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# WHO "*THEY*" ARE MATTERS: Immigrant Stereotypes and Assessments of the Impact of Immigration

Jeffrey M. Timberlake*
University of Cincinnati

Junia Howell
Rice University

Amy Baumann Grau
Eastern New Mexico University

Rhys H. Williams
Loyola University Chicago

We investigate the relationship between stereotypes of immigrants and assessments of the impact of immigration on U.S. society. Our analysis exploits a split-ballot survey of registered voters in Ohio, who were asked to evaluate both the characteristics of one of four randomly assigned immigrant groups and perceived impacts of immigration. We find that associations between impact assessments and stereotypes of Middle Eastern, Asian, and European immigrants are weak and fully attenuated by control covariates. By contrast, this relationship for Latin American immigrants is strong and robust to controls, particularly in the areas of unemployment, schools, and crime. Our findings suggest that public views of the impacts of immigration are strongly connected to beliefs about the traits of Latin American immigrants in particular.

## INTRODUCTION

The American public historically has been deeply divided over the strategies and tactics of U.S. immigration policy (Simon 1985; Simon and Alexander 1993; Zolberg 2006), and little has changed in recent years. National immigration reform proposals introduced by the last two presidential administrations and state-level measures such as California's Proposition 187 in 1994, Arizona's SB 1070 in 2010, and Alabama's HB 56 in 2011 have all generated heated public debate. Recent polling data show that the majority of Americans favor tougher immigration policies, particularly those that target unauthorized immigration. Nevertheless, a substantial minority expresses more lenient views, and on some survey items there is a nearly uniform distribution

*Direct all correspondence to Jeffrey M. Timberlake, Department of Sociology, University of Cincinnati, P.O. Box 210378, Cincinnati, OH 45221-0378; e-mail: jeffrey.timberlake@uc.edu

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across response categories,[1] indicating profound disagreement among the populace (PollingReport.com 2011).

A vast literature on public attitudes toward immigration in the United States attempts to account for this divergence of opinion. Commonly examined dependent variables include public support for prompt action regarding immigration policy (Harwood 1986; Dunaway, Branton, and Abrajano 2010; Hopkins 2010), the perceived impact of immigration on the United States[2] (Haubert and Fussell 2006; Paxton and Mughan 2006; O'Neil and Tienda 2010), and desired policy outcomes, frequently operationalized as advocating more or less immigration (Espenshade and Hempstead 1996; Chandler and Tsai 2001; Pérez 2010). Independent variables of primary interest largely fall into two groups—traits of respondents and traits of immigrants—with the bulk of the literature concentrated in the former category. For example, prior research has estimated effects of respondents' race (Espenshade and Hempstead 1996; Chandler and Tsai 2001; Wilson 2001; Berg 2010), income level (Citrin et al. 1997; Scheve and Slaughter 2001; Neal and Bohon 2003), education (Neal and Bohon 2003; Berg 2010; Rustenbach 2010), political affiliation (Espenshade and Hempstead 1996; Chandler and Tsai 2001; Pantoja 2006), immigrant status (Espenshade and Calhoun 1993; Buckler, Swatt, and Salinas 2009), and contact with immigrant communities (e.g., Hood and Morris 1998; Pantoja 2006; Berg 2009a, 2009b, 2010; Hopkins 2010; O'Neil and Tienda 2010).

The breadth and depth of research on the effects of *respondent* characteristics stands in contrast to the relatively meager literature assessing the effects of real or perceived *immigrant* characteristics on public attitudes toward immigration. Yet it is highly likely that citizens base attitudes about immigration in part on their perceptions of the traits of immigrants (Ceobanu and Escandell 2010:313). Indeed, research on a host of political issues reveals that the moral status of the beneficiary population influences public attitudes on a variety of policy debates (e.g., Williams and Demerath 1991; Cress and Snow 2000; Gilens 2000; Timberlake, Lock, and Rasinski 2003). Moreover, a large share of the extant public opinion research on immigration examines attitudes toward Latin American immigrants only (Pantoja 2006; Buckler et al. 2009; Lu and Nicholson-Crotty 2010), limiting scholars' capacity to draw definitive conclusions about whether attitudes toward all immigrants, or just particular groups of immigrants, drive public opinion. Other studies compare effects of attitudes toward Latinos and other groups, but use either nonrepresentative samples (e.g., Brader, Valentino, and Suhay 2008; Pérez 2010) or measures of general racial and ethnic attitudes—including stereotypes of native-born African Americans—rather than attitudes toward immigrants in particular (e.g., Citrin et al. 1997; Hood and Morris 1997; Burns and Gimpel 2000).

We contribute to the literature on public assessments of the impact of immigration on the United States by introducing a key methodological innovation. Namely, we analyze data from a split-ballot survey to determine whether the relationship between immigrant stereotypes and assessments of the impact of immigration depends on the group of immigrants under consideration.[3] We believe this is a crucial addition to prior

research that either treats "immigrants" as a homogeneous huddled mass, or focuses solely on one group with no point or points of comparison.

Our analysis exploits the split-ballot design in two waves of the Ohio Poll, a semi-annual survey of registered Ohio voters. Respondents were randomly assigned to a "ballot"—a group of immigrants from either Latin America, the Middle East, Asia, or Europe—and asked to evaluate that group on five characteristics commonly used in research on racial and ethnic stereotypes. This design is desirable because it yields externally valid estimates while mitigating biases in conventional single-ballot surveys resulting from respondent fatigue and social desirability effects (Schuman and Presser 1981; Sniderman and Grob 1996; Timberlake and Estes 2007). Most importantly for our purposes, this procedure ensures that respondents' answers to questions about one immigrant group are not affected by prior responses about one or more of the other groups. Hence, we can be more confident that our results reflect Ohioans' true attitudes than if respondents had answered questions about all four groups of immigrants simultaneously.

## LITERATURE REVIEW AND HYPOTHESES

In this section, we develop hypotheses about the relationship between stereotypes of immigrants and perceived impacts of immigration. We first consider the overall association between immigrant stereotypes and impact assessments and then address the interaction between stereotypes and immigrant region of origin.[4]

### Immigrant Stereotypes

Past research shows significant associations between prejudice toward immigrants and various immigration-related outcomes (e.g., Citrin et al. 1997; Hood and Morris 1997; Wilson 2001; Pantoja 2006). These findings are not surprising; indeed, it seems self-evident to expect that if immigrants are perceived to be, for example, intelligent, self-sufficient, and not prone to criminality, survey respondents will tend to be sanguine about the impact of immigration. Conversely, to the extent that immigrants are perceived to have undesirable characteristics, respondents will tend to express negative views about immigration. As we discuss below, the five stereotype items and the five immigration impact items we employ in this article load on single principal component factors, with high inter-item reliability. As such, we believe that the scales we generate from these items reflect two general underlying constructs: normatively positive or negative attitudes toward immigrants, and normatively positive or negative assessments of the impacts of immigration. We expect that positive stereotypes of immigrants are related to more positive perceived impacts of immigration on U.S. society (and, of course, vice versa). More formally, we propose:

> Hypothesis 1: There is a positive relationship between immigrant stereotypes and impact assessments.

**The Interaction of Immigrant Stereotypes and Region of Origin**

The primary purpose of this article is to assess whether the link between stereotypes of immigrants and perceptions of the impact of immigration depends on the immigrant group under consideration. In this section, we explain our theoretical reasoning behind this interaction hypothesis.

Our discussion is heavily conditioned by a methodological implication of the survey design, in which respondents from a single state were randomly assigned to answer questions about immigrants from four global regions. Because of this random assignment, the distributions of all variables are the same across ballots (except for sampling variability), meaning that we cannot "explain" any observed differences in the stereotype slopes by introducing covariates into regression models. For example, if variability in the relationship between stereotypes of immigrant groups and immigration impact assessments itself varied by education level, and if education were differentially distributed across the region ballots, then controlling for education would yield different estimates than would a model that does not control for education. But because there is no variability in the education distribution across ballots (again, except for sampling error), controlling for education would not affect the differences in the relationships between stereotypes and impact assessments across ballots.[5] We do control for a variety of covariates in the analysis below, partly because doing so helps us assess the overall relationship between stereotypes and impact assessments, and partly because it provides information on the independent relationship between those covariates and the dependent variable.

Hence, this section will not consist of reviewing past findings on the predictors of attitudes about immigration because such predictors would not help us answer the question we seek to answer. Rather, in keeping with the focus of this article, we evaluate the plausibility of two general explanations for variation in the relationship between stereotypes and impact assessments. We stress at the outset that we cannot provide direct evidence as to whether one explanation is more valid than the other. In order to do this, we would need identical data from a number of states that varied on measures of the causal processes we propose. For example, if Ohioans were more commonly exposed to messages that linked the problems of immigration tightly to Latin American immigrants than were residents of other states, then a measure of statewide exposure to such messages might explain variation in the relationship between immigrant group stereotypes and immigration impact assessments across states.

In sum, our analysis assesses whether and to what extent Ohioans link beliefs about the traits of immigrants to assessments of the impact of immigration more strongly for one group than another. It is a simple matter to answer this empirical question statistically, although we cannot know for sure why we observe it. Hence, in this section, we discuss group threat and social constructionist perspectives and explain why we believe these approaches might explain the patterns we observe in our data. We also note that we do not believe these two approaches are mutually exclusive; indeed, we argue that in examining the relationship between demographic change and attitudes, Hopkins (2010) synthesizes major elements of the two perspectives. The purpose of this section,

therefore, is to evaluate the applicability of group threat theory in a state with very low percentages of recent immigrants, and to discuss relevant insights of the social constructionist perspective.

*Group Threat Theory*

Perhaps the most frequently invoked explanation for why native populations express negative views toward newcomers, group threat theory (Quillian 1995) proposes that large or growing groups of immigrants threaten the social position, prerogatives, and control over valued resources of the native born. These threats generate negative stereotypes of the encroaching group, resulting in a jaundiced view of immigrants and immigration (Blumer 1958; Blalock 1967; Quillian 1995; Wilson 2001; Schneider 2008). Group threat theory has been employed in research on public attitudes toward immigration in regions with substantial immigrant populations. This research demonstrates that the close proximity of immigrants to native populations triggers fears of losing economic and political capital, resulting in negative sentiments toward immigrants. Although not originally emphasized by Blumer (1958), Quillian (1995) also found that prejudice toward immigrants was related to the economic conditions of the 12 European countries in his study. Subsequent studies have used objective and subjective measures of economic vulnerability at the individual level to test the claim that group threat is related to tenuous economic circumstances (e.g., Burns and Gimpel 2000; Wilson 2001).

