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ABSTRACT

In this study we examine the importance of assimilation and ethnic enclave residence for smoking outcomes among United States immigrants. We draw data on over 140,000 immigrants from the Current Population Survey Tobacco Use Supplements between 1995 and 2011. Several patterns emerge from our analysis. First we replicate findings from previous studies that show that longer residence in the U.S is associated with improved employment outcomes while ethnic enclave residence may hinder these outcomes. Second, we find that assimilation similarly extends to coverage of employment-based anti-smoking policies such as worksite smoking bans and smoking cessation programs while enclave residence does not substantially influence these outcomes. Third, we document complex relationships between assimilation, enclave residence, and smoking outcomes. Lastly, we find no strong evidence that immigrants reduce their smoking when faced with more restrictive state anti-smoking policies and find counter-intuitive impacts of tobacco taxes. These findings have important policy implications.

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1. Introduction

This study examines the importance of economic assimilation and ethnic enclave residence in immigrant smoking outcomes using the Current Population Survey Tobacco Use Supplements between 1995 and 2011. As such we make several contributions to the economics literature. First, to the best of our knowledge, no study has examined the importance of ethnic enclave residence for smoking outcomes. Moreover, only a handful of studies have investigated the impact of assimilation on immigrant smoking outcomes (Acevedo-Garcia et al., 2005; Angel et al., 2001; Baluja et al., 2003; Wilkinson et al., 2005) and only one allows for heterogeneity by birth country (Leung, 2013). Second, our study moves beyond simply assessing the degree to which assimilation and enclave residence are associated with smoking outcomes, and examines how these variables may protect immigrants from passive smoking at work and access to employer-sponsored smoking cessation programs. Thus, we contribute to the broader line of research that investigates how immigrants integrate into the United States labor market and extend this line of research to health habits and policies to improve these habits. Lastly, we consider a more comprehensive set of smoking outcomes and policies than previous studies by examining the intensive smoking margin and smoking bans in multiple venues.

Immigrants are potentially an important group to study as the U.S. immigrant population has increased substantially over time. In 2011 there were over 40 million immigrants living in the U.S., an increase from 31.1 million (23%) in 2000 (Pew Research Center, 2013). Also, the type of immigrant who chooses to migrate to the U.S. is changing. During previous migration waves (e.g., 1890 to 1920) immigrants often originated from Europe while more modern waves tend to migrate from Latin American and Asia (Pew Research Center, 2013). Unauthorized immigration from Mexico and Central America further altered immigrant flow composition.

Thus, more recent immigrant cohorts are on average less advantaged than previous cohorts. Moreover, after decades of integration, residential patterns in the U.S. have become more stratified along immigrant, racial, and ethnic lines (Cutler et al., 2008).

Smoking is arguably one of the most important health behaviors to study as it imposes large external costs on society. Specifically, smoking leads to $119 billion in health care costs per year (Centers for Disease Control and Prevention, 2008). Smoking increases health care costs through increased use of publicly provided health insurance (Zhang et al., 1999), and higher insurance premiums for smokers and non-smokers (Halpern et al., 2009; Pearson and Lieber, 2009). The full costs of smoking may extend to the labor market through lower productivity and increased absenteeism (Berman et al., 2013; Centers for Disease Control and Prevention, 2008; Sherman and Lynch, 2013). Lastly, exposure to passive smoking can harm non-smokers' health (Institute of Medicine, 2010).

While immigrants have lower prevalence rates of smoking and lower consumption of tobacco products conditional on smoking than natives, a non-trivial proportion smoke (16% of men and 7% of women in our sample) and at a level that can plausibly lead to negative externalities both in terms of passive smoking and financial implications. Importantly, many immigrants come from higher smoking countries (relative to the U.S.) and thus may respond differentially to standard anti-smoking polices than native born Americans (Leung, 2013). For these reasons, we argue that immigrants represent an important group to study.

We study smoking both because smoking is one of the most hazardous health habits and as a template for the broader set of health behaviors (e.g., obesity, substance misuse, and medication adherence) that may increase health care costs among immigrants. Identifying risk factors and developing policies to reduce these behaviors is timely given the U.S. spends $2.7

trillion annually on health care and these costs have consistently risen faster than the growth of the overall economy (Centers for Medicare and Medicaid Services, 2011). Although immigrants have lower average health care spending than native born Americans (DuBard and Massing, 2007; Stimpson et al., 2010; Stimpson et al., 2013), they contribute $96.5 billion per year to the nation's overall health care costs. Legal immigrants have access to public health insurance (Medicaid and Medicare) and illegal immigrants have access to emergency health care services, both leading to concern over immigrant health and health behaviors due to financial externalities. Access to health insurance for legal immigrants is likely to increase with the full implementation of the Patient Protection and Affordable Care Act (ACA). Thus, immigrants will likely contribute a larger share to public health care costs over time.

We first corroborate labor studies that show as immigrants assimilate to the U.S. they make gains in earnings and occupational prestige. We find evidence that labor supply attributes improve with time in the U.S. among women but not men. We also find that residence in an ethnic enclave may impede employment outcomes for immigrants of both sexes. Next we show that assimilation into U.S. society also allows immigrants to access employment-based anti-smoking policies and smoking cessation services. Perhaps somewhat surprisingly, we find little evidence than enclave residence influences access to these programs and services among men, and some evidence that enclave residence may hinder access among women. We identify complex relationships among assimilation, enclave residence, and smoking outcomes. Although smoking ban coverage is extended to immigrant workers, we find little evidence that cigarette taxes and venue-specific smoking bans substantially impact immigrant smoking outcomes.

2. Related Literature

In this section, we briefly review the economic literatures on assimilation and ethnic enclave residence. Each of these literatures is large and growing, and therefore it is beyond the scope of this study to comprehensively discuss each. Instead, we aim to highlight the work most relevant for our study. Moreover, there are likely interactions between degree of assimilation and the decision to reside in the enclave that we do not address in this review. Lastly, we focus here on immigration to developed countries only.

2.1 Assimilation

Economists have historically taken great interest in how immigrants assimilate, or converge towards natives, in terms of standard employment outcomes, e.g., earnings, occupation. The term assimilation suggests the extent to which an immigrant identifies with the social norms in the host country relative to the country of origin (Borjas, 1995). Economists typically model assimilation as a linear process proxied by the number of years since immigration (Antecol and Bedard, 2006; Chiswick, 1978; Leung, 2013; Lubotsky, 2007).

The immigrant literature is tightly linked with the Roy model of worker sorting (Roy, 1951). Chiswick (1978) pioneered this line of research using a Mincerian human capital framework. In this framework, the decision to immigrate is interpreted as investment in one's human capital. Immigrants are viewed as rational decision makers who maximize their lifetime utility subject to constraints. They decide to invest in their human capital, or immigrate in this case, when the expected benefits exceed expected costs. Chiswick (1978) predicted positive selection into immigration based on ability and work ethic for example; more capable workers could extract higher benefits from immigration, and face lower costs, than less capable workers. Chiswick (1978) documented that immigrants initially experience worse employment outcomes relative to natives as their skills may not be perfectly transferable to the host country labor

market, but eventually exceed natives. Multiple studies reinforced Chiswick's findings (Abbott and Beach, 1993; Borjas, 1982; Carliner, 1980). However, subsequent work questioned the extent to which immigrants are positively selected and, in turn, their ability to exceed native labor market outcomes (Borjas, 1985, 1995). Although there are important and outstanding questions in this literature regarding the degree of selection and assimilation, on net, the existing evidence shows some degree of assimilation in terms of earnings for most immigrant groups (Beenstock et al., 2010; Chiswick and Miller, 2011, 2012; Lubotsky, 2007). What we ask here is whether the economic assimilation process extends to employment-based anti-smoking policies and cessation services. These outcomes may proxy for a broader class of employment features that impact health and, in turn, health care costs and productivity.

