	Mean	SD	Mean	SD	Mean	SD
Own-Source Revenue	1,937	831	2,823	1,195	2,849	1,251
Taxes	982	663	1,432	913	1,477	933
Property Taxes	793	441	1,155	605	1,155	582
Sales Taxes	149	133	227	194	269	220
Income Taxes	40	89	50	115	53	132
Direct Expenditure	3,039	1,289	4,231	1,719	4,695	2,038
Public Goods Spending	2,360	996	3,296	1,304	3,678	1,552
Debt	2,505	1,616	4,077	2,702	4,505	2,444
Long Term Debt	2,344	1,564	3,956	2,673	4,414	2,428

Note: Shows measures of fiscal dependent variables per capita for sample of metropolitan statistical areas identified in all years of IPUMS Census data. All measures are aggregated to the level of the MSA from reports based on each of the individual subnational governments in the United States.

Source: Author's compilation of data from Census of Governments 1982, 1992, and 2002.

Unit of Analysis The unit used in the main analysis is the Metropolitan Statistical Area (MSA), an aggregation of counties that are economically and otherwise integrated with an urban center. MSAs were identified using IPUMS designations and then adjusted to consistent 1980 Census definitions where possible by adding or subtracting people in counties that had been dropped or added to the 1980 designations. Only MSAs with a population of at least 100,000 are identified in the microdata, and New England MSAs are excluded from the analysis because of their boundaries are not defined by counties, making aggregation of Census of Governments data complex. Although this obviously delimits the generalizability of the results to the rest of the country, it is also the same approach taken by Alesina, Baqir, and Easterly and therefore potentially boosts comparability between my analysis and theirs. In effect, there are a minimum of 200 MSAs in the analyses.

The MSA is selected because it is the smallest unit available for which I could obtain the data necessary to construct the birth cohorts instrument. Given that MSAs are constructed so as to define a socially and economically integrated area, it is arguably a reasonable choice of unit because increases in the immigrant share of an MSA are likely to be observed in local news, felt economically, and perceived in the course of one's daily life. There are of course reasons to prefer smaller units, and neighborhoods defined by units as small as Census tracts are often preferred in studies of contextual effects on public opinion. In this case, such units would be wholly inappropriate because they are far smaller than any of the jurisdictions involved in generating the dependent variables (cf. Trounstine, n.d.). Such small units are also a questionable choice because they may lead to violations of the Stable Unit Treatment Value Assumption if people are affected by the presence of immigrants in adjacent neighborhoods. However, it is potentially useful to examine the robustness of the results here to analyses at the county and state levels.

The analysis begins with a replication, extension and disaggregation of Alesina, Baqir, and Easterly's study of the relationship between ethnic diversity and spending on public goods in 1990. This part of the analysis uses ordinary least squares regression for comparability with previous research. I replicate the results for 1990, test whether they hold also in 1980 and 2000, and then break diversity into its largest racial and ethnic group components to determine whether blacks, Latino natives, or Mexican foreign-born are driving the relationships Alesina, Baqir, and Easterly (1999) find. I then implement the instrumental variables strategy discussed in detail in Chapter 2 to assess the causal effect of Mexican immigration on public goods spending, taxation, and public debt.

RESULTS

Cross-sectional Replication and Disaggregation I begin by replicating, updating to 2000 Census data and supplementing with 1980 Census data, and extending Alesina, Baqir, and Easterly's cross-sectional analysis of diversity and public spending. Column 1 of Table 3.3 shows coefficients and standard errors clustered by metro area for their measure of *Diversity* in regressions predicting the key measures of public spending. Columns 2-4 replace *Diversity* with measures of immigrant and native ethnic group shares. Note that columns 2-4 show coefficients drawn from a single multivariate regression model while the *Diversity* coefficient in Column 1 is drawn from a separate regression. In the interest of space, full regression results are not presented here but are available upon request. All regressions control for population size, poverty rate, percent sixty-five or older, percent between five and eighteen years old, percent with a college degree, mean and median income as well as the ratio of mean to median income (a measure of income inequality), and the unemployment rate. In addition to the ethnic group shares shown, each regression generating columns 2-4 controls for the non-Mexican population share and the Asian population share.

Table 3.3: Cross-Metro Area Associations between Taxes, Expenditures, and Debt and Ethnic Composition, Census: 1980, 1990, and 2000

	(1) Diversity	(2) Group Shares		
		Mexicanfb	Black	Hispanic Native
A. Share				
Highways/Roads	-0.0186 (0.0123)	-0.0219 (0.0418)	-0.0327** (0.0160)	-0.0500* (0.0302)
Education	-0.1350** (0.0562)	-0.3182 (0.2382)	-0.1229 (0.0963)	-0.1853 (0.1298)
Police	0.0395*** (0.0089)	0.0801* (0.0455)	0.0353*** (0.0119)	0.0314 (0.0213)
Health/Hospitals	0.1430*** (0.0491)	0.1667 (0.2625)	0.1985** (0.0824)	0.1509 (0.1527)
Fire	0.0144** (0.0068)	0.0453 (0.0293)	0.006 (0.0097)	-0.0171 (0.0166)
B. Per Capita⁽¹⁾				
Property Taxes	-3.3757 (2.3885)	-3.7314 (13.8164)	-5.5595 (3.4276)	9.5420 (6.8098)
Sales Taxes	1.3959 (1.2832)	1.6433 (6.7889)	2.1571 (2.1164)	0.4188 (3.3392)
Core Public Goods Expenditures	2.8782 (5.4473)	89.8110** (38.0260)	0.1317 (8.8711)	-11.5273 (10.5638)
Highways/Roads	-0.5890 (0.6869)	0.8194 (4.1334)	-1.8541** (0.8612)	-2.1993* (1.3254)
Education	-26.1496* (15.3349)	28.4944 (86.1582)	-56.8842** (23.0124)	-12.8973 (30.2078)
Police	1.9105*** (0.4792)	6.7945** (2.7891)	1.0758 (0.6959)	1.2225 (0.8039)
Health/Hospitals	7.7378*** (2.6889)	19.3914 (18.7814)	10.2229** (4.5303)	5.7714 (6.7214)
Fire	0.7261*** (0.2673)	3.6868** (1.5081)	0.1824 (0.4024)	-0.8817 (0.6640)
Debt	21.0873 (13.4234)	43.3834 (72.2122)	24.8948 (19.3012)	64.4183 (41.8426)
C. Per \$1M (2010) in Personal Income				
Property Taxes	-4.7403 (4.3301)	59.4749** (23.7293)	-0.6382 (0.5212)	0.0103 (0.1021)
Sales Taxes	2.2201 (2.3800)	25.8362** (10.1364)	0.1760 (0.3590)	-3.7738 (4.0293)

Core Public Goods Expenditures	0.6380 (0.9340)	19.9317*** (5.3978)	-4.9490 (7.1080)	-4.3190** (1.9940)
Highways/Roads	-12.1105 (8.8093)	18.8244 (51.1954)	-27.5385** (11.1263)	-33.9614* (19.4381)
Education	-81.7842* (42.4900)	138.3522 (217.3471)	-183.3295*** (64.8663)	-60.5823 (88.0405)
Police	25.1761*** (6.1340)	90.3519*** (32.7340)	14.4645 (9.0015)	18.0532* (10.7372)
Health/Hospitals	108.1348*** (38.6602)	238.8813 (247.8932)	144.2442** (64.9893)	91.0423 (99.3672)
Fire	8.8562** (3.6254)	49.1188*** (18.8551)	1.238 (5.4537)	-11.7648 (9.4249)
Debt	0.0279 (0.0191)	22.1596 (13.5892)	0.0013 (0.0030)	0.0036 (0.0079)

Minimum N=600

(1) Education per capita is measured as spending per school-aged resident (5-18)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Author's Compilation of IPUMS-USA Microdata, Census 2000

In general, the results for *Diversity* closely resemble those Alesina, Baqir, and Easterly present. *Diversity* is negatively correlated across metro areas with spending on roads/highways, and education, though the coefficients for the first of these are not statistically significant. It is positively correlated with spending on police, fire, and health/hospitals. This picture is consistent with the theory that ethnic diversity dampens spending on productive public goods. Though Alesina, Baqir, and Easterly do not make this point explicitly, it is also consistent with the notion that ethnically diverse areas spend more than more homogeneous areas in dealing with the fallout from social ills such as crime, chronic poverty, and disease. The effects are similar regardless of whether one analyzes shares of total spending, spending relative to need or per person, and spending relative to available personal income. They are also robust to the replacement of the primary core public goods expenditure measure with one that excludes health spending. Coefficients are marginally smaller but not close to differing significantly from those displayed. At the same time, the results do not hold consistently for the relationship of diversity with these measures per capita.

Table 3.3 also strongly suggests that different components of ethnic diversity drive the associations with different spending outcomes. As columns 2-4 indicate, the negative results for roads and education results tie most closely to the percentage of a metro area's population that is black. By contrast, the Mexican immigrant population share has no consistent relationship to these categories of spending. This fact calls into question whether disdain for spending public funds on services that benefit immigrant Mexicans plays any role in driving the negative association of ethnic diversity and these spending categories. Since education is among the most strongly redistributive aspects of public spending, the non-relationship with the share of the population that is Mexican immigrant also conflicts with the idea that a concentration non-citizens at the low-end of the income distribution dampens pressure for redistribution. The black population share also appears to drive the positive relationship between health and hospitals

spending and ethnic diversity, while the Mexican foreign born population share is consistently positively related to spending on police and fire.

The main purpose of Table 3.3 is to establish the importance of disaggregating measures of ethnic diversity, since different components of it may manifest different relationships to the outcomes of interest: there is no evidence here of any singular “diversity effect.” In the remainder of this section, we pursue the question of whether there is an immigration effect, and in particular whether immigration from Mexico fosters a consistent pattern of changes in local public finances. The aim of the previous section was to explore whether Alesina, Baqir, and Easterly’s findings concerning the effect of ethnic diversity on local public finances apply to Mexican immigration in the first place. If their hypothesis is taken at face value, we would have expected that in the disaggregated analysis each group’s share of the population would have contributed to the effect they identify for *Diversity* overall. It turns out that there is little in the cross-sectional analysis to support an impact of Mexican immigration in particular. Yet the absence of such an effect in cross-sectional analyses should not be convincing for the reasons articulated in Chapters 1 and 2. It remains possible that the negative correlations between black population share and fiscal outcomes are in some way genuine causal relationships while the self-selection of immigrants into U.S. localities introduces severe bias into the correlations between the Mexican-born population share and local public goods. The next section thus turns to instrumental variables analysis to generate a more credible assessment of whether immigration from Mexico has had the sorts of effects on public finances that hypotheses 1, 2, and 4 in this chapter would lead us to expect.

Analysis of Long-Differences Using OLS and IV To address the problems of confounded correlation and post-treatment bias discussed above, I turn to an analysis of changes over long differences to assess immigration’s effects. Simply put, if immigration from Mexico has changed the distribution or extent of public spending in the manner that the Alesina, Baqir, and Easterly (1999) or McCarty, Poole, and Rosenthal (2006) models would lead us to expect, then exogenous variation across localities in the extent of Mexican immigration they have received should be clearly, consistently, and negatively associated with changes in their public finances.

Table 3.4 presents the results from the critical tests of this hypothesis. In structure it parallels Table 3.3, but now the results presented are for the effect of inter-Census changes in *Mexicanfb* and each measure of public spending. Rather than including time-varying control variables the strategy here is to present OLS results and then to observe whether using either instrument indicates the presence of bias in the OLS estimate. Given that the analysis rests on the claim that the instrument is exogenous with respect to the outcomes of interest, including statistical controls is unnecessary. The idea is that the quasi-random assignment of Mexican immigrants “pushed” as a result of being part of large historic birth cohorts and channeled to a set of U.S. metro areas through networks whose existence predates the period of analysis already ensures that the causal estimates will not be confounded. Moreover, as described in Chapters 1 and 2, introducing statistical controls could generate post-treatment bias: we might control for part of immigration’s effect by including a variable in the equation that mediates the impact of immigration on public finances. In fact, although most results are indistinguishable from zero, instrumental variables estimates are often quite different from OLS, and the direction of the bias is not consistent category to category. This suggests that the selection mechanisms biasing correlations between the Mexican immigrant population share and public finance outcomes are different depending on the outcome in question. Though I do not pursue it here, this finding is intriguing in its own right since it suggests that immigrants may be attracted to localities that

furnish expansive levels of public goods in some categories and deterred from localities whose “fiscal bundles” (Tiebout 1956) emphasize others.

Table 3.4: Long Differences Estimates of the Effect of Immigration from Mexico on Public Spending Categories

	OLS	Network Instrument	Birth Cohorts Instrument
A. Share			
Highways/Roads	0.0452 (0.0439)	0.1062 (0.0752)	0.2125 (0.1354)
Education	0.0442 (0.1799)	0.1177 (0.2575)	0.025 (0.4160)
Police	0.0268 (0.0371)	-0.0394 (0.0559)	-0.0727 (0.0759)
Health/Hospitals	-0.1370 (0.1571)	0.0704 (0.2090)	-0.2192 (0.3216)
Fire	0.0143 (0.0218)	-0.0032 (0.0326)	-0.0316 (0.0456)
B. Per Capita⁽¹⁾			
Highways/Roads	3.4057 (2.6221)	7.8499* (4.5683)	17.6795** (7.1695)
Education	78.8538 (96.1023)	146.6777 (146.8961)	300.3374 (214.3778)
Police	7.4559** (3.0121)	6.0740 (4.1927)	9.4229 (6.8400)
Health/Hospitals	4.1721 (9.6783)	26.0421 (18.6236)	3.209 (17.2709)
Fire	3.2997** (1.5666)	3.2202 (2.3371)	4.6522 (3.5697)
C. Per \$IM (2010) in Personal Income			
Highways/Roads	33.6574 (34.3453)	89.9520 (61.6374)	157.7602* (85.2746)
Education	104.928 (254.7958)	297.1536 (488.0698)	266.8477 (505.3253)
Police	45.5604 (42.1046)	2.4540 (65.3861)	-6.6128 (99.2896)
Health/Hospitals	6.4689 (129.3967)	226.6271 (238.2030)	-138.3715 (244.5386)
Fire	19.7895 (20.7501)	5.3895 (33.3091)	-12.2946 (47.2764)

Minimum N=400

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(1) Education per-capital measured as per pupil spending

Sources: Author's compilation of IPUMS-USA Census 1980-2000 microdata, MMP survey data, INEGI Anuarios Estadísticos 1936-1990, 1982, 1992, and 2002 Census of Governments.

The major theme in these results is that there is no support for the premise that immigration from Mexico has had more than small effects on the distribution of government spending. This is true for instrumented and non-instrumented estimates. Because results using the birth cohorts instrument are the least likely to exhibit endogeneity bias, we focus on the third column.

In Panel A, the coefficients represent the effect of a one percentage point increase in the Mexican population share on the percentage of direct expenditures dedicated to the specified category. Contrary to the notion that immigration would erode the share of government spending dedicated to productive public goods, the estimates for highways/roads and education are both positive, though both are quite small and statistically indistinguishable from zero. However, we can reject (at the .05 level) that a one percentage point increase in *Mexicanfb* induces any more than a .05 percentage point decrease in the share of spending dedicated to highways and roads and more than a .79 percentage point decrease in the share of spending dedicated to education. In every category we can reject even a one percentage point increase or decrease in its share of government expenditures in response to a one percentage point increase in the Mexican immigrant share of the population. To put this in perspective, consider that the mean ten-year increase in the Mexican immigrant population share among all metro areas in the sample is slightly less than two percentage points. Thus we can reject that immigration from Mexico has been responsible for more than a two percentage point shift over a ten year period in either direction in the share of all direct spending dedicated to any of these categories. This is hardly consistent with the notion that Mexican immigration has played a significant role in making local public goods production more difficult to achieve.

Panel B of Table 3.4 shows that the minimal effects of Mexican immigration on public spending apply also to spending relative to population and need. Immigration significantly increases spending per capita on highways and roads and has, if anything, a positive effect on education per pupil. In fact, the effect on education spending per capita (not shown) is significant and positive, but this is in part due to immigration's effect on age distribution. Estimated effects on police, health/hospitals, and fire spending per capita are also positive but insignificant.

If, by spurring industry and increasing productivity and profitability, immigration from Mexico increases metro areas' total personal income, this increased "fiscal capacity" (Peterson 1981) might in turn increase local governments' disposable income and generate increased spending. Our data indicate that this did, in fact, occur. Regressing the change in the log of per capita personal income on the change in the Mexican foreign born population share, using the birth cohorts instrument, indicates that a one percentage point increase in the immigrant share increases personal income by approximately 1.7% ($p=0.02$) over the course of the decade. There is no evidence of a significant boost in the median per capita income in response to immigration, and indeed an analogous births-instrumented regression shows that the mean-to-median ratio increases approximately .7% in response to a one percentage point increase in the immigrant share ($p<.01$).

This increased spending could mask what would otherwise have been negative effects of ethnic diversity on spending on public goods or counteract what would otherwise have been reductions in redistributive pressure brought on by an increase in the ratio of median citizen to mean family income (see, e.g., Meltzer and Richard 1981; McCarty, Poole, and Rosenthal 2006). I speculate that local governments might have gotten richer due to immigration and, though

maintaining earlier levels of spending relative to need, not updated spending on public goods or redistributive projects to keep pace with increased capacity.

Panel C of Table 3.4, which shows the effects of immigration from Mexico on public spending in each category per million dollars of total personal income, helps rule out this possibility. The only statistically significant effect is a positive impact of immigration on spending on roads and highways, again convincing evidence against the notion that immigration-fueled increases in ethnic diversity have compounded the difficulty of providing local public goods. Effects on education spending as a share of personal income are also positively signed, though with large confidence intervals in the instrumented results. Nonetheless, we can reject at the .05 level that a one percentage point increase in the Mexican foreign-born population share decreases education spending by more than \$724 per million dollars of personal income – less than .1%.

Fiscal Aggregates: Total Spending, Taxes, Debt, and Intergovernmental Revenue The results reported so far strongly suggest that immigration from Mexico has had no major negative influence on the distribution and extent of local public spending. We turn now to an examination of immigration's effects on fiscal aggregates with the aim of ascertaining its effect on local fiscal discipline.

Effects on Total Spending, Own-Source Revenue, Intergovernmental Revenue, and Debt

	OLS	Network Instrument	Birth Cohorts Instrument
Total Direct Spending Per Capita	81.2177* (45.8448)	154.8773** (74.4167)	237.8077*** (89.7154)
Total Own-Source Revenue Per Capita	65.4447** (32.6490)	90.0174* (52.4627)	107.7393* (62.4061)
Income Tax Per Capita	-2.1604*** (0.6143)	-1.2807** (0.5721)	-2.5529*** (0.8912)
Property Tax Per Capita	27.3151** (13.3651)	11.3169 (20.9202)	34.3652 (31.2548)
Sales Tax Per Capita	3.6924 (3.9839)	4.0769 (5.2331)	2.1113 (8.3409)
Utilities Revenue	-1.8786 (8.4151)	20.2027* (11.4467)	25.1730 (15.9869)
Charges and Miscellaneous General Revenue	35.2283** (14.1427)	66.3558** (28.9415)	59.2688** (27.4622)
Total Inter-governmental Revenue Per Capita	24.8808 (19.9925)	71.7438** (34.2319)	74.6634* (39.4323)
State Inter-governmental Revenue	12.7300 (17.2255)	43.0030 (30.3483)	50.1061 (34.7342)
Federal Inter-governmental Revenue	5.9494* (3.3843)	13.9945*** (5.1742)	12.6182* (6.6199)
Total Debt Per Capita	113.8677 (85.8423)	189.9926** (90.4363)	578.1925*** (166.0651)

Minimum N=400

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Models estimated at the level of the MSA. Robust standard errors in parentheses.

Table 3.5 shows a large increase in total direct spending per capita in response to a one percentage point increase in the Mexican immigrant share. This by no means contradicts the many findings in Table 3.4, since the point estimates were generally positive and in some cases quite large, meaning that the effect on public spending in the aggregate would be expected to be larger and less noisy than the impact of immigration on each individual spending category. Additional analysis of results using the birth cohorts instrument (not shown) indicates that about half the effect on total direct spending is attributable to significant increases in per-capita education spending (\$71 per capita, $p < .05$)¹² and spending on utilities (\$57 per capita, $p < .01$). Smaller but still statistically significant ($p < .05$) increases in the per capita amount spent on housing, roads (see Table 3.4, Panel B), judicial affairs, and retirement pensions contributed an additional \$55 per person. The remainder appears to be an accumulation of small and statistically insignificant increases in a wide variety of categories, and there is no significant negative effect on spending in any category. Notably, contrary to Alesina, Baqir, and Easterly's (1999) contention that ethnic diversity fosters particularly large increases in patronage spending that can be targeted to particular groups, the category of unclassified spending shows no increase whatsoever in response to immigration.

Contrary to the notion that immigration makes it more difficult for local governments to exact tax revenue from their residents, there is an offsetting increase in localities' ability to raise revenue. While the OLS and birth cohorts instrument results show some evidence of an increase in per capita property tax (cf. Boustan et al. 2010) in response to immigration, the latter is not significantly different from zero, and the network instrument shows signs of a much smaller increase. Minor in practical impact but pertinent theoretically, however, is the fact that immigration does slightly (though significantly) reduce per capita income taxes per capita, a contrast to the effects on spending and other sources of revenue. Since income taxes are the generally the most progressive source of revenue available to localities, this lends some support to the thesis that low-wage immigration reduces redistribution, either because it lowers the mean family income while leaving the median citizen income intact (McCarty, Poole, and Rosenthal 2006) or because the median voter tends to reject taxation when the benefits redound heavily to ethnic out-groups (see, e.g., Lind 2007; Alesina, Baqir, and Easterly 1999). However, few localities raise much money through income taxes, and so it is not clear that the result has much general applicability.¹³

In fact the largest revenue increases appear to come from increases in utilities revenue (though this only partly offsets the additional utility expenditure per capita reported in the previous paragraph) and especially in the nebulous category of "charges and miscellaneous revenue." Further analysis (not shown) indicates that about half of this effect is accounted for by increases in "miscellaneous revenue," which, according to the Census Bureau's Government Finance and Employment Classification Manual (2006) includes payments-in-lieu-of-taxes from private utilities, but other elements of this category are not specified. Breaking down the effects on each category of charges is not much help either, as the great majority of the immigration-

¹² Recall that the results shown in Table 3.4 indicated a large but not quite statistically significant increase in education spending per pupil. The per-capita effect is of course smaller but also statistically significant.

