

Ethnic Concentration and Bank Use in Immigrant Communities

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Despite the many benefits of bank use, large portions of the U.S. population remain unbanked. One of the largest is immigrants, where the incidence of being unbanked is over 13% higher than among natives in 2001. We document growth in the nativity gap in bank use over time. We also test the importance of immigrant enclaves, defined as areas with high concentrations of immigrants from the same region, in explaining the increasing differential in bank use. Combining data from the Survey of Income and Program Participation, Census, and FDIC we find that immigrants living in enclaves are significantly less likely to have a bank account. We take steps to isolate one particular channel through which this might operate: the use of informal financial services provided by co-ethnics in enclaves. The results suggest that demand-side preferences may have power in explaining the persistence of the nativity gap in bank use in the United States.

JEL Classification: G21, J10

1. Introduction

Despite the prevalence and expansion of bank use in the United States, large portions of the U.S. population remain unbanked (Rhine, Greene, and Toussaint-Comeau 2006). One of the largest is immigrants, where the incidence of being unbanked is over 13% higher than among natives.¹ Financial institutions confer many benefits to their users, including providing safe and low-cost savings instruments, and the means to increase investment in a home or business. Without formal banking relationships it is more difficult for households to acquire financial assets, and researchers and policymakers are concerned that this may inhibit the wealth accumulation of unbanked households. For immigrant households, this may have long-term impacts if nonparticipation in the formal financial sector inhibits their economic success and slows their incorporation into the American economy (Bernanke 2004).

Given concerns over economic assimilation and bank use, several recent articles have explored the determinants of unbanked status among immigrants. Among other factors, education, income, wealth, family size, and institutional quality in the source region are all found to be correlated with bank use (Osili and Paulson 2006; Rhine and Greene 2006; Paulson and Rhine 2008). One factor that has been mentioned but not specifically explored, however, is the

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¹ Author's calculations from the 2001 Survey of Income and Program Participation (SIPP).

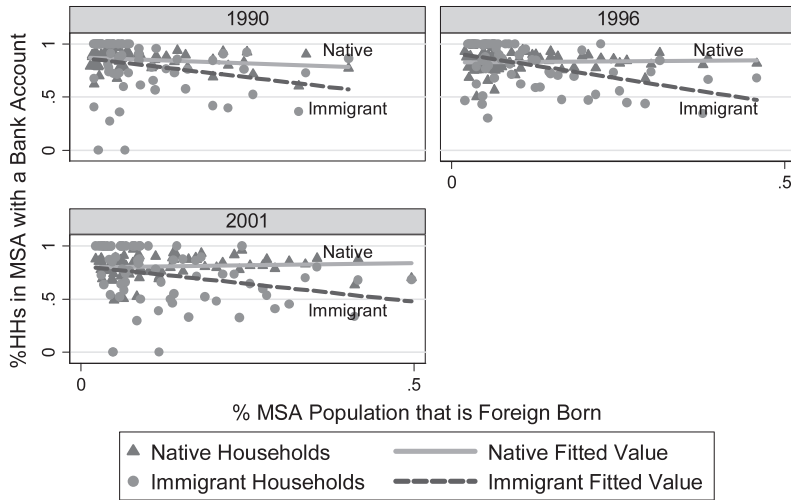


Figure 1. Average Bank Use by MSA and Immigrant Concentration
 Caption Slope coefficients & standard errors: Natives: 1990 $-.23 (.10)**$, 1996 $.04 (.11)$, 2001 $.08 (.11)$; Immigrants: 1990 $-.74 (.34)*$, 1996 $-.97 (.22)***$, 2001 $-.67 (.29)**$ Note: Authors' calculations from SIPP 1990, 1996, and 2001 (household-weighted calculations); U.S. Census bureau (population-weighted calculations). Each point represents an MSA, plotted based on bank use by native or immigrant households within the MSA and the MSA concentration of foreign-born residents.

concentration of immigrants from the same source region, or residency in an immigrant enclave. Theoretically enclaves can impact bank use through multiple, competing channels. On the one hand, enclaves provide a network of co-ethnics through which immigrants may access informal financial services, thereby decreasing demand for a bank. Indeed, there are numerous case studies documenting the use of informal financial instruments in U.S. immigrant communities (Light and Deng 1995; Bond and Townsend 1996). On the other hand, enclaves may provide residents with access to information networks through which they may learn about banking services or job opportunities, thereby increasing their income and, potentially, their acquisition of language skills or legal status. These factors, in turn, would increase demand for banks. These compensating factors yield an ambiguous prediction on the direction of the enclave effect on being banked. Thus the overall impact of enclaves on immigrant bank use remains an open empirical question.

To identify a relationship between enclaves and bank use we exploit detailed household financial behavior information from the Survey of Income and Program Participation (SIPP), the only large-scale data set that has information on immigrant status, location, and bank use. We use the 1990, 1996, and 2001 waves of the SIPP and find that the nativity gap in bank use—between native-born and foreign-born households—has actually grown over time, from 8% in 1990 to 13.8% in 2001. Furthermore, a cursory look at the data, shown in Figure 1, suggests there is a negative correlation between immigrant bank use and the concentration of immigrants in a metropolitan statistical area (MSA). In this figure each circle represents an MSA, charted based on the immigrant concentration (from Census data) and average household bank use (based on SIPP). The correlation between immigrant concentration and immigrant bank use is consistently negative. No similar correlation exists for native households, except in 1990. These correlations, while unconditional, suggest that immigrant concentrations may differentially impact immigrant bank use relative to that of natives.

To identify whether the size of, and increase in, the nativity gap can be explained by immigrant enclaves, we rely on variation in origin-specific immigrant concentration within MSAs over time and the differential effect this variation has on natives and immigrants. Specifically we assume that after controlling for other factors in the demand and supply of bank services, any additional impact of the source region–MSA concentration of immigrants on immigrant bank use constitutes an enclave effect.

After combining information on co-ethnic residence patterns from the U.S. Census with rich individual data and MSA-level characteristics, we find evidence that immigrant co-ethnic enclaves have a statistically significant negative impact on the likelihood that immigrant households have a formal banking relationship. The magnitude of our estimates is large, suggesting that for a 10% increase in concentration, the probability an immigrant household has a bank account falls between 2.4% and 3.0%. We argue this constitutes a negative enclave effect for several reasons. First, we exploit the rich individual-level data in the SIPP to control for numerous observable household characteristics. Second, we find no similar effects of immigrant concentration for native households, even those from similar ethnic backgrounds to immigrants. Third, we find no effects of overall immigrant concentration on the bank use for immigrant households. What matters is the concentration of immigrants from the same source region. This suggests that our estimates capture enclave effects rather than unobserved MSA characteristics that might be related to large immigrant populations.

Further, we also probe the robustness of our results to various model and data specifications. Overall our results are robust to additional controls for unobservable factors at the MSA or individual level, which may jointly determine immigrant location and bank use, including the location of quasi-banks, changes in preferences, and self-selection bias. To the degree that we effectively mitigate these concerns, our results provide evidence that immigrant enclaves drive down the use of formal banking services by immigrant households. Given the increases in immigrant concentrations over time, we argue that this can partially explain the rising nativity gap.

