Geographic Mobility and Globalization Backlash:

Evidence from the NAFTA Import Shock and Populist Votes for Ross Perot*

Thomas M. Flaherty†

October 18, 2022

[Link to latest version.]

Abstract

The geographic mobility of citizens significantly influences the political effects of globalization. Within import-shocked regions, immobile voters support anti-trade populism while those who can migrate to globalizing cities favor trade openness. To test this theory, I train a machine learning model on 9.7 million Census observations of actual geographic mobility to predict an individual's probability of migrating between US commuting zones. I pair this with a panel of voters tracked across the 1992-1996 presidential elections, and a regional import shock from NAFTA between those elections. Among immobile respondents, NAFTA caused a 30 percentage point increase in the probability of voting for the anti-trade populist Ross Perot while mobile voters continued to support mainstream candidates. These patterns are consistent with NAFTA's effects on respondents' wages, employment, and trade policy preferences, but not with alternative cultural hypotheses. These findings challenge the growing consensus that economic concerns over trade have only muted political consequences.

*This material is based upon work supported by the National Science Foundation Graduate Research Fellowship Program under Grant No. 2018241622. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the author(s) and do not necessarily reflect the views of the National Science Foundation. I thank John Ahlquist, J. Lawrence Broz, Ryan Brutger, Fabian Eckert, Gordon Hanson, David Lake, Megumi Naoi, Ronald Rogowski, and Christina Schneider for helpful comments. I would also like to thank participants of the Omni Methods Group at UCSD, 21 May 2020; ISA, 6 April 2021; the GSIPE Virtual Trade Conference, 27 April 2021; and the Consortium on the American Political Economy 2021 summer academy.

†University of California, San Diego, Department of Political Science, email: t1flaher@ucsd.edu, website: https://www.thomasflaherty.com.

1 Introduction

What are the electoral consequences of international trade? Recent globalization backlashes by populist parties have renewed interest in this question.[1] However, standard trade theories[2] frequently fail to explain public opinion on trade or vote choices, especially when tested with individual-level data (Hafner-Burton et al., 2017; Margalit, 2019; Frieden, 2022). Reviewing the evidence, some scholars conclude that economic theories are "of little use in explaining mass attitudes on trade," and only marginally better at predicting populist voting (Margalit, 2019).[3]

However, prior studies may have missed important heterogeneity in how individuals experience trade's economic effects. Specifically, the standard approaches, which characterizes the individual "pocketbook effects" of trade based on voters' industry (Gourevitch, 1986; Frieden, 1991) or occupation (Rogowski, 1987), appear insufficient given significant inequality within these groups (Flaherty and Rogowski, 2021). I argue that the effects of trade on individual policy preferences and voting become more evident when we adapt the standard model to account for two empirical trends: (1) rising interregional inequality and (2) individual heterogeneity in geographic mobility.

Since the 1980s, globalization has significantly contributed to a "Great Divergence" between prosperous core cities and "left-behind" peripheries (Moretti, 2012; Autor, 2019). These spatial divisions are not simply an artifact of differences in the average characteristics

[1]A recent survey cites no fewer than 150 such papers (Walter, 2021). One prominent vein investigates the consequences of the "China shock" on district vote shares, especially for populist parties, across a variety of democracies: the US (Feigenbaum and Hall, 2015; Autor et al., 2016; Ferrara, 2022); Brazil (Freitas et al., 2020); the UK (Colantone and Stanig, 2018*a*); Italy (Caselli, Fracasso and Traverso, 2020; Barone and Kreuter, 2020); Germany (Dippel et al., 2017); France (Malgouyres, 2017); and across Europe more generally (Colantone and Stanig, 2018*b*; Milner, 2021).

[2]The Open Economy Politics (OEP) tradition argues that trade and immigration policy preferences follow from how these policies directly affect voters' income through their industry of employment (Gourevitch, 1986; Frieden, 1991) or through their occupational skills (i.e., education) (Rogowski, 1989).

[3]Similarly, in their review of the trade attitudes literature, Hafner-Burton et al. (2017) conclude that "after more than a decade of careful empirical research, there is little evidence that voters actually define their interests in these rational, materialist ways."

of residents. Rather, workers of all backgrounds—regardless of education, age, gender, race and other observables—have historically received significant "urban wage premiums," from living in globalizing cities (Kline and Moretti, 2013; Ganong and Shoag, 2017).[4] The reverse has occurred in peripheral regions: global competition has hollowed-out entire communities, affecting even non-traded sectors (Hakobyan and McLaren, 2016; Autor, Dorn and Hanson, 2013). These differences between regions can reach twice the magnitude of the differences within business cycles (from boom to bust) (Kline and Moretti, 2013).[5]