¹³ Recall that all estimates discussed here pertain to local governments' revenues from their own sources. Thus state and federal income taxes are in no way factored into these results.

induced increase (\$25 per capita, $p < .01$) is found in the “Charges, All Other Not Elsewhere Classified” category.¹⁴

The crucial point in Table 3.5 is that although own-source revenue rises in response to immigration enough to partially offset increases in spending, there is still a great deal of incremental spending that must be funded either through intergovernmental transfers or debt. The instrumented results suggest that immigration increases both intergovernmental revenue and debt per capita substantially (though only debt increases relative to total personal income).

The estimated effect of immigration on accumulated debt using the birth cohorts instrument is several times larger the OLS estimate and is highly significant. A one percentage point increase in the immigrant share of a metro area’s population adds nearly \$600 per capita to its local governments’ accumulated debt. This corroborates Alesina, Baqir, and Easterly’s (1999) finding that intergovernmental transfers and debt fill in the gap between increased spending and decreased or unchanged revenue.

There are two interpretations of the finding that immigration from Mexico increases debt. Alesina, Baqir, and Easterly (1999) take their similar result for ethnic diversity as a sign that governments in ethnically diverse localities struggle with fiscal discipline and run larger deficits: patronage increases, and governments run deficits because it is difficult in ethnically diverse settings to reach agreement about how revenue should be raised – that is, which groups should be expected to pay what. But it might also be that getting municipal bonds approved in the face of increased ethnic diversity remains politically feasible whereas raising taxes engenders increased opposition. Rugh and Trounstine (2011) argue that politicians in ethnically diverse localities are able to structure bond offerings in a way that sustains funding for public goods even when the public becomes more hostile to fiscally liberal policies.

My results are consistent with Rugh and Trounstine’s (2011) findings. It seems that in fact immigration has not reduced productive public spending, perhaps because the hypothesized effects on public opinion are not borne out empirically or because localities can ill-afford to cut back on expenses that serve the needs of local industry (e.g. Peterson 1981) and that politicians have strategically turned to debt financing to sustain spending levels. Increased spending appears to reflect increases in investment in public goods rather than patronage, as Alesina, Baqir, and Easterly (1999) propose. Because certain expenditure categories are too vague to identify patronage clearly, it is impossible to test this with the Census of Governments data. But the estimate of immigration’s *positive* effect on education spending per capita (see Table 3.4, Panel B) is large though statistically insignificant, and the other coefficients are mostly positive, though more modestly so, reflecting their smaller share of localities’ budgets.

Further calling into question the fiscal stress interpretation is that the added debt municipalities have taken on in response to immigration appears to come at no increased marginal cost. It may be that immigration raises the prospects for near-term economic growth so that more debt may be taken on without increasing the cost of servicing it. By contrast, if debt were being incurred as a last resort to close a budget gap generated by unproductive patronage spending and an inability to raise adequate revenues, the cost of borrowing would likely increase as a result of immigration. The results using the birth cohorts instrument for the effect of

¹⁴ According to the Census Bureau manual, this category includes, “Charges not covered by any of the above categories [education, hospitals, commercial activities, and others], such as those derived from court and recording fees, police, fire, correction, defense, public welfare, public nursing homes, public libraries, and health activities” (p. 4-37).

immigration on the log of interest expenses per dollar of total debt (not shown) imply that a 1 percentage point increase in the Mexican immigrant share produces a 2% drop in the interest rate. Though this result falls short of statistical significance, it suggests that more debt is accrued in high-immigration localities without a concurrent increase in the cost of holding debt. Thus the evidence is consistent with the notion that local governments that have received many immigrants from Mexico may be taking on more debt to finance more developmental spending because they can do so relatively cheaply, perhaps a more attractive alternative than raising taxes and risking capital flight. In this case, local governments' turn to debt as a financing mechanism might reflect not only a response to immigration-fueled anti-tax sentiment among their constituents but a sound economic decision given their prospects for growth.

Turning to intergovernmental revenue, both Alesina, Baqir, and Easterly (1999) and Boustan et al. (2010) find that ethnic diversity is positively associated with the amount of revenue local governments receive from federal and state transfers. Obviously decisions over transfers do not reflect local governments' decisions, but state and federal governments may in effect be stepping in to help local governments that are facing difficulty raising revenue and directing it toward productive public goods maintain spending levels relative to need. Thus Alesina, Baqir, and Easterly (1999) suggest that the increases are a sign of decreased fiscal discipline and elsewhere speculate that they may be laden with patronage.

Table 3.5 demonstrates that their finding is corroborated in the case of Mexican immigration. Here we see again what appears to be evidence of substantial endogeneity or measurement error-based attenuation bias in the OLS results, as the instrumented results are often several times larger in magnitude than those generated by OLS. Estimates of immigration's effect on increases in intergovernmental revenue per capita from state governments are larger but noisier than estimates of its effect on transfers from the federal government. It is difficult to pin down the source of the increase. Among the categories of state transfers, immigration has significant positive effects on health and hospitals (\$9 per capita, $p < .1$), and a catch-all "other" category (\$19 per capita, $p < .05$) but notably not on transfers related to education or transportation. Among the federal transfer categories, positive and significant effects are found for only for transit (\$6 per capita, $p < .05$), though not for highways. Positive effects that narrowly miss statistical significance are also found for housing and community development (\$5 per capita, $p = .10$), and a federal catchall "other" category (\$4 per capita, $p = .15$). There are again no significant effects for education or, in this case, for health and hospitals. Federal and state governments may be stepping in to address shortfalls in local governments' ability to finance the costs of sustaining large immigrant populations, but given that we reject the hypothesis of decreased tax revenue overall, this fiscal stress may be a byproduct of increased economic demand for public goods and services rather than increased political resistance to financing those services.

To summarize, immigration from Mexico has slightly increased total spending on public goods, slightly increased or left unchanged own-source revenue, raised intergovernmental revenue, and added substantially to debt per capita. The questions of why spending increases in response to immigration outstrip own-source revenue increases and why immigration causes debt to mount clearly merit further investigation, but there is good reason to believe that they do not simply represent fiscal stress resulting from immigration-induced changes to the political will to finance redistribution and public services, and the increase in debt may even be a sign of economic health.

ROBUSTNESS

This section verifies that findings concerning the null effect of immigration from Mexico on public goods spending and redistribution are robust to controls for state and federal transfers to local governments, to analysis of expenditures at the county, rather than metro area, level, and to substituting recent and potentially unexpected growth in immigration for the level of immigration.

Controlling for Intergovernmental Revenue Are federal and state governments redressing what would otherwise be an immigration-induced erosion of the ability to produce public goods or redistribute income? The federal government has at times compensated states for the cost of providing illegal immigrants emergency medical care and education and of incarcerating illegal immigrants convicted of crimes. More generally, Mexican immigrant children from low-income families add to federal transfers in accordance with programs such as Title I. These transfers may subsidize additional spending even if localities would otherwise have cut back.

Even observing a negative effect of immigration on certain public spending categories once intergovernmental revenue is controlled would not constitute clear evidence for this possibility, however. Local governments that receive more revenue from parent governments might for that reason alone cut back on their own spending. On the other hand, rejecting more than minimal negative effects of immigration from Mexico on public spending once intergovernmental revenue is controlled would argue strongly against the proposition that intergovernmental transfers are masking immigration-induced problems with the production of local public goods.

Although controlling for intergovernmental revenue does reduce the positive impact of immigration on public spending, all coefficients that were positive in Table 3.4 remain positive. The effect of a one percentage point increase in immigration on total spending is estimated to be a \$97 increase per capita, though this is no longer quite significant (standard error = \$63). Spending on roads is still expected to increase \$13 per capita in response to a one percentage point increase in immigration ($p < .1$) and education spending per pupil is predicted to rise \$25 (though here the standard error is \$143, and the network instrument generates an estimated reduction of spending per pupil of \$120 with a standard error of \$83). I also separated total intergovernmental revenue into state and federal contributions, which made little difference. Finally, I re-estimated models predicting expenditures per pupil on education and on roads per capita controlling for federal and state transfers designated within those particular categories. The resulting coefficients remained positive, and a Chow test failed to reject the null hypothesis that they were the same as those indicated above ($p = .31$ for education and $p = .52$ for roads).

Changing the Unit of Analysis MSAs are not political jurisdictions and therefore it is possible that the results suffer from aggregation bias. Larger MSAs and some smaller ones are often aggregations of counties – usually only two or three but sometimes as many as ten. Thus there is a possibility that immigration contributes to increases in public spending in counties adjacent to central cities while causing public goods provision problems in central cities. In this case, immigration's local negative effect in urban counties would be masked by a countervailing positive effect in less immigrant-heavy areas. To address this possibility, all results were re-run using the largest urban counties (population greater than 100,000) identified in the IPUMS data as the unit of analysis. The birth cohorts instrument is unavailable for this analysis because there are no data linking Mexican migrant-sending regions to specific U.S. counties. I thus rely on inferences from analyses using the network instrument and OLS. In fact the county-level

analysis shows no differences of note from the MSA analysis. There are small positive effects on public goods spending per capita and as a share of income, null effects on all tax categories and large positive effects on debt of a similar magnitude to those shown in Table 3.5.

A second liability arising from the choice of MSA as unit of analysis is that local governments in the United States are “creatures of the state.” Different states delegate fiscal authority quite differently to their constituent cities and counties. Some states, such as California, limit property taxes and have the dominant responsibility for school funding. This may limit the degree to which local governments within a state can vary in their taxing and spending decisions in response to immigration and bias the results upward. We have already observed that the core results are robust to controls for intergovernment transfers from the state. Another robustness check is to aggregate all measures to the state level. Here it is possible to use OLS, the network instrument, and the births instrument. The results are unfortunately not especially informative because of the standard errors that result from confining the sample to fifty states observed over two decadal change periods.

Recent Growth Hopkins (2009) has proposed that recent growth in ethnic diversity, rather than the level of ethnic diversity, results in the erosion of public goods provision. Sudden growth in ethnic diversity may destabilize property values and cause long-term residents to consider moving out, thus reducing their willingness to support government projects with long time horizons. Unexpected increases may also increase the visibility of ethnic minorities and, in the case of foreign-born Mexican population growth, raise the political salience of hostility to immigration (Hopkins 2010).

This theory suggests that ten-year increases in the Mexican immigrant share should predict declines in spending on public goods. In an analysis over long differences, however, it means that acceleration in the immigrant share (changes in the change – that is, the net influx between 1990 and 2000 minus the net influx between 1980 and 1990) should predict decadal changes in spending. If the influxes are to be unexpected, however, the relevant sample is likely to be metro areas that had initially low levels Mexican immigration.

Neither the birth cohorts instrument nor in the network instrument are sensitive enough to be valid predictors of the acceleration in the Mexican immigrant share.¹⁵ OLS analysis, however, did produce some results consistent with the effects the recent rapid growth theory entails. In the full sample of metro areas, regressing changes in public spending, taxation, and debt on acceleration in the immigrant share turned up no significant effects among the dependent variables listed in Tables 6 and 7. However, when the sample was limited to metro areas whose Mexican population share in 1980 was under 1% (N=132). Spending per capita was predicted to drop \$164 ($p < .1$) in response to a one percentage point acceleration in the growth of the Mexican immigrant population share, per capita taxes to drop \$84, though this estimate is not statistically significant ($p = .26$), and total debt to drop \$389 per capita, again not significant ($p = .20$). Interestingly, the drop in spending was driven in large part by a predicted \$353 per pupil drop in education spending. These effects are more than cut in half, however, and fade to statistical insignificance when four outliers¹⁶ evident in inspections of residuals plots are removed from the analysis, so it is important to treat these results cautiously.

¹⁵ Both instruments do remain valid in random effects analyses. These were run both on the full sample and on cities with initially small Latino immigrant population shares. In both cases only null results were obtained, generally with positive or near-zero coefficients.

¹⁶ These outliers are Springfield, IL; Sarasota, FL; Roanoke, VA; and Hickory-Morgantown, NC.

SUMMARY

This chapter finds no evidence that immigration from Mexico has reduced public spending on public goods. Even in a cross-sectional analysis that mimics Alesina, Baqir, and Easterly (1999), the negative relationship of diversity and education and roads/highways spending, to the extent it is replicated for the 2000 sample of MSAs, is driven by the percent black of Hispanic native-born, not the percent Mexican foreign-born. The instrumental variables analysis of long differences shows that, if anything, immigration has had a positive effect on some aspects of productive public spending. Even as a share of total personal income developmental spending has not declined.

Turning back to the hypotheses that began the chapter, we can rule out negative effects of immigration from Mexico on public goods expenditures and on taxation. However, the results are quite consistent with Rugh and Trounstein's (2011) analysis and with the corresponding hypothesis that politicians are able to continue funding public spending in the face of ethnic diversity by designing electorally viable large-scale debt programs. This does not mean that immigration has led to fiscal irresponsibility through greater patronage spending without taxation to fund it, thus increasing debt and leading to greater local reliance on intergovernmental transfers. The evidence instead is that the incremental debt is no more costly for immigrant-receiving locales than others indicates that instead the accumulation of debt may be economically expedient rather than reflective of fiscal strain. And there is no evidence in these results to suggest that patronage spending rises in response to immigration-fueled diversity and upsets the fiscal balance.

The remaining two chapters probe alternative explanations for the failure and even reversal of the dissertation's core hypothesis. Chapter 4 explores whether the spike in Mexican immigrant legal status and citizenship following IRCA can account for the positive effects of increased Mexican immigrant stock on public goods provision. Chapter 5 then considers how immigration from Mexico has affected public attitudes toward spending and taxation in the context of fiscal deficits. Assessing whether such effects exist at all and, if they do, whom they are concentrated among and whether they are unidirectional or heterogeneous bears on some of the key limitations in the application of the core models linking ethnic diversity to public goods provision.

CHAPTER 4

THE IMPACT OF NATURALIZATION ON PROVISION OF NON-EXCLUDABLE PUBLIC GOODS: EVIDENCE FROM THE 1986 IMMIGRATION REFORM AND CONTROL ACT'S GENERAL LEGALIZATION PROGRAM

The results from Chapter 3 failed to support the widely held belief that mass immigration from Mexico eroded public spending on non-excludable goods and services between 1980 and 2000. If anything, my analysis based on a methodologically innovative instrumental variable approach found that spending on public goods appears to have increased in response to immigration. With taxes remaining little changed, the outcome was a budgetary gap filled in large part by relatively inexpensive debt. The question then becomes why the results were contrary to expectation.

This chapter focuses on one possible explanation. It tests whether increases in immigrants' own political power have increased spending on public goods, possibly counteracting greater support among natives for retrenchment. In particular, it uses variation in citizenship status as a key element of variation in Mexican immigrants' political power. It asks whether it is the case that increases in the percentage of Mexican immigrants in a U.S. locality who are U.S. citizens (holding the level of Mexican immigration constant) cause increases in its level of public goods provision. Did the naturalization of Mexican immigrants during the period 1980-2000 mitigate any corrosive effect of Mexican immigration on public goods provision in the U.S. and help bring about the generally null results in Chapter 3?¹⁷

To answer this question, I examine the delayed impact of the 1986 Immigration Reform and Control Act's (IRCA) general legalization program for illegal immigrants on Mexican immigrants' naturalization rates. Since different counties had different numbers of Mexican migrants eligible for IRCA legalization, IRCA generated differently sized naturalization shocks in different counties once those given legal status were able to become citizens. I ask whether the sizes of counties' IRCA-induced mid-1990's naturalization rate spikes among Mexican immigrants caused increases in local U.S. governments' spending on the provision of non-excludable goods and services.

Analyzing the impact of IRCA's legalization programs on local policy outcomes is also an important task in its own right. Attempts to offer a "path to citizenship" to some or all of America's 12 million illegal immigrants repeatedly find their way onto the U.S. political agenda. One way to make educated guesses about the impact of a path to citizenship for illegal immigrants is to attempt to ascertain the effects of the last such program. Yet the consequences of the last enactment of legalization have received surprisingly little scholarly attention (an exception is Orrenius and Zavodny 2012). There are no studies of aggregate political impacts and few even of economic impacts, leaving only wildly divergent and ideologically tinged forecasts produced by interest groups and think tanks to go by. Thus beyond shedding light on a

¹⁷ This is not to suggest that access to the franchise is the sole way of exercising political influence (cf. Fox, Bloemraad, and Kesler's 2013 analysis of the non-negative impact of non-citizenship rates on state welfare spending). How other manifestations of Mexican immigrants' local political power in the U.S. bear on public goods provision is an interesting question in its own right but one I do not know of any way to address because engagement in alternative forms of political activity is endogenous to the local conditions and policies that we might expect it to aim at altering.

potentially important reason that Mexican immigration appears not to have reduced the provision of public goods between 1980 and 2000, understanding the political effects of IRCA legalizations may furnish insights into how another mass legalization policy could transform local politics and policy in the United States.

More broadly, this chapter homes in empirically on how the low naturalization rates of today's immigrants influence public policy outputs. Scholars have examined the consequences of limited immigrant political participation for the behavior of political parties (Hajnal and Lee 2012), drawing a contrast with the critical role parties and their urban machine affiliates used to play in the incorporation of immigrants (Dahl 1961, Wolfinger 1965, but see Erie 1988). But examinations of the consequences of non-citizenship itself have gone unexamined, and how high non-citizenship rates influence public policy itself has received no scholarly attention that I am aware of. This chapter seeks to fill these gaps.

THEORY

A well-known class of economic model of preferences over redistributive taxation and immigration pits the preferences of natives against the preferences of low-skilled immigrants. Natives' support for taxation wanes with immigration since immigrants pay less in taxes than they receive in transfers, meaning that the total amount that each member of the society receives in transfers diminishes. Natives pay the same amount in taxes but receive less in return. Though these models emphasize utility from material self-interest, group psychological sources of utility from redistributive taxation may also foster a preference for low levels of taxation when natives are faced with immigrant influxes. Low-income immigrants, however, are expected to support high rates of taxation in these models because they pay little in taxes relative to the reward they reap in redistributed income.

Examinations of the interplay of low-income immigration and redistribution often assume uniform rates of voting over policy and explore, under different choice parameters, when immigration would be expected to raise or lower the tax rate. Tax revenue net of deadweight loss funds redistributive transfers that are uniform across all members of the public (Razin et al. 2002; Dolmas and Huffman 2003; see also Mayr 2007, whose model allows natives to vote over the level of immigration as well as the tax rate). Preferences over the tax rate are then tied to the costs and benefits the median income voter reaps (cf. Meltzer and Richard 1981). That is, the median voter chooses the tax rate that optimizes the difference between the amount he receives in transfers and the amount he loses through taxation. Intuitively, low-income voters tend to support more taxation, high-income voters less.

When low-income immigrants have little or no influence over political outcomes – for example where low-skilled immigrants are for the most part non-citizens or non-voters – we have seen that the potential pool of beneficiaries of redistribution increases and so the median income citizen or voter gets less return on each dollar he or she pays in taxes than he or she otherwise would (McCarty, Poole, and Rosenthal 2006). This weakens support for taxation that is in effect redistributive and might have been expected to tie immigration from Mexico to lower local provision of public goods in the United States. However, when immigrants gain access to the franchise or wield political influence in some other way, the expectation is that their preferences will to some extent counteract or even overwhelm the increasing reluctance of natives to support taxation, preventing retrenchment or even fostering higher tax rates.

We noted that Mexican immigrants' low rate of naturalization relative to other immigrant groups limited the potential influence of immigrant political power as a countervailing

mechanism. However a third of eligible Mexican immigrants do naturalize. Moreover, Mexican immigrants, naturalized and not, tend to express fiscally liberal positions in surveys and endorse an energetic government that produces an expansive array of public services (Bowler and Segura 2012). They have also been far more likely to be affiliated with the Democratic Party and to vote Democrat than to be or vote Republican (Bowler and Segura 2012). Thus, all else equal, any increase in the share of Mexican immigrants who hold U.S. citizenship would likely push policy to the left.

Yet, through a different channel, increases in the naturalization rate of Mexican immigrants could also have a negative impact on public goods provision. Naturalization of Mexican immigrants may exacerbate challenges rooted in preference heterogeneity. As discussed in Chapter 1, Alesina, Baqir, and Easterly (1999) propose that different ethnic groups' divergent preferences over the forms a public good takes reduce the amount of the good that people prefer to produce. This is because the necessary compromises over the form of the good make the good less valuable to each individual. Enfranchisement of Mexican immigrants might increase the degree to which their (perhaps divergent) preferences must be taken into account in designing public goods and services and thereby turn natives against the expansive provision of those goods.

For Alesina, Baqir, and Easterly, ethnic diversity makes provision of public goods less appealing and tempts politicians to resort instead to providing private, targeted goods (i.e. patronage). Even making public goods less appealing in absolute terms, however, co-ethnic comity and immigrants' need for immediate, targeted goods and services might make patronage a more appealing alternative. Politicians may seek to co-opt immigrants through patronage rather than more expansive provision of public goods in the manner of the urban machines of yore. These machines dealt in patronage much more than in policy and thrived on the provision of targeted private goods as a way of maintaining the loyalty of immigrant groups. Even without the preference heterogeneity mechanism, the enfranchisement of immigrants might lead to less public goods production because patronage might become a more attractive substitute.

If the predominant effect of naturalization is to increase the expression of Mexican immigrants' political preferences in local policy, one would expect that (H1) an increase in the naturalization rate of Mexican immigrants in U.S. localities will increase the local public provision of non-excludable goods and services. At any level of Mexican immigration, a higher share of Mexican immigrants who are U.S. citizens should cause a liberalization of fiscal policy and greater provision of public goods and a lower share of Mexican immigrants who are citizens should have the opposite effect. But if the predominant effect is to exacerbate problems rooted in immigrant-native preference heterogeneity, one might expect the opposite effect, that (H2) increases in the naturalization rate would reduce the provision of public goods. Both mechanisms may be present. The hypotheses are alternatives because they make conflicting predictions about the net effect of naturalization, which is in any case all that I can estimate here.

Both of these expectations are tempered by Mexican immigrants' public preferences in policy might be limited by their generally low turnout rates, even among eligible voters (Citrin and Highton 2002). Those who select into naturalization may manifest other participatory characteristics, but this is not necessarily the case with those who were granted legal status and a chance to become citizens through IRCA (cf. Barreto, Ramirez, and Woods 2005). Low turnout is especially relevant in low salience elections that often determine the direction of local politics and policy. The null hypothesis of no effect will be retained if the mechanisms presumed dominant in H1 and H2 are in fact of similar magnitude and cancel one another out or if neither

materializes because naturalization does not greatly increase the political involvement or influence of Mexican immigrants.