Hence, the fundamental proposition of group threat theory is that large immigrant populations trigger animosity among the native born (Wilson 2001). However, data from the 2006 through 2010 American Community Survey (ACS) show that the foreign-born population in Ohio is only 3.8 percent. From the four regions in question, these percentages are 1.2 percent for Asian immigrants, 1.1 percent for European immigrants, 0.81 percent for Latin American immigrants, and 0.21 percent for Middle Eastern immigrants (authors' calculations).[6] Therefore, because none of these groups represents a sizable fraction of the population, the application of group threat theory as traditionally conceptualized is questionable in the state of Ohio and, we would argue, in other states with small immigrant populations.

However, new research establishing the effect of the politicization of demographic shifts is expanding the possible applications of group threat theory. In particular, Hopkins (2010) examines the interaction of increasing immigrant concentration and the politicization of those increases on public attitudes toward immigration policy. He concludes that the effects of growing immigrant populations are conditional on the politicization of those shifts. In other words, public reactions to increases in the immigrant population depend on whether those increases are framed as threatening by "salient national rhetoric" (Hopkins 2010:40). Hence, although Ohio has relatively few recent immigrants, it is possible that intense local and national media attention to immigration may be sufficient to trigger anti-immigrant responses. It is this politicization, or the intervention of social processes between demography and attitudes, that leads us to consider the social constructionist perspective.

*The Social Construction of the Latino "Problem"*

We argue that in a state with a very small foreign-born population, beliefs about the traits of immigrants and the impact of immigration must ipso facto come largely from sources other than daily, firsthand experiences with immigrants. In short, the "problem" (or lack thereof) of immigration must largely be socially constructed. Of course, to say that something is "socially constructed" is not to imply that it is "just symbolic." Rather, this claim highlights the fact that extant social phenomena are available to be interpreted in a number of ways. For something—a person, a city, an event, an ethnic group, a religious icon—to develop a particular set of meanings that become widely shared or even predominant, those meanings must be "constructed." A constructionist perspective has become particularly fruitful in the study of social problems (Best 2003). Scholars in this tradition focus on the processes through which social phenomena become "problems" and how they become problems of a particular kind (Best 1987). Those processes lead to "typifications" (Best 1995), a cognitive reification in which the social phenomenon—such as an event or ethnic group—carries with it an assumed problem, an assumed solution, and a place in the "natural" order of things (Gusfield 1980).

Thus, it is with immigration as a "problem" for the United States. While there is an undeniable structural reality to the concentration of immigrants in a particular region or city, Hopkins (2010) finds that this has become a problem more generally because the typifications associated with immigration have become politicized. And, we would argue, recent research has shown that one of those typifications is that the problem of immigration to the United States is primarily an issue with recent Latin American immigrants.

This, of course, was not always the case. Indeed, along with the ebbs and flows of immigration have been ebbs and flows in anti-immigrant political and social attitudes among native-born Americans. Historically, anti-immigrant attitudes have tended to focus on immigrants who had high visibility because of dense settlement in major cities, distinctly different patterns of dress or religio-cultural customs, or darker skin color. Such antipathy has also varied regionally in the United States, as not all parts of the country receive immigrants in the same numbers or from the same regions of the world (Simon 1985; Harwood 1986; Simon and Alexander 1993; Higham 1998; Daniels 2004; Zolberg 2006). So, for example, the most virulent anti-immigrant sentiments of the 19th century were reserved for Irish immigrants in the Northeast (Ignatiev 1995), and later Chinese immigrants in California (Higham 1998; Daniels 2004). Hence, the "problem" of immigration and typifications of certain groups as un-American or undesirable have been applied to groups from other regions at different times and in different places and with different rationales.

Over the past several decades, we argue that much of the "salient national rhetoric" surrounding immigration has been focused on immigrants from Latin America in general and Mexico in particular. For example, Chavez (2008) analyzes what he calls the "Latino Threat Narrative." This story emphasizes the differences between

contemporary Latino immigrants and the immigrants who preceded them. Whereas prior groups eventually became part of the American national fabric, Latino immigrants—so the narrative goes—are either unwilling or unable to assimilate. This concern has resonated with policy makers (e.g., [now former] Congressional Representatives Tom Tancredo [R-Colorado] and Duncan Hunter [R-California]), political pundits (e.g., Glenn Beck, Lou Dobbs, and Michael Savage), certain intellectuals (e.g., Huntington 2004), and the public at large. Similarly, Newton (2008) reviews the history of the National Origins Acts of the early 1920s as casting a collection of different groups as "excludable" because of their status as not assimilable. She then shows how the notion of "illegal" has become a primary marker of Mexicans in the United States, thereby attaching the problem of illegality to a particular group of immigrants (2008:21). Finally, Jacobson (2008) examines the 1994 debate in California over Proposition 187 as a distinct event in the creation of this typification. That Mexicans and other Latinos did not share "American" values, or wanted to retain their own ethnic culture rather than assimilate into the putatively ethnically neutral American culture, became evidence that they were utterly "other." That they would break the laws of their supposedly new homeland and enter illegally became evidence that they could not truly become American.

Importantly, however, Dávila (2008) notes that not all representations of Latinos are negative, or cast them as a problem. There is a public discourse that emphasizes that Latinos are patriotic, hardworking, and socially conservative, with strong religious and family values. In effect, there is a symbolic battle over the public construction of who Latin American immigrants are and whether they are beneficial to the United States. This battle, Dávila claims, "seems to over-ethnicize or de-ethnicize Latinos by presenting them as a threat or as contributors to the 'national community'" (2008:4). Thus, we argue that Latin American immigrants are viewed as a threat when they are portrayed as distinctly different from the United States's predominant Anglo-Saxon cultural norm, and not a threat when they are portrayed as hewing closely to "traditional American values."

In sum, the social constructionist perspective would not simply posit that native-born Americans view Latin American immigrants more negatively than other groups (although at least one study shows this to be a plausible conclusion [Timberlake and Williams 2012]). What this perspective does imply is that the recent national immigration debate has been so tightly linked to Latin Americans that stereotypes of Latin American immigrants—both positive and negative—are particularly tightly linked to assessments of the impact of immigration. In contrast, although citizens vary in the extent to which they hold positive or negative stereotypes of Asian, Middle Eastern, or European immigrants, we hypothesize that this variation is not as strongly related to assessments of the impact of immigration. More formally, we propose:

> Hypothesis 2: The relationship between stereotypes and impact assessments is stronger for Latin American immigrants than for other immigrant groups.

Findings from prior research have provided some support for this hypothesis. For example, Wilson (2001) found significant effects of immigrant stereotypes on opposition to legal immigration and policies benefiting undocumented immigrants. Similarly, Buckler et al. (2009) found significant effects of negative stereotypes about Latinos on the public's support for deportation. Other examinations of prejudice toward Latinos (Burns and Gimpel 2000; Pantoja 2006; Lu and Nicholson-Crotty 2010; Pérez 2010) have provided evidence that stereotypes of Latinos affect beliefs about the impact of immigration.

Despite the important contributions of these studies, each has either relied on nonrepresentative samples (Brader et al. 2008; Pérez 2010) or examined attitudes toward either a general category of immigrants (Wilson 2001), or more commonly, Latinos only (Pantoja 2006; Buckler et al. 2009; Lu and Nicholson-Crotty 2010). In the analyses that follow, we use more representative survey data to test directly whether the relationship between immigrant group stereotypes and assessments of the impact of immigration is strongest among respondents considering Latin American immigrants.

The precise ordering of the relationship between stereotypes and impact assessments for the other three groups we believe to be more an empirical question than one with clear expectations derived from prior research. We are inclined to speculate that in the post-9/11 era, citizens would more tightly link stereotypes of Middle Eastern immigrants to impact assessments than they would for other groups (see, for example, Cainkar [2009] on suspicions about Arab and Muslim Americans). We also speculate that the relationship between stereotypes and impact assessments ought to be weakest among European immigrants, perhaps because of low levels of salience of recent European immigration in the minds of Ohio's citizens. Alternatively, expectations might be generated from various versions of assimilation theory (see the overview in Alba and Nee 1997). The logic would posit that perceived "other-ness" is associated with lower levels of assimilation by different ethnic groups and that lower levels of assimilation are connected with a more acute sense that the groups are "immigrants." Such othering might heighten the extent to which negative assessment of the group is connected to negative assessment of the impact of immigration. Thus, because of historical presence in the United States, perceived cultural similarity with Anglo-Saxon norms, and the capacity to be "racialized" as white (see Omi and Winant 1994) some groups (such as the French Americans) enjoy a "symbolic ethnicity" (Waters 1990) which makes them less likely to perceived as either immigrants or as the carriers of social problems. Racial distance, such as the visible differences in phenotype among Asian populations or in skin color among Middle Eastern groups, may lead native-born citizens to typify such immigrants as "others" and hence may lead to beliefs that immigration is problematic. What Portes and Zhou (1993) have called "segmented assimilation" means that some social and cultural distinctions remain even among economically successful groups (such as Korean or Cuban Americans), potentially leading native-born citizens to view these groups as "foreign."

The general categories we used in our survey—European, Asian, Middle Eastern, Latin American—were illustrated with a variety of countries as specific examples. For

example, we used both Western (French) and Eastern (Polish) Europeans as exemplars. That very variety may have undercut the extent to which respondents tended to assess all immigrants on the general level of group assimilation. Furthermore, we did not ask specific questions about the relative assimilation or "American-ness" of various groups—we asked about "immigrants" from these global regions. Thus, while it is reasonable to think that European immigrants were regarded least negatively because of higher levels of assimilation, followed next by Asian, and then Middle Eastern immigrants, these expectations are not derived from either of the theory streams we examine here and therefore remain speculative.

## DATA AND MEASURES

### Data

The data for this article come from two waves of the Ohio Poll, a semiannual survey of social and political attitudes of registered voters in Ohio. The poll was conducted via telephone by the Institute for Policy Research (IPR) at the University of Cincinnati (Institute for Policy Research [IPR] 2008a). Wave 1 was fielded in November 2007 and wave 2 in May 2008; thus, the data were gathered prior to debates surrounding President Obama's election and reintroduction of the Development, Relief, and Education for Alien Minors (DREAM) Act and the passage of SB 1070 in Arizona and HB 56 in Alabama. Although the second wave of the survey went into the field after the early 2008 Republican primaries (which devoted some debating time to immigration), we found no significant differences in the means of any of the variables between the two waves. Thus, we believe our analyses reflect Ohioans' attitudes under relatively average economic and political conditions, with immigration a salient issue for most Ohioans, but prior to both the global economic crisis in fall 2008 and the notable spikes in news coverage that occurred in 2009 and 2010.