Also since the 1980s, rising core-periphery inequality has coincided with significant decreases in the average geographic mobility of Americans—i.e., how often individuals migrate between regions (Molloy, Smith and Wozniak, 2011; Cooke, 2011). By most measures, internal migration is at a 30-year low.[6] However, this trend is not universal; most of the variation in internal migration reflects *individual* heterogeneity. Even within occupations and industries, observably similar voters vary dramatically in their ability to relocate to higher growth areas(Bonin et al., 2008; Molloy, Smith and Wozniak, 2011). This heterogeneity causes concern because geographic mobility not only contributes to the Great Divergence, but also represents a primary way that individual voters adapt to rising interregional inequalities (Moretti, 2012; Greenland, Lopresti and McHenry, 2019; Autor, 2021). By incorporating aspects of interregional inequality and geographic mobility into standard trade theories, my analysis recovers substantively large effects of self-interest on trade policy preferences and individual votes for anti-trade populism.

Specifically, I develop a theory where a combination of (a) individual geographic mobility and (b) regional trade shocks drive trade policy preferences and voting for anti-trade populists.[7] Building on the standard occupation-based model, I show how political

[4]Although since 2010, rising housing costs have started to erode, although not fully erase, the urban wage premium enjoyed by low-skill workers (Autor, 2020).

[5]The US is no exception: similar patterns of persistent regional divergence have been observed in Italy, Spain, France, and Germany (Elhorst, 2003).

[6]However, migration is still common. About 40 percent of Americans born in the US end up living in a different state (Jia et al., 2022).

[7]I specifically incorporate lessons from the so-called "new" economic geography tradition started by

coalitions over trade shift from class-based cleavages (owners of capital versus labor, or equivalently, skilled versus unskilled) to geographic cleavages (core versus periphery) as geographic mobility decreases. This occurs in part from the introduction of agglomeration externalities—i.e., neighbor-to-neighbor spillover effects that occur when traded activities concentrate in different regions. These externalities produce regional inequality for observably similar occupations, with both skilled and unskilled workers receiving "urban wage premiums" from living in globalizing cities versus declining peripheries. However, because these effects of trade depend on location, geographic mobility serves as the crucial mediator at the individual level. Those voters who lack the ability or desire to move—who are anchored in place—rationally focus on the spillover effects within their own communities. Voters with high geographic mobility, however, possess the migratory means to arbitrage regional differences in opportunity, and are thus well-adapted to thrive under globalization. With few exceptions, voters in trade-booming cities should oppose barriers to globalization and the populist politicians who propose them. Within trade shocked peripheries, voters who lack the ability to adapt via migration are more likely to lash back against globalization compared to those who can easily migrate. This perspective, in short, offers a theoretical framework that more accurately captures the highly unequal effects of trade on individuals.

In applying an economic geography lens to individual attitudes and voting, the analysis confronts two empirical challenges. First, individual measures of geographic mobility are often unobserved in public opinion surveys. I resolve this with a machine learning technique that excels at measuring complex latent variables. This method uses Census microdata—specifically, the internal migration patterns of 9.7 million Americans—to construct an algorithm that accurately predicts a survey respondent's probability of migration between US labor markets (i.e., commuting zones) within the previous five years. The probabilities closely map onto the theoretical concept of mobility by accounting for the many

Krugman (1993), generalized by Fujita, Krugman and Venables (2001), and refined by quantitative spatial models (Redding, 2020).

individual and regional characteristics (e.g., housing, family, local opportunities, and other socio-economic considerations) that are important when individuals weigh the costs and benefits of moving between local economies. To demonstrate construct validity, I show that the measure's variation is consistent with the prior literature, that its use is robust to alternative proxies, and that it strongly predicts expected outcomes, among them survey attrition, economic conditions, and community-based behaviors.

Causal plausibility represents the second empirical challenge. A common critique is that measures of economic self-interest are confounded by non-material factors.[8] It is likely the case that regional trade shocks and geographic mobility correlate with non-economic characteristics like race and social networks at the community and individual level (Mansfield and Mutz, 2009; Mutz, 2018). Additionally, political parties target their national messaging to key demographics (Guisinger and Saunders, 2017).

The case of the North American Free Trade Agreement (NAFTA) has several features that help guard against these pitfalls. On January 1, 1994, the implementation of NAFTA caused a majority of US protections against Mexican imports to fall immediately to zero. As depicted in Figure 1, the pace of this shock to US industries, in terms of the average value of net imports from Mexico, was both rapid and sudden. Variation in exposure was reasonably exogenous from the perspective of any individual voter; however, institutional features also make it plausibly exogenous from the perspective of regional commuting zones. Due to the Congressional delegation of trade negotiation authority to the President after the 1934 Reciprocal Trade Agreements Act (RTAA), local protectionist interests had little influence over the content of the treaty (Bailey, Goldstein and Weingast, 1997; Hiscox, 1999). I fortify this with a differences-in-differences analysis that shows how this shock quickly caused widespread employment disruptions to commuter zones with otherwise

[8] Studies typically measure economic exposure through one's occupation by measuring respondent's education level, which is confounded by sociotropic values learned in college (Hainmueller and Hiscox, 2006). Also, to measure related exposure through one's industry, many papers use dummy variables for employment in manufacturing, which is arguably threatened by similar forms of endogeneity. For a review that summarizes these critiques, see (Kuo and Naoi, 2015).

parallel economic trends.