We are again confronted with a clear need for a credible empirical test of how non-citizenship among Mexican immigrants has in fact influenced public policy. Equivalently, I seek to understand how a sudden and, I argue, exogenous increase in the rate of citizenship among the Mexican immigrant population would impact local public finances. In the next section I outline my empirical approach to this question. It leverages IRCA's general legalization program, which contributed to a mid-1990s spike in naturalizations and examines the impact of the IRCA-driven portion of the naturalization shock to estimate the causal effect of an increase in the percentage of the Mexican immigrant population that are citizens on the same set of dependent variables used in Chapter 3. Analyzing the local average treatment effect of naturalizations that are due to IRCA is a conservative test of the effect of naturalization of Mexican immigrants on the provision of public goods. IRCA immigrants were less likely to be politically engaged after naturalizing than other immigrants were, meaning that they are less likely to exacerbate preference heterogeneity or effect a leftward shift in fiscal policy and that we are more likely to retain the null.

EMPIRICAL APPROACH

Motivation and Overview Testing whether rates of naturalization among the Mexican foreign-born influence public goods provision poses a number of familiar challenges. The implied experiment is to randomly vary the percentage of a locality's Mexican immigrants who have naturalized. Under random assignment, the association across counties of the citizenship rate with public goods provision would furnish an unbiased estimate of the causal effect of Mexican immigrants' citizenship rates on the outcomes in question. Of course localities' naturalization rates are not random. We might contemplate controlling for the overall level of Mexican immigration and a variety of other potential sources of endogeneity – features that could influence the propensity of Mexican immigrants to become citizens and also bear on public goods provision. For example, areas with a stronger Democratic Party organization or a larger Hispanic population may devote more resources to aid in naturalization and also exhibit greater population support for expansive provision of public goods and services, so we might control for those features of a locality.

However, it quickly becomes apparent that we will run into the same set of problems that caused us to reject selection-on-observables designs in the Chapter 3 analysis. Simultaneity bias is one concern. Naturalization may itself be a response to political or policy threat. Pantoja, Ramirez, and Segura (2001) identify precisely this mechanism among Latinos eligible for naturalization in mid-1990s California. Fearing the loss of benefits and political marginalization, California Latinos naturalized en-masse. This suggests that the correlation between naturalization rates during the period and public goods spending and taxation would be negatively biased: all else equal, more naturalizations would indicate a response to low existing levels of public goods spending.

Unobserved or difficult to measure factors that influence both the naturalization propensity of a locality's Mexican immigrants and the local provision of public goods are a second concern. For example, in localities with similar levels of Mexican immigration, differences in the rate of Mexican immigrants' naturalization might reflect underlying differences in the composition of the Mexican immigrant population. Differences in legal status, acculturation, integration, income or other attributes might cause variation in natives' reactions

to them and, in turn, yield variation in the level of public goods provision that we might misattribute to immigrants' naturalization rates or, more broadly, to immigrants' political power. Naturalized and non-citizen Mexican immigrants may also sort into localities that offer distinct bundles of taxation and public goods (Tiebout 1956). Naturalized Mexican immigrants who intend to remain in the United States indefinitely may prefer localities that offer expansive bundles of public goods and high taxes whereas non-citizens may envision a shorter sojourn in the county and therefore place little value on such community characteristics, focusing instead on employment prospects. Thus the naturalized share of the Mexican immigrant population may reflect differential sorting of Mexican immigrants into destinations in response to community characteristics that are correlated with the extent of local public goods provision.

The solution pursued here is again to leverage quasi-random variation in the independent variable, this time in the percentage of a locality's Mexican immigrants who are U.S. citizens. To do this, I examine the impact of a national shock in the rate of naturalization of Mexican immigrants that was brought about by the IRCA's general legalization program and that affected different urban counties to different degrees. In brief, counties with a larger share of the Mexican non-citizen population that met IRCA's eligibility criteria for legalization experienced larger increases between 1990 and 2000 in the percentage of their Mexican immigrant population that attained U.S. citizenship than did counties with lower IRCA-eligible population shares of the Mexican immigrant population. I use counties' IRCA-eligible shares of the Mexican non-citizen population along with a set of controls described below to instrument for 1990-2000 changes in the increase in the percentage of counties' Mexican immigrants who were citizens. This instrument then allows me to estimate the effect of an increase in the rate of citizenship among Mexican immigrants on the same set of local fiscal policies examined in Chapter 3. I begin by providing some background on IRCA and its legalization provisions.

IRCA IRCA was passed in 1986 after more than a decade of immigration policy gridlock. Ostensibly, it was a compromise intended to deal with the country's illegal immigrant population (Tichenor 2002). It paired legal restrictions against "knowingly" hiring unauthorized workers, backed by the threat of small employer fines but also accompanied by anti-discrimination provisions protecting legal foreign workers; strengthened border enforcement measures; and amnesty for undocumented workers who met a set of criteria including having entered the U.S. before 1982 and agricultural workers who had labored at least 90 days in United States during the previous year. Although in the aftermath of IRCA, employer sanctions and their effectiveness and discriminatory potential were major and contentious focuses of the topical literature (Briggs 1990; Bansak and Raphael 2001), it is clear in hindsight that whatever the short run effects, weak implementation and administrative failure emasculated those provisions of the law. The Reagan and Bush administrations viewed employer sanctions as regulatory interference and seldom enforced them (Tichenor 1998). The Immigration and Naturalization Service (INS) failed to centralize and coordinate its operations, and similar infractions routinely provoked substantially different penalties (Perotti 1994).

Probably the second largest focus of the post-IRCA literature concerns its short run effect on undocumented migration. This divided body of research leads to no clear conclusions. Perotti (1994) points out that border apprehensions first rose and then dropped after IRCA, indicating a possible deterrent effect that faded when Mexican sending communities observed that employer sanctions were "an annoying but surmountable barrier to U.S. entry." Donato, Durand, and Massey (1992) find that IRCA did not increase the short-run probability of attempting a first time unauthorized trip to the United States. Cornelius' (1989) interviews with

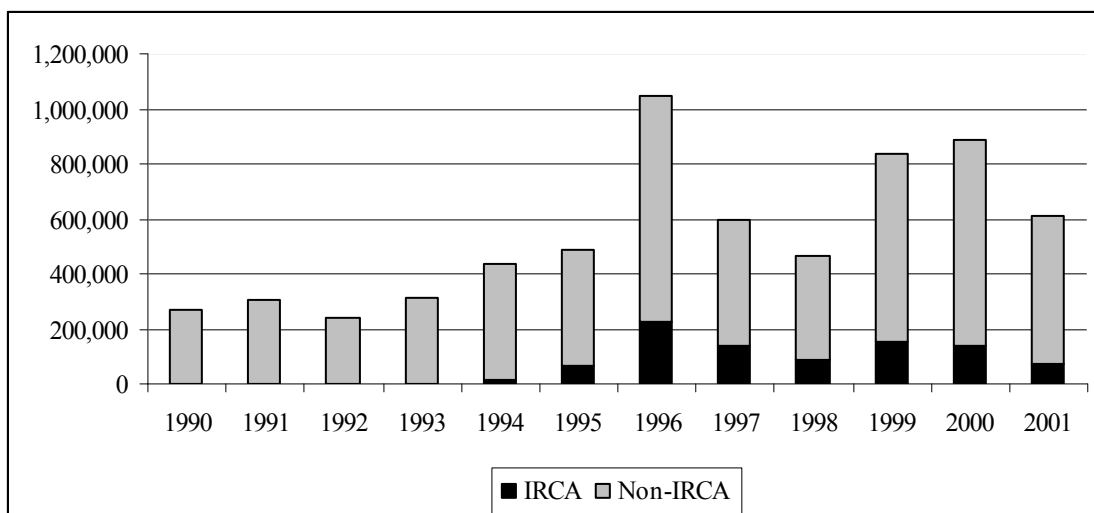
potential Mexican migrants show that, despite a high level of knowledge about IRCA, “only” 20% of interviewees who had recently thought about going to the United States but had not yet attempted the trip cited IRCA as a reason, so it appears that fear of employer sanctions or strengthened border enforcement was not a major deterrent. Yet the explosion of undocumented migration in the subsequent two decades makes much of this research moot: over the long haul illegal immigration rose at a rate several times faster than it had before, suggesting a minimal effect of the policy in this regard.

By far the most enduring consequence of IRCA, granting millions of previously illegal immigrants permanent legal status and eligibility for naturalization, has received considerably less scholarly attention. Zwane and Sunding (2006), discussed in the first chapter, focus on a collateral impact of IRCA – a short-run increase in rural counties’ immigrant agricultural laborer populations. Barreto, Ramirez, and Woods (2005), discussed briefly below, contrasts the voter turnout of IRCA immigrants with others who naturalized around the same time). There are even surprisingly few studies of IRCA’s economic impact (Orrenius and Zavodny 2012 provide a review of the existing literature).

By way of background, IRCA included two legalization programs. Its general legalization program ended up granting legal permanent residency to approximately 1.6 million people. To be eligible for this program an illegal immigrant had to have entered the country in 1981 or earlier and have resided continuously with the exception of small absences. This group received legal permanent resident status in the late 1980s and so became eligible for U.S. citizenship by the mid-1990s. Approximately 40% of those legalized under IRCA’s general legalization program had naturalized by 2001, and the IRCA immigrant naturalization flow fell to a trickle after that, with only an additional 5-6% attaining citizenship (Passel 2009).

IRCA also included a Special Agricultural Worker (SAW) legalization program for those who had been engaged in agricultural labor for at least 90 days during a twelve month period in the year prior to enactment (1985-86). Over one million SAWs were given legal status under this program. I focus on the general legalization program’s impact for several reasons. First, subsequent rates of naturalization were much higher for those legalized under the general program than for the SAWs. By 2001, naturalization rates among the SAWs (23%) also were considerably lower than among those legalized through the general program (40%) (Rytina 2002). Since this dissertation focuses on the production of non-excludable goods in urban areas the legalization of farm workers is arguably less relevant in any case. For reasons that I will make clear below, this would compromise the empirical strategy I use.

Figure 4.1: Total Naturalizations by Year in the United States Among IRCA and Non-IRCA Immigrants



Source: U.S. Immigration and Naturalization Service, Statistics Division

Estimation It is clear from Figure 4.1 that IRCA contributed significantly to naturalizations during the 1990s but was by no means the only factor involved in fostering the mid-1990s increase. Most of those who naturalized during this period were not IRCA immigrants, and there would have been a rise in naturalizations around this time even if no IRCA immigrants had become citizens. Isolating the portion of the naturalization spike that is attributable to IRCA allows us to observe the effect of increasing the rate of citizenship among a broad cross-section of immigrants who, absent IRCA, would for the most part not have been able to naturalize. Other national or state shocks in the number of Mexican immigrant naturalizations may by contrast have affected a select group of immigrants. For example, non-IRCA immigrants who responded to the Clinton-era welfare reform or California's Proposition 187 by naturalizing had to be eligible for naturalization in the first place (i.e. legal permanent residents who had lived in the country for at least five years and could meet the various language and financial requirements).

The concomitant spike in naturalizations not attributable to IRCA complicates the task of isolating counties' IRCA naturalization shocks. Immigration and Naturalization Service data on all IRCA legalizations furnish the number of legalizations of IRCA immigrants for each county by country of origin. However, using these naturalizations themselves as an instrument for changes in the naturalization rate would be inappropriate. The number of IRCA naturalizations is a function of two factors: the size of the migrant pool eligible for IRCA, which was determined before the decade I analyze and therefore could not be a result of changes in local county conditions between 1990 and 2000, and the rate at which that eligible pool in fact naturalized, which is almost certainly influenced by changes in local conditions during the decade and therefore may be endogenous. Thus I cannot simply use differences in the number of IRCA legalizations and non-IRCA legalizations to differentiate an IRCA-driven spike that is plausibly exogenous.

Instead, I use the percentage of the Mexican foreign-born population as of 1990 that was IRCA-eligible as an instrument for changes in counties' naturalization rates over the subsequent decade. For the general legalization program, this is the share of the Mexican foreign-born population that entered the United States in 1981 or earlier and had not yet become citizens, all data calculable for 152 counties with populations over 100,000 identified in the IPUMS

microdata in the 1980, 1990, and 2000 U.S. Censuses.¹⁸ However, an association between the IRCA-eligible share of the Mexican foreign-born population and the subsequent decadal change in the citizen percentage of the Mexican foreign-born population might, as Figure 4.1 indicates, have emerged irrespective of IRCA.

This potentially violates the exclusion restriction. For counties' 1990 IRCA-eligible shares of the Mexican foreign-born population to be a valid instrument for the 1990-2000 change in the percentage of the Mexican foreign-born population that was naturalized, it must (a) exert a large influence on that change and (b) have an impact on local public finances through no channel other than its impact on the percentage of Mexican immigrants who are citizens. However, an association between the 1990 IRCA-eligible population share and 1990-2000 changes in public finance outcomes could reflect other mechanisms as well. As one example, a higher 1990 IRCA-eligible population share could be associated with the length of time that a county has attracted large-scale Mexican immigration. Such counties might differ from newer destinations in many ways and have experienced differential growth in the expansiveness of their public goods provision during the 1990s for reasons that had nothing to do with increases in the naturalization rate. In other words, the size of the non-citizen population at the start of a period may be associated with the subsequent change in the naturalization rate for many reasons, some of which tap differences between county characteristics that are associated with our dependent variables. This would invalidate the instrument. On the other hand, since IRCA is a national shock, it is by definition independent of such potential mechanisms that differentiate counties, and so the portion of the association that is due to this change in policy cannot reflect the confounding impact of differences between counties that are correlated with the initial size of the IRCA-eligible population and the subsequent changes in the dependent variables.

To isolate the contribution of IRCA to this association requires making an assumption about what the association between the 1990 IRCA eligible share of the Mexican immigrant population and the 1990-2000 change in the percentage of the Mexican immigrant population that was naturalized would have been in the absence of IRCA. My strategy is to account statistically for the portion of the association that would have existed without IRCA by benchmarking the impact on naturalization of the IRCA-eligible population against the association of the non-IRCA-eligible non-citizen share with the decadal change in citizenship rates and against the association of older and more recent non-citizen shares of the Mexican immigrant population residing in each county in 1980 on the 1980-1990 change. To understand how this is implemented, consider first the equation we wish to estimate:

$$y_{ct} = a_t + bCITZ_{ct} + \varepsilon_{ct} \quad (1)$$

where y is some public expenditure outcome in U.S. county c in the year t , a is a constant term identifying effects specific to each year for which we have data, $CITZ$ is the share of the Mexican population that is citizens, and the error term captures all other county-specific influences on y that are not otherwise accounted for. We are interested in estimating coefficient b , which represents the effect of a change in the U.S. citizen percentage of county c 's Mexican foreign-born population on y . However, for reasons we discussed above, an estimate of b derived from

¹⁸ There are 26 other counties identified in all three years, however obvious issues with the data such as implausibly massive swings in population or finance estimates over a given decade force me to exclude these from the analysis.

equation 1 is likely to be biased, as the error term may contain a variety of county characteristics that give rise both to *CITZ* and *y* and because the direction of causality may be reversed.

As in Chapter 3, we begin by differencing equation 1 over a decade:

$$\Delta y_{c[t,t-10]} = \Delta a_{c[t,t-10]} + b\Delta CITZ_{c[t,t-10]} + \Delta \varepsilon_{c[t,t-10]} \quad (2)$$

This eliminates the influence of time-invariant confounds embedded in the error term but still leaves the possibility that unobserved time-varying county characteristics confound the association between the decadal change in the citizen share of the Mexican foreign-born population and the decadal change in the public finance outcome *y* captured by *b*. It also does not address the possibility that the relationship between the change in *CITZ* and the change in *y* is reciprocal. To address these problems, I wish to isolate the portion of the change in *CITZ* that is not associated with time-varying confounds and cannot be caused by change in *y*. I argue that cross-county variation in $\Delta CITZ$ that is due exclusively to IRCA and to variation in the 1990 IRCA-eligible shares of the Mexican-born population is a subset of the total variation in *CITZ* that meets these criteria for the decade 1990-2000. Since IRCA applied nationally, it cannot be associated with county characteristics that also influenced 1990-2000 changes in *y*. Nor can the IRCA-eligible migrant pool's share of the Mexican population be a byproduct of 1990-2000 changes in *y* or in other factors associated with changes in *y*.

I begin by writing the 1990-2000 change in the percentage of the Mexican foreign-born population that is made up of U.S. citizens a function of the 1990 share of the Mexican foreign-born population made up of non-citizens and other unobserved factors:

$$\Delta CITZ_{c[1990,2000]} = \gamma NONCITZ_{c[1990]} + v_{c[1990,2000]} \quad (3)$$

The coefficient γ provides the total association of the 1990 non-citizen share of the Mexican-born population in county *c* with the 1990-2000 change in the U.S. citizen percentage of the Mexican-born population in *c*. However, this association reflects the influence of both the IRCA-eligible population and the non-eligible population and the influence not only of IRCA but of all factors, some possibly embedded in the error term *v*. I therefore re-write equation 3 to break out the noncitizen share of the Mexican-born population into its IRCA-eligible and non-eligible components. I allow each component to have a distinct impact on the change in the *CITZ* and also break the impact of the IRCA-eligible population on $\Delta CITZ$ into a portion that is a result of the national IRCA shock and a portion that is not. The IRCA-eligible population is the percentage of the Mexican-born population made up of non-citizens who arrived in the U.S. no later than the year 1981. The ineligible non-citizen population is the share that arrived between 1982 and 1989.

$$\Delta CITZ_{c[1990,2000]} = \gamma_1 NONCITZ[82-89]_{c[1990]} + (\gamma_2 + I)NONCITZ[PRE82] + v_{c[1990,2000]} \quad (4)$$

It is clear that with three parameters and only two variables we cannot estimate *I*, the IRCA shock, without introducing an assumption.

One possibility would be to simply assume that $\gamma_1 = \gamma_2$, which would mean that, but for IRCA, the contribution of non-citizens arrived before 1982 to the subsequent change in the citizen percentage of the Mexican population is identical to the contribution of non-citizens who arrived in 1982 or later. Equation 4 would then become

$$\Delta CITZ_{c[1990,2000]} = \gamma_1 NONCITZ[82 - 89]_{c[1990]} + (\gamma_1 + I)NONCITZ[PRE82] + v_{c[1990,2000]} \quad (5)$$

and rearranging terms would yield

$$\Delta CITZ_{c[1990,2000]} = \gamma_1 (NONCITZ[82 - 89]_{c[1990]} + NONCITZ[PRE82]_{c[1990]}) + I NONCITZ[PRE82] + v_{c[1990,2000]} \quad (6)$$

where the sum associated with the coefficient γ_1 is left in its extended form for clarity but reduces to $NONCITZ_{c[1990]}$, that is, the whole non-citizen share of the Mexican-born population living in county c and counted in the 1990 Census. If the assumption we have made is valid, then I identifies the IRCA shock in isolation and $NONCITZ[PRE82]$ is a valid instrument for the change in the citizen share of the Mexican-born population between 1990 and 2000.

As shown in Table 4.1, a higher share of IRCA-eligible Mexican immigrants among a county's Mexican immigrant population, relative to its share of ineligible (i.e. arrived post-1982) immigrant non-citizens resulted in higher 1990-2000 increases in the naturalized share of counties' Mexican immigrant populations. The table shows an OLS regression of the 1990-2000 change in the citizen share of the Mexican immigrant population on the percentage of the 1990 Mexican immigrant population that was made up of non-citizens who arrived in the U.S. before 1982 (and were thus eligible for IRCA's general legalization program), controlling for the overall share of the 1990 Mexican immigrant population composed of non-citizens.

Here, as in the rest of the chapter, I show the results when weighted by county population size and by the size of the Mexican immigrant population. In the previous chapter, only full population weights made sense because they allowed us to estimate a weighted national effect of a change in the share of the population that is Mexican-born on the outcomes. Here, one could argue for population weights, which extrapolate to a national effect based upon the average Mexican immigrant percentage of urban counties' populations. We might also want to weight the importance of each county's contribution to the total association by the county's total population, since this determines how the average urban resident is affected by a change in the naturalization rate.

But one might also be interested in weighting by the Mexican-born population of each county for two reasons. One is that we might be interested in extrapolating the impact of an increase in the naturalization rate to the size of the Mexican-born population as a whole. An increase in the Mexican immigrant citizenship rate in a city like New York, which has a large population overall but a relatively small Mexican-born population, would thus contribute far less to our estimate of the impact of an increase in the citizenship rate of a city like Los Angeles, which has a smaller population than New York but a much larger Mexican immigrant population. A second reason for preferring Mexican immigrant population weights is that estimates of the naturalization rate among Mexican immigrants in areas with few Mexican immigrants will be far less precise – and thus introduce more attenuation bias – than will be the case in areas with large Mexican immigrant populations.

The positive coefficients on the % Pre-1982 variable indicate the extent to which a one percentage point increase in the share of the Mexican-born population made up of immigrants arrived before 1982, *relative to the share arrived after 1982*, was associated with the subsequent decade's increase in the percentage of Mexican immigrants who were citizens. The F-Statistics

easily exceed the Stock-Yogo (2005) thresholds for a strong instrument in each case, indicating that the 1990 share of the Mexican immigrant population that is composed of IRCA-eligible migrants is strongly enough associated with the 1990-2000 increase in the rate of citizenship among Mexican migrants to function as a valid instrument.

Table 4.1: Differential Effect of % Pre-1982 Non-Citizens and % Post-1982 Non-Citizens on 1990-2000 Increase in Citizenship Rate among Mexican Immigrants

	(1) Population Weights	(2) Mexican Population Weights
% Pre-1982 in 1990 (IRCA-Eligible Share of Mexican Foreign-Born Population)	0.47	0.27
	(0.07)	(0.06)
% Non-Citizen in 1990	0.64	0.83
	(0.09)	(0.06)
Constant	-0.68	-0.75
	(0.06)	(0.03)
<i>N</i>	152	152
<i>R-Squared</i>	.71	.81
<i>F-Statistic (% Pre-1982)</i>	46.8	20.3

Yet we can see that the assumption of equivalent impacts but for IRCA is at least questionable by examining the impact of noncitizens arrived before 1972 and the impact of noncitizens arrived after 1972 (both meaning the share such noncitizens account for of the total Mexican immigrant population of each county in the 1980 Census) on 1980-1990 changes in the naturalization rate. IRCA could not have contributed to these changes. Table 4.2 shows an OLS regression of the 1980-1990 change in the citizen share of the Mexican immigrant population on the 1980 Mexican immigrant population arriving before 1972, controlling for the overall share of the 1980 Mexican immigrant population composed of non-citizens. The positive coefficients on the % Pre-1972 variable indicate the extent to which a one percentage point increase in the share of the Mexican-born population made up of immigrants arrived before 1972, *relative to the share arrived after 1972*, was associated with the subsequent decade's increase in the percentage of Mexican immigrants who were citizens. Though, the estimates are quite a bit smaller than in Table 4.1, they are significant at $p < .05$ when Mexican population weights are employed, and in both case they are consistent with substantively large differences in the impact of older and newer immigrants' contribution changes in the citizenship rate between 1980 and 1990, which would inflate our estimate of IRCA's contribution to subsequent rises in naturalization.