An important and related concept is the "healthy immigrant effect". On arrival to the host country immigrants typically have better health than natives. This effect may be attributable to positive selection on health into immigration, domestic legislative rules on who can immigrate, and healthier norms in the sending countries (Hull, 1979; Kennedy et al., 2006). However, immigrant health assimilates towards native health levels over time (Anson, 2004; Antecol and Bedard, 2006; Hao and Kim, 2009; Hull, 1979; Kaplan et al., 2004; Marmot et al., 1984; Palloni and Morenoff, 2001; Park et al., 2009). Potential mechanisms for the decline in health include uptake of host county health behaviors (e.g., smoking, consumption of high calorie foods, sedentary lifestyles, and substance use and misuse) and under-reporting of health conditions at migration (Antecol and Bedard, 2006; Biddle et al., 2007; Kennedy et al., 2006).

2.2 Ethnic enclaves

An ethnic enclave is a physical space with high ethnic concentration and is culturally distinct from the larger society. Immigrants often live in the enclave as they establish themselves

in the new country (Bartel, 1989; Borjas, 1998). Residing in the enclave at arrival may be a rational decision for many as shared language, social networks, and cultural norms within the enclave that may lower the costs (both pecuniary and non-pecuniary) to immigration. As articulated by Bertrand et al. (2000) enclave residence may impact employment outcomes through information (e.g., employment opportunity knowledge, job referral networks) and social norms (e.g., attitudes toward working, self-employment, work effort). Moreover, by reducing the need to adopt host country-specific capital (e.g., language) residence in the enclave may further impede immigrant integration and economic assimilation (Chiswick, 1991; Lazear, 1999). Lastly, Chiswick and Miller (2005) argue that immigrants may be willing to accept a lower wage job if the job is located in an enclave as this lowers the cost to accessing ethnic goods and services (e.g., traditional food). While a lower reservation wage may increase employment, it may reduce overall earnings and job match quality. Taken together the impact of enclave residence on employment outcomes is *ex ante* ambiguous. Indeed, whether enclaves help or hinder immigrant employment outcomes depends critically on the quality of the enclave in terms of shared information, social norms pertaining to employment, and geographic location relative to good jobs. There is evidence of negative selection into enclaves: less able immigrants are more likely to reside in enclaves (Damm, 2009). Such selection suggests that enclaves may not provide high quality information and social norms

Perhaps not surprisingly, the research on enclaves is decidedly mixed. Residence in an enclave has been shown to impact immigrant employment outcomes both negatively (Bertrand et al., 2000; Collins and Margo, 2000; Kondylis, 2010; Liu, 2009; Warman, 2007) and positively (Bell and Machin, 2013; Damm, 2009; Edin et al., 2003). Further complicating analyses, immigrant characteristics (e.g., skill level, country of origin, vintage of enclave members, and

popular occupations) may determine whether enclave residence impacts these outcomes (Åslund and Fredriksson, 2009; Beaman, 2012; Borjas, 2005; Patel and Vella, 2012).

Turning to health outcomes, if shared information and social norms provide protection against unhealthy American practices, then residence in an enclave may improve smoking outcomes for residents. Of particular relevance for our work are several studies that suggest living in communities with higher proportions of individuals who speak the same language or share ethnic background may allow individuals to better access health and social services through sharing knowledge of service availability and steps to access these services (Aizer and Currie, 2004; Aslund and Fredriksson, 2009; Deri, 2005; Devillanova, 2008; Gresenz et al., 2009). Extrapolating from these studies suggests that enclave residence may allow residents to better access health information related to smoking (e.g., effective and low cost cessation products) and may be more likely to retain their lower smoking rates.

However, if residence in the enclave impedes immigrants' ability to obtain high quality jobs (e.g., high wages, workplaces that ban smoking, and employer-sponsored smoking cessation programs) we might expect enclaves to lead to worse smoking outcomes (in terms of smoking and passive smoking) for residents. Moreover, if immigrants who reside in enclaves work in low quality jobs that lead to job-related strain, residents may self-medicate through smoking and use of other substances. And indeed, recent studies suggest that immigrants work in more hazardous occupations than native born Americans (Davila et al., 2011; Hersch and Viscusi, 2010; Orrenius and Zavodny, 2009). Lastly, if residence in an enclave provides immigrants with access to lower price smoking products (e.g., if sellers do not enforce taxes) then enclaves may lead to worse smoking outcomes via price effects.

3. Data and empirical model

3.1 Current Population Survey Tobacco Use Supplement Data

We draw data on employment and smoking outcomes from the Current Population Survey (CPS) Tobacco Use Supplements (TUS). The TUS was administered as a supplement to the basic monthly CPS in 1992 to 1993, 1995 to 1996, 1998 to 1999, 2000 to 2003, 2006 to 2007, and 2010 to 2011. Beginning in 1994, all CPS respondents were asked about their country of birth. Thus we restrict our sample to the 1995 to 2011 TUS surveys which include 199,681 immigrants. We exclude proxy respondents and immigrants younger than 18 years as TUSs fielded after 2006 do not include respondents below age 18. After making additional sample exclusions necessary for our research design our final analysis sample includes 149,735 immigrants as described in a later section of the manuscript.

3.2 Outcomes

We examine three sets of outcomes: standard employment outcomes, access to employment-based anti-smoking policies and cessation services, and smoking outcomes. Not all variables are available in all TUS and Appendix Table A reports variable availability by year.

3.2.1 Employment outcomes

A limitation of our study is that the TUS is administered as a supplement to the basic monthly CPS survey and thus we do not have access to the rich income and employment variables that are contained in other supplements e.g., the commonly utilized Annual Social and Economic Supplement that is fielded in March. We select the most appropriate employment outcomes, but they likely do not fully capture all important aspects of the economic assimilation process: any employment at the time of the survey (coded one if the respondent is employed, and zero otherwise), annual family income (converted to 2011 dollars using the Consumer Price

Index [CPI]), and an indicator for employment in white collar job (coded one if the respondent reports a professional or managerial occupation and zero otherwise).[1]

3.2.2 Access to employment-based anti-smoking policies and cessation services

We next construct a set of separate indicator variables that measure 1) access to employment-based anti-smoking, formal, full office smoking ban; 2) working in a job with an un-enforced smoking ban[2] (a firm may have a ban on the books, as many states regulate smoking in the workplace, but may chose not to enforce the ban); and 3) working in a job that offers a smoking cessation program to employees. The employment-based variables are asked only of respondents who report working.