2. Conceptual Framework

We provide a basic model of banking participation in order to outline the channels through which enclaves affect financial behavior. A household will open and maintain a bank account if the benefits of doing so outweigh the costs:

$$Y_i = 1 \quad \text{if} \quad B_S + B_T > C_M + C_T + C_P.$$

The benefits of bank account ownership are twofold (Washington 2006). First, there is a savings benefit of bank account ownership (B_S), conferring a safe vehicle for savings and access to interest-bearing financial assets. Second, there is a transaction benefit of bank account ownership (B_T), which provides a low-cost means of transferring income into payments.² These

² Payments here can include remittances overseas, a common practice among immigrants. International remittances can be made in cash, by wire transfers through nonbank entities, and by transfers through formal banking institutions (even via automated teller machines). Suro et al. (2002) find that some of the key factors determining which type of service an immigrant uses to transfer money overseas are convenience, word of mouth, and security. While this may lead to increased use of formal bank services, the survey also finds that many immigrants are wary of banks. In a study of remittance behavior by Mexican immigrants, Ameudo-Dorantes and Bansak (2006) find that increases in access to formal banking services does not significantly alter remittance flows. We argue that immigrant preferences for remitting money overseas are incorporated into their general preferences for utilizing formal and informal financial services. Thus our modeling of the decision to use formal banking services incorporates the decision on how to remit.

benefits depend on the frequency and size of income and purchases, how income is received, household wealth, and the availability of alternatives, such as informal financial services like ROSCAs or quasi-banking services like check cashing and payday lending outlets.

The costs of owning a bank account are threefold. First, there are monetary costs, such as fees (C_M), which depend on wealth and the volume of transactions. Second, there are time costs (C_T), which are a function of the location of banks and the availability of alternatives. Third, there are other nonmonetary costs, collectively called psychic costs (C_P), which include cognitive costs associated with understanding the features of a bank account, language barriers, legal barriers, and a general distrust of or discomfort with banks. Psychic costs are difficult to quantify but likely are a function of education, prior experience with banking, legal status, language ability, and the use of banks within a household's social network.

Enclaves may affect the benefits and costs of bank account ownership in numerous directions, making the a priori prediction about the direction of causality between enclaves and bank use ambiguous. On the one hand, enclaves may increase bank use in a few ways. First, enclaves may confer information about formal banks, as the chance that a member of a households' social network uses formal banking services will increase as the size of the network grows. Similarly, networks in enclaves may confer job information and thus lead to improved labor market outcomes for immigrant residents, thereby increasing demand for banking services.³ Last, on the supply side, banks may offer more immigrant-specific services as enclave size grows.

On the other hand, enclaves may inhibit bank use through at least three channels. First, enclaves likely will increase the availability of informal financial alternatives to banks, as described earlier. Second, enclaves may yield relatively worse labor market outcomes for immigrants by inhibiting language acquisition or participation in the formal labor market, thereby decreasing the demand for banking services.⁴ Third, if quasi-banks are more likely to locate in areas with high immigrant populations, they provide an alternative to formal banking services. The specific channel we aim to test is the availability of informal financial alternatives conferred by immigrant enclaves. There is no direct measure of informal financial behavior, so we can only narrow in on this mechanism indirectly by eliminating competing explanations. In the empirical strategy outlined below we describe the approach to controlling for the other potential channels through which enclaves may impact demand for banking services.

3. Data

To discern the relationship between enclaves and immigrant bank use and to narrow in on the specific channels through which they operate we turn to the data. In the absence of an experiment randomly distributing immigrants across identical cities and observing their banking decisions, we exploit the rich data in the Survey of Income and Program Participation (SIPP). Key to our identification strategy is the variation in immigrant enclave size and

³ The strength of these network-driven explanations depends on the quality of the network. In particular, positive network externalities may require that skilled immigrants choose to stay in the enclave, something that perhaps is unlikely (Borjas 1998).

⁴ This negative network externality is perhaps more likely than the positive human capital externality listed prior, given evidence in Cutler and Glaeser (1997). However, that study examined the effects of residential segregation among native-born African Americans.

character due to the growth and dispersion of immigrants in the United States over the past few decades.

The principal source of data for our study is the SIPP. The SIPP is a nationally representative data set containing detailed information on immigrant status, region of origin, and financial market participation. We use the 1990, 1996, and 2001 panels of the SIPP. Although these data are longitudinal, because immigrant status, bank use, and household location vary little (or not at all) over time, the longitudinal structure of the SIPP is not useful for our analysis.⁵ Our unit of analysis is the household; for this reason household weights are applied throughout, a few exceptions noted. We choose households over individuals because many of the banking decisions are likely correlated across members of the same household, particularly for spouses who have joint bank accounts. We define a household as immigrant or native based on the status of the head of household⁶ and restrict attention to those age 18 to 65 who live in metropolitan areas. The result is a repeated cross section that contains 35,134 households, 85.2% of whom are native and 14.8% of whom are immigrant in the raw, unweighted sample.⁷

We define immigrant enclaves by region of origin rather than country. This is necessitated given the small number of immigrants from most countries in the SIPP but is also warranted by evidence that informal networks among immigrants exist at broad ethnic levels (see, for example, among Hispanic and Asian immigrants in Bond and Townsend 1996). Five different regions of origin are defined: Latin America, which includes Mexico, Central and South America, the Caribbean, Asia, Europe, and Other, which includes Africa, Other North America, and Oceania. Table 1 provides information on the composition of the immigrants in the sample and in the general population. Immigrants from Latin America make up the largest component, forming about 40% of all immigrants in the sample, while immigrants from Asia make up the second largest group, forming 27% of immigrants in the sample.

We measure enclaves as the concentration of immigrants from these similar source regions in a metropolitan area. This measure, derived from U.S. Census data, follows much of the immigration literature and assumes that the propensity for immigrants to rely on enclave networks is intensified the higher the concentration of similar immigrants in a metropolitan area (Borjas 1986, 1995; Bertrand, Luttmer, and Mullainathan 2000; Chiswick and Miller 2005; Andersson, Burgess, and Lane 2009). Although enclaves may be alternatively measured at the sub-MSA level, research suggests that among immigrants, networks are determined by ethnicity rather than neighborhood (Borjas 1995) and operate more broadly than within a narrowly defined neighborhood (Hellerstein, McInerney, and Neumark 2009). Utilizing a larger geographic area should bias us *against* finding a result. The Census gives reliable population estimates at the MSA level, for native-born and immigrants and, more specifically, by region of birth. This allows us to measure ethnic enclaves for immigrants from the same region for the 75

⁵ Within each panel we use the wave that most closely coincides with the relevant Census year. In the 1990 SIPP we use the fourth wave, which runs from January to April of 1991. In the 1996 SIPP we use the third wave, which runs from November 1996 to February 1997. In the 2001 SIPP we use the third wave, which runs from September to December of 2001. While each round of the SIPP is a panel containing multiple waves, we restrict attention to one wave.