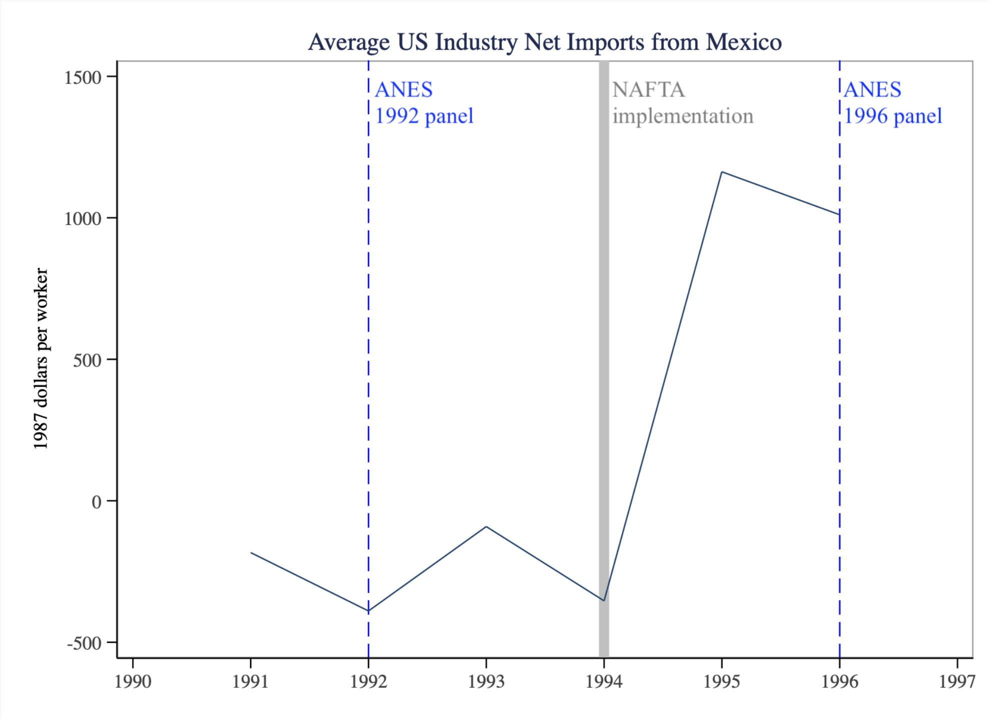

Figure 1: The trend line measures the average level of net imports to US industries from Mexico. The gray bar denotes the date NAFTA went into effect. The dashed blue lines denote the two ANES panel waves which tracked the attitudes and voting patterns of the same respondents across the 1992 and 1996 elections.

This 1994 "NAFTA shock" also helps to address endogeneity concerns at the individual level. As shown in Figure 1, the shock occurs between the timing of a panel survey started in 1992 and concluded in 1996 by the American National Elections Studies (ANES). This allows for the use of panel-data estimators that improve upon cross-sectional analyses used in prior research.[9] Specifically, fixed effects help to control for non-material correlates of geographic mobility that are specific to individuals while year fixed effects help to account for national changes in elite messaging.

Furthermore, this panel is especially relevant to the study of trade politics. Across both waves, the ANES tracked respondents' attitudes on trade policy as well as their voting

[9]For a review of this concern, see Kuo and Naoi (2015). Mutz (2018) provides a notable exception to this generalization.

patterns for the anti-trade populist Ross Perot, who made two historic runs for president. Perot's singular focus on trade policies helps to isolate voters' concerns over trade, which thus provides a clearer test of the theory than votes for similar candidates like Donald Trump.[10] Linking policy preferences to voting additionally helps to test concerns that trade's low salience limits its electoral importance (Rho and Tomz, 2017; Guisinger, 2009).

The results show a significant divergence in populist voting and trade policy preferences between those who were geographically mobile and those who were not. Those trapped in NAFTA-shocked regions became on average 30 percentage points more likely to vote for the Ross Perot. Meanwhile, those with the means to migrate, and those located in large cities, continued to support pro-trade candidates. I further show that these differential political reactions within trade-afflicted regions is driven by voters' preferences on trade policy in particular; and relatedly, by the differential effect of NAFTA on wages and employment. These trade effects operate through the hypothesized economic geography mechanisms rather than competing industry or occupational pathways. Crucially, because the attitudes and voting patterns of mobile and immobile voters move in opposite directions, analyses that ignore geographic mobility might falsely reject the economic effects of trade on attitudes and voting.[11]

Furthermore, the analyses finds little evidence in this context for the alternative thesis of cultural backlash. The close association of populist supporters with white voters (Baccini and Weymouth, 2021) who hold authoritarian and nationalist values (Ballard-Rosa et al., 2017; Colantone and Stanig, 2018*b*), and intensely oppose immigrants (Cerrato, Ferrara and Ruggieri, 2018), has led many to discount the independent role of international trade (e.g., Inglehart and Norris (2017); Mutz (2018)).[12] However, I show that exposure to NAFTA

[10]While Trump heavily campaigned on NAFTA, his campaign adopted a relatively diverse basket of non-trade issues that make Trump voting a noisy test.