Employment at a worksite that bans smoking protects against passive smoking via exposure to secondhand smoke and may increase the hassle cost of smoking. Access to workplace smoking cessation programs can reduce the costs (both monetary and non-monetary) of smoking cessation. Moreover, medical evidence suggests that well-designed worksite smoking cessation programs are effective in promoting cessation (Volpp et al., 2009).

3.2.3 Smoking outcomes

Our first smoking outcome is an indicator for any cigarette smoking use (coded one if the respondent reports smoking at the TUS interview, and zero otherwise). Next, we measure the number of cigarettes smoked in the past 30 days among current smokers. Past 30 day smoking information is obtained in a multiple variable sequence. All respondents who report smoking 100 cigarettes in their lifetime are asked whether they currently smoke no days, some days, or every day. Someday smokers are asked the number of days smoked in the past 30. We assume

[1] Specifically, we use 2-digit CPS primary industry codes 1 (management, business, and financial occupations) and 2 (professional and related occupations).

[2] The question wording is "During the past two weeks has anyone smoked in the area in which you work?" The question is asked to those workers who report an official smoking ban in the workplace.

that every day smokers smoked on all of the past 30 days. All smokers are asked the number of cigarettes they smoke per day. We construct cigarettes smoked in the past 30 days by multiplying the number of cigarettes smoked per day by the number of days smoked.[3] Lastly, we examine past year smoking cessation. Survey administrators ask a retrospective question on smoking patterns one year ago to both current and former smokers. We code as 'past year quitters' respondents who report smoking one year prior to the survey and not smoking at the survey as one, and zero otherwise.

3.3 Assimilation

To proxy for assimilation, we follow the economics literature and construct a linear measure of years since migration to the U.S. Respondents are asked for the year in which they entered the U.S. This variable is categorical, and we assign the mid-point year to each interval. When the mid-point year is not an integer (e.g., 1966.5) we round up to the nearest integer. An exception is when entry date is truncated. For example, in the 2003 TUS, the earliest value is "before 1950" and we assign immigrants an entry year of 1950. This imputation underestimates the years since migration for early arrivers. We then subtract the year of entry from the survey year to determine the years since migration. A limitation of this variable is that it does not incorporate reverse migration and overestimates years in the U.S. for those immigrants who return to the home country at some point between the reported year of entry and the survey year.

3.4 Ethnic enclave

We use data from the 1990 and 2000 5% file of the U.S. Censuses and the 2005 to 2011 American Community Surveys (ACS) to construct our ethnic enclave measures.[4] Both data sets were extracted from the Integrated Public Use Microdata Series (Ruggles et al., 2010) and

[3] Results are consistent if we use the logarithm of cigarettes smoked to address skewness.
[4] We do not utilize earlier ACS data sets (2001 to 2004) as they do not contain metropolitan statistical area of residence. As detailed later in the manuscript this information is necessary for our analysis.

contain detailed information on birth country. We limit the sample to the top 50 sending countries in the 1990 Census as sample sizes for particular countries become small outside this range. These countries comprise over 86% of immigrants in the TUS. Appendix Table B lists the top 50 countries, the number of respondents from each country in our analysis sample, the smoking rate in these countries in either 2006 or 2009 from external sources (Leung, 2013; World Health Organization, 2013), and the smoking rate for each country based on our analysis sample. An interesting feature of this table is that many immigrants in our sample arrive from countries with higher smoking rates than the U.S. For example, the smoking rate in Greece is 52%, roughly double the U.S. rate. However, TUS sample smoking rates are lower than the country rates for all countries suggesting positive selection into immigration on smoking.[5]

We construct our proxy for ethnic enclaves as the proportion of individuals from the same birth country in the metropolitan statistical area (MSA) of residence. MSA information is available for roughly 70% of the TUS, and we exclude respondents with missing MSA information. For example, if a respondent reports that he was born in Ireland and resides in the New York City MSA (NYC), we assign him the following value: $\frac{(population\ born\ in\ Ireland)_{NYC,t}}{(total\ population)_{NYC,t}} * 100$. See the Data Appendix for more details on construction of this variable.

Although it may be appealing to define the enclave at a smaller geographic level (e.g., census track), the finest geographic unit available for a substantial proportion of TUS respondents is the MSA.[6] We view our MSA-level measure as a proxy for access to enclaves. Put differently, an immigrant who lives in an MSA with a higher proportion of own-ethnics likely has better access to an enclave than an otherwise similar immigrant.

[5] The exception is Puerto Rico which is a location used in the sample even though it is not a separate country.
[6] The TUS contains county of residence for roughly 30% of the sample, but our sample sizes become too small to estimate our regression models and we choose not to utilize this information in our study.

3.5 State anti-smoking policies and characteristics

We include two standard anti-smoking policies in our regression models: the state cigarette tax in dollars (Orzechowski and Walker, 2012) and a venue-specific smoking ban index (Centers for Disease Control and Prenvetion, 2012). We convert the cigarette taxes to 2011 dollars using the Consumer Price Index (CPI). Our venue-specific smoking ban index includes information on bans in government worksites, private worksites, restaurants, shopping malls, and bars. The variables indicate the severity of the smoking ban (range: 0 to 3) and higher values indicate more restrictive bans. We sum across these 5 venues to construct an index of smoking ban severity, and the allowable range for the summed variable is 0 to 15. We include policies in all regressions for consistency, although results are robust to their exclusion. We expect these policies to be most relevant for our smoking outcomes. Lastly, we include the state unemployment rate as both employment and smoking outcomes may respond to changes in the business cycle (Ruhm, 2005).

3.6 Other control variables

In all regression models we include age (25 to 34 years, 35 to 44 years, 45 to 54 years, 55 to 64 years, and 65 and above years, with 18 to 24 years as the omitted category), race/ethnicity (African American, Hispanic, and other race, with white as the omitted category), educational attainment (high school, some college, college graduate, and post college, with less than high school as the omitted category), and marital status (divorced, separated, or widowed, and never married, with married as the omitted category).

3.7 Empirical model

In this section we describe our empirical strategy for identifying the association between economic assimilation and ethnic enclave residence, and the outcomes described above. Our core model is a regression that takes the following form:

(1) $S_{ibmt} = \alpha_0 + \alpha_1 A_{ibmt} + \alpha_2 E_{bmt} + P_{st}\alpha_3 + X_{ibmt}\alpha_4 + B_b\alpha_5 + M_m\alpha_6 + D_t\alpha_7 + \varepsilon_{ibmt}$

S_{ibmt} is an employment or smoking outcome for individual *i* born in country *b* residing in MSA *m* in year *t*. We utilize a linear probability model when the outcome is binary and least squares when the outcome is continuous. The key explanatory variables are A_{ibmt} (number of years since migration) and E_{bmt} (percent of MSA own-ethnic). P_{st} is a vector of state policies and characteristics, and X_{ibmt} is a vector of personal characteristics. B_b, M_m, and D_t are vectors of birth country, MSA of residence, and year fixed effects.

Inclusion of birth country fixed effects controls for difficult to observe characteristics that are correlated with birth country and our outcomes. Moreover, inclusion of these fixed effects partially addresses non-random selection into migration that may vary by birth country. By including MSA fixed effects in our regression models we utilize within MSA variation in our predictor variables to estimate relationships. As noted by Borjas (1985) years since migration and survey year linearly determine the year of arrival in a pooled cross-sectional analysis as utilized here. Thus, the survey year fixed effects control for cohort effects (e.g., changes in cohort quality over time). Moreover, the year fixed effects capture national trends in our outcomes. ε_{ibmt} is the random error term. We cluster standard errors around the birth country, although our results are robust to clustering at the MSA level. All models are estimated separately by sex given different labor market patterns across men and women.