⁶ Defined as the reference person in the SIPP. Other studies define households more restrictively, considering couple immigrant households to be those in which both partners are foreign born (Cobb-Clark and Hildebrand 2006). We consider this definition in section 5.

⁷ Immigrant is defined as foreign born, regardless of subsequent citizenship status. Two exceptions are individuals born to U.S. citizens abroad and Puerto Ricans, who are counted as natives. Also, we remove 61 households that have extreme bank to total wealth values.

Table 1. Immigrants in Sample

Region of Origin	1990			1996			2001		
	N ^a	% Immigrants in Population ^b	Average MSA Concentration ^c	N ^a	% Immigrants in Population ^b	Average MSA Concentration ^c	N ^a	% Immigrants in Population ^b	Average MSA Concentration ^c
Latin America	500	33.0%	4.6%	796	39.5%	6.7%	783	39.8%	8.4%
Caribbean	146	9.2%	1.5%	232	11.7%	1.8%	175	8.5%	2.1%
Europe	227	19.2%	2.4%	304	16.4%	2.5%	281	14.9%	2.6%
Asia	299	24.9%	3.7%	522	27.6%	4.7%	540	29.1%	5.6%
Other ^d	165	13.7%	0.7%	91	4.8%	0.9%	145	7.8%	1.1%
Total	1337	100.0%	12.9%	1945	100.0%	16.6%	1924	100.0%	19.8%

^a SIPP sample.

^b Based on the SIPP samples, using individual weights.

^c MSA concentration from the 1990 and 2000 Census. 1996 values are interpolated (see text).

^d Other includes Africa, Oceania, and other North America.

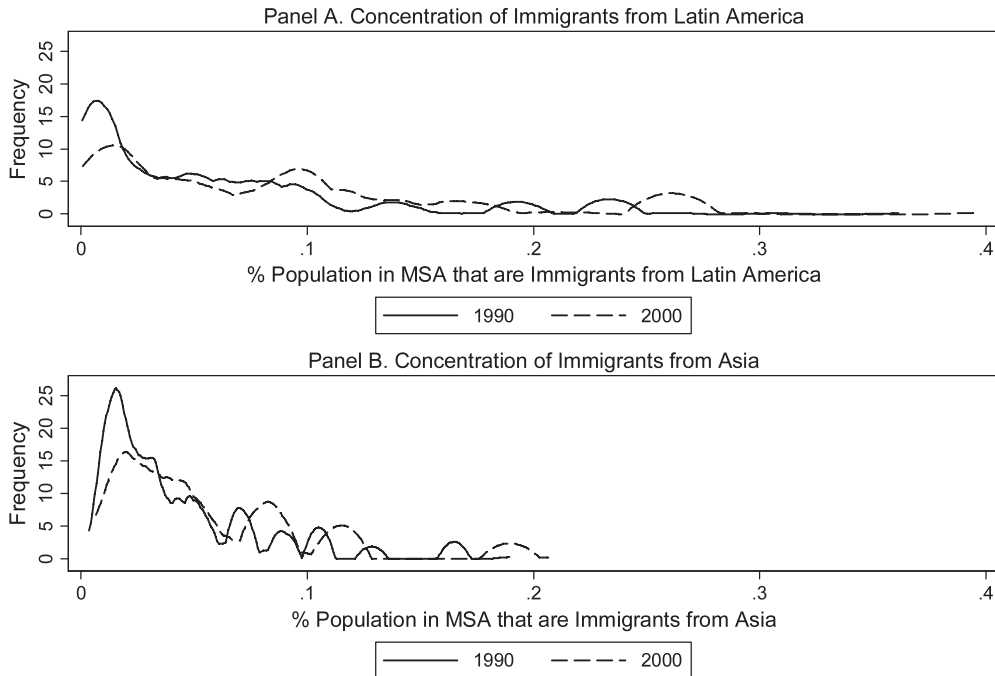


Figure 2. Distribution of Immigrant Concentration across MSAs (MSAs included in 1990 and 2001 SIPP only)
 Note: Authors' calculations from 1990, 2000 Decennial Census (population-weighted).

MSAs in our sample.⁸ There is significant variation in this enclave measure even at the MSA level, as illustrated by Figure 2. The concentration of the two largest immigrant groups—Asian and Latin American—reveal significant dispersion across MSAs and large changes across time.

We classify a household as “banked” if any adult age 18–65 has a bank account, which includes checking accounts, savings accounts, certificates of deposit, and money market deposit accounts.⁹ Given the importance of banks for asset accumulation, we also focus on interest-bearing bank accounts separately.¹⁰ Summary statistics on the unconditional means for bank

⁸ We include only MSAs that appear in the 1990, 1996, and 2001 samples. This leads to an exclusion of 26 MSAs which do not appear in all of the panels. The 1990 SIPP wave is matched to the 1990 Decennial Census; the 2001 wave is matched to the 2000 Decennial Census. The 1996 wave is matched to data interpolated between the 1990 and 2000 censuses using a cubic spline derived from four decennial censuses: 1970, 1980, 1990, and 2000. We also test the sensitivity of our results to a linear spline interpolation. These results are available upon request and are similar in all cases.

⁹ The SIPP uses various strategies to impute data that are missing for individuals because they were not present for a particular survey wave or because they did not answer particular questions. There are concerns that imputation rates for bank ownership status might be higher for immigrant households than native ones, and that these differences may skew our results. We therefore reestimate all of our results on the subsample that excludes households with imputed bank status. This removes very few observations as imputation rates are low for an entire household on a characteristic (bank ownership) with numerous questions. The results on the nonimputed status subsample are similar to our original estimates. They are available upon request.

¹⁰ These include everything except non-interest-bearing checking accounts. We consider the division by interest-bearing and non-interest-bearing accounts because the 1990 SIPP does not separately account for checking accounts. Approximately 86% of households with a bank account have at least one interest-bearing account.

Table 2. Nativity Gaps in Bank Use

	Native Born		Immigrant	Nativity Gap
<i>Panel A: Unconditional Nativity Gaps</i>				
% with bank account				
Year 1990	86.0%		78.2%	7.8% ***
Year 1996	83.4%		71.2%	12.2% ***
Year 2001	83.6%		69.8%	13.8% ***
% with interest-bearing bank account				
Year 1990	75.1%		67.4%	7.7% ***
Year 1996	72.1%		57.4%	14.7% ***
Year 2001	72.2%		55.6%	16.6% ***
	All Bank Accounts		Interest Bearing	
<i>Panel B: Conditional Nativity Gaps, Regression Results^a</i>				
Immigrant	-0.029 (0.012)**	0.002 (0.015)	-0.037 (0.012)***	0.010 (0.018)
Immigrant*1996		-0.029 (0.017)*		-0.051 (0.020)**
Immigrant*2001		-0.055 (0.014)***		-0.077 (0.018)***
Observations	34,875	34,875	34,875	34,875
R ²	0.274	0.274	0.277	0.278

Coefficients are population weighted. Robust standard errors in parentheses.