Table 4.2: Differential Effect of % Pre-1972 Non-Citizens and % Post-1972 Non-Citizens on 1980-1990 Increase in Citizenship Rate among Mexican Immigrants

	(1) Mexican Population Weights	(2) Population Weights
% Pre-1972 in 1980	0.23 (0.11)	0.07 (0.15)
% Non-Citizen in 1980	0.54 (0.06)	0.88 (0.10)
Constant	-0.47 (0.04)	-0.63 (0.04)
<i>N (Counties)</i>	152	152
<i>R-Squared</i>	.54	.63

Note: Standard errors in parentheses. % Pre-1972 is the percentage of Mexican immigrants in the county who are non-citizens and arrived in the U.S. prior to 1972. % Non-Citizen is the overall percentage of Mexican immigrants in the county who are non-citizens. Thus the reference category is % Post-1972, the percentage of Mexican immigrants in the county who are non-citizens and arrived in 1972 or later. The coefficients for % Pre-1972 therefore estimate the difference in the impact of older and more newly-arrived Mexican immigrant non-citizens on the subsequent decade's change in the Mexican immigrant percent citizen.

It seems unwise, therefore, to assume that pre-1982 and post-1982 immigrants would have exerted similar influences on the 1990-2000 changes in the percentage of Mexican immigrants who were citizens. Instead, I introduce the association between each component of the non-citizen pool and the change in the percent citizen from the previous decade as benchmarks. In other words, I use the differences exhibited in Table 4.2 as a way of gauging what the differences in Table 4.1 would have been in the absence of IRCA, and I assume any additional differences are due to IRCA.

The IRCA shock, I , will be absent from the equations estimated in Table 4.2 relating older and newer non-citizen shares of the Mexican immigrant population to 1980-1990 growth in the naturalization rate among Mexican immigrants. IRCA could not have differentially influenced the naturalization of any immigrant groups until 1994. But it will contribute to the 1990-2000 differentials estimated in Table 4.1. I therefore use a difference-in-difference design, to isolate the IRCA shock. As I argue below, this shock can be estimated as the degree to which pre-1982 noncitizens' greater contribution, relative to post-1982 non-citizens', to increases in the citizenship rate between 1990-2000 exceeds pre-1972 non-citizens' greater contribution, relative to post-1982 noncitizens', to 1980-1990 increases in the citizenship rate.

To enable identification of I , the IRCA shock, I must make the assumption that, but for the influence of IRCA, the influence of the earlier and later non-citizen pools on the subsequent decadal change in the percent citizen would have changed by equal amounts. That is, earlier and later non-citizen pools may have a different influence on the subsequent decade's change in percent citizen in 1980-1990 and 1990-2000. And the earlier and later non-citizen pools' influences in each decade may differ from one another. But I assume that the difference between their influences in each decade must be the same apart for the IRCA intervention, which should make the influence of the older immigrant pool larger in 1990-2000 than in 1980-1990 by more

than the more recent immigrant pool. For the periods 1980-1990 and 1990-2000 I introduce the following equations:

$$\Delta CITZ_{c[1990,2000]} = (\gamma_1 + k)NONCITZ[82 - 89]_{c[1990]} + (\gamma_2 + I + k)NONCITZ[PRE82] + \nu_{c[1990,2000]} \quad (7a)$$

and

$$\Delta CITZ_{c[1980,1990]} = \gamma_1 NONCITZ[72 - 79]_{c[1980]} + \gamma_2 NONCITZ[PRE72]_{1980} + \nu_{c[1980,1990]} \quad (7b)$$

I then take the difference in the 1990-2000 and 1980-1990 differences by subtracting equation 7b from equation 7a. Rearranging terms yields the first-stage equation

$$\begin{aligned} \Delta \Delta CITZ_{c[1980,1990;1990,2000]} = & \gamma_1 \Delta NONCITZ[72 - 79; 82 - 89]_{c[1980;1990]} + \gamma_2 \Delta NONCITZ[PRE72; PRE82]_{c[1980;1990]} + \\ & I \Delta NONCITZ[PRE82]_{c[1990]} + k(\Delta NONCITZ[82 - 89]_{c[1990]} + \Delta NONCITZ[PRE82]_{c[1990]}) + \Delta \Delta \nu_{c[1980,1990;1990,2000]} \end{aligned} \quad (8)$$

The double difference in *CITZ* is the change in the percentage of the Mexican immigrant population of county *c* that held U.S. citizenship between 1990 and 2000 minus the change between 1980 and 1990. Differencing twice means that we need only worry about confounds that are not only time-varying but that time-vary in different ways between 1980-1990 and 1990-2000, as indicated by the double differenced error term. Note that the variable multiplied by the coefficient *k* simplifies to the percentage non-citizen of the Mexican immigrant population in each county in 1990, though for clarity as to the algebraic manipulations and for the purposes of elaborating a sensitivity test I describe below, I have left this in its current form.

Contingent on our assumption, the coefficient *I* now identifies IRCA's contribution, in isolation, to the impact of the percentage of the Mexican population in each county made up of non-citizens who arrived before 1982 (relative to the percentage arriving later) on the 1990-2000 increase in the naturalized percentage of Mexican immigrant population. In other words, with the controls displayed above, I argue that the percentage of the Mexican immigrant population that were non-citizens arriving in the U.S. before 1982 is a valid instrument for the increase in the increase in the Mexican immigrant population's percent citizen between 1980-1990 and 1990-2000.

In principle the assumption that *k* is the same for older and newer immigrant groups may lead me to overestimate or underestimate the IRCA shock. In practice, the most likely sources of error are the mid-1990s national welfare reform or state-specific interventions such as the crackdown on immigrant benefits in California. Both of these events are believed to have inspired increases in the rate of naturalization (e.g. Pantoja, Ramirez, and Segura 2001). For two reasons, it is likely that these produced a greater increase in the naturalization rate of newer than older immigrants over the course of the 1990s relative to during the 1980s, since more recently-arrived legal permanent residents were more vulnerable to some of these provisions. To the extent that these events provided a greater inducement to emigrate from the U.S. and return to

Mexico among more recently-arrived non-citizens than those with more time in the country, the error would be in the same direction, since the differentially higher departure of more recent non-citizens would also increase the degree to which their 1990 share of the Mexican foreign-born population would produce a greater increase in the percent citizen between 1990 and 2000 than their 1980 share of the Mexican foreign-born population produced in the 1980-1990 period. In either case, I would underestimate the impact of the IRCA shock on the 1990-2000 change in the percentage of the Mexican-born who were citizens.

I address this potential for error in two ways. First, there is a particular reason to worry about differential error in California counties, I verify that the analysis is robust to the omission of California counties. Second, I test the sensitivity of the estimates that the instrumental variables strategy yields to discrete variations in the assumption that the impact of older and newer noncitizen populations on the subsequent decade's change in the percent of Mexican immigrants that are citizens changes by the same amount in both groups. I observe the impact on all results of allowing the growth in the impact of the newer group of non-citizens to be anywhere modestly or much larger than the growth in the impact of the older group of non-citizens. In other words, I test the sensitivity of my main results to different degrees of underestimation of the IRCA shock. Formally, assume that the impact of the newer non-citizen group on the subsequent change in the percent citizen increased between 1980-1990 and 1990-2000 by n times more than the impact of the older non-citizen group did. Equation 7a then becomes

$$\Delta CITZ_{c[1990,2000]} = (\gamma_1 + k)NONCITZ[82 - 89]_{c[1990]} + (\gamma_2 + I + nk)NONCITZ[PRE82] + v_{c[1990,2000]} \quad (9)$$

Now subtracting equation 7b from equation 9 yields

$$\Delta \Delta CITZ_{c[1980,1990,1990,2000]} = \gamma_1 \Delta NONCITZ[72 - 79; 82 - 89]_{c[1980,1990]} + \gamma_2 \Delta NONCITZ[PRE72; PRE82]_{c[1980,1990]} + I \Delta NONCITZ[PRE82]_{c[1990]} + k(n \Delta NONCITZ[82 - 89]_{c[1990]} + \Delta NONCITZ[PRE82]_{c[1990]}) + \Delta \Delta v_{c[1980,1990,1990,2000]} \quad (10)$$

The larger n is, the more closely the sum tied to coefficient k approximates the IRCA-ineligible non-citizen populations' share of the Mexican foreign-born population in 1990. I estimate the alternate first-stage equation 8 for values of n of 1 (equivalent to equation 6), 1.25, 1.5, 1.75, 2, 4, and 8 using the 152 counties with populations greater than 100,000 identified in the 1980, 1990, and 2000 IPUMS Census microdata.

Table 4.1 demonstrates that the instrument satisfies the inclusion restriction, i.e., it is sufficiently strongly correlated with the endogenous regressor, the change in the citizenship rate of the Mexican-born population. The variable entries use the notation described in Equation 5b. The key coefficients are shown in the first row. They are the estimates of I , the degree to which IRCA itself raised the impact of the 1990 presence of pre-1982 immigrants relative to post-1982 immigrants on subsequent 1990-2000 naturalizations over and above the impact of the 1980 presence of pre-1972 immigrants on subsequent 1980-1990 naturalizations. Clearly the estimate depends on the weighting scheme employed, but in all cases the F-statistic on the excluded instrument exceeds 10. For comparison, the Stock-Yogo (2005) critical value for 10% maximal

bias is 16.4, and the critical value for 15% maximal bias is 9.0. Thus even in the weakest specification, which omits California counties entirely and uses general population weights that likely amplify measurement error in all the variables involved in the equations, there is unlikely to be a great deal of bias in the instrumented estimates to come relative to the amount of bias there would have been in the analogous OLS specification.

Note that here and in all subsequent analyses, I control for the difference in the change of the total Mexican foreign-born population between 1980-1990 and 1990-2000 ($\Delta\Delta\text{Mexicanfb}$). This is because, by granting legal status and the possibility of citizenship to millions of immigrants, IRCA also increased family reunification-based legal migration. For this reason, counties with a higher intensity of IRCA legalizations and naturalizations would have also had a greater potential for receiving larger influxes of new Mexican migrants. This would violate the exclusion restriction if these additional Mexican immigrants were a channel other than naturalization through which IRCA affected the dependent variables of interest. The relevant F-statistics are scarcely affected if this variable is omitted. The negative coefficients on the double difference in the percentage of the population that is Mexican foreign-born make sense. The more the Mexican immigrant population increases, the more it means that additional non-citizens were introduced during the course of the decade. This works against any increases in the percentage of Mexican immigrants who are citizens.

Table 4.3 Main First Stage Specifications (DV = 1990-2000 Change in % of Mexican-Born Population that Were U.S. Citizens Minus 1980-1990 Change in the Same Quantity, i.e. $\Delta\Delta CITZ_{[1980-1990;1990-2000]}$)

	(1) Population Weights	(2) Mexican Population Weights	(3) Population Weights, Omits California	(4) Mexican Population Weights, Omits California
$NONCITZ_{[PRE82]}_{1990}$	0.31^{***} (0.08)	0.55^{***} (0.08)	0.29^{**} (0.09)	0.50^{***} (0.10)
$NONCITZ_{1990}$	0.84 ^{***} (0.06)	0.59 ^{***} (0.08)	0.85 ^{***} (0.07)	0.69 ^{***} (0.09)
$\Delta NONCITZ_{[72-79;82-89]}_{[1980;1990]}$	1.00 ^{***} (0.03)	1.02 ^{***} (0.04)	0.99 ^{***} (0.03)	0.99 ^{***} (0.04)
$\Delta NONCITZ_{[PRE72;PRE82]}_{[1980;1990]}$	0.95 ^{***} (0.06)	0.88 ^{***} (0.06)	0.95 ^{***} (0.06)	0.86 ^{***} (0.07)
$\Delta\Delta MEXICANFB$	-1.59 ^{***} (0.40)	-1.38 ^{***} (0.30)	-2.03 ^{**} (0.71)	-2.05 ^{**} (0.61)
Constant	-0.75 ^{***} (0.03)	-0.65 ^{***} (0.05)	-0.75 ^{***} (0.03)	-0.68 ^{***} (0.05)
<i>N</i>	152	152	125	125
<i>R-Squared</i>	.96	.92	.96	.93
<i>F-Statistic (Noncitz[PRE82])</i>	14.8	48.4	10.8	23.5

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

To give a sense that the first stage results are not being driven by some outlier counties, I show a residual plot of the expected value of the instrument given the covariates against the expected value of the double difference in the share of the Mexican population that are citizens. Both are expressed as percentages. The plot should also make clear that California and Texas counties are not dominating the result. Although they tend to have large Mexican-born populations, they are not outliers here in the sense that the share of their non-citizen populations that is IRCA-eligible is not outside the norm.

RESULTS

I now leverage these IRCA shocks in the 1990-2000 change in the percentage of Mexican immigrants who were naturalized in each of 152 counties identified in U.S. Census IPUMS microdata. Given the specification of the instrument, all estimates derive from difference-in-difference equations. OLS equations have the following specification:

$$\Delta\Delta y_{c[1980,1990;1990,2000]} = \Delta\Delta CITZ_{c[1980,1990;1990,2000]} + \Delta\Delta Mexicanfb_{c[1980,1990;1990,2000]} + \Delta\Delta \varepsilon_{c[1980,1990;1990,2000]} \quad (11)$$

For analogous instrumental variables equations, I use the specification in Equation 8 to estimate the double difference in the change in the percent citizen among Mexican immigrants and then substitute this estimate into the second-stage equation in place of the double difference in the citizenship percentage and including all controls shown in Equation 8. Where I test the sensitivity of my estimates to differences in k , I employ equation 10 instead of equation 8.

Fiscal Aggregates: Public Spending, Taxes, and Debt Table 4.4 presents estimates for the impact of an IRCA-induced one percentage point increase in the naturalization rate of a county's Mexican immigrant population on fiscal aggregates per capita and as a share of each million dollars of a county's personal income. As in Chapter 3, per capita measures are indicative of taxation, spending, and debt proportional to need or demand whereas per-income measures are indicative of these metrics proportional to capacity, similar "effort" commonly used in the literature on the determinants of U.S. states' provision of welfare. For definitions and descriptive statistics pertaining to all dependent variables, please consult Chapter 3.

Table 4.4: Impact of IRCA-Induced 1 Percentage Point Rise in Citizenship Rate Among Mexican Immigrants on Fiscal Outcomes Per Capita

	OLS		IV		IV (Omits CA)	
	(1)	(2)	(3)	(4)	(5)	(6)
Total Spending	-3.2*	-6.35*	88.2*	49.5**	99.3	39.8
	(1.7)	(3.4)	(49.2)	(23.8)	(63.1)	(33.1)
Public Goods	-1.4	-2.7	65.4*	36.3*	72.5	35.4
	(1.2)	(2.3)	(36.3)	(19.1)	(46.8)	(24.3)
Core Public Goods	0.2	-1.5	48.2*	26.3*	53.4	23.1
	(0.8)	(2.5)	(26.7)	(14.5)	(34.8)	(20.2)
Potential Patronage	-0.2	-0.4	2.3	8.3	0.5	0.6
	(0.3)	(0.7)	(4.2)	(4.5)	(4.5)	(2.8)
Total Taxes	-0.5	0.8	39.6**	28.5**	37.9*	26.3*
	(1.1)	(1.7)	(17.8)	(14.3)	(19.9)	(14.0)
Property Taxes	-0.8	-0.4	28.4**	24.1**	26.9**	16.0*
	(0.9)	(1.5)	(10.7)	(7.7)	(12.9)	(9.1)
Sales Taxes	0.3	1.4**	4.0	0.4	3.6	3.3
	(0.3)	(0.6)	(3.5)	(5.4)	(3.7)	(3.5)
Income Taxes	-0.3	-0.3	9.1	5.0	9.6	7.1

	(0.3)	(0.3)	(6.1)	(4.1)	(6.3)	(4.4)
Total Debt	-0.1 (4.2)	0.6 (9.5)	74.6 (57.9)	-7.6 (54.8)	95.6 (70.3)	-101.9 (82.4)
Long Term Debt	1.5 (3.9)	3.6 (9.1)	72.1 (55.0)	-22.9 (49.4)	90.9 (67.8)	-106.2 (81.0)
<i>N (Counties)</i>	152	152	152	152	125	125
<i>Weights</i>	Full Population	Mexican Population	Full Population	Mexican Population	Full Population	Mexican Population

Note: Each entry is the coefficient on the variable *ddcitz*, the difference in the changes in the percentage of the Mexican-born population that were citizens between 1990-2000 and 1980-1990 in an OLS or IV regression predicting the difference in the changes of each specified fiscal outcome between 1990-2000 and 1980-1990. Each model includes a control for the 1990-2000 vs. 1980-1990 difference in differences in the Mexican population share as well as, in the IV specifications only, other controls detailed in Table 4.3. All outcomes are adjusted to 2010 dollars per capita. Robust standard errors are reported in parentheses.

* p<.1 **p<.05 ***p<.01

Sources: Author's compilation of 1982, 1992, and 2002 Census of Governments and IPUMS 1980, 1990, and 2000 Census microdata

Turning first to the impact of an increase in the citizenship rate on spending per capita, we see that the OLS estimates yield significant but small negative effects, consistent with a minor suppressive effect of citizenship on spending. However, when we apply the IRCA instrument to these results, the estimated effects become quite large and positive, on the order of several tens of dollars per capita, though the standard errors are also quite large in the IV, so I cannot offer a precise estimate of these effects. This suggests that the OLS results are biased downward, possibly reflecting the fact that at least some naturalizations during this period were a reaction to efforts at retrenchment targeting immigrants in particular. OLS results may also suffer from serious attenuation bias toward zero. Measurement error increases relative to signal when one differences variables, and a double-difference compounds this problem. The instrument may help rectify errors generated in twice differencing the percentage of the Mexican immigrant population that are citizens, a quantity that is no doubt measured with considerable error in the first place in counties with smaller Mexican populations or small populations overall.

It is clear from the subsequent three rows that the great majority of this increase is accounted for by greater spending per capita on public goods. Whereas the OLS results hover near zero, the instrumented results suggest a substantial positive impact of IRCA-induced naturalizations on counties' public goods spending per capita, though here the standard errors are too large to reject the null hypothesis of no effect at $p < .05$. Removing law enforcement related goods and services from the public goods measure does not much alter the result, as shown, and I verified that neither does removing spending on health and hospitals. Omitting the 25 California counties for which IPUMS microdata are available from the analysis further inflates the standard error on the estimates but does not influence the point estimates themselves, suggesting that the result is not being driven by the distinctive political environment related to immigration in that state during the 1990s or the massive number of IRCA legalizations that took place in LA County. The impact on potential patronage spending, by contrast, is positive but small and insignificant.

The results shown in Table 4.4 are consistent with two explanations. One is that mass naturalizations increased Mexican immigrants' political power and led to more liberal public spending outcomes. A second is that natives' favorability toward spending on public increased as the illegal immigrant beneficiaries in their midst became legal residents and then full-fledged U.S. citizens. The results are inconsistent with the hypothesis that increases in the naturalization rate would exacerbate problems of public goods provision rooted in preference heterogeneity among citizens and thus lead to lower levels of public goods provision. The results are also inconsistent with the related hypothesis that naturalization would foster greater political reliance on patronage to appeal to new voters. Both may occur in response to increased naturalization, but it is clear from the positive effects of naturalizations on public spending that the countervailing positive effects overwhelm any such impact.

Turning to per-capita impacts on taxes, we again see paltry estimates in the OLS analysis turn to substantial effects in the IV regressions. The increases are again positive and on the order of several tens of dollars per capita in response to a one percentage point increase in the share of the Mexican-born population that is naturalized. They are mainly a product of increases in property tax receipts. Recall that these are tax receipts rather than tax rates, so they may emerge from impacts on property assessments or rates. Importantly, given the cap on property tax increases in California since the passage of Proposition 13, omitting California counties from the

analysis leaves these results perfectly intact. Impacts on sales taxes and income taxes are smaller and mostly insignificant, though also positive.

These results point again to a net liberalization of fiscal policy in response to an increase in the citizenship rate among Mexican immigrants. They are inconsistent with the dominance of the preference heterogeneity mechanism that would have suggested higher rates of citizenship would make it more difficult to raise revenues to fund public goods whose form is subject to compromise between members of different ethnic groups. Tax increases are on the same order of magnitude as those observed for spending per capita and emerge for the most part from increases in property taxes. They are smaller across the board than impacts on spending, but the differences are not statistically significant. Estimating the impact of an increase in citizenship rates on the deficit - difference between total own-source tax revenue and direct spending per capita - using the same models as employed above yields an insignificant estimate of a \$20 per capita increase ($p=.33$). Thus we cannot reject that increases in public spending associated with increases in naturalization of Mexican immigrants are fully funded by increased taxes.

In contrast to impacts on per capita spending and taxation, estimates of the impact of a higher citizenship rate among Mexican immigrants on debt are highly sensitive to specification and unstable even among the IV estimates. All the estimates are statistically insignificant ($p>.1$) and whether the point estimate is positive or negative depends immensely on whether population weights or Mexican population weights are employed. It would seem that there is no consistent impact of a rise in the naturalization rate on accrued debt.

Table 4.5 tests the impact of increases in the naturalization rate of Mexican immigrants on fiscal aggregates per capita as a share of per capita income. Coefficients are multiplied by 100 to show the impact of a one percentage point increase in the U.S. citizen share of the Mexican-born population on taxation, spending, and debt per capita as a percentage of per capita income. In contrast to the estimates in Table 4.4, the results are mostly null, though still mostly positive in the IV specifications. They are consistent with a small positive or neutral effect of a change in the percentage of Mexican-born residents who are citizens on spending and taxation as a percentage of personal income. To appreciate how small the estimates are, note that, for example, the statistically insignificant .09 point estimate for total spending in model 3 means that a one percentage point increase in the U.S. citizen share of a county's Mexican-born population is estimated to increase direct expenditures by less than one tenth of one percent of total personal income (or, put differently, to increase per capita expenditures' share of per capita income by .0009).