4. Results

4.1 Summary statistics

Tables 1A (men) and 1B (women) report summary statistics for both the immigrant sample and the native born samples, the latter is included for comparison purposes. On average, immigrants have worse employment outcomes than native born Americans. They are less likely to be employed, have lower family income, and are less likely work in a white collar job ($p<0.01$). An exception to this pattern is that immigrant men are *more* likely to report being employed at the survey than native men. This difference may be due to the fact that immigrants are more likely to be of working age than the native born population.

Turning next to our measures of employment-based anti-smoking policies and cessation services, we find a similar pattern: immigrants are less likely to work at a worksite that fully bans smoking in working areas and are less likely have access to a worksite smoking cessation program. Interestingly, immigrant men are slightly *less* likely to be employed at a worksite that does not enforce its smoking ban (8% vs. 9%) while a comparable proportion of immigrant and native women report this outcome (5%). Consistent with healthy immigrant theories, our sample of immigrants is less likely to smoke than the native born population of adult smokers (16% vs. 22% among men and 7% vs. 18% among women) and smoked fewer cigarettes in the past 30 days (306 vs. 500 among men and 287 vs. 413 among women). Immigrant and native men are equally likely to report quitting in the past year (9%) but immigrant women are more likely than native women to report a past year quit (10% vs. 9%).

The average number of years since migration to the U.S. among immigrant men and women is 18.77 and 19.72 respectively. Immigrant men and women live in MSAs with 2.85% and 2.60% of the population from their country of birth respectively. Immigrants tend to reside in states with more restrictive smoking policies as measured by cigarette taxes and venue-

specific smoking bans, and higher unemployment rates. Other personal characteristics suggest that immigrants are on average less advantaged than natives.

4.2 Employment outcomes regression results

Table 2 reports selected results from regressions of our standard employment outcomes (labor supply, earnings, and occupation) on measures of assimilation, ethnic enclave residence, and other controls. Among men, we find little evidence that either assimilation or ethnic enclave residence predicts the probability of employment: for each the coefficient is small and statistically indistinguishable from zero. Turning to women, we find evidence that longer residence in the U.S. is associated with a higher probability of employment. Specifically, 1 additional year residing in the U.S. is associated with a 0.3 percentage point (0.6%) increase in the probability of employment. Among both men and women we find that assimilation into U.S. society allows immigrants to move up the employment ladder: the more years an immigrant resides in the U.S. the higher is his/her family income and the more likely he/she is to work in a white collar job (relative to a blue collar or service job). For example, an additional year in the U.S. is associated with a 0.24 percentage point (1%) and 0.31 percentage point (1%) increase in the probability of working in a white collar job among immigrant men and women respectively. However, residence in an ethnic enclave may impede these transitions: residence in MSAs with higher proportion own-ethnic is associated with lower family incomes among both men and women. A higher percent own-ethnic is associated with a lower probability of white collar work among men: a 1 percentage point increase in the percent own-ethnic is associated with a 0.21 percentage point (1%) decrease in the probability of a white collar job.

We include our state anti-smoking policies in these regressions for completeness not because we believe that that they should influence these outcomes. Somewhat surprisingly, we

find that among women, cigarette taxes are positively associated with the probability of working in a white collar job while the smoking ban index is negatively associated with family income. These associations likely capture residential sorting rather than true causal effects. For example, cigarette taxes are typically higher in states with more highly skilled workers (e.g., California, New York). We do not observe these relationships in the male sample.

4.3 Access to employment-based anti-smoking policies and cessation services regression results

In Table 3 we assess whether the associations between our immigration measures (assimilation and ethnic enclave residence) and standard employment outcomes (Table 2) extend to employment-based anti-smoking policies and cessation services. We find strong evidence that the positive links between economic assimilation into the U.S. labor market are associated with improved access to anti-smoking policies and smoking cessation services through employment among both men and women. For example, among men, an additional year of residence in the U.S. leads to a 0.2 percentage point (0.2%) increase in the probability of working at a job with a full smoking ban in work areas and a 0.2 percentage point (1.8%) increase in the probability of having access to a worksite smoking cessation program. Findings among women are consistent, and, in addition, assimilation is negatively associated with the probability of an unenforced worksite smoking ban (we observe a similarly signed coefficient among men, but the association is imprecisely estimated).

Turning to our proxy for ethnic enclave residence, percent own-ethnic in the MSA of residence, we find a different pattern of results. Among men residence in an ethnic enclave is not a strong predictor of our employment-based anti-smoking policies and cessation services variables. Among women, we find some evidence that residing in an enclave is associated with a higher probability of an unenforced worksite smoking ban: a 1 percentage point increase in the

percent own-ethnic in the MSA of residence is associated with a 0.08 percentage point (1.6%) increase in the probability of an unenforced smoking ban. This coefficient is only marginally statistically significant ($p < 0.10$), however.

We find evidence, particularly among men, that state venue-specific smoking bans and the protection they provide from passive smoking are passed through to immigrant workers: the state smoking ban index is positively associated with the probability of reporting a worksite smoking ban and negatively associated with the probability that the worksite smoking ban is not enforced (the former association is imprecise among women). For example, a 1 unit increase in the smoking ban index (recall that the mean of this variable is 6.8 on a scale that ranges from 0 to 15) is associated with a 0.32 percentage point (0.4%) increase in the probability of an office smoking ban and a 0.23 (2.9%) reduction in the probability of an unenforced ban among men.

4.4 Smoking outcomes regression results

We next consider smoking outcomes. We identify sex-specific relationships between assimilation and the probability of being a current smoker: longer residence in the U.S. is associated with a lower (higher) probability of smoking among men (women). Our findings suggest that an additional year in the U.S. is associated with a 0.07 percentage point (0.4%) decrease in the risk of smoking among men and a 0.07 percentage point (0.9%) increase in the risk of smoking among women.

This seeming gender inconsistency may be explained by something akin to regression to the mean. Immigrant women smoke at a rate of 39% of that of native women (7% vs. 18%) but immigrant men smoke at almost 73% of the rate of native men (16% vs. 22%.). Thus for women, assimilation may lead to immigrant women behaving more like native women as they reduce the larger discrepancy in smoking behavior. The smoking rates by gender are much more

equal in the U.S. by gender as compared to other countries (World Health Organization, 2013). This pattern of gender-specific results is consistent with recent work by Leung (2013), however.

Interestingly among both male and female smokers, additional years in the U.S. is associated with a higher number of cigarettes smoked in the past 30 days: 1 additional year of residence is associated with 1.7 (0.5%) and 1.3 (0.4%) additional cigarettes smoked in the past 30 days among male and female smokers. The increase in the number of cigarettes smoked with assimilation may be explained by the relatively light immigrant smokers being those most likely to quit. Assimilation is not a strong predictor of successful quits among men, while more years in the U.S. is positively associated with a successful quit in the past year among women (an additional year is associated with a 0.09 percentage point, or 0.9%, increase in this outcome). This latter finding for cessation is consistent with the increase in number of cigarettes smoked with time in the U.S. among immigrant women documented earlier in this manuscript. Moreover, perhaps immigrant women are better able to access and successfully utilize smoking cessation services once they integrate into the U.S. labor market than immigrant men.