[11]I.e., the average of negative and positive coefficients converges to zero.

[12]According to this increasingly popular narrative, anxiety over the declining status of historically dominant racial groups drives support for authoritarian populists who promise to reverse cultural change forcefully (Ballard-Rosa, Jensen and Scheve, 2022). If trade matters, it does so indirectly by triggering racial resentments (Baccini and Weymouth, 2021).

did not change how voters of any mobility level felt towards various racial or ethnic groups. I also show in a placebo outcome test that the NAFTA shock had no effect on how voters evaluated immigration policies, despite its obvious relation to political discourse over NAFTA and to US-Mexico relations more generally. These findings challenge an increasingly popular conclusion that globalization backlashes are driven primarily by identity politics (Inglehart and Norris, 2017; Mutz, 2018; Margalit, 2019). Rather, economic factors appear to exert an independent and substantively large effect.

This paper contributes to a large literature on economic self-interest that spans both international political economy and American politics. At least since Campbell et al. (1980)'s *The American Voter*, scholars have questioned, often with the same data presented here, the importance of material self interest in politics. Perhaps nowhere has this debate been more intense than in the area of trade politics. To suggest that economic exposure, particularly to globalization, drives trade policy preferences and support for anti-trade parties rubs against the last three decades of research on individual attitudes.[13] Yet more troubling for economic theories is the lack of empirical evidence for a link between trade policy preferences and voting.[14] By emphasizing voters' material connections to local economies, this paper contributes to a growing literature that explores the local sources of economic voting and democratic accountability (Ebeid and Rodden, 2006; Healy and Lenz, 2017; Larsen et al., 2019; de Benedictis-Kessner and Warshaw, 2020; Ansell et al., 2022).

This paper also advances our understanding of anti-globalization populism. While I focus on the case of NAFTA and Ross Perot for reasons of causal identification, the theory generalizes to recent globalization backlashes by populist parties in the US and in Europe. As a disruptive political outsider, Perot rode a historic wave of support based on his promise to protect the American people from harmful trade agreements imposed by a corrupt elite.

[13] For excellent reviews, see Hafner-Burton et al. (2017); Margalit (2019).

[14] This reflects the well-known "so-what" critique of trade preferences research (Kuo and Naoi, 2015)—a criticism substantiated by findings that average voters lack information about trade (Hainmueller and Hiscox, 2006; Rho and Tomz, 2017) and do not find it salient enough to affect their vote (Guisinger, 2009)

I argue that Trump, by adopting Perot's populist message on NAFTA, rode a similar wave of support two decades later. This time however, the wave was amplified by significant decreases in the geographic mobility of average Americans (Autor, 2020), and by the rise in Chinese imports which dwarfed that from NAFTA (Autor, Dorn and Hanson, 2013). In the conclusion, I give special consideration to how various electoral contexts, such as those since 1996 in the US, and those under proportional representation rules in Europe, likely amplify these effects of economic geography.

2 Missing Spatial Elements in Attitudes and Voting

Theories of trade in political science, particularly in the Open Economy Politics (OEP) tradition (Lake, 2009), have long made the analytical bet that economic models provide useful predictions of political interests.[15] Most predictions rest on competing assumptions over the extent of actors' mobility across *sectors*.[16] In Stolper-Samuelson's world of costless intersectoral mobility, interests unify around broad factors of production—i.e., owners of land, labor, and capital can form distinct policy positions (Rogowski, 1989). However, in Ricardo-Viner's world of little to no intersectoral mobility, these broad coalitions break down to the industry level, with exporting and import-competing industries fighting over policy (Gourevitch, 1986; Frieden, 1991; Hiscox, 2002).[17]

Oddly, theorists have seldom considered *geographic* mobility—whether factors can move physically from one region (i.e., local labor market) to another within a country. Yet, geographic mobility may matter as much as, or more than, intersectoral mobility given significant evidence of agglomeration externalities to trade (e.g. Hakobyan and McLaren (2016); Autor, Dorn and Hanson (2013)).

[15] That is, the OEP tradition uses economic models to predict how international trade should affect individuals' private welfare, which in turn predicts support for, or opposition to, trade policies.

[16] Specifically, the ease with which workers and capital can be redeployed from one industry to another.

[17] More recent OEP theories consider the extent of factor mobility between occupations (Owen and Johnston, 2017) and between firms within the same industry (Kim, 2017; Kim and Osgood, 2019).