The original sample size was 2,253; however, after the deletion of cases with missing data, the final sample size was 2,109. In both waves of the poll, respondents were randomly assigned to one of four ballots, with each ballot focusing on one immigrant group. Hence, about one-quarter of the respondents (split about equally across the two waves) considered stereotype questions for immigrants from Latin America, the Middle East, Asia, and Europe. As expected by the randomized design, chi-squared tests yielded no significant differences in the distributions of the independent variables across ballots.

#### *Data Limitations and Strengths*

Although these Ohio Polls are, to the best of our knowledge, unique in their capacity to generate unbiased estimates of attitudes toward immigrants from four global regions, the data are not without limitations. First, because the sampling frame was registered voters, our findings do not generalize to all Ohio citizens. Second, the response rates (18 percent and 20 percent for waves 1 and 2, respectively, using AAPOR's RR4 calculation (American Association for Public Opinion Research

[AAPOR 2006]) and cooperation rates (25 percent and 31 percent, using AAPOR's COOP4 calculation [AAPOR 2006]) are low in absolute terms. Although all analyses are weighted to account for survey nonresponse, it is possible that some unknown degree of nonresponse bias is present in these data.[7] Finally, the data are from just one state, limiting our capacity to generalize to other states or the nation as a whole.

Nevertheless, we argue that there are advantages to examining attitudes in a state like Ohio. Ohio has recently been an important swing state in presidential electoral politics, and understanding attitudes among registered voters in bellwether states such as Ohio may provide clues to the eventual outcome of immigration reform debates. In terms of contributions to scholarly research, many prior studies have focused on public opinion in high immigrant–receiving areas such as Southern California (Espenshade and Calhoun 1993; Neiman, Johnson, and Bowler 2006), Texas (Binder, Polinard, and Wrinkle 1997), and, increasingly, new destinations such as Oregon (Padín 2005) and the Southeast (Neal and Bohon 2003; Saenz et al. 2003; Marrow 2009; O'Neil and Tienda 2010). In such places, direct interaction with immigrants is likely to be much higher than in Ohio.

Indeed, as noted above, 2006 through 2010 ACS data indicate that less than 4 percent of Ohioans are foreign born, compared to much higher percentages in traditional immigrant gateways, such as California (27 percent), New York (22 percent), or Texas (16 percent), and "new destination" states such as Nevada (19 percent) and Georgia (10 percent). Our analysis indicates that just seven of Ohio's 88 counties feature percentages of foreign-born residents greater than 4 percent, only one of which has a foreign-born concentration greater than 8 percent. Furthermore, each of these seven counties is located in the three largest metropolitan areas (Cleveland, Cincinnati, and Columbus), meaning that residents of large swaths of Ohio have little direct contact with recent immigrants.

This makes a state like Ohio ideal for understanding the outcomes of immigration rhetoric, more or less untainted by substantial demographic change, on public attitudes toward immigrants and immigration. We do not claim that it is better to measure public opinion in a state with few recent immigrants than to do so in a state with many, nor do we claim that there is necessarily anything unique about Ohio relative to other low-immigration states. Rather, we argue that our sample yields interesting information because observed public attitudes in Ohio are not likely to be because of quotidian conflicts over bilingual school instruction or low-wage jobs that might exist in, say, southern California or Arizona or even a true new destination like Georgia or Nevada. Rather, we argue that the effects of those conflicts—nativist fears over the "flood" of Latin American immigrants—can be detected even in a state with very few such immigrants. Put somewhat differently, we believe Ohio is interesting not because it is a new destination, but because it is very nearly a "non-destination." In this respect, our analysis provides new information about the extent to which impact assessments are attached to stereotypes of particular groups, even in a relatively immigrant-free context.

**Measures**

*Dependent Variables*

The dependent variables are questions about the likelihood of five outcomes occurring because of immigration. These items were preceded by the following preamble:

> What do you think will happen as a result of more immigrants coming to this country? Is each of these possible results very likely, somewhat likely, not too likely, or not at all likely? First/what about . . . (INSERT ITEM) . . . is it very likely, somewhat likely, not too likely, or not at all likely this will happen as a result of more immigrants coming to this country?

The five items analyzed in this article include, in the order presented in the questionnaire: (1) "higher levels of unemployment," (2) "lower quality schools," (3) "making it harder to keep the country united," (4) "higher levels of crime," and (5) "a terrorist attack in America." Each item was coded so that higher values corresponded to lower probabilities of each problem occurring, with 1 representing "very likely," 2 meaning "somewhat likely," 3 representing "not too likely," and 4 meaning "not at all likely." We use this coding strategy so that normatively positive stereotypes and normatively positive impact assessments are both at the upper end of their respective scales. We created an "immigration impact scale" of the five items by first averaging the unstandardized scores and then standardizing that average.[8] Exploratory factor analysis revealed that these items loaded on a single principal component factor (Eigenvalue = 2.63; all factor loadings above 0.60), and Cronbach's alpha was 0.77. Analyses employing this scale appear in Figure 1 and Table 2, and analyses of the separate items appear in Tables 3 and 4 and Figure 2.

*Focal Independent Variables*

The independent variables of interest are immigrant region of origin, immigrant stereotypes, and the interaction of these two variables. Region of origin was measured with dummy variables indicating the ballot to which respondents were assigned (i.e., Latin America, Middle East, and Asia, with Europe the omitted category). We created an immigrant stereotype scale by standardizing the average of five "semantic differential" items commonly used in surveys of racial and ethnic stereotypes, such as the General Social Survey (Davis and Smith 2000) and the Multi-City Study of Urban Inequality (Bobo et al. 2000). For these items, respondents were asked to rate the immigrant group to which they had been assigned on seven-point scales representing whether that group tended to be (1) rich versus poor, (2) intelligent versus unintelligent, (3) nonviolent versus violent, (4) self-supporting versus on government assistance, and (5) willing to fit in with Americans versus staying separate from Americans. We present the preamble to the stereotype module and the "rich versus poor" question below.

> Now I have some questions about different groups in our society. I'm going to describe a 7-point scale that I'd like you to use to describe the characteristics of people in different groups. In the first statement a score of 1 means that you think people in that group tend to be "rich," a score of 7 means that you think people in that group tend to be "poor," and a score of 4 means you think the group has no leaning one way or the other. Of course, you may choose any number, 1, 2, 3, 4, 5, 6, or 7, that comes closest to where you think people in the group stand.
>
> In general, where would you rate [GROUP] on this scale, where 1 meant RICH, and 7 meant POOR?

For the Latin America ballot, "GROUP" in the question above was replaced with the following text: "Immigrants from Latin American countries, such as Cuba, Brazil, and Mexico." For the Middle East group, respondents were asked about "Immigrants from Middle Eastern countries such as Jordan, Saudi Arabia, and Iran." For the Europe ballot, respondents were asked about "Immigrants from European countries such as Ireland, France, and Poland." Finally, for the Asia group, respondents were asked about "Immigrants from Asian countries, such as Japan, Korea, and China."[9] We coded each stereotype item so that higher values indicate more normatively positive evaluations. These items loaded on a single principal component factor (Eigenvalue = 2.25, all factor loadings above 0.50), and Cronbach's alpha for the scale was 0.69.

*Control Variables*

We include in our analysis a number of variables shown in prior research to influence immigration attitudes (Timberlake and Williams 2012). First, we control for income, a four-category measure contrasting individuals earning less than $20,000 per year to those earning $20,000 to $39,999, $40,000 to $59,999, and greater than $60,000. We imputed missing values of income and include a dummy variable scored 1 if imputed, 0 otherwise in the regression analyses.[10] Second, we control for education, contrasting those with some college or a college degree to those with a high school degree or less. Urban residence is measured via dummy variables for city and suburb, with rural areas or small towns the omitted category. We measure several political orientation variables, including binary indicators for self-reported party identification (Democrat and Independent/other versus Republican) and political ideology (from a three-category variable with liberal, moderate, and conservative response choices). Media exposure is the relative frequency of consumption of five types or sources of news: national, local, television, radio, and Internet. We first generated a summative scale of these items and then recoded the variable to represent the percentage of the maximum possible score. Hence, this measure ranges from 0 to 100, with 50 representing a person who scored half of the maximum.[11] Finally, we controlled for race/ethnicity (non-Latino white, non-Latino black, and other, including a trivial [1.1 percent] percentage of Latino respondents), age (four dummy variables representing age categories), sex (1 if male, 0 if female), and wave of the Ohio poll (a dummy variable scored 1 if wave 1, 0 if wave 2). Descriptive statistics for all variables used in the analysis appear in Table 1 below.

**TABLE 1.** Descriptive Statistics for the Variables Used in the Analysis: The Ohio Poll, 2007–2008

| Variable | Mean | Standard deviation | Range | |
|---|---|---|---|---|
| | | | Low | High |
| Dependent variables | | | | |
| Higher levels of unemployment | 1.78 | 0.90 | 1 | 4 |
| Lower quality schools | 2.27 | 1.01 | 1 | 4 |
| Harder to keep country united | 2.21 | 0.97 | 1 | 4 |
| Higher levels of crime | 1.91 | 0.89 | 1 | 4 |
| Terrorist attack in America | 2.04 | 0.94 | 1 | 4 |
| Immigration impact scale (unstandardized) | 2.05 | 0.68 | 1 | 4 |
| Focal independent variable | | | | |
| Immigrant stereotype scale (unstandardized) | 4.17 | 1.13 | 1 | 7 |
| Control variables | | | | |
| Income | | | | |
| Less than $20,000 | 0.12 | — | 0 | 1 |
| $20,000 to $39,999 | 0.25 | — | 0 | 1 |
| $40,000 to $59,999 | 0.21 | — | 0 | 1 |
| $60,000 and over | 0.42 | — | 0 | 1 |
| Education | | | | |
| High school degree or less | 0.36 | — | 0 | 1 |
| Some college | 0.30 | — | 0 | 1 |
| College graduate | 0.34 | — | 0 | 1 |
| Urban residence | | | | |
| City | 0.51 | — | 0 | 1 |
| Suburb | 0.30 | — | 0 | 1 |
| Rural area | 0.19 | — | 0 | 1 |
| Party identification | | | | |
| Democrat | 0.48 | — | 0 | 1 |
| Republican | 0.37 | — | 0 | 1 |
| Independent/other | 0.14 | — | 0 | 1 |
| Political ideology | | | | |
| Liberal | 0.18 | — | 0 | 1 |
| Conservative | 0.37 | — | 0 | 1 |
| Moderate | 0.45 | — | 0 | 1 |
| Media exposure | 66.02 | 18.12 | 0 | 100 |
| Race | | | | |
| White | 0.86 | — | 0 | 1 |
| Black | 0.10 | — | 0 | 1 |
| Other | 0.04 | — | 0 | 1 |
| Male | 0.39 | — | 0 | 1 |
| Age | | | | |
| 18 to 29 | 0.08 | — | 0 | 1 |
| 30 to 44 | 0.24 | — | 0 | 1 |
| 45 to 64 | 0.42 | — | 0 | 1 |
| 65 and older | 0.25 | — | 0 | 1 |

Notes: Sample size = 2,109. Both the immigration impact and the immigrant stereotypes scale were coded so that higher values relate to normatively positive impacts and stereotypes.