The relationship between enclaves and smoking outcomes appears to be sex-specific: residence in an enclave may protect men against smoking but may hinder women who smoke in their attempts to quit. Consistent with recent work (Leung, 2013), and troubling from a health policy perspective, we find little evidence that increases in cigarette taxes or implementation of more restrictive venue-specific smoking bans are associated with the risk of smoking or probability of quitting. The coefficients on these variables are generally imprecise and often counter to our predictions. Moreover, we find only weak evidence that smoking policies are associated with changes in smoking patterns on the intensive smoking margin (i.e., the number of cigarettes smoked in the past 30 days). Among male immigrant smokers a $1 increase in the

state cigarette tax is associated with 22 fewer cigarettes (7%) smoked in the past 30 days, and the implied tax-elasticity is -0.07. Surprisingly, for women the findings suggest that *increases* in the cigarette tax are associated with *increases* in the number of cigarettes smoked and this association is arguably both statistically and practically significant: a $1 increase in the cigarette tax is associated with 24 more cigarettes smoked in the past 30 days (8.2%). We suspect that this latter finding is likely attributable to residential sorting of immigrant women into high cigarette tax states rather than true causal effects. The coefficients on more restrictive venue-specific smoking bans are significantly indistinguishable from zero for both sexes.[7]

5. Robustness checks and extensions

We next examine the robustness of our findings to a number of sensitivity checks to assess the stability of our findings and extensions to the core analysis.

5.1 Defining the enclave at an alternative geographic level

In our core regressions, we construct our measure of the ethnic enclave at the MSA level. However, this requires that we exclude respondents that cannot be linked to MSA of residence information. Moreover, respondents that cannot be linked to MSA information are more likely to be rural thus this exclusion may impose sample selection bias. We next re-construct our enclave measures at the state level. The tradeoff here is that the state is likely an even cruder level at which to model the enclave than the MSA. The benefit, however, is that we do not exclude rural respondents and sample selection concerns are minimized. Moreover, we are able to utilize additional ACS surveys, specifically the 2001 to 2004 surveys, in our enclave construction algorithm as all ACS surveys contain state of residence information (as noted earlier

[7] In unreported analyses we interacted the assimilation variables with the anti-smoking policy variables. We also separate the smoking ban index by venue (i.e., bans in restaurants, bars, government worksites, private worksites, and shopping malls). No clear patterns emerged from these analyses. These results are available on request.

in the manuscript, this is not true for MSA of residence). Thus less imputation is required to construct the enclave measure. More details on variable construction are available on request.

We re-estimate Equation (1) using our state-level enclave measures. Our results are presented in Appendix Tables C (employment outcomes), D (access to employment-based anti-smoking policies and cessation services), and E (smoking outcomes). Importantly for our study the results are broadly consistent with the core model results presented in Tables 2, 3, and 4.

5.2 Mexican immigrants

Mexican immigrants represent the largest group of immigrants in our sample and receive a fair amount of attention in policy debates. In unreported analysis, we re-estimate our models for respondents born in Mexico only. In these regressions we remove the birth country fixed effects and cluster standard errors around the MSA. The findings are broadly consistent with those reported earlier this manuscript. However, the coefficient estimates generated in the smoking outcome regressions are generally smaller in magnitude and less precisely estimated than those generated in the full sample. These findings suggest that Mexican immigrants are even less responsive to standard anti-smoking policies than the broader group of immigrants.

5.3 State anti-smoking policy dynamics

Smoking is an addictive habit and it may take time for smokers to adjust their smoking outcomes following a price increase (either monetary as measured by the cigarette tax or non-monetary as measured by venue-specific smoking bans). We include contemporaneous anti-smoking policies in our core regressions and this may mask the policy impacts if such dynamics are present. In unreported analyses, we re-estimate our regressions using a one year lag in policy variables. Results are generally consistent in sign but show even weaker associations between the policies and smoking outcomes.

5.4 Addressing between state unobservables

In our core models we control for MSA fixed effects as we construct our measure of the ethnic enclave at this level. We measure our state policies at a higher level of aggregation, however. To better address difficult-to-observe characteristics that are correlated with both the state policies and our outcome variables we replace the MSA fixed effects with state fixed effects and re-estimate our models. The findings, available on request, are consistent in terms of sign, magnitude, and statistical significance to the results reported in this manuscript.[8]

6. Conclusions

In this study we provide new and policy relevant evidence on the roles of economic assimilation and enclave residence in immigrant smoking. We first confirm findings from earlier studies that show assimilation allows immigrants to achieve greater economic success in terms of labor supply, earnings, and occupational prestige. We then extend this perspective and show that assimilation similarly conveys better working conditions in terms of coverage by workplace smoking bans and access to smoking cessation programs. In contrast however, residence in an enclave may impede these outcomes. We find mixed patterns of results for smoking outcomes with differing patterns by sex. Specifically, both assimilation and enclave seem to reduce the smoking rates of men. But for women, assimilation increases the smoking rate. This seeming inconsistency by sex is likely to different male and female smoking rates in the sending countries: male (female) immigrants tend to arrive from countries with relatively high (low) male (female) smoking rates (Leung, 2013). Thus, males experience healthy assimilation while females experience unhealthy assimilation (at least in the context of smoking). The smoking rate by sex is much more equal in the U.S. as compared to the comparable rate in other countries.

[8] We attempted to utilize an instrumental variable framework following Bertrand et al (2000), but our IVs were underpowered and we chose not to present these findings.

Moreover, the increase in the number of cigarettes with assimilation may be explained by the relatively light immigrant smokers being those most likely to quit. Some of these findings are difficult to explain fully and likely relate to a complex structure of conflicting impacts. More detailed data are needed to more fully understand the structure of these relationships.

We find evidence that assimilation increases coverage of immigrants by workplace smoking bans. Even if all bans are not strictly enforced, this is likely beneficial for immigrants. The benefits can be of two kinds. The direct impact of a smoking ban would be to reduce smoking, but we do not find this in our results. We do not have any measures of the indirect, beneficial impact of reducing exposure to the passive smoking of other workers. Unfortunately our data set does not contain direct measures of passive smoke exposure, such as cotinine levels. That cigarette taxes are statistically indistinguishable from zero in participation and quitting behavior regressions is surprising given the vast literature on this topic (Chaloupka and Warner, 2000), however it is consistent with recent work by Leung (2013). Perhaps living in an enclave allows tax avoidance by obtaining cigarettes from non-taxed sources. We find some evidence that higher taxes reduce the number of cigarettes smoked in the past 30 days but only among men. More work is needed to understand these null findings for public policy effects.

While we extend the literature in multiple dimensions and use a rich and extensive data set, the study has several limitations. Although we address selection into migration, at least partially by including birth country fixed effects in our regressions, it is unlikely that we are able to address all sources of bias. Moreover, we are unable to adequately account for compositional changes among the immigrant population over time (inclusion of survey fixed effects addresses this concern to some extent). Our measure of assimilation is vulnerable to both rounding error and recall bias, and does not capture reverse migration. Also, although we rely on self-reported

smoking outcomes which probably contain error, self-reports have been shown to be valid for statistical analyses (Patrick et al., 1994). Lastly our proxy measure for financial success is family income, not personal earnings or wages.