These null findings appear to be attributable to the measurement error emerging from IPUMS-based estimates of the double-differences in per capita income, which introduces noise into the dependent variables and attenuates the point estimates. They do not mean that the results in Table 4.4 are simply attributable to rises in personal income concomitant with rises in naturalizations of Mexican immigrants or that a naturalization-driven rise in per capita income mediates these effects. Controlling for the difference in differences of county per-capita income between 1980-1990 and 1990-2000 does not alter any of the results in Table 4.4 more than marginally and leaves all significance levels intact.

Table 4.5: Impact of IRCA-Induced 1 Percentage Point Rise in Citizenship Rate Among Mexican Immigrants on Fiscal Outcomes As Share of Personal Income

	OLS		IV		IV (Omits CA)	
	(1)	(2)	(3)	(4)	(5)	(6)
Total Spending	0.00 (0.00)	0.00 (0.01)	0.09 (0.09)	0.03 (0.05)	0.10 (0.12)	0.00 (0.06)
Public Goods	0.00 (0.00)	0.00 (0.01)	0.08 (0.07)	0.03 (0.04)	0.08 (0.09)	0.02 (0.05)
Core Public Goods	0.00 (0.00)	0.01 (0.01)	0.07 (0.05)	0.03 (0.03)	0.06 (0.07)	0.00 (0.04)
Potential Patronage	0.00 (0.00)	0.00 (0.00)	0.00 (0.01)	0.01 (0.01)	0.00 (0.01)	0.00 (0.01)
Total Taxes	0.00 (0.00)	0.01* (0.00)	0.05 (0.03)	0.03 (0.02)	0.04 (0.03)	0.02 (0.02)
Property Taxes	0.00 (0.00)	0.00 (0.00)	0.04* (0.02)	0.03* (0.01)	0.04 (0.02)	0.01 (0.02)
Sales Taxes	0.00* (0.00)	0.00** (0.00)	0.00 (0.00)	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)
Income Taxes	0.00* (0.00)	0.00** (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)

	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Total Debt	0.00 (0.01)	0.01 (0.02)	0.09 (0.10)	-0.09 (0.09)	0.12 (0.13)	-0.25 (0.15)
Long Term Debt	0.01 (0.01)	0.02 (0.02)	0.09 (0.10)	-0.10 (0.09)	0.11 (0.12)	-0.26 (0.15)
<i>N (Counties)</i>	152	152	152	152	125	125
<i>Weights</i>	Full Population	Mexican Population	Full Population	Mexican Population	Full Population	Mexican Population

Note: Each entry is the coefficient on the variable *ddcitz*, the difference in the changes in the percentage of the Mexican-born population that were citizens between 1990-2000 and 1980-1990 in an OLS or IV regression predicting the difference in the changes of each specified fiscal outcome between 1990-2000 and 1980-1990. Each model includes a control for the 1990-2000 vs. 1980-1990 difference in differences in the Mexican population share as well as, in the IV specifications only, other controls detailed in Table 4.3. All outcomes are adjusted to reflect effects on the fiscal outcome per capita as a percentage of per capita income. Robust standard errors are reported in parentheses.

* p<.1 **p<.05 ***p<.01

Sources: Author's compilation of 1982, 1992, and 2002 Census of Governments and IPUMS 1980, 1990, and 2000 Census microdata

Sensitivity Embedded in these estimates is the assumption that $n=1$ (see equations 7-10 above), that is, that but for IRCA legalizations, the pace of citizenship acquisition of the older and newer immigrants would have changed between 1980-1990 and 1990-2000 by the same amount. We also noted some reasons to suppose that in fact the rate would have increased by more among newer immigrants. Testing the robustness of the results to the omission of California counties helps assuage these concerns, since an especial impetus to naturalize among California's newer immigrants, who were vulnerable to losing out on an array of benefits, has been documented in previous research noted above. However, the same inducement may have been present in some measure nationally as well, as newer immigrants were more vulnerable to the mid-1990s national reform of welfare.

In equation 10, we derived a way of testing the robustness of the estimates to the degree of error in our assumption. If, but for IRCA, newer immigrants would actually have increased their rate of naturalization between 1990-2000 relative to 1980-1990 by more than older immigrants would have, we would be underestimating the IRCA shock. Table 4.6 re-estimates some of the key results from Tables 4.4 and 4.5, using Mexican population weights, using different values of n . Although it would be idea also to test the robustness of the results to overestimates of the IRCA shock (i.e. values of n lower than 1), the instrumental variable no longer has sufficient power below approximately $n=.75$, the lowest value I show. Beneath that value, estimates of the IRCA shock are negative. This is possible if IRCA's influence on naturalizations was drowned out by its inducement of non-naturalized immigrants to stay in the U.S. longer than they would have. But the 40% rate of naturalization among those participating in IRCA's general legalization program means nearly all the rest of the 60% would have had to intend to leave the U.S. but then stayed due to legalization.

Table 4.6: Sensitivity of Estimates to Violations of Assumption that $n=1$

	Value of n (see equation 10 above)							
	0.75	1 (Estimate)	1.25	1.5	1.75	2	4	8
<i>Per Capita</i>								
Total Spending	81.4 (39.1)	49.5 (23.8)	37.6 (19.5)	31.4 (17.5)	27.6 (16.4)	25.0 (15.6)	17.5 (13.7)	14.4 (12.9)
Core Public Goods	32.8 (24.8)	26.3 (14.5)	23.9 (11.3)	22.7 (10.0)	21.9 (9.2)	21.3 (8.8)	19.8 (7.9)	19.2 (7.7)
Total Taxes	45.5 (24.3)	28.5 (14.3)	22.2 (11.1)	19.0 (9.5)	16.9 (8.6)	15.6 (7.9)	11.6 (6.3)	9.9 (5.6)
<i>Share of Personal Income</i>								
Total Spending	0.02 (0.08)	0.04 (0.05)	0.04 (0.04)	0.05 (0.03)	0.05 (0.03)	0.05 (0.03)	0.05 (0.03)	0.06 (0.03)
Core Public Goods	0.00 (0.06)	0.03 (0.03)	0.04 (0.02)	0.04 (0.02)	0.05 (0.02)	0.05 (0.02)	0.05 (0.02)	0.06 (0.02)
Total Taxes	0.04 (0.03)	0.03 (0.02)	0.03 (0.01)	0.03 (0.01)	0.03 (0.01)	0.03 (0.01)	0.03 (0.01)	0.02 (0.01)
<i>F-Stat on Instrument</i>	16.0	49.0	89.0	127.3	159.9	186.1	266.0	285.1

Table 4.6 leaves little cause for concern over the sensitivity of the estimates to the assumption that $n=1$. All estimates in the table remain positive, and they generally increase in statistical significance the greater the underestimate of the magnitude of the IRCA naturalization shock. Clearly the magnitude of the estimates does vary with n , but, in general, all estimates in Table 4.6 fall within or very close to the 95% confidence intervals around the estimates presented in Tables 4.4 and 4.5. Nothing in Table 4.6 would lead us to reject the conclusion that increases in the naturalization rate lead, on balance, to increases in per capita taxation and public goods spending and have very small effects on these dependent variables as a share of personal income.

Individual Public Goods Spending Items Finally, I look at impacts on each major public goods spending item examined in Chapter 3 per capita, as a share of personal income, and as a share of total direct expenditures. The results are shown in Table 4.7. Panel A contains results derived using Mexican population weights while Panel B shows results using full population weights. The first two columns show effects on the dependent variables measured per capita (or in the case of education spending, per school-age person – i.e. the population between 5 and 18 years old) for OLS and IV specifications. The second two columns render results for expenditures as a share of personal income. Here, since the percentages are quite small, I have left four decimal places to distinguish vanishing results from zero and differentiate among the various mostly insignificant coefficients. The third two columns display what impact a rise in Mexican citizenship rates had on the share that each public goods item comprised of total direct expenditures in the county. Note that the third two columns, like the second two whose scaling is discussed above in the description of Table 4.5, are scaled to reflect percentages, so, for example, the impact of a one percentage point increase in the Mexican citizen rate on the share of all expenditures dedicated to roads is .02 percentage points, *not* 2 percentage points.

What stands out most is that the IV results suggest a positive effect of a rise in the naturalization rate on public goods spending in a range of categories as measured per capita. Not surprisingly, given the small size and great county-to-county variability in most of the categories, precise estimates are difficult to come by. In the Mexican population-weighted results, seven of nine coefficients are positive compared to two that are negative, and three of the positive coefficients are statistically significant at least at $p < .1$ where neither negative coefficient is. In the total population-weighted results, eight of nine coefficients are positive, though only two of these are significant.

Table 4.7: Impact of Rise in Citizenship Rate Among Mexican Immigrants on Categories of Public Goods Provision

<i>A. Mexican Population Weights</i>	Per Capita		Share of Personal Income (%)		Share of Total Direct Expenditures (%)	
	OLS	IV	OLS	IV	OLS	IV
Roads	0.01 (0.39)	3.96* (2.33)	0.0005 (0.0006)	0.0052 (0.0032)	0.00 (0.01)	0.02 (0.04)
Education ¹	0.80 (7.59)	113.74** (61.16)	0.0093 (0.0058)	0.0098 (0.0254)	0.08*** (0.03)	-0.08 (0.15)
Parks	-0.99 (0.54)	3.15 (2.17)	-0.0014 (0.0008)	0.0048 (0.0036)	-0.01 (0.01)	0.05 (0.03)
Sewers	-1.03 (0.54)	-0.51 (2.89)	-0.0015 (0.0009)	-0.0020 (0.0055)	-0.02 (0.01)	-0.05 (0.06)
Trash	-0.49 (0.40)	1.54 (1.45)	-0.0005 (0.0007)	0.0015 (0.0026)	-0.01 (0.01)	0.03 (0.03)
Police	0.30 (0.36)	5.15** (2.01)	0.0011 (0.0007)	0.0078* (0.0040)	0.01 (0.01)	0.07* (0.04)
Fire	-0.15 (0.15)	-0.42 (1.18)	0.0001 (0.0003)	-0.0015 (0.0023)	0.00 (0.00)	-0.02 (0.02)
Health	-0.58 (1.13)	4.96 (5.60)	(0.0006) (0.0020)	0.0091 (0.0093)	0.01 (0.02)	0.09 (0.11)

Housing	-1.17*	2.15	-0.0017*	(0.0004)	-0.02**	0.00
	(0.51)	(2.64)	(0.0008)	(0.0043)	(0.01)	(0.06)

<i>B. Population Weights</i>	Per Capita		Share of Personal Income		Share of Total Direct Expenditures	
	OLS	IV	OLS	IV	OLS	IV
Roads	0.00	4.01*	0.0002	0.0047	0.00	0.00
	(0.18)	(2.10)	(0.0003)	(0.0041)	(0.01)	(0.04)
Education ¹	2.57	143.47**	0.0029**	0.0349	0.03*	0.00
	(3.93)	(61.16)	(0.0013)	(0.0285)	(0.01)	(0.12)
Parks	-0.06	3.66	0.0000	0.006	0.00	0.06
	(0.18)	(2.85)	(0.0003)	(0.0051)	(0.00)	(0.04)
Sewers	-0.20	4.77	-0.0003	0.0063	0.00	0.02
	(0.23)	(3.21)	(0.0005)	(0.0066)	(0.01)	(0.05)
Trash	0.03	1.62	0.0003	0.0013	0.00	0.01
	(0.17)	(1.61)	(0.0003)	(0.0034)	(0.00)	(0.04)
Police	-0.17	2.19	-0.0001	0.0021	0.00	-0.01
	(0.12)	(2.15)	(0.0003)	(0.0046)	(0.00)	(0.04)
Fire	0.00	-0.31	0.0002	-0.0019	0.00	-0.03
	(0.07)	(1.20)	(0.0001)	(0.0025)	(0.00)	(0.02)

Health	-0.16 (0.57)	8.45 (7.77)	0.0001 (0.0011)	0.0116 (0.0145)	0.01 (0.01)	0.06 (0.10)
Housing	-0.97** (0.34)	4.88 (3.59)	-0.0016* (0.0006)	0.0027 (0.0069)	-0.02* (0.01)	0.03 (0.06)
<i>N (Counties)</i>	152	152	152	152	152	152

¹ NB: Entries in the per capita column for education are spending on education per school-aged pupil (i.e. aged 5-18).

Note: Each entry is the coefficient on the variable *ddcitz*, the difference in the changes in the percentage of the Mexican-born population that were citizens between 1990-2000 and 1980-1990 in an OLS or IV regression predicting the difference in the changes of each specified fiscal outcome between 1990-2000 and 1980-1990. Each model includes a control for the 1990-2000 vs. 1980-1990 difference in differences in the Mexican population share as well as, in the IV specifications only, other controls detailed in Table 4.3. Robust standard errors are reported in parentheses.

* p<.1 **p<.05 ***p<.01

Sources: Author's compilation of 1982, 1992, and 2002 Census of Governments and IPUMS 1980, 1990, and 2000 Census microdata.

These results translate into positive, though for the most part very small and insignificant increases in spending in each public goods category as a percentage of per capita income. For example, even the fairly large increase in per-pupil education spending of \$113 or \$143 per pupil, depending on which weights are employed, indicates that spending on education rises in response to a one percentage point increase in the naturalization rate of Mexican immigrants by less than one twentieth of one percent of the total personal income generated by a county. This serves to remind that even large and significant impacts when viewed in the light of spending per person reflect small changes in spending as a share of income – so small that it seems likely that the great majority of citizens would not detect them, let alone trace them to immigration or a change in the rate of citizenship. This makes sense given the small share of the population we are generally talking about. When weighted by population, the mean Mexican foreign-born population share in the counties in this sample was approximately 5%. When weighted by Mexican population size, this rises to 12%. Thus a 1 percentage point increase in the share of these Mexican-born people who are citizens implies a change in status for roughly between .05% and .15% of the population.

Effects on each public goods item as a share of total expenditures are also vanishing. This reflects the fact that items appear to have increased in response to naturalization almost across the board in absolute terms. It also reflects a growth in spending not dedicated to public the goods examined here. Thus although, as the lion's share of expenditure, increases in public goods spending account for the majority of increases in total spending in response to the naturalization rate, we cannot rule out that an increase in the naturalization rate causes spending on items that are not easily classifiable as public goods to increase as well. This is reflected in the positive, though relatively small and statistically insignificant, coefficients on per capita spending on potential patronage shown in Table 4.4.

ROBUSTNESS

Naturalization of Immigrants from Countries other than Mexico Though 75% of those legalized through IRCA were of Mexican origin (Rytina 2002), illegal immigrants from other countries also received legal status and naturalized at rates 10-20 percentage points higher than Mexicans legalized through the general legalization program. Thus it is important to ensure that the results here are not being driven by legalizations and subsequent naturalizations of other non-Mexican immigrants. While such an effect would be of interest in its own right, it would limit the comparability of these results with those in Chapter 3. I therefore re-estimated all models reported in this chapter with a control for the difference between the 1990-2000 and 1980-1990 changes in the percentage of the non-Mexican foreign-born who were citizens. This variable is positively associated in most cases with spending on public goods. But in no instance does its inclusion in the model have more than a minimal and statistically insignificant impact on the estimated effect of Mexican immigrant naturalizations on the dependent variables of interest.

Federal Reimbursements and other Intergovernmental Transfers As discussed in Chapter 3, it is important to ensure that these results are not simply a byproduct of federal or state transfers to local governments. I verified that controlling for intergovernmental transfers from states and the federal government in the aggregate, in analyses of each category, yields no estimates that differ significantly ($p < .05$) using Chow tests from those presented above for any spending category, for spending on public goods overall, and for total direct expenditure.

However, In addition to reimbursement programs for states' incarceration of illegal immigrants convicted of crimes and for emergency health services, IRCA provided for a State

Legalization Impact Assistance Grant that would help reimburse the cost of providing welfare, health services, and public education to the legalized population. In each year of 1988-1991, 1 billion dollars were allocated, minus a total of five hundred million in administrative costs (GAO 1991). Though reimbursements were targeted to welfare, health, and education, this may have left additional funds for states to spend on other public goods or increased transfers to counties to do so. Thus federal transfers to states and possibly from states to counties might have subsidized increased spending on public goods and led to the generally positive effects reported here.

These transfers occurred mostly during the 1990s, so it is unclear whether they would continue to have a discernible effect on the estimates in the 2002 Census of Governments. It is even possible, if local governments anticipated reimbursement, that they might have inflated their public goods spending in 1990, leading to downward bias in my estimates because of the inflation of the 1980-1990 changes and the diminution of the 1990-2000 changes. In either case, the actual volume of transfers in 2002 would not necessarily be indicative of the additional funds made available over the course of the 1990s. To address this issue, I re-estimate models for public goods spending overall with a control for the total volume of federal transfers to the state in which each county is located over the period 1988-2000. Data on annual federal transfers to each state are from the Census' Annual Survey of Governments, which provides fiscal data for state and large local governments in the intervening years between Censuses of Governments. The point estimates in the IV specifications with all counties included remain positive (\$40.2 and \$16.4 respectively for a one percentage point increase in the citizenship rate among Mexican immigrants), though they drop slightly below statistical insignificance ($p = .14$ and $p = .18$ respectively). Point estimates for increases on spending overall remain statistically significant.

CONCLUSION

These nuances in the data aside, the best summary of the results in Tables 4.4, 4.5, and 4.7 is that an increase in the naturalization rate among Mexican immigrants appears, on balance, to cause increases in public spending per capita and taxation, driven for the most part by increases in spending on core public goods such as education and infrastructure and increases in property tax receipts. However, these impacts on spending are small relative to total personal income and do not materially alter the distribution of public spending across categories. Again, this does not mean that the mechanisms linking intensified differences over the form public goods take between newly minted Mexican-born U.S. citizens and U.S. natives does not in some measure dampen these effects. It means, however, that either the positive response of natives to producing greater volume of public goods when they are shared with U.S. citizens rather than non-citizens or the greater political influence of enfranchised Mexican immigrants themselves outweighs any such negative impact.

Returning to the question that motivated the chapter: to what extent can citizenship among Mexican immigrants help account for failure of Mexican immigration to erode the provision of public goods. To shed some light on this question, consider that the increase of the Mexican-born population share among the urban counties considered in this analysis from 2.1% in 1980 to 3.5% in 1990 to 5.8% in 2000. Yet the percentage of the Mexican population that was naturalized actually increased to a small degree, from 22.9% in 1980 to 23.4% in 1990, to 24.5% in 2000. In other words, as immigrant influxes have risen, the rate at which Mexican immigrants naturalize has remained roughly constant, helped to a significant degree by IRCA's legalization programs between 1990 and 2000. If no post-1980 immigrants from Mexico had naturalized, we would instead have observed the rate of citizenship drop from 22.9% in 1980 to approximately

5.5% by 2000. Extrapolating from our estimates in Table 4.4, this would have had substantial effects in negating a negative effect of immigration from Mexico. Consider the estimate for the effect of immigration on core public goods per capita (+\$26 dollars per capita for each additional percentage of the Mexican-born population that achieves U.S. citizenship with weights for Mexican population size). If the citizenship rate among the Mexican-born population had indeed declined by 17.4 percentage points, the impact of the 1980-2000 rise in the Mexican population share (3.7 percentage points) would have yielded \$458 less in per capita spending, or \$124 less in response to each percentage point increase in the Mexican-born population. The analogous figure for total taxes is \$135 less in response to each percentage point increase in the Mexican population.

Of course considerable caution is in order in extrapolating from these estimates. Each comes with a large confidence interval that means its true impact of continued acquisition of citizenship among the U.S.-born Mexican population could be considerably larger or smaller. Moreover, the hypothetical scenario I have used for the purposes of illustration, in which none of the Mexican immigrant stream arriving between 1980 and 2000 acquires citizenship, is clearly extreme. Yet this chapter does sustain the plausibility of the notion that despite their relatively low rate of naturalization compared to other immigrants, Mexican immigrants' steady rate of citizenship acquisition over the period of study is one important reason that influxes have not led to the erosion of public goods provision in U.S. localities.

CHAPTER 5: DOES MEXICAN IMMIGRATION AFFECT AMERICANS' FISCAL POLICY PREFERENCES?

Chapter 3 showed that immigration from Mexico between 1980 and 2000 did not decrease local public goods spending or dampen overall levels of taxation. Cross-locality correlations between ethnic diversity and lower spending on public goods identified in prior research (e.g. Alesina, Baqir, and Easterly 1999) appear to be driven by the sizes of black populations rather than concentrations of Mexican immigrants. When analyzed longitudinally and with the use of instrumental variables described in Chapters 1 and 2, immigration from Mexico had, if anything, a positive impact on public goods spending. Furthermore, it appears that localities have financed the incremental spending caused by the influx of Mexican immigrants primarily with increases in relatively inexpensive public debt.

My analyses so far have followed most of the existing literature in focusing on actual policy outcomes as dependent variables. But as I elaborated in Chapter 1, the various theories linking Mexican immigration to lower public goods spending assume a mediating role for public opinion. However, despite this common assumption, the nature of the opinion-to-policy link is rarely spelled out. This omission is important because there are a number of reasons to suppose that Mexican immigration might not produce policy change even if it affects public preferences over fiscal policy, making natives less supportive of taxation and public goods spending. For one, the political power of Mexican immigrants themselves might counteract natives' support for retrenchment. I explored this in Chapter 4, through an analysis of IRCA's contribution to a spike in naturalizations among Mexican immigrants between 1990 and 2000. Indeed, although Mexican immigrants naturalize at considerably lower rates than most other immigrants, the acquisition of citizenship among many Mexican immigrants does help explain why Mexican immigration has had a non-negative impact on local public goods provision. For another, status quo policies tend to be "sticky" in democracies due to the existence of multiple veto points and possibly also status quo bias and risk aversion among political leaders. In a federal system, subnational units face the additional peril of capital flight if they cut developmentally oriented spending. These points work against the expectation that immigration will have a corrosive impact on public goods provision. But they leave open the possibility that immigration will reduce mass support for the provision of public goods.

This chapter therefore turns attention to the impact of Mexican immigration on public preferences over state fiscal policy. I analyze the relationship between states' levels of Mexican immigration and their non-Hispanic native-born residents' responses to two pertinent survey questions asked consistently in the 2006, 2008, 2010, and 2012 Cooperative Congressional Election Studies. Both questions solicit respondents' preferences about how their own states' budget deficits should be addressed. The first asks respondents to choose between using tax increases and spending cuts and specifically mentions three important areas of public goods when articulating the possibility of cutting spending. The second asks whether any tax increases should come from taxes on income or on sales. Therefore though these questions do not directly tap preferences over the amount governments should spend on public goods, the first gauges the public's support for preserving taxes needed to fund public goods and pose a clear tradeoff between sustaining the provision of public goods and keeping taxes low, and the second gauges whether citizens are willing to impose progressive income taxes if a tax increase were necessary

or prefer more regressive sales taxes. Taken together, the two tap whether immigration from Mexico increases the native public's fiscal conservatism.