## FINDINGS

### The Immigration Impact Scale

*Immigrant Group Differences in the Stereotype Scale Slopes*

Figure 1 presents the results from ordinary least squares regressions of the standardized immigration impact scale on region of origin (the randomly assigned ballot), the immigrant stereotype scale, and their interaction. The coefficients used to generate this figure, as well as standard errors and results of significance tests, appear in model 1 of Table 2. Table 2 shows the results before (model 1) and after (model 2) accounting for the effects of all control variables. Because the dependent variable is standardized, the coefficients are interpreted as the average change in standard deviations of the immigration impact scale per one-unit change in the independent variables.

In terms of the hypotheses noted above, several findings are relevant. First, Hypothesis 1 predicted that there is a positive main effect of stereotypes on impact assessments. This hypothesis is confirmed by the positive (0.145) and significant (at $\alpha = 0.05$) coefficient for the immigrant stereotype scale in model 1 of Table 2. Because the omitted category is Europe, this coefficient is interpreted as relating to stereotypes of European immigrants. Hence, assessments of the impact of immigration change at a rate of about 15 percent of a standard deviation per standard deviation change in stereotypes of European immigrants. Because each of the interactions between group and stereotypes is positive, this indicates that there is a positive relationship between

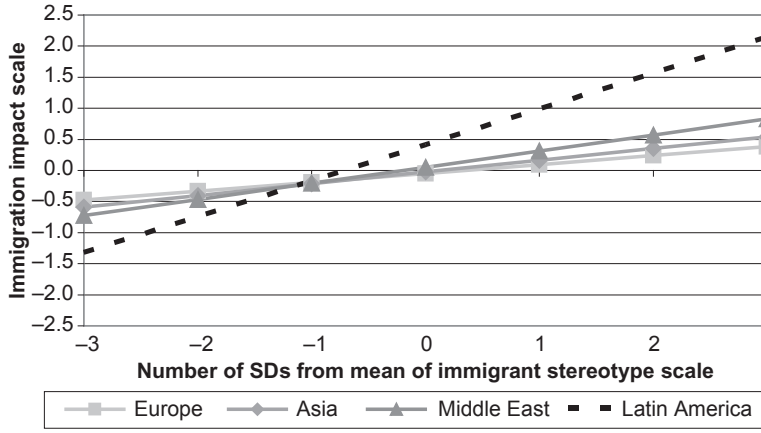


**FIGURE 1.** Relationships between Immigrant Stereotypes and Assessments of the Impacts of Immigration, by Immigrant Region of Origin: Ohio Poll: 2007–2008.

*Notes*: $N = 2{,}109$. Data from a model with no control covariates. Both the immigration impact and the immigrant stereotypes scale were coded so that higher values relate to normatively positive impacts and stereotypes. Europe slope significantly different from zero ($t = 2.53$; $p = 0.011$; two-tailed test). Asia and Middle East increments to Europe slope not significant at conventional levels. Latin America slope significantly different from Europe slope ($t = 5.38$; $p < 0.001$; two-tailed test).

**TABLE 2.** Coefficients and Robust Standard Errors from Ordinary Least Squares Regressions of the Standardized Immigration Impact Scale: The Ohio Poll, 2007–2008

| | Model 1 | | Model 2 | |
|---|---|---|---|---|
| Parameter | Coefficient | SE | Coefficient | SE |
| Region/ballot (versus Europe) | | | | |
| Asia | 0.019 | 0.071 | 0.011 | 0.068 |
| Middle East | 0.088 | 0.073 | 0.066 | 0.068 |
| Latin America | 0.461*** | 0.080 | 0.414*** | 0.073 |
| Immigrant stereotype scale | 0.145* | 0.057 | 0.090 | 0.053 |
| Interactions | | | | |
| Asia × stereotype scale | 0.037 | 0.078 | 0.061 | 0.073 |
| Middle East × stereotype scale | 0.133 | 0.085 | 0.114 | 0.078 |
| Latin America × stereotype scale | 0.438*** | 0.081 | 0.409*** | 0.075 |
| Control variables | | | | |
| Income (versus less than $20,000) | | | | |
| $20,000 to $39,999 | — | — | –0.146 | 0.082 |
| $40,000 to $59,999 | — | — | –0.101 | 0.090 |
| $60,000 and over | — | — | –0.040 | 0.088 |
| Education (versus high school degree or less) | | | | |
| Some college | — | — | 0.163* | 0.064 |
| College graduate | — | — | 0.471*** | 0.064 |
| Urban residence (versus rural area) | | | | |
| City | — | — | 0.090 | 0.069 |
| Suburb | — | — | –0.046 | 0.072 |
| Party identification (versus Republican) | | | | |
| Democrat | — | — | 0.163** | 0.058 |
| Independent/other | — | — | –0.044 | 0.080 |
| Political ideology (versus Conservative) | | | | |
| Liberal | — | — | 0.498*** | 0.081 |
| Moderate | — | — | 0.222*** | 0.055 |
| Media exposure | — | — | 0.001 | 0.001 |
| Race (versus black) | | | | |
| White | — | — | 0.155 | 0.093 |
| Other | — | — | 0.106 | 0.158 |
| Male | — | — | 0.048 | 0.049 |
| Age (versus 65 and older) | | | | |
| 18 to 29 | — | — | 0.090 | 2.470 |
| 30 to 44 | — | — | 0.066 | 0.570 |
| 45 to 64 | — | — | 0.058 | –0.560 |
| Imputed income | — | — | 0.057 | 0.220 |
| Survey wave 1 | — | — | 0.049 | 0.840 |
| Constant | –0.041 | 0.049 | 0.158 | –4.650 |
| R2 | 0.086 | | 0.193 | |
| No. of cases | 2,109 | | 2,109 | |

***$p < 0.001$; **$p < 0.01$; *$p < 0.05$, two-tailed tests. Both the immigration impact and the immigrant stereotypes scale were coded so that higher values relate to normatively positive impacts and stereotypes.
Notes: See Table 1 for reference categories of control covariates.

stereotypes and impact assessments for all immigrant groups under consideration. This can be seen by the positively sloping lines for all groups in Figure 1. Post-estimation statistical tests (not shown here) indicate that all four slopes are significantly different from zero at the 0.05 significance level or less.[12]

In model 2 of Table 2, the stereotypes coefficient for European immigrants (0.09) declines to statistical nonsignificance. This indicates that when all control variables are accounted for, there is no statistically significant independent association between stereotypes of European immigrants and impact assessments. However, the sum of the "immigrant stereotype scale" coefficient and the interaction terms for the other three groups indicates positive and significant slopes for all three non-European groups (see note 11). Hence, we find support for Hypothesis 1, with the sole exception being the association between stereotypes of European immigrants and impact assessments in the presence of controls.

Hypothesis 2 predicted that stereotypes of Latin American immigrants more strongly predict impact assessments than the stereotypes of the other groups. We find strong confirming evidence for this hypothesis. Note the steeper slope of the dotted line representing Latin American immigrants in Figure 1. The data in model 1 of Table 2 show that the gap between the Latin America slope and the Europe slope is 0.438 ($p < 0.001$), indicating that an additional standard deviation on the stereotypes scale results in more than a two-fifths of a standard deviation greater change when the immigrant target group is from Latin America than when it is from Europe.

Furthermore, although this gap is slightly attenuated in model 2, the coefficient remains significant at the 0.001 level. In analyses not shown directly in Table 2, we found that the Latin America stereotypes slope is steeper than that for Asian and Middle Eastern immigrants as well, with or without controls, and all with $p$-values less than 0.001. Hence we conclude that Hypothesis 2 is confirmed. That is, although there is variation in stereotypes of all immigrant groups, and although there is variation in assessments of the impact of immigration, the relationship between these two variables is much stronger for Latin American immigrants than for the other three groups. Again, this finding indicates that Ohioans do not link their beliefs about the traits of Asian, Middle Eastern, or European immigrants strongly to assessments of the impact of immigration. In contrast, these impacts are tightly linked to traits of Latin American immigrants.

*Control Variables*

Before turning to a comparison of the constituent items in the immigration impact scale, we briefly consider the relationships of the control variables to impact assessments. First, as found in much past research (e.g., Pantoja 2006; Fennelly and Federico 2008; O'Neil and Tienda 2010), more educated Ohioans were more positive about the impact of immigration. Specifically, those with a college degree scored nearly one-half of a standard deviation higher on the immigration impact scale than the omitted category, those with a high school degree or less. Those with some college scored about 16 percent of a standard deviation higher.[13] Also in keeping with past research (e.g., Burns

and Gimpel 2000; Chandler and Tsai 2001; Neiman et al. 2006), we found that political conservatives were more negative about the impact of immigration than were moderates or liberals. Second, Democratic Party affiliation and liberal political ideology were significantly related to normatively positive views about the impact of immigration.

No other control variable showed a statistically significant relationship with the dependent variable, including our measure of media exposure. Given our argument that public attitudes in a low-immigration state must come at least somewhat from the media, this finding bears some discussion. Some past research has shown that print and televised media can have effects on public opinion (Brader et al. 2008; O'Neil and Tienda 2010). We suspect that the null finding in the present study may be because of the fact that total volume of media coverage is not how immigration attitudes are formed; rather, it is through reading and viewing specific types of media. For example, in a study of attitudes in two counties in North Carolina, O'Neil and Tienda (2010) found that watching "Lou Dobbs Tonight" on CNN was associated with a more negative view. Unfortunately, the Ohio Poll did not contain items on specific programming; hence, we were unable to replicate the findings in that study.

**Differences between Immigration Impact Items**

Table 3 and Figure 2 disaggregate the immigration impact scale into its five component items. We perform this analysis to investigate whether the effects of stereotypes vary not only by immigrant group but by the particular impact under consideration. Results from statistical tests for differences in these interaction effects across impact items can be found in Table 4. Because the dependent variables in this latter set of analyses are Likert-type items, with 1 representing "extremely likely" to 4 representing "extremely unlikely," we use ordered logistic regression techniques (Long 1997). These analyses report coefficients indicating the effect of a one-unit change in the independent variable on the log odds of being in one higher (or lower) category on the four-category dependent variable.[14] All standard error estimates reported are adjusted for the Ohio Poll's complex sampling design. Because the coefficients are in the nonintuitive log odds metric, we occasionally transform the results into predicted probabilities using post-estimation procedures for Stata developed by Long and Freese (2005). We present selected results from these transformations in Figure 2 below.