Our also study provides new information on the effectiveness of a standard health policy lever in a growing segment of the U.S. population at a critical time. The federal government is currently proposing to increase the federal cigarette tax by 94 cents, nearly doubling the current rate ($1.01). Our results suggest that this tax increase, although the largest increase in the federal cigarette tax history, will not lead to substantial reductions in smoking among the majority of immigrant smokers. Instead, the tax may be regressive and crowd out consumption of other goods and services, or the enclave may protect them from bearing the full tax due to tax avoidance (Busch et al., 2004; Colman and Remler, 2008).

Importantly, what we learn in this study for tobacco regulation could inform regulation of other health behaviors such as obesity and substance misuse. For example, obesity has reached an epidemic level in the U.S. with just over 35% of the adult population being obese (Flegal et al., 2012). A 'fat tax' has been suggested to fight obesity (Allais et al., 2010). Our findings suggest that living in an enclave may shelter immigrants from taxes and other policies, while assimilation may expose them to such public policies. That is, the healthy immigrant may not only adopt the harmful U.S. behaviors but may be prevented from being helped by public health policies. This suggests that to help some of the immigrant groups, other ways to motivate smoking cessation must be used (Sindelar and O'Malley, 2013). In order to fully understand the heterogeneity in the impact of public health policies, the behaviors of immigrants should be better understood as they represent an increasing segment of our society.

Table 1A. Summary statistics among men

	Immigrants	Natives	*p*-value for difference
Dependent variables	**Prop/mean**	**Prop/mean**	**--**
Employment outcomes			
Employed	0.76	0.72	0.0000
Family income (1,000s)	67.05	88.87	0.0000
White collar status	0.24	0.35	0.0000
Smoking policies at work			
Full office smoking ban	0.78	0.80	0.0000
Office smoking ban not enforced	0.08	0.09	0.0001
Work cessation plan	0.11	0.19	0.0000
Smoking outcomes			
Smoke (at survey)	0.16	0.22	0.0000
Cigarettes smoked in the past 30 days	305.83	500.17	0.0000
Quit (past year)	0.09	0.09	0.3443
Assimilation and enclave variables			
Years since migration	18.77	--	--
Percent own-ethnic in MSA of residence	2.85	88.97	0.0000
State anti-smoking policies and characteristics			
Cigarette tax (dollars)	1.02	0.86	0.0000
Smoking ban index (range 0-15)	6.71	5.27	0.0000
State unemployment rate	5.93	5.53	0.0000
Personal characteristics			
18-24 years	0.12	0.14	0.0000
25-34 years	0.25	0.19	0.0000
35-44 years	0.24	0.21	0.0000
45-54 years	0.18	0.19	0.0000
55-64 years	0.10	0.13	0.0000
65+ years	0.11	0.14	0.0000
White	0.71	0.85	0.0000
African American	0.06	0.13	0.0000
Other race	0.23	0.03	0.0000
Hispanic	0.53	0.07	0.0000
Less than high school	0.35	0.12	0.0000
High school	0.24	0.30	0.0000
Some college	0.16	0.28	0.0000
College	0.25	0.30	0.0000
Married	0.66	0.58	0.0000
Divorced[1]	0.09	0.14	0.0000
Never married	0.25	0.29	0.0000
N	70,314	418,955	

Notes: Observations with missing information excluded from the sample. The observation count is based on the number of individuals who responded to the smoking status question.
[1]Includes divorced, separated, and widowed.

Table 1B. Summary statistics among women

	Immigrants	Natives	*p*-value for difference
Dependent variables	**Prop/mean**	**Prop/mean**	**--**
Employment outcomes			
Employed	0.52	0.60	0.0000
Family income (1,000s)	65.37	80.42	0.0000
White collar status	0.28	0.39	0.0000
Smoking policies at work			
Full office smoking ban	0.83	0.86	0.0000
Office smoking ban not enforced	0.05	0.05	0.3977
Work cessation plan	0.16	0.20	0.0000
Smoking outcomes			
Smoke (at survey)	0.07	0.18	0.0000
Cigarettes smoked in the past 30 days	286.89	413.44	0.0000
Quit (past year)	0.10	0.09	0.0458
Assimilation and enclave variables			
Years since migration	19.72	--	--
Percent own-ethnic in MSA of residence	2.60	89.02	0.0000
State anti-smoking policies and characteristics			
Cigarette tax (dollars)	1.04	0.86	0.0000
Smoking ban index (range 0-15)	6.78	5.22	0.0000
State unemployment rate	5.90	5.52	0.0000
Personal characteristics			
18-24 years	0.10	0.13	0.0000
25-34 years	0.22	0.18	0.0000
35-44 years	0.23	0.20	0.0000
45-54 years	0.18	0.18	0.0000
55-64 years	0.12	0.13	0.0000
65+ years	0.14	0.18	0.0000
White	0.67	0.83	0.0000
African American	0.07	0.15	0.0000
Other race	0.26	0.02	0.0000
Hispanic	0.48	0.06	0.0000
Less than high school	0.33	0.12	0.0000
High school	0.25	0.31	0.0000
Some college	0.18	0.30	0.0000
College	0.23	0.27	0.0000
Married	0.61	0.51	0.0000
Divorced[1]	0.21	0.25	0.0000
Never married	0.18	0.24	0.0000
N	79,421	482,003	

Notes: Observations with missing information excluded from the sample. The observation count is based on the number of individuals who responded to the smoking status question.
[1]Includes divorced, separated, and widowed.

Table 2. Selected associations among assimilation, enclaves, and employment outcomes among immigrants

	Employed	Family income (1,000s)	White collar status[1]
Men			
Proportion/mean	0.76	67.90	0.25
Years since migration	-0.0004	0.6575***	0.0024***
	(0.0005)	(0.1064)	(0.0006)
Percent own-ethnic	-0.0002	-0.1848**	-0.0021***
	(0.0003)	(0.0888)	(0.0007)
Cigarette tax (dollars)	0.0045	-0.5638	-0.0032
	(0.0059)	(1.0016)	(0.0056)
Smoking ban index	-0.0004	-0.2141	-0.0008
	(0.0009)	(0.1389)	(0.0008)
N	60,954	54,446	43,955
Women			
Proportion/mean	0.52	65.39	0.28
Years since migration	0.0033***	0.4630***	0.0031***
	(0.0004)	(0.0877)	(0.0006)
Percent own-ethnic	0.0010**	-0.1035*	0.0001
	(0.0004)	(0.0591)	(0.0007)
Cigarette tax (dollars)	0.0046	0.0415	0.0178***
	(0.0075)	(0.8562)	(0.0058)
Smoking ban index	0.0016**	-0.2103**	-0.0012
	(0.0006)	(0.0998)	(0.0008)
N	68,907	61,335	34,450