I estimate how the percentage of a state's population that is made up of Mexican immigrants influences responses to these questions. If Mexican immigration does limit support for public goods spending and the taxes needed to fund it, we would expect higher levels of Mexican immigration to be associated with more conservative responses to these questions (i.e. more support for spending cuts than tax hikes and more support for sales tax increases than income tax increases). I also examine whether Mexican immigration has a different impact on the fiscal preferences anti-immigrant and pro-immigrant residents and on people with differing ideological and partisan predispositions. A significant interaction between the level of immigration and individuals' stable orientations to politics could mean that immigration from Mexico fosters greater ideological constraint between attitudes about fiscal policy and attitudes about immigration policy. It may thus contribute to partisan polarization on these issues.

My primary analysis uses fixed effects specifications. These specifications permit me to estimate how variation in the level of Mexican immigration within states over this period was associated with responses. I also test the robustness of the results to random-effects specifications that incorporate static cross-state variation and, in the limited cases where it is possible, to the use of instrumental variables for Mexican immigration. Data constraints I will explain below force me to adopt states, rather than localities, as units of analysis, to use as a dependent variable a general question about state fiscal policy rather than specific questions about public goods spending, and to study a later period than in the previous two chapters. These aspects of the design to some extent limit direct comparability with the previous chapters of the dissertation, but, as I argue, they still permit a test of immigration's role in promoting conservative attitudes toward taxation and spending.

The research described here is to my knowledge the only existing effort to assess the impact of immigration on fiscal preferences other than those pertaining to welfare and other explicitly redistributive domains. I am aware of only one other study that seeks to link local ethnic diversity to preferences over public goods spending (Trounstine, n.d.). Critically, however, I analyze a question soliciting preferences over fiscal policy in respondents' own states whereas prior research has not been able to convincingly achieve congruence between the unit at which contextual measures are taken and the unit invoked in questions gauging fiscal policy preferences. I begin with a brief review of theory and then derive three hypotheses that guide the analysis.

THEORY

Each theory presented in Chapter 1 to link immigration from Mexico to lower public goods spending and taxation implies a mediating link through public opinion. Natives may resent Mexican immigrants' use of non-excludable public goods. They may perceive the consumption of publicly provided goods as eroding the boundary between citizen and non-citizen, legal and illegal, or those who meet the subjective criteria for national membership and those who do not by dint of incomplete acculturation or ethnic difference. Racial prejudice or beliefs that immigrants are fiscally burdensome or even choose to migrate in order to reap public benefits in the United States may exacerbate this resentment and spill over to lower support for spending on government-provided goods and services. Immigration may therefore turn public opinion against expansive provision of public goods and increase hostility to the taxes that fund them (Alesina, Baqir, and Easterly 1999). Or immigration-fueled linguistic and ethnic diversity

may weaken the ties citizens feel to the public sphere generally (Putnam 2007) and cause them to value public investment in goods and services less.

An alternative way that Mexican immigration may influence natives' fiscal preferences is by altering the costs and benefits individuals derive from taxation and public spending.

Divergences between Mexican immigrants' and U.S. natives' preferences over the form that public goods take could force subnational governments to forge compromises that are unappealing to immigrants and natives alike. This might reduce the amount individuals are willing to pay in taxes to fund these goods (Alesina, Baqir, and Easterly 1999). And finally, the addition of non-citizen households at the low end of the U.S. income distribution, whose net contribution to the public coffers will be lower than those of most natives, dilutes the return that natives wealthier than the average Mexican immigrant receive on taxed income that is put toward any rivalrous good (cf. McCarty, Poole, and Rosenthal 2006, who apply this theory to explain the convergence of rising income inequality and retrenchment nationally in the U.S.). This is expected to reduce support for taxation. Mexican immigrants' low average income and low rate of naturalization support their use as a critical test case for this theory.

Each of these models has been invoked to make predictions about diversity or immigration's policy effects but in fact each makes a more direct prediction about their impact on public opinion. They all lead to the expectation that increases in immigration from Mexico reduce support for public goods spending and citizens' willingness to pay taxes to fund these goods. All else equal, I hypothesize (H1) that an increase in states' Mexican immigrant population shares should reduce their residents' support for public goods spending and taxation.

Moreover, if immigration from Mexico does have an impact on public opinion, that impact need not be uniform. A growing body of literature finds that responses to immigration depend not only on situational "triggers" but also on underlying psychological orientations. Different people's fiscal attitudes may respond to immigration in different ways. An increase in Mexican immigration may generate a more negative reaction to public goods provision among those hostile to immigrants than among those who are sanguine about immigration. Those who support immigration and are not riled by immigrants' use of public services might be unaffected by immigration. Some of the most pro-immigrant natives may even respond to increases in immigration with greater support for public goods provision if they perceive these goods to attract or retain immigrant families or if they believe immigrants are worthy beneficiaries.

This leads to the hypothesis (H2) that the impact of increases in Mexican immigration will vary depending on individuals' prior attitudes toward immigration. Specifically, an increase in a state's immigrant population share will have a particularly pronounced negative impact on anti-immigrant natives' attitudes toward public goods provision and a muted or even positive impact on pro-immigrant natives' attitudes. Confirmation of this hypothesis would be found in a statistically significant interaction between a state's immigrant population share and individuals' immigration attitudes.

Heterogeneity in the effect of immigration from Mexico on fiscal preferences is related to the question of whether and how these influxes influence mass political polarization. That is, if H2 is confirmed, then assuming that anti-immigrant publics are not more fiscally liberal than pro-immigrant publics even absent the presence of many immigrants, increased immigration from Mexico would increase the divide between pro- and anti-immigrant citizens over issues of taxation and spending. This hypothesis is comparable to Citrin, Levy, and Wright's (forthcoming) finding that multicultural policy in Europe widens the gap in political support between pro- and anti-immigrant citizens. Anti-immigrant citizens tended to have lower levels

of political support irrespective of immigration, and there was no evidence that multicultural policy reduced political support on the whole. But the strength of the association between opposition to immigration and support for regime incumbents and institutions increased when countries adopted more extensive sets of multicultural policies. Citrin, Levy, and Wright speculate that this may contribute to the electoral fortunes of extreme parties of the right by “fusing” political discontent and anti-immigrant sentiment.

The United States has not yet confronted any analogous threat of political destabilization from extreme right-wing parties, but the political consequences of this effect, as well as of any other such interactions could be substantial. Debate continues over whether greater polarization among elected officials and elites in U.S. politics since the 1970s is reflected in greater divisions within the mass public (Fiorina et al. 2005; DiMaggio et al. 1996; Abramowitz and Saunders 2008; Carsey and Layman 2006). However, there is evidence of partisan sorting (Fiorina et al. 2005), as ideologues have switched to the political party that is more congenial to their ideological perspectives. Issue- or ideology-based sorting, accompanied by the passive adoption of the party line on issues people feel less strongly about (Carsey and Layman 2002), can lead to greater issue constraint (Converse 1964) – that is, that one’s view on one issue is predictive of his view on others. Even if the extremity of the public’s issue attitudes has not increased during this period, greater ideological constraint and partisan sorting mean that the public increasingly resembles cohesive ideological blocs that agree about most things internally and disagree about most things with the other side.

To the extent that increases in immigration from Mexico have increased the association between immigration attitudes, liberal-conservative identification, and partisanship, on the one hand, and attitudes about taxation and spending on the other, they will have fostered greater sorting and increased constraint. That is, Americans’ fiscal attitudes, which have long aligned with ideology and partisanship, may hew to these attachments to a greater degree because of immigration from Mexico. And immigration attitudes, long known as an issue that generates political coalitions that cross-cut the ideological and partisan space (Tichenor 2002; Zolberg 2006) might become more tightly connected to party and ideology.

Testing H2 also helps shed light directly on the ingroup preference mechanism tying immigration from Mexico to fiscal attitudes. If people’s preferences over the level of taxation and spending are truly informed by the social identity of those who consume public goods and services, we should observe that those who have favorable attitudes toward the group respond significantly less negatively toward its presence in their state than those who have positive attitudes toward the group. This result would also be consistent with the mechanism linking immigration to reduced social capital especially among those hostile to immigration.

But a theory that attributes immigration’s effect on fiscal preferences to self-interest rather than to group identities or feelings about immigrants’ use of public goods services per se would not predict any difference between how the fiscal preferences of those hostile to and sanguine about immigration would react to increases in immigration. If people in states with many Mexican immigrants are become less supportive of taxation simply because it alters the marginal financial costs and benefits to them personally, there is no obvious reason that the magnitude of these effects should vary depending on how one feels about immigrants. Instead, we should observe (H3) significant interactions between the level of Mexican immigration and some measure of individual income. This is because individuals with higher incomes experience a higher increase in tax burden to fund additional public goods and services that low-income immigrants benefit from. The hypothesis is that Mexican immigration causes a greater rightward

shift in the fiscal preferences of individuals with higher incomes than of individuals with lower incomes.

There is almost no empirical research testing these or even related hypotheses concerning the impact of racial or ethnic context on public preferences over fiscal policy. Large related literatures, described more fully in Chapter 1, have examined the effects of immigration on public support for the welfare state in Europe and of local and state minority population shares on attitudes toward welfare policy in the United States (Stichnoth and van der Straeten 2010). Yet outside of the case of black population shares in the United States, which are consistently found to be correlated with reduced support for redistribution, this body of research is quite divided. So the well documented “racialization” of attitudes toward some redistributive policies (e.g. Gilens 1999) may apply specifically to the case of blacks in the United States and public welfare and not carry over to the presence of Latinos or other public spending domains (cf. Fox 2004; Hero and Tolbert 1996). Even findings linking states’ black percentages to public attitudes about welfare are difficult to interpret because it is not clear to what extent respondents view welfare as a state policy and because questions frequently prime considerations about aggregate national spending rather than spending in one’s own state.

Consequently, there is little in the way of direct empirical support for this proposition or even for more general claims about analogous effects of ethnic diversity. The one exception (Trounstine n.d.) analyzes General Social Survey data from the late 1990s and early 2000s and observes a negative association between the level of diversity in respondents’ cities and their support for spending in three categories controlled in large part by local government: highways and bridges, law enforcement, and parks and recreation. Though Trounstine’s study does not speak to the impact of immigration in particular, focusing instead on diversity, it is an important as a first step toward observing the impact of ethno-racial diversity on public attitudes toward public goods spending.

Still, an important limitation of Trounstine’s data calls into question whether her findings actually gauge the impact of local ethnic diversity on local fiscal policy preferences, the mechanism implied in all pertinent theories. The General Social Survey questions she analyzes are worded as follows: “We are faced with many problems in this country, none of which can be solved easily or inexpensively. I’m going to name some of these problems, and for each I’d like you to tell me whether you think we’re spending too much money on it, too little money on it, or the right amount” and followed by references to each of the three items.

The explicit reference to “problems in this country” suggests that respondents could have been primed to think about these issues nationally rather than as related to their own localities. Though it is plausible that local diversity would influence views about national spending, this is not the prediction of any model linking diversity to fiscal attitudes. Instead, they all make the prediction that diversity will influence views about *local* spending. There is no obvious reason embedded in these theories that the aggregate national amount people prefer to be spent on roads should be more than minimally associated with local levels of diversity. The national level of diversity is obviously constant across cities and should be the operative factor in determining to what extent additional spending benefits out-group members, results in public projects whose form departs from natives’ preferences, fosters withdrawal from national politics, and alters the median income voter’s economic calculation of national taxation and redistribution’s costs and benefits. It is possible that the local level of diversity impacts perceptions of the national level (cf. Wong 2010), but there is no evidence that this is in fact the mechanism through which local

diversity influences preferences over aggregate national spending. The correlations Trounstine reports are therefore not readily interpretable in light of existing theories.

Trounstine offers two footnotes to defend the use of these questions as measures of preferences on “public goods items that respondents might assign to local government” (p.12), but this is beside the point. Even if respondents do habitually view these spending categories as local responsibilities, the GSS questions can easily be taken to solicit opinions about how much is spent in the national aggregate rather than in one’s own locality. It is also questionable whether research she invokes by Atkeson and Partin (2001) and Schneider and Jacoby (2003) actually sustains voters’ ascription of these policy domains to “non-federal” (a term used in neither study) governments.¹⁹ And since both studies deal only with a state-federal distinction they provide no evidence concerning voters’ ascription of or preferences over local governments’ responsibility for the spending categories they largely control. Schneider and Jacoby even discuss this distinction, stating, “Our survey questions asked respondents only about national and state governments. Local governments were not mentioned. This strategy was deliberate...” and mention the that state-federal distinctions have been “emphasized” in American federalism and “modern political rhetoric” (p. 251). Thus not only do the authors not regard their study as a reflection of federal-nonfederal distinctions generally, they are explicit about their motivations to focus on the state-federal distinction rather than the local-state or local-federal division of duties. Thus it seems unlikely that GSS respondents would have ascribed the policy areas Trounstine analyzes to city rather than to state governments.

Thus although most theories linking Mexican immigration and diversity more generally to the provision of public goods presume a mediating role for public opinion, no existing research has tested whether the public’s fiscal attitudes respond to immigration or to other contextual racial factors. I proceed to an explanation of my approach to this question.

¹⁹ Specifically, Trounstine claims that Atkeson and Partin suggest that developmental policies “are seen by voters as non-federal responsibilities. [Atkeson and Partin] also find that respondents give non-federal officials responsibility for the economy” (fn. 11, p. 12). In fact, their finding with respect to public opinion is that New Mexico residents surveyed in 1995 accorded more responsibility to New Mexico’s governor than its senators for “New Mexico’s economy” – hardly a surprise, though perhaps more surprising is that 11% of the sample held senators more responsible and 37% believed responsibility to be shared equally. None of the other categories analyzed in the article is similar to spending on parks and recreation or on transportation, the policies the GSS questions ask about, and the results suggest that a large share of voters believe the federal government has much more responsibility for some local policies (e.g. crime prevention) than it actually does. Schneider and Jacoby’s (2003) analysis of a 1999 Survey of *South Carolinians* also does not really show that “nearly 3/4ths of their respondents view the maintenance of ‘public roads, bridges, dams, and the like’ as a non-federal responsibility and better than half view ‘reducing crime’ and ‘reducing unemployment’ as non-federal.” Respondents were not asked to guess whether federal or state governments actually have responsibility over each domain. They were asked how they believed these responsibilities *should* be allocated: “Do you think the federal or the state government should take the lead in trying to...” (Appendix A, p. 261), and, while it is possible that these opinions reflect respondents’ knowledge of the real division of power, this possibility is not explored.

DATA AND METHOD

Independent Variables The data used to create all contextual variables used in the analysis come from my computations from American Community Survey microdata available from IPUMS. These variables are self-explanatory and are aggregated to the U.S. state level in 2006, 2008, 2010, and 2012. Descriptive statistics for these variables are shown in Table 5.1. This period is chosen to match the period for which the survey data used to create the dependent variable are available (see below). Usefully, whereas Mexican immigration had risen sharply for decades through 2006, the U.S. recession combined with a slowing of Mexican population growth to reduce the level of Mexican immigration for the next several years (Passel 2009). Thus whereas in earlier parts of the dissertation I leveraged variation in the degree to which different localities experienced increases in their Mexican immigrant populations, here I am able to compare not only units with different levels of increase but units whose Mexican immigrant population shares decreased as well. Though most of my analysis is based on fixed-effects regressions, I also make use of Mexican government data (INEGI's Anuarios Estadísticos) and of survey data from the Mexican Migration Project to construct an instrumental variable for Mexican immigration. For more information on these data sources, please consult Chapters 2 and 3 [pages].

Data for all individual level independent variables and for the dependent variable come from the 2006, 2008, 2010, and 2012 waves of the Cooperative Congressional Election Study's (CCES) common content. The CCES has surveyed used a sample matching technique to conduct a nationally representative Internet survey of tens of thousands of Americans in the run-up to presidential and mid-term general elections and then again after the election. The CCES provides basic demographic information on age, sex, educational attainment, household income, homeownership, and race/ethnicity. It also asks the standard American National Election Study question about partisan identification, which I use to construct a re-scaled measure of party affiliation that runs from 0 = strong Democrat to 1 = strong Republican. I also use a re-scaled version of the standard five-point liberal-conservative ideology question, where 0 = very liberal and 1 = very conservative.

Table 5.1: Descriptive Statistics for Variables Used in the Analyses (Data Pooled Years 2006, 2008, 2010, 2012)

Variable	Mean	25th Percentile	50th Percentile	75th Percentile	Standard Deviation
% Mexican FB	3.3%	0.7%	1.5%	3.8%	3.7%
% Black	11.8%	5.5%	11.1%	14.6%	7.9%
% Asian	4.1%	1.8%	2.8%	4.8%	4.0%
% Latino non-Mexican FB	10.7%	3.5%	6.6%	17.2%	8.8%
% FB non-Mexican	9.0%	3.8%	6.6%	14.1%	6.1%
% College Degree	19.9%	17.9%	19.4%	22.0%	3.2%
Mean HH Income (2010 dollars)	39,212	33,671	37,499	44,496	6,620
Unemployment Rate	0.086	0.065	0.086	0.108	0.026
Log Population	15.91	15.42	15.97	16.69	0.87
Female	0.53	0.00	1.00	1.00	0.50
Own Home	0.65	0.00	1.00	1.00	0.48
Educational Attainment	3.16	2.00	3.00	5.00	1.48
Age	46.73	33.00	47.00	59.00	16.04
Household Income	59,332	27,775	52,250	81,000	39,397
Black	0.11	0.00	0.00	0.00	0.32
Asian	0.01	0.00	0.00	0.00	0.10
Party ID	0.48	0.17	0.50	0.83	0.36
Ideology	0.55	0.50	0.50	0.75	0.26
Support Path to Citizenship (0 or 1)	0.41	0.00	0.00	1.00	0.49
Oppose Increased Border Patrol	0.40	0.00	0.00	1.00	0.49
Oppose Police Question Suspected Ill Imm	0.55	0.00	1.00	1.00	0.50
Immsupport (Index of Previous Three)	0.45	0.00	0.33	0.67	0.37
% Spending Cuts	60.95	48.00	56.00	82.00	25.72
% Sales Tax Increases	58.42	48.00	54.00	76.00	26.09
<i>DV</i>	60.08	49.00	56.50	75.00	21.74

The 2006, 2010, and 2012 CCES asked questions tapping respondents' views on immigration policy. No questions about immigration policy are available in the 2008 common content. Moreover, only in 2010 and 2012 were identical questions asked. To create an index of immigration attitudes (*immsupport*) in 2010 and 2012 I average responses to the three items from a repeated question that asked respondents "What do you think Congress and the government should do about immigration?" and instructed them to select all items that applied. The three items that repeated in both years were "Grant legal status to all illegal immigrants who have held jobs and paid taxes for at least 3 years, and not been convicted of any felony crimes," "Increase the number of border control on the US-Mexican border," and "Allow police to question anyone they think might be in the country illegally." I coded each response dichotomously where 0 meant disagreeing with granting legal status and agreeing both with increased border enforcement and allowing police to question suspected illegal immigrants and 1 meant support for legalization and opposition to both enforcement measures. I then took the mean of these three responses. The three items have an alpha reliability coefficient of .68 and are correlated with one another at between .33 and .39. Descriptive statistics are provided in Table 5.1.

Dependent Variable The dependent variable in all analyses is constructed from two questions about how respondents prefer that their own states confront budgetary deficits. The questions share a preamble:

If your state were to have a budget deficit this year it would have to raise taxes or cut spending, such as on education, health care, welfare, and road construction.

The first asked,

What would you prefer more, raising taxes or cutting spending? Choose a point along the scale from 100% tax increases (and no spending cuts) to 100% spending cuts (and no tax increases). The point in the middle means that the budget should be balanced with equal amounts of spending cuts and tax increases. If you are not sure, or don't know, please check here.

The second asked,

If the state had to raise taxes, what share of the tax increase should come from increased income taxes and what share from increased sales taxes? Choose a point along the scale from 100% from sales (and none from income) to 100% from income (and none from sales). The point in the middle means that any increase in taxes should come equally from sales and income taxes. If you are not sure, or don't know, please check here.

82% of the sample across the four years answered the questions. To construct the dependent variable, I recode these variables so that 100 = all spending cuts / all sales taxes (in both cases the most fiscally conservative response) and 0 = all tax increases / all income taxes (in both cases the most fiscally liberal response). I then take the mean of responses to the two questions to create a variable I term *fiscal*. For descriptive statistics of these variables, please see Table 5.1.

These questions have several useful properties for present purposes. Importantly, they question solicits opinions about one's own state and not about fiscal policy in the national

aggregate, and so any associations between state contextual variables and responses can be sensibly interpreted in light of the existing theories I invoked. They also pose a clear tradeoff between taxation and spending, which may have prevented some respondents from unthinkingly resisting any spending cuts. Finally, they refer explicitly to two categories of spending that are non-excludable public goods – education and roads, and, one that is to non-excludable to a degree (immigrants can be excluded from health care provision but not from emergency health care). On the downside, the reference to “welfare” may have evoked concerns about redistribution rather than public goods provision. Moreover, since they refer to state-level spending, they are not directly comparable to claims about “local” diversity and public goods provision that I have explored in earlier chapters.

Some might argue that statewide increases in immigration might be less perceptible than those in one’s city or county. On the other hand, I have argued elsewhere (Levy n.d.) that the use of smaller units may increase bias at least as much as they introduce it. One risk is a violation of the stable unit treatment value assumption: immigration elsewhere in the state or in a neighboring county or city may influence public attitudes and bias causal inferences. It is also true that states have often been the locus of immigration-related debates, as California’s experience with Proposition 187, several states’ efforts to sue the federal government for the cost of providing services to illegal immigrants, and recent state crackdowns on illegal immigration make clear. At a minimum these questions are useful in gauging how state demographics influences fiscal attitudes. No other questions I am aware of furnish the advantages they confer.

Method Because I am interested in the effect of immigration on natives’ attitudes, I restrict the sample to those who reported in the CCES that they were born in the United States. And because Latinos’ reactions to Mexican immigration are likely to differ substantially from those of other groups, I restrict the sample also to those who do not report Latino ethnicity. Unfortunately, only the 2010 and 2012 CCES followed up on a single question about race/ethnicity that asked respondents to choose between white, black, Asian, Hispanic, and several other categories with a question asking about Hispanic ethnicity. In each of those years several hundred respondents who self-identified on the first race/ethnicity question as “white” then reported Hispanic ethnicity. However, in the 2006 and 2008 CCES this follow-up was not available. For comparability over time, I therefore retain only the first question as a measure of race/ethnicity and use this to restrict the sample. This means that it is likely some of those I have retained in 2006 and 2008 would have identified as Hispanic had they been offered this follow-up. In 2010 and 2012 these respondents amount to approximately 2% of the white sample.