We begin by noting that although there are some differences in the point estimates of the region of origin and immigrant stereotype scale "main effects" across impact items (the top four rows of Table 3), none of the pairwise differences was significant at the 0.05 level (the top four rows of Table 4). Hence, we focus our attention on the interactions between region of origin and stereotypes. Here, a statistically significant finding would indicate that the difference between, say, the Latin American stereotypes slope and the European stereotypes slope was greater for one impact item than for another. These "differences in differences" point estimates and results from significance tests are reported in Table 4.[15] For example, note the coefficient 0.480 in the "Crime minus Terror" column for Latin American immigrants. This coefficient indicates that

**TABLE 3.** Coefficients and Robust Standard Errors from Ordered Logistic Regressions of the Unstandardized Items in the Immigration Impact Scale: The Ohio Poll, 2007–2008

| | Unemployment | | Schools | | Country united | | Crime | | Terror | |
|---|---|---|---|---|---|---|---|---|---|---|
| Parameter | Coefficient | SE | Coefficient | SE | Coefficient | SE | Coefficient | SE | Coefficient | SE |
| Region/ballot (versus Europe) | | | | | | | | | | |
| Asia | −0.049 | 0.150 | 0.020 | 0.142 | 0.092 | 0.150 | −0.163 | 0.151 | 0.020 | 0.154 |
| Middle East | −0.014 | 0.145 | 0.126 | 0.149 | 0.223 | 0.143 | 0.118 | 0.140 | −0.050 | 0.139 |
| Latin America | 0.563*** | 0.155 | 0.705*** | 0.146 | 0.396* | 0.157 | 0.690*** | 0.137 | 0.637*** | 0.145 |
| Immigrant stereotype scale | 0.151 | 0.124 | 0.101 | 0.102 | 0.076 | 0.118 | 0.108 | 0.110 | 0.169 | 0.110 |
| Interactions | | | | | | | | | | |
| Asia × stereotype scale | 0.129 | 0.175 | 0.128 | 0.143 | −0.128 | 0.166 | 0.187 | 0.172 | 0.148 | 0.169 |
| Middle East × stereotype scale | 0.170 | 0.174 | 0.405* | 0.163 | 0.096 | 0.175 | 0.151 | 0.163 | 0.044 | 0.162 |
| Latin America × stereotype scale | 0.738*** | 0.184 | 0.783*** | 0.153 | 0.348* | 0.169 | 1.045*** | 0.154 | 0.565*** | 0.160 |
| Control variables | | | | | | | | | | |
| Income (versus less than $20,000) | | | | | | | | | | |
| $20,000 to $39,999 | −0.113 | 0.185 | −0.084 | 0.183 | −0.282 | 0.193 | −0.419* | 0.175 | −0.271 | 0.192 |
| $40,000 to $59,999 | 0.109 | 0.185 | −0.118 | 0.191 | −0.428* | 0.206 | −0.262 | 0.180 | 0.030 | 0.202 |
| $60,000 and over | 0.103 | 0.186 | −0.157 | 0.184 | −0.190 | 0.203 | −0.153 | 0.173 | 0.101 | 0.195 |
| Education (versus high school degree or less) | | | | | | | | | | |
| Some college | 0.368** | 0.137 | 0.111 | 0.135 | 0.263 | 0.140 | 0.263 | 0.135 | 0.278* | 0.135 |
| College graduate | 0.769*** | 0.130 | 0.445*** | 0.129 | 0.569*** | 0.135 | 1.002*** | 0.139 | 0.762*** | 0.136 |
| Urban residence (versus rural area) | | | | | | | | | | |
| City | −0.037 | 0.133 | 0.040 | 0.136 | 0.244 | 0.136 | 0.151 | 0.143 | 0.264 | 0.144 |
| Suburb | −0.046 | 0.138 | −0.241 | 0.143 | 0.091 | 0.144 | −0.071 | 0.150 | 0.012 | 0.150 |
| Party identification (versus Republican) | | | | | | | | | | |
| Democrat | −0.106 | 0.124 | 0.119 | 0.122 | 0.226 | 0.116 | 0.284* | 0.118 | 0.329** | 0.117 |
| Independent/other | −0.444* | 0.176 | −0.143 | 0.159 | 0.015 | 0.173 | −0.079 | 0.168 | −0.095 | 0.158 |
| Political ideology (versus Conservative) | | | | | | | | | | |
| Liberal | 0.576*** | 0.166 | 1.034*** | 0.170 | 0.558*** | 0.160 | 0.780*** | 0.165 | 0.703*** | 0.165 |
| Moderate | 0.350** | 0.119 | 0.510*** | 0.109 | 0.225* | 0.111 | 0.298** | 0.114 | 0.389*** | 0.112 |
| Media exposure | 0.002 | 0.003 | −0.002 | 0.003 | −0.001 | 0.003 | 0.003 | 0.003 | 0.004 | 0.003 |
| Race (versus black) | | | | | | | | | | |
| White | 0.446* | 0.214 | 0.346 | 0.187 | −0.152 | 0.190 | 0.248 | 0.202 | 0.444* | 0.207 |
| Other | 0.345 | 0.367 | 0.653* | 0.299 | −0.501 | 0.355 | −0.199 | 0.360 | 0.307 | 0.325 |
| Male | 0.300** | 0.102 | 0.046 | 0.097 | −0.022 | 0.099 | −0.112 | 0.101 | 0.079 | 0.099 |
| Age (versus 65 and older) | | | | | | | | | | |
| 18 to 29 | 0.085 | 0.189 | 0.529** | 0.176 | −0.041 | 0.180 | 0.573** | 0.179 | 0.527** | 0.185 |
| 30 to 44 | −0.187 | 0.142 | 0.377** | 0.135 | −0.182 | 0.136 | 0.317* | 0.142 | −0.033 | 0.136 |
| 45 to 64 | −0.161 | 0.126 | 0.112 | 0.120 | −0.157 | 0.119 | 0.013 | 0.128 | −0.082 | 0.121 |
| Imputed income | 0.098 | 0.120 | 0.071 | 0.118 | −0.022 | 0.118 | −0.045 | 0.122 | 0.066 | 0.117 |
| Survey wave 1 | 0.311** | 0.102 | 0.051 | 0.097 | −0.045 | 0.098 | 0.180 | 0.099 | −0.209* | 0.099 |
| Cut points | | | | | | | | | | |
| 1 | 1.186** | 0.396 | −0.087 | 0.323 | −0.825* | 0.340 | 0.764* | 0.378 | 0.824* | 0.358 |
| 2 | 2.780*** | 0.399 | 1.411*** | 0.324 | 0.888** | 0.342 | 2.563*** | 0.382 | 2.478*** | 0.361 |
| 3 | 4.347*** | 0.415 | 3.062*** | 0.330 | 2.281*** | 0.351 | 4.586*** | 0.403 | 4.238*** | 0.373 |
| Pseudo $R^2$ | 0.053 | | 0.054 | | 0.025 | | 0.083 | | 0.092 | |
| No. of cases | 2,090 | | 2,073 | | 2,073 | | 2,075 | | 2,075 | |

***$p < 0.001$; **$p < 0.01$; *$p < 0.05$, two-tailed tests.

Notes: The immigration impact items and the immigrant stereotypes scale were coded so that higher values relate to normatively positive impacts and stereotypes.

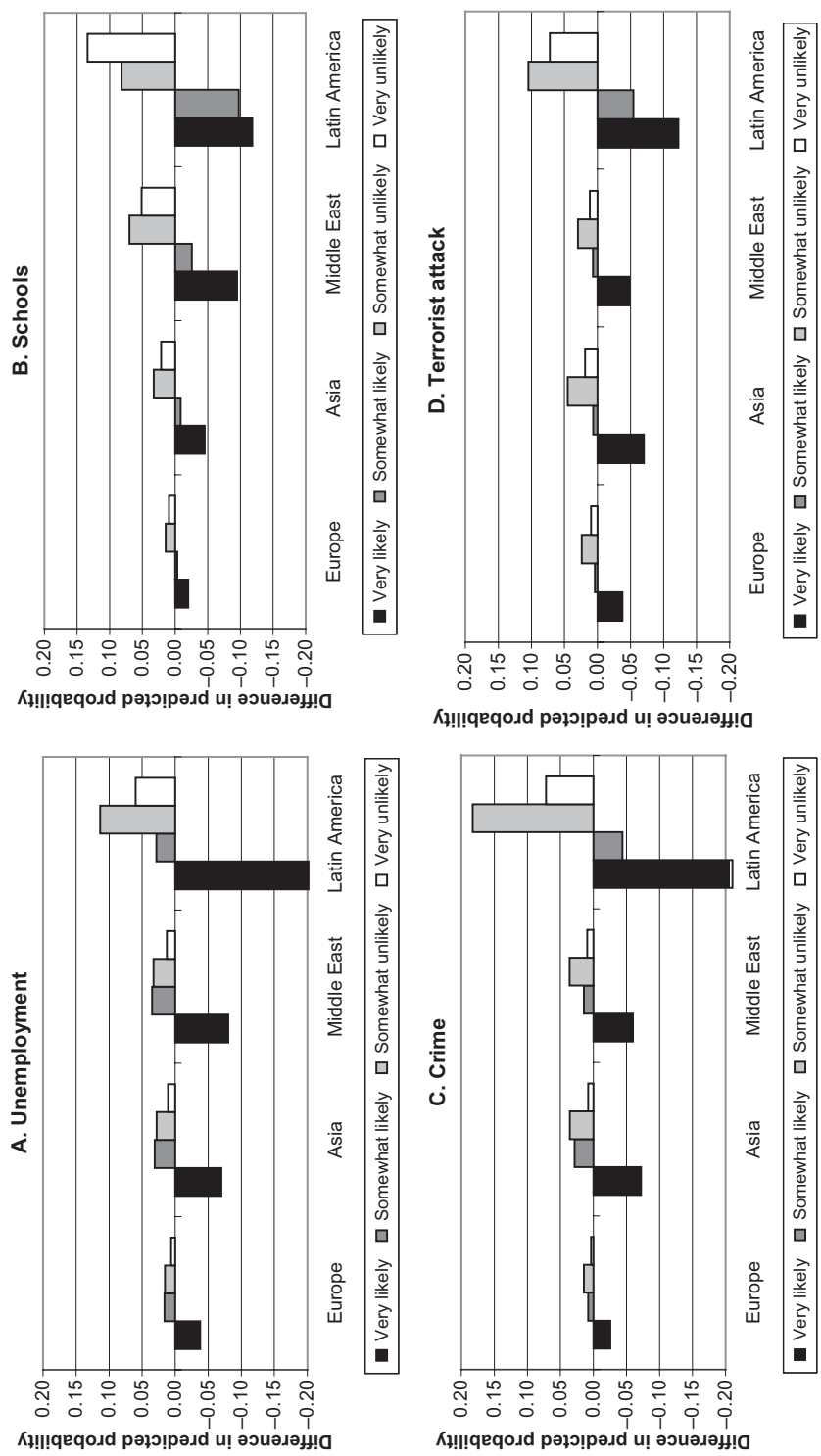


FIGURE 2. Effects of Centered Discrete Change in Immigrant Stereotypes on Predicted Probabilities of Immigration Impact Response Categories, by Immigrant Group: Ohio Poll, 2007–2008.