^a Controls include gender, ethnicity, education, age, age squared, marital status, salaried employment status income in year 2000 dollars, wealth (quintiles), home ownership, number of adults, number of children under 18, MSA-level unemployment and immigrant concentration, total bank branches (FDIC), MSA and year fixed effects and for immigrants, permanent residency status and time in the United States.

*** $p < 0.01$.

** $p < 0.05$.

* $p < 0.1$.

use are presented in panel A of Table 2. The unconditional means show a large and statistically significant nativity gap. In 1990, 86% of native households had a bank account while only 78.2% of immigrant households had a bank account, constituting a nativity gap of 7.8%. In 1996 the nativity gap rises to 12.2%, and in 2001 the gap increases further to 13.8%. The increases in the nativity gap are even larger for interest-bearing accounts alone. Thus while bank use declines over the 11-year period for both natives and immigrants, the declines are greater for immigrant households, showing that they are becoming relatively more unbanked.

Furthermore, as shown in panel B of Table 2, the nativity gaps largely persist after we control for numerous household and MSA-level characteristics. As expected, the size of the yearly nativity gaps falls, decreasing by more than one half. In the case of 1990 the nativity gap is explained by differences in socioeconomic and MSA characteristics. However, the conditional nativity gap remains for 1996 and 2001 and remains larger for interest-bearing accounts. This confirms the results from Figure 1, which showed a more acute nativity gap-concentration relationship in 2001 than other years. It also confirms that the nativity gap has worsened over time, and is not simply a function of changes in observable household characteristics.

4. Empirical Findings

Model Specification

We estimate the propensity to have a bank account using the following linear probability model:

$$B_{ist} = \alpha + \beta_1 X_{it} + \beta_2 Z_{st} + \beta_3 \lambda_{sjt} + \beta_4 \lambda_{sjt} * \delta_j + \delta_j + \delta_t + \delta_s + \varepsilon_{ijst} \quad (1)$$

where B_{ist} is the measure of bank use for a household headed by individual i in MSA s at time t .¹¹ We estimate separate models of bank use of all types and bank use for savings, by interest-bearing accounts in particular. This helps isolate the nontransaction motives for holding an account of different type; we expect, if there is a relationship between enclaves and immigrant bank use, that it will be relatively larger for interest-bearing accounts. Both native and immigrant households are included in all specifications, with dummy variable δ_j indicating the household is headed by an immigrant from region j . The key relationship we want to identify is that between B_{ist} and the immigrant region-specific enclave measure, λ_{sjt} . Because the enclave measure is common to all MSA residents, we take a number of approaches to isolate the immigrant source region-specific effect on bank use.

First, we use MSA fixed effects δ_s to control for time-varying factors common to households in a given MSA. Second, we include native-born households in the model to account for other MSA-specific factors specifically correlated with immigrant enclave size. Thus our key coefficient of interest, β_4 , identifies the differential impact of enclaves on bank use of immigrants relative to native born within an MSA. The MSA fixed effects and inclusion of natives in the model help control for characteristics that systematically shift demand for banking services, such as the availability of quasi-banking alternatives, which we cannot control for directly. Other MSA-level channels that we control for directly are represented by Z_{st} . This includes the availability of banking services, measured by Federal Deposit Insurance Corporation bank location data.¹² We also control for MSA unemployment rates, as they are related to economic activity that attracts labor, potentially increasing the concentration of immigrants, as well as capital, potentially increasing the supply of banks.

Household characteristics that drive the costs and benefits of banking services are included in X_{ist} . These include controls related to the demand for banking services due to the volume of transactions: wealth (quintiles), total household income, the number of children, and the number of adults in the household. Total income also controls somewhat for labor market outcomes. We also include controls for education, ethnicity, gender, age, marital status, and time in the United States (for immigrants), as well as dummies for salaried work and, in some estimations, dummies for business ownership. To control for prior banking experience, we proxy with home ownership, as this is likely to be highly correlated with previous financial

¹¹ We choose the linear probability due to the ease of interpreting interaction effects (Ai and Norton 2003). For robustness we also estimate logit models. Results are available upon request.

¹² The FDIC provides information on all branches of FDIC insured institutions in the country as of June 30 of each year. We use data from 1994, the earliest year available, 1996, and 2001, which gives us the total number of bank branches in every MSA for all years in our sample. Although we would like to control for the number of fringe bank outlets such as check cashers or payday lenders, comprehensive historical data are not currently available. We rely on MSA fixed effects as well as MSA-time varying characteristics, such as immigrant concentration, to pick up most of the variation in location of these establishments.

market participation. For immigrant households, we use proxy variables to control for language ability and legal status, factors that are likely related to demand for banking services. Neither are directly available in the SIPP during the time period we consider, but the combination of age, time in the United States, gender, and education are highly correlated with both (Orrenius and Zavodny 2005; Passel and Cohn 2008). In addition, we include a dummy variable for permanent residency status, differentiating between households who are definitely legal (naturalized or permanent resident) and those who are more likely to be undocumented (reporting neither).¹³

A challenge to the identification of β_4 is the potentially endogenous location choice of immigrants. The selection into immigrant enclaves may be systematically related to the use of formal banking services. If industrious immigrants, for example, are more likely to live outside immigrant enclaves and simultaneously more likely to have a bank account, then our coefficient of interest may pick up immigrant ability rather than informal networks. If, however, lower ability immigrants are deflected from cities with high concentrations of immigrants in the face of stiff labor market competition (Light 2006), then our estimates may be biased downward. Ideally one would exploit a natural experiment that randomly assigns individuals to cities and observe their banking choices¹⁴ or make use of an instrumental variable for concentration. Since neither are an option here, we instead rely on two approaches. First, we include the rich set of individual characteristics described above, which control for much of the potential source of skill difference across households. Beyond this, we exploit a common hypothesis in immigrant location choice literature, namely that immigrants are more responsive to the *level* of location-specific characteristics rather than predicted *changes* in these characteristics (LaLonde and Topel 1991) and estimate a model with fixed effects by year. This assumes that the propensity for immigrants to locate in particular areas is largely a function of previous settlement patterns, rather than current economic characteristics. We recognize, however, that this will not completely control for self-selection bias if immigrants with particular unobservable characteristics are changing their settlement patterns over time. Thus in the robustness check section we take a number of approaches to address potential unobserved individual heterogeneity.

If we have effectively controlled for the other factors that impact a household's decision to hold a bank account, β_4 will capture the role that social networks within enclaves play in determining immigrant bank use. If the main channel through which these networks operate is the provision of informal financial services, we expect the sign to be negative. If, however, the main channel is through exposure to banking services, we expect the sign to be positive.

¹³ It is impossible to precisely measure legal status as neither the SIPP nor any comprehensive representative data set includes indicators of legal versus illegal status. It would be difficult to obtain accurate responses on legal status in a large-scale survey, even if the question were asked. However, researchers estimate that nationally representative surveys similar to the SIPP (Census and CPS) have been able to capture the large majority of the illegal population. In the 1990 SIPP individuals are not asked separately about their naturalization status and permanent residency status. We categorize non-naturalized individuals as non-permanent residents for this wave.