Economic models of trade describe agglomeration externalities as productivity spillovers between a traded activity and its neighbors within the region.[18] In general, proximity increases the productivity of traded goods/services, thus allowing them to become engines of growth for local economies (Moretti, 2012). Agglomeration unlocks this productivity potential by: reducing employer-employee search costs (Marshall and Marshall, 1920; Berry and Glaeser, 2005; Kline and Moretti, 2013); facilitating the exchange of knowledge and technology (Marshall and Marshall, 1920; Glaeser et al., 1992); and, reducing supply-chain costs (Marshall and Marshall, 1920; Ellison, Glaeser and Kerr, 2010; Acemoglu et al., 2016). Consequently, expanding growth in traded activities attracts non-traded activities—e.g., barbershops and restaurants—which further reinforces the traded sector's productivity by reducing transportation costs between producers and consumers (Krugman, 1993).[19] These dependencies make various industries and activities indirect benefactors to the local production of traded goods/services. Furthermore, the enhanced productivity of traded activities benefits local property owners by appreciating the value of their assets (Scheve and Slaughter, 2001); and, because many local governments are funded through property taxes, consumers of public services like education, fire and police protection, parks and recreation, infrastructure, and public housing also benefit (Feler and Senses, 2017). The critical point of these externalities is that traditionally considered losers (low skilled or import-competing workers) can leap to higher wages by migrating to global cities where export agglomerations generate widespread growth.

Individual-level studies that use the "China shock"[20] have largely missed the importance of these externalities, let alone geographic mobility. The effects of regional shocks

[18]Theorists will recognize agglomeration externalities as a significant violation of the constant returns to scale assumption found in our traditional OEP models. These externalities are a form of increasing returns, or sometimes called external economies of scale—e.g., a doubling of inputs within an agglomerated region more than doubles outputs.

[19]The logic is circular: non-traded services depend on demand from the local traded activities, and the local traded activities come to depend on demand from proximity to a large market of non-traded services.

[20]I.e., the instrumental variable that captures US regional exposure to Chinese import competition (Autor, Dorn and Hanson, 2013).

on individual attitudes and voting have often been attributed to sociotropism (Dippel, Gold and Heblich, 2015; Colantone and Stanig, 2018*a*,*b*). That is, voters are averse to the effects of trade on others, regardless of their own pocketbook circumstances. This has led to conclusions that populist voting among public and service sector workers is irrational when, in fact, this is perfectly consistent with economic exposure through local externalities. This interpretation is not surprising given that current OEP theories currently restrict the notion of one's pocketbook to educational skills and industry of employment.[21] Rarely have we considered that the pocketbook effects of trade could spillover from one individual to her neighbors.

Bisbee (2018); Broz, Frieden and Weymouth (2021) provide important exceptions to this generalization. They recognize that the results the China shock are not just sociotropic, but also reflect pocketbook spillovers across individuals within a region.[22] The next section builds on this important insight with a theory that situates the notion of spillovers into the broader phenomenon of agglomeration and geographic mobility. Doing so reveals that different spillovers work in opposite ways, depending on local agglomeration patterns, and that their economic and political consequences depend crucially on the extent of migration between regions. Furthermore, by reconciling the OEP framework with economic geography, the paper encourages further theoretical and empirical work on how individuals form economic relationships to, and between, places.

[21] While most work focuses on the employment effects of trade, a third branch of OEP considers consumption effects (Baker, 2005).

[22] Writing well before the China shock papers, Scheve and Slaughter (2001) find evidence of local spillovers that operate through housing markets.

3 An Economic Geography Theory of Trade's Distributional Politics

Compared to the standard models that emphasize class conflict or industry competition, the introduction of geographic mobility and agglomeration externalities produces spatial political alignments. In short, when globalization concentrates growth (stagnation) within certain regions, and that prosperity (hardship) spills over to all residents of that region, interregional inequality emerges—what Moretti (2012) calls the "Great Divergence."[23] This divergence benefits geographically mobile factors of production (e.g., owners of labor or capital) who can arbitrage the spatial differences caused by international trade. In contrast, immobile factors anchored to afflicted regions go down with their ship. I discuss at the conclusion how this power of "place" provides lessons for the study of other policy issues that produce regional inequalities.

Imagine a very simple picture of two regions and, within each region, two factors—call them human capital and labor; or, equivalently, the skilled and the unskilled. If we assume a wealthy economy like the US, it will be abundant in skill but poorly endowed in low-skill labor relative to its trading partners. If we apply the standard Stolper-Samuelson assumptions, then expanding trade will benefit the skilled but harm the unskilled, *regardless of their location*.[24]

Now suppose, however, that skill-intensive goods (e.g. software), are produced most efficiently in one region—call this region the core—while low-skill-intensive goods (e.g. textiles) are produced most efficiently in the other second region—call it the periphery. These

[23] Of course, widening disparities between regions also arise from skill-biased technological change. However, international trade is considered a major driver of both regional inequality (Autor, Dorn and Hanson, 2013) and technological change (Bourguignon, 2015, 81).