Notes: Data for this figure come from Table 3, with all covariates set to the grand mean except for immigrant group and the immigrant stereotypes scale. See note 1 for further description of the use of SPost in Stata (Long and Freese 2005).

**TABLE 4.** Point Estimates of Differences in the Effects of the Independent Variables on the Unstandardized Items in the Immigration Impact Scale: The Ohio Poll, 2007–2008

| | Unemployment minus: | | | | Schools minus: | | | Country united minus: | | Crime minus: |
|---|---|---|---|---|---|---|---|---|---|---|
| Parameter | Schools | Country united | Crime | Terror | Country united | Crime | Terror | Crime | Terror | Terror |
| Region/ballot (versus Europe) | | | | | | | | | | |
| Asia | −0.069 | −0.141 | 0.114 | −0.069 | −0.073 | 0.183 | 0.000 | 0.255 | 0.072 | −0.183 |
| Middle East | −0.140 | −0.237 | −0.132 | 0.036 | −0.097 | 0.008 | 0.175 | 0.105 | 0.273 | 0.168 |
| Latin America | −0.142 | 0.167 | −0.127 | −0.074 | 0.309 | 0.015 | 0.068 | −0.294 | −0.241 | 0.054 |
| Immigrant stereotype scale | 0.050 | 0.075 | 0.043 | −0.018 | 0.025 | −0.007 | −0.068 | −0.032 | −0.094 | −0.061 |
| Interactions | | | | | | | | | | |
| Asia × stereotype scale | 0.000 | 0.257 | −0.059 | −0.020 | 0.257 | −0.059 | −0.020 | −0.315 | −0.277 | 0.039 |
| Middle East × stereotype scale | −0.235 | 0.075 | 0.019 | 0.127 | 0.310 | 0.254 | 0.362 | −0.056 | 0.052 | 0.108 |
| Latin America × stereotype scale | −0.045 | 0.390 | −0.308 | 0.173 | 0.434 | −0.263 | 0.218 | −0.697** | −0.217 | 0.480* |
| Control variables | | | | | | | | | | |
| Income (versus less than $20,000) | | | | | | | | | | |
| $20,000 to $39,999 | −0.029 | 0.169 | 0.306 | 0.158 | 0.198 | 0.334 | 0.187 | 0.137 | −0.011 | −0.147 |
| $40,000 to $59,999 | 0.227 | 0.537 | 0.371 | 0.079 | 0.310 | 0.144 | −0.148 | −0.166 | −0.458 | −0.292 |
| $60,000 and over | 0.260 | 0.293 | 0.256 | 0.002 | 0.033 | −0.004 | −0.258 | −0.037 | −0.292 | −0.254 |
| Education (versus high school degree or less) | | | | | | | | | | |
| Some college | 0.257 | 0.105 | 0.105 | 0.090 | −0.152 | −0.152 | −0.167 | 0.000 | −0.015 | −0.015 |
| College graduate | 0.325 | 0.200 | −0.233 | 0.008 | −0.125 | −0.558** | −0.317 | −0.433* | −0.192 | 0.241 |
| Urban residence (versus rural area) | | | | | | | | | | |
| City | −0.077 | −0.281 | −0.189 | −0.302 | −0.204 | −0.111 | −0.224 | 0.093 | −0.020 | −0.113 |
| Suburb | 0.195 | −0.137 | 0.025 | −0.058 | −0.332 | −0.170 | −0.253 | 0.162 | 0.079 | −0.083 |
| Party identification (versus Republican) | | | | | | | | | | |
| Democrat | −0.225 | −0.332 | −0.390* | −0.435* | −0.107 | −0.165 | −0.210 | −0.058 | −0.103 | −0.045 |
| Independent/other | −0.300 | −0.459 | −0.364 | −0.348 | −0.158 | −0.064 | −0.048 | 0.094 | 0.110 | 0.016 |
| Political ideology (versus Conservative) | | | | | | | | | | |
| Liberal | −0.458* | 0.018 | −0.204 | −0.127 | 0.476* | 0.254 | 0.332 | −0.222 | −0.145 | 0.078 |
| Moderate | −0.160 | 0.124 | 0.052 | −0.040 | 0.284 | 0.212 | 0.121 | −0.072 | −0.164 | −0.092 |
| Media exposure | 0.004 | 0.003 | −0.001 | −0.002 | −0.001 | −0.005 | −0.006 | −0.004 | −0.005 | −0.001 |
| Race (versus black) | | | | | | | | | | |
| White | 0.100 | 0.598* | 0.198 | 0.002 | 0.497 | 0.098 | −0.098 | −0.399 | −0.596* | −0.196 |
| Other | −0.308 | 0.846 | 0.544 | 0.038 | 1.154* | 0.852 | 0.346 | −0.302 | −0.808 | −0.506 |
| Male | 0.254 | 0.323* | 0.412** | 0.221 | 0.069 | 0.158 | −0.033 | 0.089 | −0.101 | −0.191 |
| Age (versus 65 and older) | | | | | | | | | | |
| 18 to 29 | −0.444 | 0.126 | −0.488 | −0.442 | 0.570* | −0.044 | 0.002 | −0.614* | −0.567* | 0.047 |
| 30 to 44 | −0.564** | −0.005 | −0.505* | −0.154 | 0.559** | 0.059 | 0.410* | −0.499* | −0.148 | 0.351 |
| 45 to 64 | −0.273 | −0.004 | −0.173 | −0.079 | 0.268 | 0.099 | 0.194 | −0.169 | −0.075 | 0.094 |
| Imputed income | 0.027 | 0.120 | 0.143 | 0.032 | 0.093 | 0.116 | 0.005 | 0.023 | −0.088 | −0.111 |
| Survey wave 1 | 0.260 | 0.356* | 0.130 | 0.520*** | 0.096 | −0.129 | 0.260 | −0.225 | 0.164 | 0.389** |
| Cut points | | | | | | | | | | |
| 1 | 1.273* | 2.011*** | 0.422 | 0.362 | 0.738 | −0.851 | −0.911 | −1.589** | −1.649*** | −0.060 |
| 2 | 1.369** | 1.891*** | 0.217 | 0.301 | 0.522 | −1.152* | −1.068* | −1.674** | −1.590** | 0.084 |
| 3 | 1.285* | 2.066*** | −0.240 | 0.109 | 0.781 | −1.524** | −1.176* | −2.305*** | −1.957*** | 0.348 |

***$p < 0.001$; **$p < 0.01$; *$p < 0.05$, two-tailed tests.

Notes: The immigration impact items and the immigrant stereotypes scale were coded so that higher values relate to normatively positive impacts and stereotypes.

the difference between the Latin American and European slopes was about half of a standard deviation greater when respondents were asked about the impact of immigration on crime than about the likelihood of a terrorist attack.

An inspection of the "Interactions" panel in Table 3 reveals that, in contrast to Table 2 where the only significant interaction effect occurred for Latin American immigrants, in the "schools" column, we observe a significantly steeper stereotypes slope for Middle Eastern immigrants relative to European immigrants. This indicates that there is a stronger association between immigrant stereotypes and assessments of the impact of immigration on school quality for raters of Middle Eastern immigrants than for raters of European immigrants. In this same panel, we find that the slopes relating stereotypes of immigrants to the five immigration impact items are significantly steeper for Latin American immigrants than for European immigrants (see the "Latin America × stereotype scale" row in Table 3). In addition, post-estimation tests reveal that these slopes are steeper for Latin American immigrants than for Asian and Middle Eastern immigrants, with the sole exception of the Middle East versus Latin America contrast for "keeping the country united."

Interestingly, our data show that the association between stereotypes and terrorism is statistically equal for Middle Eastern, European, and Asian immigrants and significantly *weaker* than the relationship for Latin American immigrants. This indicates that whatever drives Ohioans' beliefs in the impact of immigration on the likelihood of terrorist attacks, it is not particularly strongly related to stereotypes of immigrants from the Middle East (see also Figure 2D). It is always difficult to explain a null or counterintuitive finding such as this, but we believe that the lack of this seemingly obvious relationship in the post-9/11 era—that is, a strong association between stereotypes of Middle Eastern immigrants and perceptions of the likelihood of a terrorist attack—underscores the centrality of Latin American stereotypes for understanding immigration impact assessments. In other words, although respondents vary in their belief that a terrorist attack is made more likely because of immigration, and although they vary in their stereotypes of immigrants from different regions, the correlation between these two variables is much stronger for Latin American immigrants than for the other three groups.

Our second finding of interest is the weak association between stereotypes and the likelihood that immigration makes it harder to "keep the country united." On this item, there were no significant differences in slopes for European, Asian, or Middle Eastern stereotypes, and the gap between the Latin American and the other group slopes was the smallest among the five items. In statistical terms, the "Immigrant stereotype scale" coefficient is nonsignificant in the "Country united" column of Table 3, and the sums of that coefficient and each of three non-Latin American stereotype interaction effects are also nonsignificant at the 0.05 level.

Finally, the data in Table 3 indicate that the impact items to which respondents tied stereotypes of Latin Americans most closely—at least compared to European immigrants—were unemployment, schools, and crime. For these items, the Latin American stereotype slopes were 0.738, 0.783, and 1.045 logits greater than the

European stereotype slopes, respectively. The crime gap between the Latin American and European slopes was significantly greater than the "keeping the country united" gap, and in addition the "crime" versus "terror" difference in differences was significant at the 0.05 level (see Table 4).

To put these findings in more intuitive terms, we generated predicted probabilities for respondents who were statistically equal in all respects except for (1) the group of immigrants they rated and (2) whether they rated those immigrants one-half of a standard deviation above or below the mean on the immigrant stereotypes scale.[16] Figure 2 presents results from these simulations for the four immigration impact items for which group stereotypes were particularly salient (at least in the case of Latin American immigrants).