¹⁴ Other authors have used similar quasi-experiments to examine enclave effects on various labor market outcomes in Scandinavia (Edin, Fredriksson, and Aslund 2003; Damm 2009). In the U.S. context however, similar natural experiments are not large enough to exploit.

Table 3. Bank Use

	All Accounts (1)	Interest Bearing (2)
Head of household an immigrant from region <i>j</i> :		
Latin America	-0.011 (0.028)	-0.001 (0.016)
Caribbean	0.043 (0.026)	0.030 (0.023)
Asia	0.003 (0.019)	-0.034 (0.023)
Europe	-0.004 (0.015)	-0.003 (0.016)
Other	0.070 (0.019)***	0.050 (0.021)**
Concentration immigrants from region <i>j</i> in household's MSA		
Latin America	-0.095 (0.276)	-0.208 (0.282)
Caribbean	0.001 (0.913)	-0.741 (1.482)
Asia	1.166 (1.088)	1.129 (0.847)
Europe	-2.619 (2.415)	-0.800 (2.649)
Other	3.137 (2.617)	4.253 (4.052)
ConcentrationImmigrants _{Region <i>j</i>}	-0.241	-0.305
*Immigrant _{Region <i>j</i>}	(0.119)**	(0.089)***
Observations	34,875	34,875
R ²	0.275	0.279

Other variables include gender, ethnicity, education, age, age squared, marital status, salaried employment status, income in year 2000 dollars, wealth (quintiles), number of adults, number of children under 18, MSA-level unemployment and immigrant concentration, bank branches (FDIC), MSA and year fixed effects, permanent residency status and time in the United States. Full estimation results in Appendix. Coefficients are population weighted. Robust standard errors in parentheses.

*** *p* < 0.01.

** *p* < 0.05.

* *p* < 0.1.

Estimation Results

We estimate Equation 1 using a linear probability model. The results are presented in Table 3. Column 1 contains results for all bank accounts while column 2 contains results for interest-bearing accounts only. We report population weighted coefficients with robust standard errors, clustered by MSA.¹⁵

Overall we find that immigrants living in enclaves are significantly less likely to own bank accounts, as shown by the negative coefficient on the immigrant interaction term in each column. Our estimates for all bank accounts (column 1) suggest that if the concentration of immigrants from a household's source region increases by 10%, the probability that the average immigrant household has a bank account relative to similar natives and immigrants from different regions of origin falls by 2.4%.¹⁶ As expected, the magnitude of the coefficient on interest-bearing bank accounts (column 2) is even larger, at 3.05%. The results have implications for changes in immigrant concentrations over time. They suggest that, even after controlling for changes in observable characteristics, as the percentage of immigrants from

¹⁵ The results for the full set of controls are shown in the Appendix. The signs and precision of the covariates are similar to those found in other studies (Cobb-Clark and Hildebrand 2006; Osili and Paulson 2006).

¹⁶ The effects are identified by changes within MSA, across time. However, models that utilize only the time variation or variation across MSAs yield similar results. In fact, when either set of fixed effects (rather than both) are employed, the point estimates are larger and strongly statistically significant. Overall, we find that the MSA-level variation carries more weight as compared with time variation. For the reasons discussed we find the model identifying from variation within MSA over time to be preferable.

certain source regions increases in a city the probability that immigrants from that region have bank accounts falls. The direct concentration variable estimates suggest that there is no consistent, significant relationship between immigrant enclaves and MSA native-born co-residents. The models identify a differential impact of immigrant concentration on banking behavior of *immigrants*. Since we control for other MSA and household characteristics that could yield this negative relationship, our conceptual framework suggests that the increased availability of informal financial services within enclaves are a key explanation of our results. While we attribute these enclave effects to informal networks in immigrant communities, we recognize concerns that the effects may be attributable to unobservable individual or MSA-level factors that are distinct from informal networks. In the following section we address concerns regarding the endogeneity of the immigrant concentration variable.

5. Robustness Checks

In this section we examine the robustness of our main findings. The main concerns surrounding our results are that unobservable factors at the MSA or household level may drive the relationship between immigrant concentration and immigrant bank use, leading to a spurious correlation between the two. This would threaten our argument that the channel through which immigrant concentration impacts bank use is the availability of social networks. We address each concern in turn.

MSA-Level Heterogeneity

We start with concerns that our results are being driven by certain outlier cities with high immigrant concentrations. We examine the distribution of immigrant concentrations and truncate the sample at the 90th percentile, removing MSAs for which immigrants comprise more than one third of the total population, a natural break in the data.¹⁷ Only four MSAs lie above this threshold: Miami, Los Angeles, San Francisco, and McAllen, Texas. Results from the sample that excludes these four MSAs are shown in columns 3 and 4 in panel A of Table 4. It is important to note that this removes over one third of the immigrants from our sample. For comparison we also present the results from the full sample in columns 1 and 2.

The results show that the relationship between immigrant concentration and bank use among immigrants is not being driven by cities with large immigrant concentrations. In fact, the opposite seems to be the case, as the coefficients on the interaction term are twice as large in the truncated sample as the full one. One possibility for these results is that the channels through which enclaves increase bank use operate more strongly in cities with potentially larger enclaves. These channels include the provision of job information that leads to improved labor market outcomes and language acquisition and the tailoring of banking services to particular communities, which may enhance information about and comfort with formal banks. We have no way of knowing with certainty if this is the case. However, the results suggest that any

¹⁷ Given that the vast majority of immigrants in our sample live in MSAs with high concentrations of immigrants, truncating the sample at the 90th percentile is preferable to the 75th percentile or the median. For example, 80% of the immigrants in our sample live in MSAs with immigrant concentrations above the median.

Table 4. Robustness Checks for MSA-Level Heterogeneity

	Full Sample		Above 90th Percentile Remove	
	All Accounts (1)	Interest Bearing (2)	All Accounts (3)	Interest Bearing (4)
<i>Panel A: Removing Potential Outlier MSAs</i>				
ConcentrationImmigrants _{Region} [*]	-0.241	-0.305	-0.757	-0.690
Immigrant	(0.119)**	(0.089)***	(0.198)***	(0.216)***
Sample	Full	Full	Truncated ^a	Truncated ^a
Observations	34,875	34,875	29,221	29,221
R ²	0.275	0.279	0.278	0.275
<i>Panel B: ConcentrationImmigrants from Other Regions of Origin</i>				
ConcentrationImmigrants _{Region j-1} [*]	0.019	-0.079	0.156	0.191
Immigrant	(0.093)	(0.092)	(0.120)	(0.121)
Sample	Full	Full	Truncated ^a	Truncated ^a
Observations	34,875	34,875	29,221	29,221
R ²	0.275	0.279	0.277	0.275
	OLS ^b		IV ^c	
<i>Panel C: Control for Economic Shocks</i>				
ConcentrationImmigrants _{Region j} [*]	-0.226	-0.311	-0.360	-0.490
Immigrant _{Region j}	(0.120)*	(0.164)*	(0.097)***	(0.227)**
Observations	33,740	33,740	33,740	33,740
R ²	0.273	0.273	0.276	0.276

All models include gender, ethnicity, education, age, age squared, marital status, salaried employment income in year 2000 dollars, wealth (quintiles), home ownership, number of adults, number of children under 18, MSA-level unemployment and immigrant concentration, total bank branches (FDIC), MSA and year fixed effects and for immigrants, permanent residency status and time in the United States. Coefficients are population weighted. Robust standard errors in parentheses.