The main analysis uses fixed-effects regression where the key independent variable is the percentage of a state’s population that is Mexican foreign-born and the dependent variable *fiscal*. The inclusion of fixed-effects confines the analysis to comparisons of co-variation within states over time. Fixed-effects rule out spurious relationships emerging from stable differences between states that are correlated with their levels of Mexican immigration and their citizens’ fiscal preferences. I also include controls for a variety of contextual and individual variables that could vary over time. Although there is a consequent potential for post-treatment bias, I show results with and without these controls to help ensure that differences are minor. I also attempt to account for some unobserved sources of endogeneity by controlling for year fixed-effects in all specifications and, where possible, testing the sensitivity of my results to the inclusion of state-specific linear time trends. The former account for any overall trends in the dependent and independent variables while the latter account for any approximately linear trends within states over time. Relative to the great majority of topical research in political science, which mostly

draws on random-effects specifications that do not rule out time-invariant confounds, or even depends on simple cross-sectional regressions, these specifications are very conservative.

Still, unobserved confounds could be present among the universe of factors that co-vary over time with demographics and influence the dependent variable and are not accounted for with my control variables or by the state-specific linear time trends. In previous chapters I have argued for the use of an instrumental variable to overcome these problems. However, the birth cohorts instrument I used earlier is far weaker during this time period, partly because network ties linking Mexican migrants' sending and destination regions weakened in the 1990s and partly because the U.S. recession appears to have caused somewhat idiosyncratic shifts in the Mexican-born population in the U.S. Moreover, the instrument was designed for variation in immigration over long periods (earlier, I used decades). Here the time intervals are only two years each and the total interval is only six years. Since the birth cohorts-determined supply of eligible migrants is very similar in proximate years, there is little variation to draw on over small time intervals. However, I am still able to test the robustness of some results to a variant of the birth cohorts instrument. The instrument for each U.S. state is the sum of all birth cohorts lagged 18-75 years in each Mexican state, each weighted by the percentage of immigrants born in each Mexican state who traveled to each U.S. state until 2006. For details on the reasoning behind this technique, please see Chapter 2. The instrument is strongly predictive in random-effects specifications but is not sufficiently powerful to use in most fixed-effects specifications. However, it approaches being powerful enough in fixed-effects specifications during the post-recession recovery period (2010-2012) when the sample is restricted to states with a population of greater than 1,000,000. Thus where the instrument has at least some power I test whether the basic result holds up in instrumental variables specifications. I discuss these findings and other robustness checks after presenting the main results.

RESULTS

I begin by discussing results bearing on the chapter's first hypothesis, that higher levels of Mexican immigration in a state would decrease its residents' support for taxation and public spending and would cause residents to opt for less progressive forms of taxation if forced to choose. Table 5.2 shows the results of fixed-effects models that test this hypothesis. All models include dummies identifying each state and each year and pertain to all 50 states and the years 2006, 2008, 2010, and 2012. To account for the correlation of residuals within states, I cluster the standard errors by state. Thus bear in mind that the confidence intervals on these results are derived from four temporal observations on fifty clusters, for a total of 200 contextual units. The first column displays the association between the share of a state's population that is Mexican foreign-born and *fiscal* with no controls included. Since the Mexican FB variable is scaled 0-1 and the dependent variable is scaled 0-100, the bolded coefficient implies that a one percentage point increase in the Mexican foreign-born population share would yield a one point increase in the average resident's response to these questions. This is easily statistically significant and supports H1 since higher values of *fiscal* mean more support for spending cuts and for sales taxes rather than income taxes.

Table 5.2: Effect of Mexican Foreign-Born Population Share on U.S. Native Non-Hispanics' State Budgetary Conservatism

	(1) No Controls	(2) Contextual Controls	(3) Contextual and Demographic Controls	(4) All Controls	(5) No Controls	(6) Contextual Controls	(7) Contextual and Demographic Controls	(8) All Controls
<u>Contextual Variables</u>								
Mexican FB %	100.1909^{***} (33.6194)	84.4648^{**} (37.0974)	80.1946^{**} (39.1112)	96.1258^{**} (36.3269)	83.0788^{**} (36.4971)	119.7483^{**} (47.3933)	102.1683^{**} (48.3857)	135.8153^{***} (44.7704)
Black %		-18.7089 (34.8117)	-11.6457 (33.8177)	-6.5677 (31.7995)		25.5378 (38.4272)	22.2225 (37.0183)	26.9064 (36.7671)
Asian %		-116.3419 ^{**} (52.7002)	-68.1185 (55.1738)	-82.6621 (51.1277)		25.8534 (77.3042)	47.4814 (74.2523)	-31.7433 (79.9261)
Latino % (Non Mexican)		19.7228 (29.8095)	14.8723 (31.8293)	37.0932 (30.2342)		112.1240 ^{**} (42.6874)	106.1997 ^{**} (47.0931)	92.2301 ^{**} (38.0730)
FB% (Non-Mexican)		18.4061 (45.1168)	-3.1492 (49.7072)	2.0431 (49.3426)		40.1617 (50.9774)	32.0846 (52.3300)	30.9048 (54.1171)
College Deg %		-12.8590 (19.1518)	-6.8025 (22.4609)	-14.9929 (20.2511)		-9.0340 (21.9910)	-3.1863 (25.3203)	-8.8929 (22.8486)
Mean HH Inc		-0.0001 (0.0001)	-0.0001 (0.0001)	-0.0001 (0.0001)		-0.0000 (0.0001)	0.0000 (0.0001)	0.0001 (0.0001)
Unemployment Rate		-25.2839 [*] (14.8566)	-25.2192 (15.6927)	-13.5647 (15.6772)		-18.3769 (16.4871)	-12.3498 (16.5861)	-5.3216 (18.0376)
Log Pop		1.1595 (5.5487)	1.6457 (5.7636)	-4.3419 (6.0220)		1.9059 (11.2876)	-1.0784 (10.8230)	-16.5760 (11.0825)
<u>Individual Controls</u>								
Female			-3.0306 ^{***}	-1.0193 ^{***}			-3.0321 ^{***}	-1.0221 ^{***}

			(0.2720)	(0.2977)		(0.2725)	(0.2987)	
Owns Home			4.1006*** (0.2901)	2.3901*** (0.2753)		4.0979*** (0.2917)	2.3837*** (0.2761)	
Education			-1.2134*** (0.1454)	-0.5873*** (0.1093)		-1.2151*** (0.1458)	-0.5868*** (0.1093)	
Age			0.0337*** (0.0081)	-0.0198** (0.0077)		0.0335*** (0.0081)	-0.0201** (0.0078)	
HH Income			0.0000*** (0.0000)	0.0000*** (0.0000)		0.0000*** (0.0000)	0.0000*** (0.0000)	
Black			-5.4083*** (0.4446)	1.0611*** (0.3046)		-5.4235*** (0.4452)	1.0488*** (0.3059)	
Asian			-2.2492** (0.9448)	-0.5717 (0.8181)		-2.2308** (0.9297)	-0.6158 (0.8249)	
Party ID				13.5739*** (0.3998)			13.5672*** (0.3978)	
Ideology				24.4550*** (0.6786)			24.4636*** (0.6767)	
Constant	60.9311*** (0.4451)	53.9132 (83.3239)	44.4515 (86.6735)	111.6689 (90.1422)	-313.8506*** (98.8325)	-197.8468 (585.4762)	103.4183 (548.3381)	27.3177 (552.2688)
<i>State Linear Time Trends</i>	No	No	No	No	Yes	Yes	Yes	Yes
<i>N (individuals)</i>	124303	124303	109305	103198	124303	124303	109305	103198
<i>adj. R²</i>	0.016	0.016	0.054	0.266	0.017	0.017	0.055	0.266

Note: Standard errors, clustered by state, in parentheses. Results are for years 2006, 2008, 2010, and 2012 and include all 50 states in each year. All models include state and year fixed-effects (not shown).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The second column includes contextual controls for the black population share, the Latino population share, the Latino population share that is not Mexican foreign-born, the foreign-born population share that is not Mexican, the share of a state's population that holds a college degree, the mean household income (substituting the median does not alter any result), the unemployment rate, and the natural log of the state population. As can be seen at a glance, the effect of the Mexican foreign-born population share remains quite strong in the predicted direction. By contrast, only the unemployment rate and the percent Asian are statistically significant among the controls, both predicting more liberal fiscal attitudes. The non-impact of the percent black deserves special mention in light of the large literature tying black population shares to lower white support for welfare. In fact, when the fixed-effects are removed, the percent black does have a small but statistically significant positive association with *fiscal*, which is consistent with previous research. Its insignificance in the fixed-effects regressions is likely due to the limited variation in this variable within states over time. It is beyond the scope of this research to say whether results in prior literature that depend on negative cross-state correlations between black population share and support for welfare spending are in fact credible.

The third column adds an array of individual demographic controls. These relate to the dependent variable in theoretically sensible ways. Females, those with more education, blacks, and Asians express more liberal fiscal preferences. Homeowners, those with higher income, and older respondents harbor more conservative preferences. All of these effects are statistically significant. The key result for the Mexican population share remains virtually unchanged. By contrast, none of the effects of the other contextual variables remains statistically significant. This is clear evidence for the distinctive importance of Mexican immigration as a demographic force that shapes Americans' fiscal attitudes.

The fourth column adds controls for individual partisan identification and liberal-conservative ideology. These controls help account for any changes in the political composition of states' populations over the period of study. They also risk introducing post-treatment bias since either orientation could be influenced by Mexican immigration. On the other hand, as enduring political identities, they are likely to be less subject to short-run changes than attitudes about fiscal policy. In fact, these controls do alter the relationship of some demographic factors to *fiscal*. But, if anything, they slightly strengthen the estimated effect of Mexican immigration on *fiscal*. Finally, the last four columns test whether these results withstand controls for unobserved state-specific time trends. The key result is remarkably robust, even strengthening though not to a statistically significant degree.

I turn next to a test of the second hypothesis, which is that there would be a statistically significant interaction between an individual's support for immigration and a state's level of Mexican immigration. If group attitudes are an important factor in linking Mexican immigration to fiscal preferences, then the more pro-immigrant a respondent, the less an increase in his state's immigrant population should influence his fiscal attitudes. The more anti-immigrant a respondent, the more we would expect an increase in immigration to his state to induce opposition to public spending and taxation and preferences for regressive taxation. This hypothesis also receives strong and robust corroboration, as shown in Table 5.3.

Table 5.3: Heterogeneity in the Effect of Mexican Immigration on Fiscal Preferences by Level of Support for Immigration, 2010 and 2012

	(1) No Controls	(2) Contextual Controls	(3) Contextual and Demographic Controls	(4) All Controls	(5) All Controls + Controls for Other Interactions With Mexican FB	(6) All Controls + Controls for Other Interactions with Immsupport
<i>Main Effects</i>						
% Mexican FB	157.1347^{***} (56.0793)	239.3975^{***} (62.1633)	245.5855^{***} (59.1434)	289.8502^{***} (51.8514)	255.8315^{***} (53.9120)	232.8134^{***} (57.8362)
Immsupport	-19.8107^{***} (0.4581)	-19.8060^{***} (0.4629)	-18.2840^{***} (0.5173)	-9.0594^{***} (0.5030)	-9.5700^{***} (0.5294)	-18.1236^{***} (1.7131)
% Non-Mexican FB Latino		55.8650 (47.2527)	98.8733 ^{**} (46.1886)	113.2483 ^{**} (49.6231)	111.9100 ^{**} (49.7478)	98.0880 ^{**} (44.2583)
% Non-Mexican FB		-90.0271 (71.3183)	-113.9502 (70.2081)	-99.6796 (73.6785)	-98.3264 (73.7226)	-66.6135 (75.4448)
Unemployment Rate		-23.3689 (33.8914)	-26.8189 (33.8526)	-33.1274 (34.6702)	-33.2014 (34.6970)	-40.3219 (32.8599)
Party Identification				10.3434 ^{***} (0.4847)	10.5452 ^{***} (0.6286)	
Libcon				21.8592 ^{***} (0.8497)	20.0083 ^{***} (0.9817)	
<i>Interactions</i>						
Mexican FB*Immsupport	-60.2224^{***} (7.0706)	-60.3991^{***} (7.0518)	-60.5023^{***} (6.9274)	-41.2842^{***} (5.1769)	-24.8407^{***} (6.0503)	-70.5360^{***} (16.2020)
Mexican FB * Libcon					56.7634 ^{***} (12.9657)	
Mexican FB * Party ID					-6.7883 (9.2196)	

Unemployment * Immsupport						5.4803 (16.2671)
% Non-Mexican FB * Immsupport						-12.9861 (10.0781)
% Non-Mexican FB Latino * Immsupport						7.2841 (9.5067)
Constant	66.5865*** (0.9063)	-357.3476 (249.7771)	-382.6877 (259.9280)	-256.3215 (219.7015)	-251.1561 (220.3265)	-263.6036 (219.9113)
<i>N</i>	75827	75827	65897	62321	62321	65897
adj. <i>R</i> ²	0.157	0.157	0.166	0.288	0.288	0.166

Note: Standard errors, clustered by state, in parentheses. Results are for years 2010, and 2012 and include all 50 states in each year. 2006 and 2008 are excluded because consistent questions tapping individuals' immigration attitudes were not included. All models include state and year fixed-effects (not shown).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In the interest of space, I do not display the results for the control variables shown in Table 5.3 and confine the table to results to variables involved in interactions in one of the specifications. In all these models, the bolded coefficient on % Mexican FB can now be interpreted as the predicted impact of an increase in Mexican foreign-born population share on *fiscal* among respondents who expressed the most staunchly anti-immigrant views – that is, who scored a zero on *immsupport*. The coefficient on *immsupport* can be interpreted as the predicted effect on *fiscal* of moving from the most anti- to the most pro-immigrant position on this variable among those living in states with virtually no Mexican immigrants.

The interaction between these two variables indicates by how much the effects of each of these variables depends on the level of the other. For example, if we switch *immsupport* from zero to one, the most pro-immigrant stance, we subtract somewhere between 25 and 75 from the coefficient on the Mexican foreign-born population share. As predicted, all of these interactions are negative, meaning that, as predicted, anti-immigrant citizens' fiscal preferences take a more conservative turn in the face of Mexican immigration than pro-immigrant citizens' fiscal preferences do. Even those scoring in the most pro-immigrant category, however, are predicted to become more fiscally conservative when confronted with Mexican immigration. This result should not be interpreted, however, as meaning that even the most pro-immigrant citizens would react negatively to Mexican immigration. A large number of those who dislike the idea of police scrutiny of illegal immigrants, who feel border enforcement need not be increased, and who support a path to citizenship for the highly vetted illegal immigrant specified in the CCES may still bristle at Mexican immigrants' use of public goods and services and to illegal immigrants' access in particular. A question that isolated those who would not react this way from those who would might have allowed us to identify a subset of the population for whom Mexican immigration would have no impact on fiscal preferences and resulted in even larger estimates of the interaction effect. Nonetheless, there is clear support across specifications for heterogeneity in pro- and anti-immigrants' responses to immigration, with both responding by becoming more fiscally conservative but the magnitude of anti-immigrant publics' response in this regard significantly outstripping that of pro-immigrant publics.

In addition to all the controls shown in Table 5.2, the last two columns of Table 5.3 include verify that the key interaction effect is not a spurious byproduct of similar interactions between the Mexican immigrant population share and other political orientations that are associated with immigration attitudes. The nature of these interactions is interesting in and of itself because of the possibility that core political dispositions help determine how individuals respond to demographic change, but they are also alternative explanations for the interaction effect of immigration level with individual immigration attitudes that I have demonstrated. I also verify that the effect is not a reflection of interactions between contextual factors other than the Mexican immigrant population share with individuals' immigration attitudes. In particular I examine the state unemployment rate and as a reflection of economic conditions, the non-Mexican foreign-born population share, and the non-Mexican Latino population share. Each of these factors is correlated with the level of Mexican immigration and could also have a hand in dividing pro- and anti-immigrant publics' fiscal preferences. In a weaker economy, anti-immigrant citizens might become especially prone to cut spending that benefits “them” while pro-immigrant citizens would likely not form such a judgment. To the extent that prejudice against Latinos or aversion to sharing with non-natives generally or with immigrant groups other than Mexicans drives a wedge between the fiscal preferences of pro- and anti-immigrant publics,

we might find a significant interaction between these factors and attitudes about taxation and spending.

Notably, the result survives controls for each type of interactions. It remains statistically significant in all specifications, even though the correlation of liberal conservative ideology and *immsupport* is .49. Ideology also shows a strong and significant interaction with the Mexican foreign-born population size. This is logical since conservatives may be more concerned about perceived use of public services by those deemed undeserving or be more psychologically invested in protecting the boundaries of the national community from outsiders. However, there is no significant interaction with partisanship. Since partisanship and ideology are correlated in the sample at approximately .7, caution in order. The other interactions I test in Model 6 between contextual factors – the unemployment rate, the foreign-born population share that is not of Mexican origin, and the Latino but non-Mexican foreign born population share – and *immsupport* are all small and statistically insignificant. The latter two suggest the distinctive role of Mexican immigrants in shaping fiscal preferences, rather than of Latino presence or the foreign-born population share more generally.

This supports for the in-group preference mechanism since the degree to which one's fiscal attitudes are responsive to immigration from Mexico depends in substantial measure on one's attitudes toward immigrants. However, a different interpretation is also plausible, and it is impossible using survey data with no repeat observations on individuals over time to say for certain which is correct. It is possible that increases in Mexican immigration differentially altered the immigration attitudes or, perhaps less likely, the ideological identifications of fiscally liberal and conservative citizens instead of differentially altering the fiscal preferences of pro- and anti-immigrant citizens and of liberals and conservatives. Those who already believed taxes burdensome and spending excessive may have become more hostile to immigrants that they viewed as additional fiscal strains while those harboring liberal fiscal attitudes may not have reacted this way. It is less clear why fiscal conservatives' and fiscal liberals' ideological self-identifications would have been influenced differently by immigration, but this cannot be ruled out.

Regardless of the mechanism at work, the implication of these interactions is that influxes of Mexican immigration have fostered greater constraint between immigration attitudes and fiscal preferences and between ideological self-identification and fiscal preferences. Beyond making the fiscal preferences of the public more conservative overall, they have increased the association between preferences over immigration with beliefs about appropriate levels of taxation and spending and between these beliefs and liberal-conservative identity. Both reflect greater sorting of the native, non-Hispanic public into cohesive ideological camps.

It is unclear how politically consequential these effects are. The best I can do here is to give some sense of their magnitude relative to the measures employed in this research. Consider that the largest Mexican immigrant state population shares are a little over 10%. Model 5 in Table 5.3 predicts that the gap between pro- and anti-immigrant publics' fiscal preferences is 2.5 points on the 100 point scale larger than it would have been absent any immigration from Mexico and that the gap between self-identifying strong conservatives and liberals is 5.6 points larger than it would have been. Given that the predicted gaps absent any immigration are 10 points and 20 points respectively, these figures translate into a 25% increase in polarization over fiscal preferences between ideological extremes and between the most pro- and anti-immigrant publics.

Finally, I hypothesized (H3) above that if immigration from Mexico operated on fiscal attitudes by altering the individual cost-benefit calculus associated with taxation, we should observe a stronger effect among those with higher incomes, who incur most of the tax burden, than among those with lower incomes. To test this, I re-estimated all the models shown in Table 5.3 but substituting an interaction of respondent household income with Mexican FB. Although the coefficients on the interaction terms were all positive, consistent with the idea that higher income individuals' fiscal conservatism intensifies in the face of Mexican immigration, in no instance was a statistically significant result obtained. In all cases, the income was positively and significantly associated with *fiscal*, but in no case did Mexican immigration significantly boost this association. This was true regardless of whether a control for the interaction of Mexican immigrant share and *immsupport* was included. Insignificant results were also obtained when I proxied for income using home ownership and when I used the log of household income instead of income. Thus there is little to support H3 in these results. To be sure, public preferences over how a state should handle a budget deficit are a function of one's own income, but this relationship is not made stronger by an influx of Mexican immigrants. This calls into question mechanisms invoking additional personal tax burden with no additional return to taxation as an explanation for the relationship between Mexican immigration and fiscal preferences, though it is still possible that this relationship is operative at the national level, as McCarty, Poole, and Rosenthal (2006) argue.

ROBUSTNESS

I subject these results to two robustness checks and then discuss why bias from the promulgation of state anti-immigrant laws poses little threat to their validity. First, I test the core results' robustness to the sample of states. Given the distinctive importance of California, Texas, and Illinois as receiving states for Mexican immigration, it is important to verify that my results are not driven solely by these states. Despite the increasing geographic spread of Mexican immigration in the past two decades (Singer 2004), my own compilation of IPUMS American Community Survey data for 2012 shows that those three states combined accounted for just shy of two-thirds of the Mexican foreign-born population residing in the United States. If immigration from Mexico influences the attitudes of liberals and conservatives differently, the liberalism of California and Illinois' non-Latino publics or the conservatism of Texas' could have a strong impact on the total effect. The fixed-effects specifications I have used help address this issue. Although California and Texas are extreme outliers in their Mexican foreign-born population shares, they are not outliers when it comes to changes in those shares between 2006 and 2012.

Second, I examine robustness to specification. I have argued for the superiority of a fixed-effects estimator in this case. This is because there is a strong likelihood that it will be impossible to account in a piecemeal fashion for all covariates that drive both the level of immigration and citizens' attitudes toward spending and taxation. However, some might be curious to know whether there is a cross-state relationship as well. On the other side, although the fixed-effects specifications I have relied on are conservative relative to modeling strategies used in most of the topical political science literature and are remarkably robust to a large battery of contextual and individual control variables as well as controls for unobserved state-specific linear time trends, it would be ideal to achieve identification through the use of an instrumental variable. I have made a number of arguments about possible endogeneity bias even in fixed-effects estimation when it comes to studying the impact of immigration and have claimed that

instrumental variables strategies can be a powerful way of addressing these issues. However, the techniques I used in Chapter 3 are only robust during the latter part of the period and then only weakly so. I therefore invoke these as corroboration for the results derived from the descriptive analysis rather than as the main body of evidence to support the chapter's main arguments.