Figure 2 shows that respondents who rated European immigrants one-half of a standard deviation above or below the mean on the stereotypes scale had virtually the identical probabilities of reporting any of the response categories. Notice in all four panels of Figure 2 that the four bars, ranging from "very likely" to "very unlikely" are close to the zero point for European immigrants, indicating that respondents who were a full standard deviation apart from each other in their stereotypes of those immigrants tended to report the same categories on the dependent variable (i.e., the difference in the probability of reporting any of the categories is close to zero). By contrast, respondents who rated Latin American immigrants one full standard deviation different on the stereotypes scale reported very different categories on the dependent variable.

For example, Figure 2A shows the difference in the probability of reporting that it is "very likely" for unemployment to get worse as a result of immigration was 0.20 for respondents who differed by one full standard deviation on the Latin American stereotypes scale (note the large black bar in the Latin America column of Figure 2A). As an additional example, Figure 2C shows that the gap in predicted probabilities of rating crime as "very likely" to get worse as a result of immigration for respondents who were one standard deviation apart in ratings of European immigrants was −0.03. Again, this indicates that even though respondents differed in their stereotypes of European immigrants, this did not translate into variation in the perceived impact of immigration on crime. For equivalent raters of Latin American immigrants, this gap was −0.21 (note the large negative bar for Latin American immigrants in Figure 2C). Hence, as stereotypes of Latin American immigrants become more negative, respondents are much more likely to report a negative impact of immigration on crime. Furthermore, the large positive bar indicating a "somewhat unlikely" chance that immigration will make crime worse indicates that respondents with more positive views of Latin American immigrants hold relatively sanguine views about the impact of immigration. These findings demonstrate our central point in this article—that Ohioans' assessments of the impact of immigration are much more sensitive to their stereotypes of Latin American immigrants, both positive and negative, than their stereotypes of any of the other immigrant groups.

*Control Variables*

Finally, in this section, we review the relationships in Table 3 between control variables and the five impact assessment items taken separately. First, we found just two statistically significant coefficients for income groups, relative to the omitted category—respondents with personal incomes less than $20,000 per year. These findings conform to those in Table 2, which found no strong relationship between income and the immigration impact scale. Also in keeping with our findings from Table 2, we found much more robust effects of higher education, with college graduates reporting much more normatively positive assessments of each of the five immigration impacts than their counterparts with a high school degree or less.

In terms of political variables, party identification was most strongly related to assessments of the impact of immigration on crime and terrorism, with Democrats reporting more sanguine attitudes about the impact of immigration in these domains than Republicans. As in Table 2, we observed much stronger effects of political ideology, with both liberals and moderates scoring on the higher end of the five impact items than conservatives, indicating greater beliefs among the former two groups in the normatively positive impact of immigration. Race, sex, and age were not consistently strongly related to impact assessments, with the exception that younger voters reported less concern over the impact of immigration on schools, crime, and terror than did senior citizens.

Finally, we observed one positive and one negative relationship between survey wave and assessments of the impact of immigration. The positive coefficient concerned the impact of immigration on unemployment. It is conceivable that the period between the fielding of the two waves coincided with Ohioans' increasing sense that the economy was showing signs of recession, and therefore adjusted their views on the impact of immigration between these two waves. The negative relationship concerned the impact of immigration on the likelihood of a terrorist attack, and indicates that, controlling for all other variables in the model, wave 1 respondents were more likely to believe that immigration made a terrorist attack more likely, relative to their wave 2 counterparts. We speculate that this may have to do with a generally increasing sense that the "war on terror" was working. For example, the "surge" of U.S. troops in Iraq occurred during 2007. It is conceivable that growing signs of its success in tamping down violence in Baghdad may have led to a general sense that terrorism was becoming less of a problem overall, which may have led to a reduction in public beliefs that immigration was a likely cause of terrorism. We note that these are the only survey wave differences, and believe it could be useful to discover if our speculations are correct. However, we cannot assess them with these data.

## CONCLUSIONS

The primary goal of this article was to investigate the extent to which the relationship between stereotypes of immigrants and assessments of the impact of immigration varies by the immigrant group in question. We noted that because Ohio has very low

percentages of foreign-born residents (less than 4 percent from 2006 through 2010 ACS data), it seemed unlikely that group threat theory as traditionally conceived could drive differences in the relationship between stereotypes and impact assessments. We argued that such differences would be much more likely to be generated by state- and national-level constructions of the "problem" of immigration. Because such debates have focused on Latin American immigrants, we reasoned that the relationship between stereotypes and impact assessments would be strongest for that group. Our findings supported this hypothesis in that the slope of the line relating stereotypes to impact assessments was significantly steeper when the group under consideration was Latin Americans than when it was one of the other three groups. Put conversely, our findings show that respondents do not link stereotypes of Asian, Middle Eastern, or European immigrants tightly to their assessments of the impact of immigration. This does not mean there is no variation in respondents' stereotypes of the non-Latino groups; only that this variation is not systematically related to variation in respondents' assessments of the impact of immigration.

Past research has hinted at these findings by examining effects of attitudes toward Latinos, or attitudes toward Latinos and whites. Nevertheless, we believe our study provides the most solid evidence available on this question for three reasons. First, unlike Pantoja (2006), Buckler et al. (2009), and Lu and Nicholson-Crotty (2010), each of which examined effects of attitudes toward Latinos only; we expanded the analysis to four groups of immigrants. Hence, we were able to show that stereotypes about Latin American immigrants do not have the same effect as stereotypes of other groups of immigrants, at least on assessments of the impacts of immigration. Second, unlike Brader et al. (2008) and Pérez (2010), who manipulated the immigrant target group in a laboratory setting, we analyze data from a probability sample, in particular of Ohio registered voters. Finally, unlike Citrin et al. (1997), Hood and Morris (1997), and Burns and Gimpel 2000, our analysis relies on stereotypes of immigrants, not of racial or ethnic groups in general. Although it is unclear whether our results would have been different if we had asked about, say, "Latinos," "Arabs," "Asians," and "whites," we are confident at least that our measures target the groups implied by our dependent variables, that is, immigrants.

Finally, the results in Tables 3 and 4 and Figure 2 show that Ohioans most closely link stereotypes of Latin American immigrants to concerns about the impact of immigration on unemployment, school quality, and crime. Ohioans linked Latin American stereotypes to expectations of a terrorist attack only weakly, and still more weakly to concerns about keeping the country united. This latter point is significant, for it demonstrates that despite the concerns of some anti-immigration commentators (e.g., Huntington 2004), Ohioans do not, on average, link stereotypes of Latin American immigrants (or any other immigrant group, for that matter) strongly to national unity and cohesion.

Hence, we believe our findings indicate that the rhetorical yoking of immigration to Latin Americans has been most salient in terms of relatively daily and concrete concerns. In other words, to the extent that Ohioans' actual or virtual (and media-filtered)

experiences with Latin American immigrants have led them to be optimistic or pessimistic about the impact of immigration, it seems that these associations have been more focused on their jobs, their children's schools, and the crime in their neighborhoods. It remains to be seen whether these "home and hearth" concerns will continue to fuel high levels of antipathy toward Latin American immigrants, or whether, as Latinos continue to move into new destinations in Ohio and other similar states, these animosities will begin to subside.

Immigration has waxed and waned in its intensity as a political and social issue. Historically, the crests in controversy and intensity have occurred either as a result of economic contractions in which competition for resources leads to concerns about immigration, or during periods marked by high numbers and density of noticeably different "others." Since September 11, 2001 both conditions have been present in the United States, coupled with heightened security concerns, and immigration has again become a "hot button" issue. As we have demonstrated, the immigration concern is overwhelmingly directed at immigrants from Latin American countries.

Our analyses do not enable us to adjudicate between the "group threat" and "social constructionist" perspective, nor do we believe these two theoretical approaches are incompatible. We argue that the fundamental claim of "group threat" theory is that the presence of out-group members triggers a set of responses by the in-group; chiefly negative affect and the mobilization of mechanisms to exclude the encroaching group from access to resources like political power, education, and jobs. But where there is a small local presence of the out-group, in order for there to be a threat response among the in-group that threat must be communicated somehow, likely via "salient national rhetoric" (Hopkins 2010:40). We argue that this is the theoretical terrain of social constructionism. That is, group threat qua group threat would argue that it is the presence of out-group members that causes in-group members to develop negative attitudes and exclusionary mechanisms. We argue, however, that even in a state with few out-group members, which (according to orthodox group threat theory) should provide no threat and therefore no variation in the relationship between stereotypes and impact assessments, there can be variation in such relationships, because of the construction of one or more groups as a particular threat or problem.

As we and others such as Chavez (2008) have demonstrated, current anti-immigration reaction is primarily about "the Latino threat." We hypothesize that this narrative is primarily communicated through media sources such as newspapers, television, and the Internet, particularly television programs that take a specifically anti-immigrant stance and the Web sites of anti-immigration groups such as Federation for American Immigration Reform (http://www.fairus.org), NumbersUSA (http://www.numbersusa.org), or Americans for Immigration Control, Inc. (http://www.immigrationcontrol.com). Although we found no relationship between a measure of total media exposure and impact assessments, we do not think that this necessarily vitiates the claim that public attitudes in non-destination states are determined by messages communicated via the media. We suspect that had we had measures of exposure to particular Internet sites and conservative political pundits,

we would have found strong relationships between media consumption and impact assessments.

We conclude by noting that an important narrative theme within the American story line is that "we are a nation of immigrants," and millions of Americans are proud of and display their own heritage of migration, adaptation, and prosperity. Anyone who follows the public discourse on the current politics of immigration cannot escape noticing the number of times people preface their opinions—both for and against many different versions of immigration reform—with the claim to feel positively about immigration in principle, or with a bow to our "nation of immigrants" history, or by recounting their own family's story. However, as our findings show, reactions to immigration are filtered through attitudes toward the characteristics citizens believe particular immigrant groups hold. In brief, who "they" are matters.

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## NOTES

[1]For example, a *USA Today/Gallup Poll*, fielded January 14 through 16, 2011, asked respondents whether Congress should "take steps to deny automatic citizenship to children born in the United States whose parents are illegal immigrants." Relative frequencies for each of the four response categories (strongly favor to strongly oppose) ranged between 22 percent and 28 percent (PollingReport.com 2011).