^a MSAs not included in the truncated sample are Miami, Los Angeles, San Francisco, and McAllen, Texas.

^b OLS results with no immigrant concentration other than interaction, for comparison.

^c Second-stage results. First-stage results available upon request.

*** $p < 0.01$.

** $p < 0.05$.

* $p < 0.1$.

enclave effects in the full sample are not being driven by cities with large immigrant populations.

We next address concerns about unobserved MSA-level characteristics. If our empirical design and controls do not adequately account for MSA variation in the propensity for households to have banking relationships, our results may spuriously attribute diminished bank use among immigrants to immigrant-specific enclave effects rather than to general MSA effects. To test for this potential spurious correlation, we run falsification tests, replacing the key interaction term with one that interacts immigrant status with the concentration of immigrants from regions *other* than an individual's source region ($conc_{s(j-1)t} * \delta_{imm,j}$). We argue that if our story holds, the importance of immigrant concentration lies in immigrants from the same region of origin living in an MSA, not in the concentration of immigrants from other regions. For example, the bank use of a European immigrant should not be impacted by the concentration of immigrants from Asia living in their MSA if informal networks are the main channel through which enclaves operate. We therefore reestimate bank use with this interaction term. Results from the full sample are shown in columns 1 and 2 in panel B of Table 4. For comparison we also

consider the truncated sample that excludes the four MSAs with immigrant concentrations above the 90th percentile. These results are shown in columns 3 and 4. Overall we find limited evidence to support the hypothesis that the concentration of immigrants from other source regions impacts the bank use of immigrant households. Three out of four of the coefficients are positive and none are significant. These results further support our argument that the main channel through which immigrant concentration impacts bank use is the availability of informal networks.

Third, we utilize a common instrumental variables approach to account for unobservable economic shocks to MSAs that may not be fully captured in our MSA-level controls. These shocks could jointly determine immigrant bank use and immigrant concentration and thus bias our findings. We instrument for immigrant concentration for the year in question using immigrant concentration in 1970, exploiting the chain-based migration patterns of immigrants to the United States.¹⁸

The results from the second stage are reported in columns 3 and 4 in panel C of Table 4. The number of observations in the instrumental variable (IV) estimation falls due to the absence of information on 1970 immigrant concentration for several MSAs in the sample. Since we omit the control variable $conc_{sjt}$ to simplify the IV calculation and we also present baseline ordinary least squares results omitting concentration measures other than the interaction term and MSAs that have no 1970 Census data for comparison. These are in columns 1 and 2 in panel C of Table 4. We find that instrumenting does not change the magnitude of our baseline results much. The standard errors on the all bank account regression increase, but statistical significance is maintained. These results lend confidence to the conclusion that our results are not driven purely by economic shocks.

Individual-Level Heterogeneity

We next address concerns that changes in the unobserved, individual-level characteristics of immigrants within MSAs over time drive our results. For example, if enclaves are increasingly made up of immigrants with greater preference for not using banks, we will obtain a negative relationship between concentration and bank use for reasons that have nothing to do with social networks.

To the extent that unobserved preferences for bank use are correlated within ethnic groups, we can use native households as a test for whether or not the estimated effects are unique to immigrants. If the immigrant enclave size in fact measures immigrant informal networks rather than changing ethnic preferences, we anticipate that immigrant enclave size should have no power in explaining differences in bank use between native-born whites and native-born Latinos. We test this by estimating bank use only on the sample of native households and interacting concentration with Mexican American natives, all Latin American natives, and Asian American natives. These interaction terms tell us, for example, if Mexican American households living in MSAs with a higher concentration of Mexican immigrants are more or less likely to have a bank account than comparable non-Mexican American natives.

¹⁸ Historical patterns of settlement of immigrants are a strong predictor of current settlement within source country groups (Cortes 2008). However, we expect the 1970 distribution of immigrants across MSA to be unrelated to current economic shocks. To construct the IV, we essentially reallocate immigrants in year t according to their distribution in 1970 as follows: $\sum_j \frac{I_{j1970}}{I_{j1970}} \times J_{jt}$ where I is the number of immigrants and s represents MSA. We calculated 1970 statistics from the Decennial Census. Native-born are reallocated in the same fashion. The ratio of immigrant to native plus immigrant within MSA instruments for our key variable of interest. We estimate bank use using two-stage least squares. First-stage results are available upon request.

The results, presented in panel A of Table 5, show no evidence that immigrant concentration has a similar effect on natives of similar ethnic backgrounds. The coefficients on the interaction terms are either insignificant or *positive* and significant, showing that the negative enclave effect is specific to immigrants.¹⁹ To the extent that immigrant preferences regarding banks are similar to those of co-ethnic natives, these results suggest that changes in preferences do not drive the enclave effects we find. Furthermore, this test is evidence that the enclave effects are unique to immigrants.²⁰

Next, we are concerned with potential sorting into MSAs based on individual ability or industriousness. As we mentioned earlier, it is impossible to completely control for unobserved ability given the lack of an instrument for immigrant concentration. We consider, however, a proxy variable in the form of self-employment.²¹ Highly industrious individuals may be more likely to open their own business and therefore to be self-employed than less industrious individuals. Self-employment also serves as an additional check on the labor market channel for immigrant enclaves. We therefore reestimate bank use including a measure of self-employment in addition to all of the controls in the initial estimation. The results are in panel B of Table 5. We see no significant changes in the results, as the coefficients on the interaction terms remain negative and significant.²²

The next piece of evidence that selection does not drive the findings utilizes evidence from the literature that unobserved skill varies across immigrant arrival cohort. We thus hold arrival cohort constant and reestimate the relationship between bank use and immigrant concentration. We consider two years of arrival cohorts—1970–1980 or 1980–1990—and track the bank use of these synthetic cohorts over time.²³ Borjas (1985) suggests that unobserved immigrant quality declined over these cohorts. In the analysis we also restrict the age range of the natives in the sample to match those of the restricted immigrant cohort group. The results are shown in panel A of Table 6. Overall we find no evidence that unobserved cohort ability biases our results. As shown in columns 3 and 4, the estimated enclave effects are larger for the newer arrival cohort of immigrants who arrived between 1980 and 1990. The coefficients, however, remain negative in all cases.

¹⁹ We define households based on the immigrant status of the head of household. This, however, may lead to inappropriate classifications for mixed households defined as households comprised of native-immigrant couples. For robustness, we check if the results are robust to removing these mixed households from the sample. This removes approximately 1300 households from the analysis. We find no significant change in the results, which are available upon request.

²⁰ We also estimate the full model on subsamples of Hispanic and Asian households in case the importance of immigrant concentration varies by ethnicity, something not captured by the ethnicity dummy variables. First, we find that the nativity gap is larger and increasing more severely in the subsamples of Hispanic and Asian households. We also find that the coefficients on the interaction term remain negative and even increase in size in the Hispanic subsample. They also remain highly significant in the Hispanic and Asian subsamples (−0.51 and −0.27, respectively) but do not retain significance in the Asian subsample, likely due to small sample size (approximately 1400 observations). Results are available upon request.