[24] This follows standard Heckscher-Ohlin logic: countries abundant in unskilled labor, like China, will produce labor-intensive goods like toys more efficiently (i.e. at lower marginal cost) than countries like the US that have relatively few—and thus more expensive—workers. Furthermore, the distributional effects to factors are independent of location because the standard constant returns to scale assumption implies zero benefits to location (Fujita, Krugman and Venables, 2001).

regional differences in productivity reflect the agglomeration externalities described above.[25] Assume for now, as an extreme case, that neither the skilled nor the unskilled can move between these two regions.

Within the core region, positive externalities to trade dominate. If sufficiently strong, these indirect effects of trade may offset the direct losses faced by low-skill workers in the core. For example, every software engineer hired in a "core" region like Silicon Valley triggers the growth of five additional jobs outside that sector in the same region—taxi drivers, housekeepers, baristas and waiters—all eager to capture a slice of the core's growing pie (Moretti, 2012, p.13).[26] High-skill workers located in the core doubly win: trade increases the local and global demand for their talents (Rogowski, 1989), and agglomeration allows them to satisfy this demand with much greater efficiency—and thus higher wages—than had they employed their same talents in the periphery.

Observers of the China Shock literature will recall the *negative* agglomeration externalities featured within the periphery (Autor, Dorn and Hanson, 2013). Here, the same spillovers that profitably link low-skill workers in the core to high-skill exports flow in the opposite direction (Moretti, 2012). When import shocks hit the backbone of a local economy, the hardship depresses demand for the goods and services provided locally by skilled workers: legal counseling, web design, management, and medical exams. Trade shocks also reduce the value of residents' homes and starve the local government of revenue and thus the public goods on which residents depend. Again, if we assume no interregional mobility, even the skilled workers lose from trade, and the larger these agglomeration externalities, the more the indirect effects of trade may swamp the direct effects. Autor, Dorn and Hanson (2013) find that the negative local employment effects of Chinese manufacturing imports

[25] This setup (loosely) takes inspiration from the core-periphery model in Krugman (1993)'s *Geography and Trade* where a combination of agglomeration forces and factor mobility dynamically produces asymmetric geographic concentration. To focus on static predictions, I exogenously fix this asymmetry from the outset.

[26] John Deere, a manufacturer of farming equipment, provides a useful firm-level example. Despite import-competition, it profitably makes heavy tractor equipment via its linkage to booming US agricultural exports. It does this in large part by locating in Waterloo, Iowa—the heart of America's soybean and wheat exports.

applied to workers at all education levels, and to workers in non-manufacturing industries. For every manufacturing job lost in an afflicted region, 1.6 jobs are eventually lost outside of that sector (Moretti, 2012, p. 24). As for the periphery's low-skill workers, they doubly lose: in addition to the above negative externalities, trade exposes them to direct competition with low-skill abundant countries.

If this picture were right, the political consequences would be obvious: in the core where positive externalities to trade dominate, both the skilled and the unskilled would support the expansion of trade, with the skilled perhaps being markedly more favorable. In the globally uncompetitive periphery, both the skilled and the unskilled would oppose trade. This reasoning is consistent with the enormous interregional income inequality found *within* skill groups. Both janitors and lawyers earn significantly higher incomes (net of housing costs) in the tri-state New York area (NY, NJ, CT) compared to their colleagues in the Deep South, a pattern which generalizes across time and skill groups Ganong and Shoag (2017).[27]

Suppose now that we make the picture more realistic by admitting that *some* mobility between regions is possible. In the aggregate economy, this would begin to arbitrage-away the core-periphery inequalities created, in part, by agglomeration (Moretti, 2012): low- and high-skill workers seeking to adjust to local trade shocks would leave the periphery (Hakobyan and McLaren, 2016; Greenland, Lopresti and McHenry, 2019), thereby relieving downward wage and unemployment pressures there, but also competing-away gains found in the core. Increasingly higher levels of *aggregate* geographic mobility help to equalize factor prices across regions, thus producing a world closer to that postulated by Stolper-Samuelson.[28] In reality,

[27] Note that "centrifugal" forces—e.g. congestion, unaffordable housing, and regulations—counteract the *real* expected gains to agglomeration's "centripedal" forces (Krugman, 1993). As early as 2010, rising real estate prices in "core" regions like San Fransisco have significantly eroded, although not fully erased, the gains low-skill labor receives from living there (Autor, 2020). By making core cities more attractive, but also more expensive, trade can reduce the expected real incomes of labor. This suggests a rising class conflict (labor versus capital and land owners) over trade within the core when the centripetal effects are greater than the centrifugal effects. However, the analysis focuses on the effects of NAFTA in the 1990s, well before the falling urban wage premiums to labor in the 2010s.