[2]We use such perceptions as our dependent variables in this article. We sometimes use the shorthand term "impact assessments" in place of the more cumbersome "assessments of the impact of immigration on the United States." We argue that understanding stereotypes of immigrants and beliefs about the impact of immigration on the United States are important steps in understanding Americans' policy opinions. After all, if citizens have dim views of immigrants and believe that immigration causes serious problems, they will likely support policies that curtail immigration (Chavez and Provine 2007). Hence, although our article does not directly examine policy attitudes, we believe that by studying important antecedents of those views we can contribute to scholarly focus on policy issues.

[3]Hence, we are predicting attitudes with attitudes, which should raise concerns about issues of causal order. We follow Ceobanu and Escandell's (2010) admonition that attitudes toward immigrants and immigration are different constructs; however, as Schuman (2000:304) notes, all attitudes "swim around in the same heads" (cited in Ceobanu and Escandell 2000:313). As suggested by Ceobanu and Escandell (2010:313), we posit that respondents' beliefs about the impact of immigration are partially determined by their beliefs about the desirability of the traits of immigrants; however, it is conceivable that the causal arrow runs in the opposite direction. That is, it is possible that citizens (1) decide that immigrants are helping or harming the country, which makes them conclude that (2) immigrants must have desirable or undesirable characteristics, especially (3) Latinos, who comprise the largest and most visible immigrant

group. Or, it is possible that the relationship is spurious, and that unmeasured characteristics of citizens or sociopolitical structures lead respondents to conclude simultaneously that immigrants (1) have desirable or undesirable characteristics, and (2) are helping or harming the country. Our data are cross-sectional, so there is little we can do about these issues; hence, we focus more on the variability in the relationship between stereotypes and impact assessments across groups and in general refer to "relationships" and "associations" rather than "effects."

[4]We do not consider the relationship between immigrant region of origin and impact assessments because region was randomly assigned to respondents and therefore could not be related to the dependent variable. In addition, our measures of perceived impacts preceded the questions about immigrant stereotypes. Had the question order been reversed, we might have expected some sort of priming effect; that is, subjects might have thought about the group of immigrants on their experimental ballot when considering questions about the impact of immigration. This would have undermined the methodological benefit of the split ballot, however, in that respondents would not have been balanced on all predictors of the dependent variable. This question order also renders the design nonexperimental because the manipulation of the ballot occurred after the questions that make up the dependent variable, and so cannot be said to "cause" the outcome. Rather, the design yielded samples of attitudes about four different groups of immigrants, and the split ballot simply ensures that respondents did not calibrate their answers to certain groups of immigrants on the basis of responses to questions about other groups.

[5]The exception to this rule would be if the priming of a group of immigrants in the ballot to which respondents were randomly assigned caused respondents differentially (across ballots) to alter their responses to the variables we include in model 2 of Table 2. For example, if respondents who were assigned the "Latin America" ballot were especially likely to link stereotypes of immigrants to impact assessments *and* to alter their self-reported education level, then controlling for education would attenuate the Latin America versus Europe difference in the relationship between stereotypes and impact assessments. We have found no evidence that respondents behave in this fashion; that is, that they systematically bias their answers about relatively objective sociodemographic characteristics in response to priming.

[6]Ohio also features a small population of Eastern and sub-Saharan African immigrants (totaling 0.34 percent of the population in 2006–2010). A focus in the Ohio Poll questions on issues of national security led to the inclusion of Middle Eastern immigrants. African immigrants were, unfortunately, not able to be included in the analysis.

[7]These rates are in line with those cited in recent research on declining response and cooperation rates in random digit dial (RDD) phone surveys (Pew Research Center 2012). Furthermore, two studies of the likely effects of survey nonresponse have found that RDD surveys with lower response rates do not produce dramatically different results than surveys deliberately fielded to increase response rates. For example, Keeter et al. (2000) report on a 1997 study, in which respondents were either contacted over a "standard" 5-day period or a "rigorous" 8-week period. The authors found that of 91 items included in the survey, only 15 (16.5 percent) showed significant differences in responses across the two survey conditions (i.e., "standard" versus "rigorous"). Among the items relevant to the present study, respondents were asked their "opinion of" black people, Hispanics, Asians, and Jews. Only opinions of black people varied significantly across conditions; however, the difference in responses to the modal category ("mostly favorable") was only three percentage points (66 percent in the "standard" condition and 69 percent in the "rigorous" condition) (Keeter et al. 2000:130–31). In a follow-up study

from 2003, Keeter et al. (2006) found that of 84 items surveyed, only 7 (8.3 percent) yielded statistically significant differences across the two conditions, and that the magnitude of the statistically significant findings was small (6 of the 7 significant differences ranged between 3 and 6 percentage points [767]). Among the items germane to our study, respondents in the "rigorous" condition were significantly less likely than those in the "standard" condition to report favorable opinions of Jews (22 percent versus 25 percent, respectively; 768). In addition, respondents in the "hardest to reach" group (those who refused to be interviewed at least twice before agreeing or required 21 or more calls before agreeing) were less likely to offer an opinion, either positive or negative, of racial or ethnic groups (771). These respondents were also less likely than those in the "standard" group to agree that "African-Americans [are] mostly responsible for [their] own condition" (58 percent versus 64 percent, respectively; 772). Importantly, however, there were no significant differences between the "standard," "rigorous," or "hardest-to-reach" groups in terms of average responses to the three items most directly relevant to our study. These questions asked whether (1) "immigrants [are a] burden on [the] United States"; (2) "Islam encourages violence"; and (3) "Most Muslims are anti-American" (772). Overall, the authors conclude that "there is little to suggest that unit nonresponse within the range of response rates obtained seriously threatens the quality of survey estimates" (Keeter et al. 2006:759). Hence, although it is conceivable that nonresponse bias is operating to an unknown degree in our study, the available evidence suggests that nonresponse is not likely to be a substantial source of bias in our results. Finally, data from the University of Cincinnati's Institute for Policy Research show that the Ohio Poll provides extremely accurate estimates of voter behavior (IPR 2008b). Thus, we are confident that these data provide valid estimates of attitudes toward immigration among registered voters in Ohio.

[8]More formally, each respondent $i$ has a value on the unstandardized scale $y$ equal to the average of the five unstandardized immigration problem items. The standardized scale $y^*$ is equal to:

$$y_i^* = \frac{(y_i - \overline{y})}{s_y}$$

where $y_i$ is the score on the unstandardized scale and $\overline{y}$ and $s_y$ are the mean and standard deviation of the unstandardized scale, respectively.

[9]In developing these items, we elected to list three countries from each region to clarify what was meant by immigrants from those regions. For each region, we selected a country that we reasoned might elicit a relatively positive response (Japan, Ireland, Cuba, and Jordan), an ambivalent response (Korea, France, Brazil, and Saudi Arabia), and a relatively negative response (China, Poland, Mexico, and Iran). Although these choices arguably are strong on face validity, time and budgetary constraints prevented us from verifying their content validity. In addition, because the three countries suggested from Asia were all East Asian (as opposed to South Asian countries like India or Southeastern Asian countries like Vietnam), our results should be interpreted as applying most directly to attitudes toward East Asian immigrants. However, because the preamble to the question states "Asian countries, such as . . ." it is conceivable that our results are at least somewhat generalizable to all countries respondents think of as "Asian." Finally, readers should note that the regional categories we chose are broad, and may not reflect attitudes toward immigrants from specific countries. Although we primed specific countries in the preamble to the stereotype questions, it is unclear to what extent respondents focused on those countries versus the region as a whole.

[10]We used the *impute* command in Stata to predict the categories of income with an ordered logit regression of income on race, sex, education, urban residence, age, and metropolitan area.

[11]The categories of media consumption used in the scale were local magazines or newspapers, national magazines or newspapers, television news programs, radio, or satellite radio news programs, and blogs, e-mail lists, or other online media. We consider this scale to be formative (as opposed to reflective) in nature, because it measures the level of a latent trait (media exposure) without necessarily presuming correlation between the items. In other words, we combined items into a scale to measure total media exposure, not to reduce error in our estimate of a latent trait. For this reason, we did not conduct factor or reliability analysis on this scale.

[12]Specifically, we used the *lincom* command in Stata, which tests the null hypothesis that the "Immigrant stereotype scale" slope (which relates to European immigrants) plus the other three slopes taken individually are significantly different from zero.

[13]Scholars have offered at least two explanations for the relationship between education and immigration attitudes. First, education is frequently conceptualized as an indicator of social class. Higher levels of education are likely related to higher income and occupational prestige, and therefore lower levels of labor market competition with low-skill immigrants. Second, scholars have interpreted high levels of education as representing a more tolerant stance toward immigration, and an appreciation of the cultural diversity brought by immigrants. For example, Betts (1988) argues that "cosmopolitan" citizens hold more global worldviews, and therefore hold more positive attitudes toward immigration. Such cosmopolitanism could be an effect of education or an exogenously-determined characteristic that leads cosmopolitan students to select into higher education. We cannot distinguish between these alternatives with our data.

[14]The "cut points" (CP) reported in the output are analogous to the constant term in a binary logit model. Hence, cut point 1 yields the predicted log odds of scoring the lowest value on the dependent variable (in this case, a "1") for respondents who score 0 on all independent variables in the model. Cut point 2 refers to the log odds of scoring the next highest value on the dependent variable (a "2" in our study) for respondents scoring 0 on all independent variables, and so on. In terms of predicted probabilities, for $CP_k = 1$ to $K$–1 (where $K$ is the number of categories in the ordered categorical dependent variable),

$$\Pr(y = k \mid \mathbf{x} = 0) = \frac{\exp(CP_k)}{(1 + \exp[CP_k])}$$

[15]Test statistics for these differences follow the form:

$$t_{(\beta_k^i - \beta_k^j)} = \frac{(\beta_k^i - \beta_k^j)}{\sqrt{\sigma^2_{\beta_k^i} + \sigma^2_{\beta_k^j}}}$$

where $\beta_k^i$ and $\beta_k^j$ are coefficients from impacts $i$ and $j$ (for $i \neq j$) and $\sigma^2_{\beta_k^i}$ and $\sigma^2_{\beta_k^j}$ are squared robust standard errors from Table 3. Although the ordered logistic regression model is nonlinear in terms of underlying probabilities, the logit transformation of the dependent variable makes the models "linear in the coefficients"; hence, the test statistic noted above is unbiased.

[16]We used the Stata command *prvalue*, available through the SPost package developed by Long and Freese (2005). Although ordered logistic regression models are linear in the coefficients, the underlying probability surface is nonlinear. Hence, to generate predicted probabilities, values for all covariates must be entered. We set all covariates to the sample mean and then varied the

group of immigrants under consideration and the score on the stereotype scale, either one-half of a standard deviation below the mean (zero in our case) and one-half of a standard deviation above the mean. Long (1997:77–78) calls this interval "centered discrete change."

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