²¹ In the SIPP self-employment is best measured by ownership of businesses. We regard self-employment broadly, defined as anyone who reports owning at least one business.

²² We also examined individual heterogeneity by splitting the sample by gender and education. These results are available upon request. Note that splitting the sample based on these individual characteristics does not test whether the immigrant enclave is more or less skilled since the independent variables of interest are the same across models. These results instead indicate whether more or less skilled immigrants in enclaves of various sizes are more likely to use formal banking services. For gender we find that for interest-bearing accounts the coefficient on the interaction term is similar across men and women. For education we find that the coefficients on the interaction term are larger than the baseline for households with less education (high school or less). For households whose head has a college education the coefficients are still negative but smaller and no longer significant. This may be driven by the relatively small number of highly educated immigrants in our sample, thus we hesitate to make any strong conclusions from this cut of the data.

²³ Due to sample size constraints estimating effects on the newest cohort of immigrants that arrived after 1990 is unreliable.

Table 5. Controls for Individual Heterogeneity

	All Accounts (1)	Interest Bearing (2)	All Accounts (3)	Interest Bearing (4)
<i>Panel A: Natives Only</i>				
ConcentrationImmigrantsMexico&CentralAmerica*	0.159 (0.136)	0.161 (0.099)		
Mexican American			0.353 (0.124)***	0.129 (0.116)
ConcentrationImmigrantsLatinAmerica*				
Hispanic (all Latin America)	-0.235 (0.257)	-0.131 (0.281)	-0.234 (0.259)	-0.134 (0.281)
ConcentrationImmigrantsAsia*				
Asian American	29,713	29,713	29,713	29,713
Observations	0.270	0.271	0.270	0.271
R^2				
<i>Panel B: Self-employment Included</i>				
Head of household is self-employed	0.083 (0.010)***	0.039 (0.010)***		
ConcentrationImmigrantsRegion _j *	-0.231 (0.117)*	-0.300 (0.088)***		
Immigrant Region _j				
Observations	34,875	34,875		
R^2	0.278	0.279		

All models include gender, ethnicity, education, age, age squared, marital status, salaried employment status, income, wealth (quintiles), home ownership, number of adults, number of children under 18, MSA unemployment and immigrant concentration, total bank branches (FDIC), MSA and year fixed effects, permanent residency and time in the United States. Coefficients are population weighted. Robust standard errors in parentheses.

*** $p < 0.01$.
 ** $p < 0.05$.
 * $p < 0.1$.

Table 6. Sub-samples of Immigrant Households by Cohort, Time in U.S., and Possible Legal Status

	1970–1980				1980–1990			
	All Accounts (1)	Interest Bearing (2)	All Accounts (3)	Interest Bearing (4)	All Accounts (5)	Interest Bearing (6)	All Accounts (7)	Interest Bearing (8)
Panel A: Cohort: Year of Arrival								
ConcentrationImmigrants _{Region,j} *	-0.053	-0.249	-0.410	-0.343				
Immigrant _{Region,j}	(0.135)	(0.134)*	(0.162)**	(0.147)**				
Observations	26,584	26,584	31,440	31,440				
R ²	0.271	0.274	0.276	0.277				
6–11 years								
Panel B: Time in U.S.								
ConcentrationImmigrants _{Region,j} *	-0.061	-0.578	-0.406	-0.050	-0.277	-0.341		
Immigrant _{Region,j}	(0.359)	(0.243)**	(0.220)*	(0.228)	(0.126)**	(0.097)***		
Observations	30,539	30,539	30,639	30,639	32,690	32,690		
R ²	0.275	0.277	0.274	0.274	0.269	0.273		
11+ years								
Panel C: Possible Legal Status								
ConcentrationImmigrants _{Region,j} *	-0.141	-0.197	-0.138	-0.265	-0.738	-0.808		
Immigrant _{Region,j}	(0.110)	(0.140)	(0.122)	(0.109)**	(0.358)**	(0.373)**		
Observations	31,893	31,893	33,592	33,592	3,749	3,749		
R ²	0.263	0.267	0.268	0.273	0.348	0.302		
Naturalized Citizens								
Permanent Residents ^a								
Likely Undocumented								

All models include gender, ethnicity, education, age, age squared, marital status, salaried employment status, income, wealth (quintiles), home ownership, number of adults, number of children under 18, MSA-level unemployment and immigrant concentration, bank branches (FDIC), MSA and year fixed effects, and for immigrants, permanent residency status and time in the United States. Coefficients population weighted. Robust standard errors in parentheses.

^a Includes naturalized citizens.

*** $p < 0.01$.

** $p < 0.05$.

* $p < 0.1$.

Alternately, we hold fixed the years since arrival of the immigrants in the sample, to abstract from the unobservable effects of assimilation over time (whereas the previous controls for cohort effects). To capture the potential for a differential impact of enclaves by tenure in the United States we separately estimate bank use on subsamples that include immigrants who have been in the United States fewer than 5 years, between 6 and 10 years, and more than 10 years. Results are shown in panel B of Table 6. The coefficient on interest-bearing accounts is largest for immigrants with fewer than five years in the United States (column 2), perhaps indicating less assimilated immigrants are more subject to enclave effects. However, the coefficient on all bank accounts is largest for immigrants with 6–11 years in the United States (column 3). The pattern of standard errors is not too surprising given the small number of immigrants in the 0–5 and 6–11 year groups (833 and 935, respectively). But the pattern of significant results does not present a clear picture on the role of time in the United States on our model. Nevertheless, we find a negative and significant effect for immigrants with the longest tenure, 11 years or more. This indicates that the effects are not dominated by unobserved heterogeneity of immigrants on this characteristic.²⁴

Finally, we address concerns regarding immigrant legal status. We verify that our results are not identifying only the differential propensity to use a bank account due to documentation status. Our model includes some proxies for legal status, such as the dummy for permanent resident status and demographic characteristics highly correlated with legal status (time in the United States, age, education, and ethnicity, following Orrenius and Zavodny 2005).²⁵ Here we go one step further to estimate the model separately for immigrant households of different legal status: naturalized immigrants, permanent residents, and a narrowly defined group of likely undocumented based on being nonpermanent resident and not graduated from high school. For the latter, we include only less educated native-born households as the MSA-level comparison. Results are shown in panel C of Table 6. Overall, they are in line with our expectations; however, due to small samples precision falls greatly. For naturalized immigrants (columns 1 and 2) the enclave effect is smaller and insignificant. For naturalized and permanent residents combined (columns 3 and 4), the coefficient is more negative and significant for interest-bearing accounts. Finally, for the group of most likely undocumented immigrants (columns 5 and 6), the coefficients are significantly larger than for the entire immigrant sample. Note that this latter point estimate is not directly comparable to the others or to the baseline model results, as it includes only less-educated native and immigrant households. As the coefficients remain negative among each subsample, we are confident that our results are not being driven by one legal status group of immigrants.