[28] However, the presence of large agglomeration effects can still prevent full convergence of unemployment

significant barriers to migration prevent or slow much convergence in occupational wages and unemployment between regions (Blanchard et al., 1992; Moretti, 2012; Ganong and Shoag, 2017; Autor, 2021); and, crucially, many of these barriers reflect *individual* heterogeneity.

The ability, and the desire, to migrate between regions reflects a complex combination of individual-specific characteristics—the most important ones being homeownership, education, age, marriage, the number and age of children, and a history of immobility (Bonin et al., 2008; Winkler et al., 2010; Molloy, Smith and Wozniak, 2011; Malamud and Wozniak, 2012)—as well as region-specific amenities: temperate climates, good schools, characteristics of local housing markets, and local unemployment rates (Gabriel, Shack-Marquez and Wascher, 1993; Davies, Greenwood and Li, 2001).[29] Geographic mobility is therefore best characterized as a continuous variable whose variation is specific to individuals' features and the features of regions.

When internal migration does occur, particularly in response to local demand shocks, research, unsurprisingly, confirms the intuition that workers relocate from declining to more prosperous regions (Bartik, 1991; Blanchard et al., 1992; Black, McKinnish and Sanders, 2005; Foote, Grosz and Stevens, 2019), and that this is driven largely by the desire to increase expected income (Borjas, Bronars and Trejo, 1992; Kennan and Walker, 2011). Across the 2000-2018 waves of the Annual Social and Economic Survey, employment is by far the most frequently reported reason—seventy five percent of each sample—for moving across state lines (Jia et al., 2022).[30] Internal migration thus acts as a type of human capital investment, where individuals can, as with education, increase their earnings by moving to high-growth agglomerated cities (Molloy, Smith and Wozniak, 2011).[31] Therefore, this vari-

between regions, even if we assume perfect mobility (Kline and Moretti, 2013).

[29] Migration is also pro-cyclical, especially among younger workers—that is, relocations rise under good times and fall under bad times (Fujita, Ramey et al., 2006; Saks and Wozniak, 2011; Johnson, Curtis and Egan-Robertson, 2017).

[30] This is all distinct from "residential" mobility *within* regions (e.g. from inner city to suburbs) which does not change one's exposure to region-specific trade externalities (Bell et al., 2002).

[31] Adjustment through geographic mobility is likely more important that adjustment through re-education (i.e., sectoral mobility). The latter describes re-educating for jobs that benefit from trade. This form of adjustment is less important empirically. For many, acquiring the education needed to work in high-tech is

ation in geographic mobility will not matter to residents of the prospering core, since with rare exceptions[32] they will have little incentive to move to the trade-shocked periphery.

Within the periphery, by contrast, variation in mobility will matter enormously. Consider again a sudden trade shock that simultaneously creates widespread opportunities in the core and widespread decline in the periphery. Just as in our model of perfect immobility, periphery workers with prohibitive migration costs—extremely low mobility—must absorb the full magnitude of trade's negative externalities. In other words, extreme relocation costs preclude any hope of exiting local decline; or equivalently, preclude any gainful arbitrage between regions. Local decline should therefore increase their support for trade barriers and anti-trade politicians.

On the other extreme, consider periphery workers with near perfect mobility. Recalling that the positive spillovers to trade are locked away in the core, highly mobile voters in the periphery can reasonably expect to unlock these gains by their ability to move there, even after adjusting for their expected relocation costs. In the language of migration decision models, mobile workers benefit if trade makes the expected gains to moving sufficiently higher than the expected relocation costs (Sjaastad, 1962; Bonin et al., 2008). I provide numerical examples in Appendix A of the absolute and relative benefits mobile voters receive within adversely affected regions. More intuitively, when a trade shock removes any doubt that a local community will endure long-run decline, those who can easily pick up everything and move to greener pastures have relatively little to fear compared to voters who know that they are stuck no-matter how bad it gets. Sufficiently mobile voters should therefore see relatively little appeal to populist calls to reverse trade integration since they

likely more costly than moving in terms of years of education, opportunity costs, and student debt, especially for older individuals. Furthermore, sectoral mobility alone makes little sense in the context of agglomeration externalities since the "jobs of the future" concentrate in very different regions than the "jobs of the past." Specifically, if all local sectors are in steep decline due to spillovers, the returns to investing in new skills are in fact lower than simply moving to where your current skills earn more (Autor, 2020).

[32] To keep the story simple, I ignore general equilibrium effects of in-migration within the core. Inflows of workers will eventually reduce real wages there, as appears to be the case within the largest US metros after the Great Recession.

can expect to capitalize on the rising returns to their skills found in the core. The logic is akin to Hirschman (1970) classic story of "exit and voice." High mobility voters know that they can vote with their feet (i.e., exit the region) while the low mobility voters face strong incentives to use their political voice to reverse local decline.