Notes: All equations estimated with a linear probability model (binary outcomes) or least squares (continuous outcomes) and adjust for age, race/ethnicity, education, marital status, state unemployment rate, birth country, MSA fixed effects, and year fixed effects. Standard errors are clustered by the birth country and reported in parentheses.
[1]Conditional working sample.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 3. Selected associations among assimilation, enclaves, and access to employment-based anti-smoking policies and services among immigrants

	Office work ban[1]	Work ban not enforced[1]	Work cessation plan[1]
Men			
Proportion	0.77	0.08	0.12
Years since migration	0.0017***	-0.0002	0.0022***
	(0.0003)	(0.0002)	(0.0004)
Percent own-ethnic	-0.0018	0.0003	-0.0016
	(0.0016)	(0.0005)	(0.0012)
Cigarette tax (dollars)	0.0094	-0.0079	-0.0116
	(0.0096)	(0.0054)	(0.0108)
Smoking ban index	0.0032**	-0.0023**	-0.0014
	(0.0013)	(0.0009)	(0.0015)
N	16,649	12,051	13,272
Women			
Proportion	0.82	0.05	0.16
Years since migration	0.0021***	-0.0006***	0.0023***
	(0.0003)	(0.0002)	(0.0004)
Percent own-ethnic	-0.0032	0.0008*	0.0000
	(0.0021)	(0.0004)	(0.0010)
Cigarette tax (dollars)	0.0106	0.0050	-0.0114
	(0.0092)	(0.0059)	(0.0124)
Smoking ban index	0.0007	-0.0012*	0.0018
	(0.0015)	(0.0007)	(0.0021)
N	18,290	13,612	10,969

Notes: All equations estimated with a linear probability model and adjust for age, race/ethnicity, education, marital status, state unemployment rate, birth country, MSA fixed effects, and year fixed effects. Standard errors are clustered by the birth country and reported in parentheses.

[1]Conditional working sample.

[2]Conditional smoking sample.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 4. Selected associations among assimilation, enclaves, and smoking outcomes among immigrants

	Smoke	Number of cigarettes smoked past 30 days[1]	Quit[1]
Men			
Proportion/mean	0.17	321	0.08
Years since migration	-0.0007**	1.6692***	-0.0001
	(0.0003)	(0.4461)	(0.0004)
Percent own-ethnic	-0.0008**	-1.1435	-0.0004
	(0.0003)	(0.8060)	(0.0024)
Cigarette tax (dollars)	0.0084	-21.9410*	-0.0171
	(0.0059)	(11.4723)	(0.0113)
Smoking ban index	-0.0006	-1.0722	0.0004
	(0.0007)	(1.6010)	(0.0019)
N	60,954	5,939	6,248
Women			
Proportion/mean	0.08	296	0.10
Years since migration	0.0007***	1.2532***	0.0009**
	(0.0002)	(0.3859)	(0.0004)
Percent own-ethnic	0.0001	0.7716	0.0013
	(0.0003)	(1.4984)	(0.0011)
Cigarette tax (dollars)	0.0042	24.3507**	0.0073
	(0.0042)	(10.0943)	(0.0166)
Smoking ban index	-0.0001	-0.7127	0.0009
	(0.0004)	(1.1952)	(0.0020)
N	68,907	3,604	3,826

Notes: All equations estimated with a linear probability model (binary outcomes) or least squares (continuous outcomes) and adjust for age, race/ethnicity, education, marital status, state unemployment rate, birth country, MSA fixed effects, and year fixed effects. Standard errors are clustered by the birth country and reported in parentheses.
[1]Conditional smoking sample.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Appendix Table A. Smoking outcome availability by year

Variable	Years available
Employed	1995-1996, 1998-2003, 2006-2007, 2010-2011
Family income	1995-1996, 1998-2003, 2006-2007, 2010-2011
White collar status	1995-1995-1996, 1998-2003, 2006-2007, 2010-2011
Full office smoking ban	1995-1996, 1998-1999, 2001-2003, 2006-2007, 2010-2011
Office smoking ban not enforced	1995-1996, 1998-1999, 2001-2002, 2006-2007, 2010-2011
Work smoking cessation program	1995-1996, 1998-1999, 2001-2002, 2010-2011
Smoke	1995-1996, 1998-2003, 2006-2007, 2010-2011
Number of cigarettes smoked in the past 30 days	1995-1996, 1998-1999, 2001-2003, 2006-2007, 2010-2011
Quit	1995-1996, 1998-1999, 2001-2003, 2006-2007, 2010-2011

Appendix Table B. Top 50 immigrant sending countries

Birth County	Order	N	Country smoking rate	TUS sample smoking rate
Mexico	1	39,590	0.26	0.11
Philippines	2	8,400	0.29	0.10
Puerto Rico	3	6,791	0.11	0.18
India	4	6,099	0.15	0.05
China	5	5,618	0.27	0.07
Germany	6	5,265	0.29	0.21
Cuba	7	5,256	0.39	0.13
El Salvador	8	4,840	0.22	0.08
Canada	9	4,626	0.20	0.14
Vietnam	10	4,242	0.25	0.14
Korea (Republic of Korea)	11	4,060	0.28	0.17
Dominican Republic	12	3,952	0.15	0.09
England	13	3,215	0.24	0.15
Italy	14	2,886	0.26	0.12
Poland	15	2,849	0.31	0.17
Jamaica	16	2,787	0.24	0.07
Colombia	17	2,737	0.27	0.10
Other USSR/Russia	18	2,604	0.42	0.14
Japan	19	2,581	0.27	0.18
Guatemala	20	2,384	0.13	0.08
Haiti	21	2,189	--	0.05
Ecuador	22	1,721	0.15	0.08
Taiwan	23	1,633	--	0.08
Iran	24	1,617	0.14	0.14
Peru	25	1,554	0.23	0.09
Honduras	26	1,542	--	0.10
Portugal	27	1,360	0.24	0.14
Brazil	28	1,211	0.17	0.13
Guyana/British Guiana	29	1,097	0.16	0.08
Nicaragua	30	1,039	--	0.10
Ireland	31	991	0.32	0.15
Hong Kong	32	976	--	0.09
France	33	958	0.31	0.20
Trinidad and Tobago	34	948	0.19	0.10
Laos	35	902	0.28	0.14
Greece	36	858	0.52	0.20
Thailand	37	827	0.24	0.15
Cambodia (Kampuchea)	38	782	0.23	0.10
Argentina	39	698	0.27	0.12
Israel/Palestine	40	678	0.21	0.17
Yugoslavia	41	650	--	0.28
Romania	42	646	0.35	0.15
Lebanon	43	642	0.39	0.18
Scotland	44	585	0.24	0.16
Hungary	45	519	0.38	0.13
Spain	46	508	0.32	0.21
Czechoslovakia	47	488	0.37	0.17
Panama	48	482	0.11	0.11
Netherlands	49	472	0.29	0.13
Austria	50	380	0.46	0.16

Notes: We deleted entries that could not be exactly match to country. Examples of deleted entries are abroad (not specified), Africa (not specified), and Americas (not specified). Country smoking rates pertain to 2006 or 2009 (Leung, 2013; World Health Organization, 2013). -- indicates that data for this country is not available.