Overall our results are robust to a variety of controls for heterogeneity at the MSA and individual level. We recognize that we are limited in our ability to control for the problem

²⁴ We also split the sample by age of entry. Given that year of arrival is categorical in the SIPP and the small sample size of immigrants, we construct only three categories: immigrants who arrived before age 15; immigrants who arrived between the ages of 16 and 30; and immigrants who arrived over age 30. Though large, these breaks address the concern that immigrants who arrive as children differ substantially from those who arrive in prime working age. We find negative interaction terms for all three groups. The coefficients, however, are only significant for the 0–15 and the 30+ groups. Furthermore the size of the coefficient is larger for these two groups than the 15–30 group. This suggests that any potential enclave effects are not limited to immigrants who arrived at a later age and may have slower rates of assimilation. These results are available upon request.

²⁵ Mexican birth along with education and young age are the strongest predictors of undocumented status. However, restricting our sample along all three dimensions makes the SIPP sample so small that we cannot reliably estimate the model. Thus, we use education and reported citizenship status as a reasonable proxy.

of self-selection, given the absence of a good instrument for immigrant concentration. Nevertheless, the robustness of our results leads us to be confident that the concentration effects are not purely a story of self-selection. We are therefore confident to conclude that there is a negative impact on banking participation for immigrants residing in MSAs with higher concentrations of similar immigrants.

6. Conclusion

In this article we document growth in the nativity gap in bank use over time, showing that immigrants are increasingly less likely to have a bank account than natives. We test the importance of immigrant enclaves, defined as areas with a high concentration of immigrants

Appendix 1

Immigrant Concentration (Census Year Percentage of Total Population) for Sample of MSAs

	1990		2000	
	Central America and Mexico	Asia	Central America and Mexico	Asia
Atlanta	0.71%	2.08%	4.70%	3.68%
Boston	0.72%	3.23%	1.45%	5.40%
Chicago	5.03%	3.26%	9.74%	4.92%
Dallas	5.58%	2.75%	13.02%	4.44%
Denver	1.68%	1.79%	7.61%	2.86%
Detroit	0.19%	2.12%	0.79%	4.08%
Houston	8.58%	3.69%	15.98%	5.54%
Las Vegas	3.88%	3.12%	13.07%	5.56%
Los Angeles	18.31%	9.02%	24.99%	11.52%
Miami	4.86%	1.48%	7.56%	2.22%
Minneapolis/St. Paul	0.16%	2.09%	1.59%	3.64%
New York	1.62%	5.33%	3.64%	8.35%
Phoenix	4.51%	1.53%	12.60%	2.66%
Seattle	0.38%	4.61%	2.04%	7.59%
San Francisco	5.85%	12.85%	10.26%	18.94%
Washington, DC	1.60%	4.26%	3.82%	6.43%

Source: Authors' calculations from Decennial Census (population-weighted).

Appendix 2

Full Estimation Results

Variables	All Accounts (1)	Interest Bearing (2)
Woman	-0.008 (0.005)*	-0.009 (0.004)**
Married	0.051 (0.005)***	0.049 (0.006)***
Black	-0.142 (0.011)***	-0.098 (0.010)***
Hispanic	-0.089 (0.023)***	-0.054 (0.017)***
Asian	0.015 (0.013)	0.046 (0.015)***
Education:		
Less than high school	-0.119 (0.010)***	-0.097 (0.011)***
College	0.072 (0.005)***	0.087 (0.007)***
Advanced degree	0.076 (0.008)***	0.104 (0.009)***

Appendix 2
Continued

Variables	All Accounts (1)	Interest Bearing (2)
Owns home	0.013 (0.005)***	0.013 (0.006)**
Wealth quintile 1	-0.226 (0.010)***	-0.283 (0.008)***
Wealth quintile 2	-0.042 (0.005)***	-0.070 (0.009)***
Wealth quintile 4	0.013 (0.005)***	0.061 (0.009)***
Wealth quintile 5	0.012 (0.006)**	0.091 (0.007)***
Salaried work	0.077 (0.006)***	0.089 (0.005)***
Total income	0.000 (0.000)***	0.000 (0.000)***
Total adults in household	0.019 (0.002)***	0.017 (0.002)***
Total children in household	-0.026 (0.002)***	-0.032 (0.002)***
Age	-0.005 (0.001)***	-0.008 (0.002)***
Head of household an immigrant from region <i>j</i> :		
Latin America	-0.011 (0.028)	-0.001 (0.016)
Caribbean	0.043 (0.026)	0.030 (0.023)
Asia	0.003 (0.019)	-0.034 (0.023)
Europe	-0.004 (0.015)	-0.003 (0.016)
Other	0.070 (0.019)***	0.050 (0.021)**
Nonpermanent resident	-0.028 (0.020)	-0.004 (0.027)
Tenure in United States <5 years	-0.037 (0.019)*	-0.064 (0.022)***
Tenure in United States 5–10 years	-0.034 (0.017)**	-0.019 (0.014)
Concentration immigrants from region <i>j</i> in household's MSA		
Latin America	-0.095 (0.276)	-0.208 (0.282)
Caribbean	0.001 (0.913)	-0.741 (1.482)
Asia	1.166 (1.088)	1.129 (0.847)
Europe	-2.619 (2.415)	-0.800 (2.649)
Other	3.137 (2.617)	4.253 (4.052)
MSA unemployment	-0.002 (0.003)	0.000 (0.004)
Number of bank branches	0.000 (0.000)	-0.000 (0.000)
Year 2001	-0.070 (0.018)***	-0.083 (0.017)***
Year 1996	-0.049 (0.011)***	-0.060 (0.011)***
ConcentrationImmigrants _{Region <i>j</i>} *	-0.241	-0.305
Immigrant _{Region <i>j</i>}	(0.119)**	(0.089)***
Observations	34,875	34,875
<i>R</i> ²	0.275	0.279

Coefficients are population weighted. Age squared and MSA fixed effects also included.

from the same region, in explaining the increasing differential in bank use. Our estimates suggest that if the concentration of immigrants from a household's source region increases by 10%, the probability that the average immigrant household has a bank account relative to similar natives and immigrants from different regions of origin falls by 2.4%. The effects we find are large and can potentially explain the persistence of the nativity gap in bank use as well as the increase in this gap over time.

The findings in this article are positive rather than normative. The role of bank accounts in the economic outcomes of immigrant households, such as wealth accumulation, is unclear. An inverse link between bank use and enclave size may be troubling if it indicates a low propensity among immigrants to save and thus progress economically. If, on the other hand, informal financial services are good substitutes for formal ones, households may make rational choices

to remain unbanked. In this case, nativity gaps in bank use may not be linked with different economic outcomes.

Our results have important implications for policymakers and financial institutions aiming to increase bank use among immigrant households. Limited demand may play a major role in deterring some immigrants from opening and maintaining bank accounts, and understanding the sources of muted demand is necessary for determining how to modify banking services to reach underserved communities. Reliance on informal networks may help explain the persistence of underserved communities and shed light on the limits of banking sector outreach to them.

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