California, Texas, Illinois The results are not sensitive to the exclusion of California, Texas, or Illinois, or any combination thereof. I re-analyzed Model 8 in Table 5.2 and Model 4 in Table 5.3, two models that include the full set of covariates. I omitted in turn each potential combination of these states. With respect to Model 8 in Table 5.1, the coefficient on Mexican foreign-born is 129.2 (SE=50.09) when California alone is omitted, 154.04 (SE=46.27) when Texas alone is omitted, and 132.42 (SE=44.19) when Illinois alone is omitted. When both Texas and California are omitted from the model, the key coefficient is 150.47 (SE=52.12). When both California and Illinois are omitted, it is 128.89 (SE=49.15). Excluding Texas and Illinois together yields a coefficient of 149.97 (SE=45.39). Finally, excluding all three states produces an estimate of 150.07 (SE=50.67). None of these estimates constitutes anywhere near a statistically significant departure from the main estimate shown in Column 8 of Table 5.1. We can therefore be confident that our confirmation for hypothesis 1 is not a mere byproduct of something distinctive about these states.

Re-analyzing Model 4 in Table 5.3 also generally confirms the robustness of the interaction effect to the exclusion of these states as well. Omitting California yields an estimate of -36.67 (SE = 7.68) on the interaction. Omitting Texas yields an estimate of the interaction of -48.47 (SE=17.37). Omitting Illinois leaves the result virtually unchanged (coefficient = -41.72, SE = 5.04). Omitting California and Texas together produces a coefficient of -35.74 (SE=15.15), while when California and Illinois are omitted the estimate is -37.64 (SE = 8.26). Without Texas and Illinois the estimated interaction is -43.04 (SE=5.45). Removing all three states from the analysis yields a coefficient on the interaction term of -38.03 (SE=19.68). The last result carries a p-value of .06 due to the reduced sample size but is statistically and substantively indistinguishable from the results shown in Table 5.3. I also explored the robustness of the last two columns' results to the omission of all combinations of these states. The point estimates remain virtually unchanged in all cases, and all estimates remain statistically significant at least at $p < .1$ except when California and Texas are removed together (coefficient = -18.62, SE = 16.50) or California, Texas, and Illinois are all removed (coefficient = -17.53, SE = 20.36) from model 5. However, the difference between these coefficients and those shown in column 5 of Table 5.3 are not remotely substantively or statistically significant.

Specification Both key results remain statistically significant at least at $p < .1$ and mostly at $p < .05$ in random-effects specifications. I re-estimate the first four models of Table 5.2 and all six models from Table 5.3 but omitting the state fixed-effects and instead adding a random intercept by state. I use the Stata 12 *xtmixed* command, grouping by state and leaving in the year dummies to account for trends in the data. I am forced to make one adjustment, dropping the control for the Latino non-Mexican population. These variables are correlated at approximately .8 across states in each year of the analysis, and the collinearity would make the estimated effects impossible to interpret sensibly. In fact, however, I did verify that the point estimates on Mexican FB change little with the inclusion of this variable, although the standard errors inflate so that some estimates are no longer significant at $p < .1$.

Starting with the re-estimation of models analogous to those presented in Table 5.2: with no covariates, the coefficient on Mexican FB is 29.66 with a SE of 10.09. When only the contextual controls are included, the estimate is 26.09 with a SE of 10.12. When individual

demographic controls are also included, the coefficient is 27.07 (SE = 10.09). Finally, when party and ideology are also controlled, making understatement of the total effect through post-treatment bias very likely the coefficient is 16.93 (SE = 9.68). The somewhat smaller size of the point estimates compared to the fixed-effects estimates may reflect the confounding influence of some time-invariant difference between historically high- and low-immigrant receiving states. Turning to the re-estimation of the interaction models analogous to those presented in Table 5.3: with no covariates, the key interaction is estimated to be -60.97 with a SE of 7.17. This is virtually unchanged when contextual controls are included (coefficient = -60.99, SE = 7.18), when individual demographic controls are also included (coefficient = -60.92, SE = 7.06), and only marginally weaker, though more precisely estimated, with controls for party and ideology (coefficient = -42.10, SE = 5.22). The interaction remains strong with controls for interactions of Mexican FB with ideology and party (coefficient = -25.54, SE = 6.02) and with controls for interactions between *immsupport* and the unemployment rate and the non-Mexican foreign-born population (coefficient = -58.80, SE = 6.21).

I am able to test the robustness of certain results to the use of the variant of the birth cohorts instrument described above. Since there is no valid instrument I am aware of for an individual attitude, I do not attempt to instrument for the interactions described above.²⁰ The instrument is predictive enough to use in a between-effects specification that includes only cross-sectional variation in the Mexican foreign-born population share ($F = 28.0$) but loses predictive power when within-state variation over time is included. Without using the instrument, the between-effects coefficient on the Mexican foreign-born population share is only 13.97 with a SE of 15.34. When the instrument is employed the estimate increases to 26.28 with a SE of 24.92. These coefficients are not significantly different but do suggest some negative bias in the cross-sectional estimates even if the instrument is not fully resolving endogeneity issues in these specifications. The estimates are highly sensitive to the case of California, and they rise to 25.83 (SE=17.33) without the instrument and 58.92 (SE=36.17) with the instrument, the last estimate falling just short of statistical significance at the .1 level. The estimates are also quite sensitive to the inclusion of Illinois. With both California and Illinois excluded the estimate becomes 77.34 with a SE of 39.82. Surprisingly, excluding Texas inflates the coefficient further, though not significantly so, to 87.36 (SE=62.31). In short, although the cross-sectional results all remain positive, the estimates are smaller than the results derived from using within-state variation only. This appears to be due in part to endogeneity bias, since all coefficients increase when the instrumental variable is employed, and also to a high degree of sensitivity to the inclusion or exclusion of individual states. On those grounds, I argue that the fixed-effects results are more credible, though in either case the best guess is that immigration from Mexico increases fiscal conservatism. Though the instrument is for the most part not sufficiently predictive for use in most fixed-effects specifications, the F-statistic reaches 8.0 when only data from 2010 and 2012 are included and the sample is restricted to states with populations in excess of 1,000,000 (there

²⁰ Attempts to use one (see, e.g., McLaren's 2012 analysis of the effect of opposition to immigration on trust in government in Britain) raise serious questions about whether the exclusion restriction is satisfied. McLaren uses a question about how many immigrant friends one has as an instrument for attitudes toward immigration. Although the instrument is highly predictive, there is no reason to suppose that causality runs in the direction its use assumes, let alone that there is no causal channel linking one's number of immigrant friends to one's trust in government other than the effect of having immigrant friends on one's support for immigration.

are 43 of these). The point estimate is 111.20, very similar to the results derived without the use of the instrument. This provides some additional reassurance that endogeneity is not driving the results shown in Table 5.2 though F-statistic is weak enough that the instrument may continue to contain up to between 15 and 20% of the bias in the OLS result, and of course the reassurance applies only to the subsample of the data for which the instrument can be used.

Potential for Bias from State Laws Cracking Down on Immigration Could state laws aimed at cracking down on illegal immigrant employment or requiring local or state law enforcement to check immigration status introduce bias into these results? As summarized on the National Council of State Legislatures' website, states passed hundreds of such laws, and the start of the flurry coincided with the beginning of the period of study. Certainly the states in which these sorts of laws were promulgated tended to be more conservative than average. And in at least some cases they appear to have significantly changed the Mexican immigrant population share. For example, I calculate that in the wake of Arizona's E-Verify and employer sanctions law (Bohn, Lofstrom, and Raphael forthcoming), between 2008 and 2010 the Mexican state's immigrant population share declined 1.6 percentage points from a high of about 10%, or by approximately 17%.²¹

The potential for such time-invariant partisan or ideological differences between states to confound the relationship of interest would be a major issue in an analysis that leveraged cross-sectional variation. But my use of state fixed-effects, which control for all such stable covariates, should go a long way toward allaying such concerns. State-specific linear time trends should additionally control for any temporally coinciding linear progressions of a state toward a more conservative or liberal stance on immigration policy and of its citizens toward a more liberal or conservative stance on fiscal policy. That the instrumental variable, where at least somewhat potent, produces results that are not discrepant from those in the main analysis offers further reassurance. Finally, given that anti-immigrant laws were overwhelmingly adopted in states controlled by Republican legislatures and reduced the immigrant population, it is likely that they would result in an understatement of the true effect of Mexican immigration on fiscal attitudes.

SUMMARY

This chapter has analyzed the impact of Mexican immigration on public preferences over state fiscal policy. The results strongly support the hypothesis that immigration from Mexico makes non-Hispanic native-born Americans more inclined to opt for spending cuts over tax increases and for sales tax increases over income tax increases as ways of addressing hypothetical budget deficits in their states. Given that the questions employed as the dependent variable in the analysis refer explicitly to three types of public goods in posing the spending-taxation tradeoff, this finding supports theories linking ethnic diversity generally and Mexican immigration in particular to reduced support for the public provision of goods and services, though I cannot rule out that part of the effect is attributable to increased hostility to welfare, which is also mentioned in the questions' preamble.

²¹ This raw figure is in the same ballpark as Bohn, Lofstrom and Raphael's (forthcoming) much more sophisticated estimate, using a synthetic controls method, that Arizona's likely illegal immigrant population (defined as Mexican immigrants in the country for a short time and without a high school degree) dropped by about 20%.

We also saw that the impact of Mexican immigration on citizens' attitudes depends on two individual predispositions. The fiscal attitudes of those who support greater enforcement and reject legalization as ways of confronting illegal immigration are affected to a significantly greater degree than are the attitudes of those who prefer more limited enforcement and support legalization. Self-identifying conservatives are also more affected than liberals. This is not to say that citizens far to the left on questions of immigration policy and ideological orientation respond to immigration by becoming more fiscally liberal. The results suggest that these citizens also adopt more conservative views on matters of taxation and spending but to a lesser extent than those who identify as strongly conservative or staunchly against legalization and in favor of robust immigration enforcement.

These results are consistent with explanations grounded in Social Identity Theory for the impact of immigration on public goods provision because those who are more hostile to immigration and hence more likely to be disturbed about immigrants' consumption of public goods and services respond more strongly to Mexican immigration than do those who are less hostile to immigration. Of course attitudes toward immigration are not tantamount to group attitudes, so it is possible also, for example, that those who believe immigrants are more burdensome on the national economy react more strongly to immigrants' use of public goods than do others, even if they have nothing against immigrants or Latinos in particular and even if they do not care whether public goods are used by immigrants or natives. Yet the statistical insignificance of interactions between the level of immigration and household income and homeownership calls into question explanations that invoke immigrants' impact on the income distribution and their effect on the return citizens reap for the taxes they pay.

Further research is needed to determine if a measure of immigration attitudes capable of tapping more extremely favorable or hostile views would identify some very pro-immigrant natives who respond to immigration with increased support for taxation and public goods spending. Some research on the impact of immigration on attitudes toward immigrants (Newman 2013) finds that although citizens living in areas with little prior experience of immigration react negatively to immigrant influxes, citizens living in contexts with a long history of immigration actually respond favorably and become more supportive of immigration. The results here suggest that this may not be the case when it comes to the impact of Mexican immigration on natives' fiscal attitudes, but I cannot rule out that a more refined population subset consisting of the most pro-immigrant natives would not liberalize its fiscal attitudes in response to immigration from Mexico. The CCES sample of Latinos is neither large nor representative enough (it is far more heavily native-born than the true U.S. Latino population, likely in part due to English only interviewing) to determine, for example, whether Latinos would respond in this way.

More importantly, additional survey research could help resolve some lingering questions. Surveys offering repeated observations on individuals could also help disentangle the causal mechanisms at work in the interactions. Immigration from Mexico may have differentially influenced the fiscal attitudes of pro- and anti-immigrant citizens and of liberals and conservatives or differentially altered the immigration attitudes or ideological identifications of fiscal liberals and conservatives. Both of these mechanisms lead to greater sorting and ideological constraint and thus contribute to a kind of mass polarization. But it is theoretically important to know for sure where the source of heterogeneity lies.

More importantly, these results require corroboration using different questions, including queries that specifically ask about spending on public goods and that identify different

subnational units. Would the results differ if respondents were asked about education spending in their own district, or transit spending in their own city or county? It is impossible to know for certain, and there is no survey evidence to draw on at present to answer this question. It will also be important to corroborate this result during a different time period. One limitation of this study is that most of the evidence pertains to a period of great economic difficulty. It is possible that citizens' fiscal responses to immigration are sharper during periods of hardship than they are in times of plenty. Although I found no evidence of an interaction between the level of immigration and states' unemployment rates, it is possible that national economic conditions rather than state economic conditions are the operative force in this regard.

Still, the results presented in this chapter strongly suggest that Mexican immigration does reduce public support for taxation and public goods provision and contributes to a certain type of polarization of fiscal preferences across ideological categories and relevant issue positions. The dissertation therefore reaches a split decision: it confirms the effects on public opinion predicted by its guiding theories but finds that these effects may not translate into policy change. One reason for this, the countervailing influence of political power wielded by Mexican immigrants themselves, was considered in Chapter 4, and I have mentioned other potential explanations rooted in fiscal federalism, the low political efficacy of many of those most affected by immigration, and the stickiness of public policy generally. Of course there are methodological explanations for this divergence in my findings as well – different periods of study and different units of analysis – so it is important to be cautious in how to interpret these results jointly. In the concluding chapter, I summarize my theoretical and empirical contributions and offer a speculative interpretation of their political implications.

CONCLUSION

The impact of mass immigration from the developing world on policy and public opinion will no doubt continue to garner scholarly attention. It is a topic with important and enduring implications for a diverse array of social scientific theories linking demographic change to political phenomena. And it raises questions about how one of the defining features of our era – the unprecedented mobility of people across national boundaries – reconstitutes politics in host and sending societies.

I have sought to shed light on a critical component of this larger question by exploring what effect immigration from Mexico has had on the local production of public goods in the United States between 1980 and 2000. Several lines of social psychological and economic theory detailed in Chapter 1 converge on the prediction that Mexican immigration would have had a corrosive effect on public goods spending and taxation during the period in study. This expectation is also consistent with a recent empirical literature linking ethnic and racial diversity to lower provision of public goods (for a review, see Chapter 1). But, as I discussed in Chapters 1 and 2, the topical literature suffers from theoretical omissions and methodological flaws that call into question the validity of its inferences.

To address some of the key empirical issues, I used a novel instrumental variables design derived (Chapter 2) from variation in Mexican states' historic birth cohort sizes and persistent pre-determined networks linking Mexican states to distinct sets of U.S. localities to generate a source of plausibly exogenous variation the timing and location of Mexican immigration to the U.S. Implementing this design (Chapter 3) using population data derived from the decennial 1980, 1990, and 2000 Censuses and local fiscal data compiled from the 1982, 1992, and 2002 Census of Governments, I found no evidence to support a negative effect of immigration from Mexico on public goods spending. Instead, Mexican immigration appears, if anything, to have had a positive effect over this period on the amount local governments spend on public goods such as education and roads and a smaller positive effect on tax receipts overall. Though the resulting budgetary gap apparently prompted localities receiving greater influxes of Mexican immigrants to assume more public debt, the marginal cost of holding this debt did not increase. Thus strategic shifts from tax- to debt-funding (cf. Rugh and Trounstein 2011) appear a more likely interpretation of this result than immigration-fueled fiscal distress (cf. Alesina, Baqir, and Easterly 1999).

A critical question then becomes why my results in Chapter 3 were at odds with the predictions derived from theories connecting ethnic diversity in general and immigration from Mexico in particular with the provision of public goods. There are several classes of explanation. One is that these theories are inapplicable to the case of Mexican immigration. Given how apropos the case of Mexican immigration is to these theories, their inapplicability would raise serious questions about whether these broader theories are generalizable beyond particular cases – perhaps tribalism in the developing world and the distinctively fraught case of blacks in the United States. That is, instances where ethnic diversity has been linked to lower provision of public goods may not actually make the case that ethnic diversity would always or even usually have such effects. Consistent with this possibility, my replication and disaggregation (Chapter 3) of Alesina, Baqir, and Easterly's (1999) cross-sectional analyses indicate that the correlations they document between ethnic diversity and public goods spending do not emanate from Mexican immigration but primarily from the size of local black populations.

A second possibility is that these theories apply to Mexican immigration but are incomplete because they fail to consider important countervailing pressures on policy and other impediments to the sorts of policy changes they predict. In Chapter 4 I probed one element omitted from three of the four principal theories linking Mexican immigration to lower provision of public goods: the political influence of Mexican immigrants themselves. Economic models that explore the impact of low-skilled immigration on preferences over fiscal policy and immigration often pit natives, whose opposition to taxation increases because their share of redistributed income is diluted by an influx of immigrants who contribute less in taxes than they reap in redistributed income, against immigrants, who support more redistribution because it would impose a minimal tax burden on them relative to the amount of income they would receive. In their simplest form, these models assume universal voting – obviously an assumption that does not apply to the U.S. Mexican-born population, with its low rates of naturalization and traditional forms of participation. Nonetheless, a third of Mexican immigrants do become citizens, and their fiscal preferences appear to be quite liberal. Those preferences may push against and even overwhelm an increase in opposition to taxation and public goods spending among natives. My results suggest that the Immigration Reform and Control Act's contribution to a rise in naturalization rates during the 1990s led to significant increases in the amount localities spent on the provision of public goods and to increases in their property tax receipts. Though I the results are not precisely enough estimated to venture a clear guess at what impact of Mexican immigration would have had on local public goods provision between 1980 and 2000 if no Mexican immigrants naturalized, the point estimates generated in Chapter 4 indicate that naturalization may go a long way toward explaining the disconfirming results in Chapter 3. Moreover, naturalizations caused by IRCA involved a group of Mexican immigrants that had previously had no legal status in the U.S., meaning that they are likely a less politically involved group than those who naturalize under normal circumstances and therefore might have been expected to exert a smaller influence on local politics than other Mexican immigrants who naturalize.

Theories linking Mexican immigration to public goods provision may be incomplete in other respects as well. For example, the presumption that localities can cut back on spending on infrastructure, education, and basic services for their residents in order to align with immigration-induced shifts in public opinion conflicts with research on fiscal federalism (e.g. Peterson 1981). Local governments in the United States are constrained by competition to attract and retain a healthy tax base and so cannot adjust policies critical to this task simply to satisfy public whim. Recent experience with state and local legislation aimed at illegal immigration suggests that localities are more likely to address festering anti-immigrant sentiment with crackdowns on immigration, and even in these cases the policy response appears more symbolic than substantive. Even in centralized systems where fiscal federalist pressures do not apply, status quo policies are not so easily changed, and cut-backs on public goods would seem unlikely to generate long-term democratic popularity even if they are greeted with short-run enthusiasm in some corners.

A third possibility is that immigration from Mexico may not even alter the fiscal attitudes of the native public in the manner that all four theories require. All four theories predict that Mexican immigration makes U.S. natives less supportive of spending on public goods in their localities and more hostile to levying taxes needed to fund them. Even if these theories omit important reasons that such effects on public opinion would not yield congruent policy change, they might offer valid predictions about public attitudes. But on the other hand, whether

immigration is sufficiently salient most of the time to be a significant consideration in how the public thinks about local fiscal policy requires empirical verification, and there is virtually no empirical evidence to date bearing on whether diversity or immigration influences public attitudes on these issues.

Due to the unavailability of the necessary data, I am unable to test the impact of immigration from Mexico on public opinion at the local level and during the same time period as my examination of policy. However, the 2006-2012 Cooperative Congressional Election Studies asked respondents whether they preferred spending cuts or tax increases as a way of closing a hypothetical budget deficit in their own states and then gauged their preference for more regressive sales taxes versus more progressive income taxes. These questions served as dependent variables in my study in Chapter 5 of how variation in states' Mexican immigrant population shares over this time period were associated with native non-Hispanics' fiscal preferences. Here I find robust evidence to support the expectation that immigration from Mexico reduces support for taxation needed to fund the provision of public goods and increases endorsement of more regressive taxes over more progressive ones.

I also find that Mexican immigration has a larger effect on those more opposed to immigration, consistent with theories positing in-group bias or hostility to immigrants' use of public services as a mechanism. I do not find, however, that immigration more strongly impacts the fiscal preferences of the wealthy. This is inconsistent with some theories positing reduced individual self-interest derived from taxation as a mechanism linking Mexican immigration to fiscal preferences. Future research would benefit from survey questions directly tapping preferences over spending on public goods in their own localities and from survey measures designed to further probe the quite different mechanisms articulated in each of the four theories. It would also be useful to explore the contribution of immigration to polarization in the form of greater ideological constraint. I find that immigration from Mexico strengthened the association between liberal-conservative identification and attitudes toward fiscal policy and the association between preferences over immigration and attitudes toward fiscal policy. In other words, between 2006 and 2012,

Taken together, these results provide some explanation for why Mexican immigration between 1980 and 2000 did not have the expected effect on local governments' fiscal policy in the United States. Though I sustain social-psychological theories linking Mexican immigration to natives' fiscal preferences, there is either no consequent change in public policy or whatever change in policy does occur is overwhelmed by countervailing influences of Mexican immigration, for example, exertion of political pressure among those who naturalize. Of course these findings need to be corroborated in different temporal contexts, preferably with data on public opinion and policy from the same time period, with a consistent set of geographic units, and using different methods for causal inference. These are all critical avenues for further empirical research. On the theoretical side, these findings call attention to the great complexity involved in understanding how demographic change influences politics and policy. They also sound a note of caution about introducing facile theoretical premises that presume a straightforward translation of changes in mass preferences to public policy. By the same token, we should be cautious about interpreting associations between elements of demographic context and the nature of public policy as straightforward manifestations of how demography has influenced public attitudes, even when these interpretations strike us as intuitive or bear some relation to existing research.

Debates over immigration policy have generally focused on disagreements over immigration's social, cultural and economic impacts. My dissertation has turned attention to a potential political cost of immigration. Multiple theories lead to the prediction that immigration of those ethnically different from and poorer than the native majority will hamper governments' ability to invest in infrastructure and education and provide other goods and services to their constituents. My results suggest that such concerns are unwarranted. However, they also raise some more unsettling possibilities. If Mexican immigration leaves spending on public goods unchanged or even increases it while making the native public more opposed to public spending, it may foster a disconnect between public opinion and policy that could generate political dissatisfaction. The tendency for immigration to more tightly link ideological identities with policy attitudes toward immigration and fiscal issues alike may intensify existing divisions in the mass public. Immigration has long been viewed as an issue that cuts across ideological coalitions and spurs the formation of "strange bedfellow" coalitions. These coalitions can sometimes generate grand bargains over policy (Tichenor 2002). The more that immigration attitudes align with ideology and other issue preferences, the less likely it is that such bargains will be forged.

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