Appendix Table C. Selected associations among assimilation, enclaves, and employment outcomes among immigrants: State-level enclave measure

	Employed	Family income (1,000s)	White collar status[1]
Men			
Proportion/mean	0.76	67.90	0.24
Years since migration	-0.0004	0.6587***	0.0023***
	(0.0005)	(0.1078)	(0.0006)
Percent own-ethnic	-0.0018**	-1.0304***	-0.0023
	(0.0007)	(0.1880)	(0.0015)
Cigarette tax (dollars)	0.0001	-0.9074	-0.0026
	(0.0056)	(1.0440)	(0.0048)
Smoking ban index	-0.0005	-0.2353*	-0.0008
	(0.0007)	(0.1248)	(0.0009)
N	66,181	58,971	53,696
Women			
Proportion/mean	0.52	65.39	0.27
Years since migration	0.0034***	0.4598***	0.0032***
	(0.0004)	(0.0900)	(0.0006)
Percent own-ethnic	0.0003	-0.3668*	-0.0045*
	(0.0010)	(0.1946)	(0.0024)
Cigarette tax (dollars)	-0.0036	-0.3930	0.0157***
	(0.0077)	(0.8426)	(0.0057)
Smoking ban index	0.0010*	-0.1784*	-0.0009
	(0.0006)	(0.0997)	(0.0008)
N	74,687	66,300	41,832

Notes: All equations estimated with a linear probability model (binary outcomes) or least squares (continuous outcomes) and adjust for age, race/ethnicity, education, marital status, state unemployment rate, birth country, state fixed effects, and year fixed effects. Standard errors are clustered by the birth country and reported in parentheses.
[1]Conditional working sample.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Appendix Table D. Selected associations among assimilation, enclaves, and access to employment-based anti-smoking policies and services among immigrants: State-level enclave measure

	Office work ban[1]	Work ban not enforced[1]	Work cessation plan[1]
Men			
Proportion	0.77	0.08	0.12
Years since migration	0.0018***	-0.0003	0.0021***
	(0.0004)	(0.0002)	(0.0004)
Percent own-ethnic	0.0001	-0.0005	0.0001
	(0.0015)	(0.0011)	(0.0008)
Cigarette tax (dollars)	0.0088	-0.0066	-0.0143
	(0.0095)	(0.0068)	(0.0114)
Smoking ban index	0.0035***	-0.0024***	-0.0008
	(0.0013)	(0.0008)	(0.0013)
N	18,143	13,026	14,580
Women			
Proportion	0.82	0.05	0.16
Years since migration	0.0021***	-0.0006**	0.0023***
	(0.0004)	(0.0002)	(0.0004)
Percent own-ethnic	-0.0037***	0.0017***	0.0014
	(0.0010)	(0.0006)	(0.0016)
Cigarette tax (dollars)	0.0081	0.0079	-0.0064
	(0.0100)	(0.0057)	(0.0128)
Smoking ban index	0.0018	-0.0012	0.0010
	(0.0016)	(0.0008)	(0.0021)
N	19,850	14,626	12,038

Notes: All equations estimated with a linear probability model and adjust for age, race/ethnicity, education, marital status, state unemployment rate, birth country, MSA fixed effects, and year fixed effects. Standard errors are clustered by the birth country and reported in parentheses.

[1]Conditional working sample.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table E. Selected associations among assimilation, enclaves, and smoking outcomes among immigrants: State-level enclave measure

	Smoke	Number of cigarettes smoked past 30 days[1]	Quit[1]
Men			
Proportion/mean	0.17	321	0.08
Years since migration	-0.0007***	1.7842***	0.0000
	(0.0003)	(0.4176)	(0.0003)
Percent own-ethnic	-0.0025***	-0.8686	-0.0004
	(0.0008)	(1.4403)	(0.0010)
Cigarette tax (dollars)	0.0106	-23.3284**	-0.0010
	(0.0063)	(11.2648)	(0.0124)
Smoking ban index	-0.0005	-1.1809	-0.0007
	(0.0007)	(1.4571)	(0.0018)
N	66,181	6,397	6,747
Women			
Proportion/mean	0.08	296	0.10
Years since migration	0.0007***	1.2524***	0.0008**
	(0.0002)	(0.3681)	(0.0004)
Percent own-ethnic	-0.0008	0.1031	-0.0006
	(0.0007)	(1.9718)	(0.0024)
Cigarette tax (dollars)	-0.0004	34.4768***	-0.0003
	(0.0049)	(10.3864)	(0.0165)
Smoking ban index	-0.0001	-1.1535	0.0008
	(0.0004)	(1.2295)	(0.0020)
N	74,687	3,885	4,146

Notes: All equations estimated with a linear probability model (binary outcomes) or least squares (continuous outcomes) and adjust for age, race/ethnicity, education, marital status, state unemployment rate, birth country, state fixed effects, and year fixed effects. Standard errors are clustered by the birth country and reported in parentheses.
[1]Conditional smoking sample.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Data Appendix: Construction of ethnic enclave measures

We use data from the 1990 and 2000 5% file of the U.S. Censuses and the 2005 to 2011 American Community Surveys (ACS) to construct our ethnic enclave measures. Both data sets were extracted from the Integrated Public Use Microdata Series (Ruggles et al., 2010). The Census is fielded every ten years to enumerate and collect demographic information on the population. The 1990 and 2000 5% files of the Census include approximately 12.5 million and 14 million individuals respectively. The ACS is a large-scale nationally representative survey which is designed to replace the decennial long-form Census. Each ACS provides data for over three million individuals.

Both the Census and ACS contain detailed information on country of origin. We limit the sample to the top 50 sending countries in the 1990 Census as sample sizes for particular countries become small outside this range. These countries comprise over 86% of immigrants in the TUS. We delete observations in which the specific birth country cannot be determined. Examples include Africa (not specified) and Americas (not specified).

We construct our proxy for ethnic enclaves as the proportion of individuals from the same birth country in the metropolitan statistical area (MSA) of residence. MSA information is available for roughly 70% of the TUS, and we exclude respondents with missing MSA information. For example, if a respondent reports that he was born in Ireland and resides in the New York City MSA, we assign him the following value: $\frac{(population\ born\ in\ Ireland)_{NYC,t}}{(total\ population)_{NYC,t}} * 100.$

Because we do not have full overlap between the TUS data, and the Census and ACS surveys we must impute our enclave measure for non-Census and non-ACS years (i.e., 1995, 1996, 1998, and 1999). Moreover, not all years of our Census (recall that we rely on the 5% sample) or ACS surveys contain the universe of MSAs in the U.S. Thus, we must impute

enclave measures for missing MSAs. Since MSAs are at a much finer geographic level than the state, many of the birth country-by-MSA-by-year cells are small or empty, however. In total, we must impute information for 57% of our MSA/birth country/year cells. To provide enclave values for all MSAs in all years we utilize prediction equations that regress the percent of residents from each of the 50 included birth countries on birth country, MSA, and year fixed effects, and birth country by state fixed effects using least squares. The adjusted R^2 from this regression is 0.98, suggesting that our prediction model has reasonably good fit.

The MSA coding in the TUS (and the CPS more broadly) changed substantially between the 2003 and 2006/07 supplements (specifically, the CPS switched from using the Office of Management and Budget's June 1993 to June 2003 geographic area identification system). We match MSAs between the two classification systems, and achieve a match rate of 95%. More details on our matching procedure are available on request.

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