This political logic rests on what voters rationally *expect* to gain from trade given their mobility; however, mobility can also confer *present* benefits through wage bargaining.[33] Periphery voters whose high mobility allows them to threaten to move their skills to the core can pressure current employers to make their wages today more nationally competitive.[34] Either through expected or present payoffs, voters with very high geographic mobility derive net benefits when trade creates sufficiently large regional wage inequality.

Most voters will fall somewhere between mobility extremes. Within this spectrum of mobility, as periphery voters' barriers to moving fall—i.e. as they become more footloose—the less they can expect to suffer from local decline before it makes sense to move to the core where gains have become plentiful.[35] Mobility, in other words, determines periphery voters' level of exposure to trade's negative externalities, and thus their level of opposition to trade.

To the extent that this sketch is correct, we can say that, as geographic mobility declines, "place" should increasingly matter more than factor ownership or intersectoral mobility. In a stricken region, even owners of the nationally abundant factor can embrace populist calls for protection while highly mobile owners of the scarce factor can embrace free trade; in one prospered by trade, even owners of the nationally scarce factor can support freer trade.[36] Figure 2 summarizes these propositions.

[33]While the expected and present mechanisms compliment one another, the theory focuses more on the former for the analytical simplicity of avoiding employer-employee bargaining dynamics.

[34]The literature on capital mobility provides much evidence for this. Capital owners (i.e. firms) can successfully bargain for policy concessions *today* from local governments when they can threaten to relocate *tomorrow* to other regions (or countries) that offer looser regulations or larger subsidies, thus resulting in a regulatory "race to the bottom" (e.g. Przeworski and Wallerstein (1988); Mosley (2000)). The analysis here validates this intuition with individual wage and employment data.

[35]Said differently, as migration costs approach zero, so too do the differences in expected utility between mobile voters in the periphery and voters in the core.

[36]The essential intuition applies if we instead assume a specific-factors (Ricardo-Viner) model where we

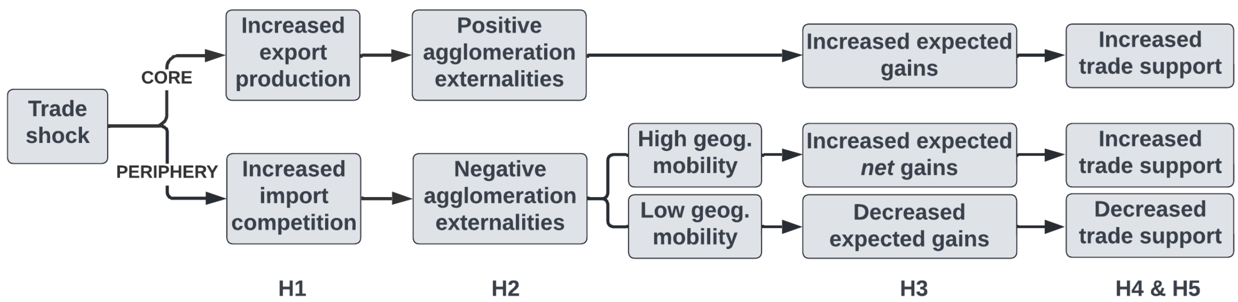

Figure 2: Economic Geography Theory of Trade Politics

The primary hypothesis is that trade shocks increase political support for trade among voters in the core, but decrease support among geographically immobile voters in the periphery; however, as a periphery voter's mobility rises, their opposition to trade diminishes such that, at very high levels of mobility, shocks can increase support for trade (H4). An auxiliary hypothesis to H4 (call it H4.1) is that, geographically mobile voters are, on average, winners of trade liberalization and should thus report higher *average* levels of support for trade, regardless of local conditions.[37] Furthermore, these effects on trade policy preferences should correspond to votes for anti-trade parties, with the most fervent support coming from the immobile voters in the periphery (H5).

These political effects should follow the hypothesized economic geography mechanisms. First, trade liberalization increases export production in the core, and increases competition for import-competing goods in the periphery (H1).[38] Second, this simultaneously increases positive agglomeration externalities in the core and negative agglomeration externalities in the periphery (H2). For the final step in the mechanism, negative externalities decrease the expected gains for immobile voters—an effect which diminishes as moving costs fall; for very high levels of mobility, externalities may increase expected gains, net of

consider industries rather than factor owners.

[37]The numerical examples in Appendix A make this clear: highly mobile voters receive net benefits in both the core and the periphery while a significant number of immobiles—those in the periphery—lose. Thus, the average level of trade support should be higher among mobiles, all else equal.

[38]In this static setup, regions that constitute the core specialize in the productions of goods and services in which the country enjoys a comparative advantage. Periphery regions specialize in the production of comparatively disadvantaged goods. In reality, industry locations shift dynamically over time (Krugman, 1993).

expected moving costs, if the benefits to moving become sufficiently higher than the relocation costs (H3). The analysis shows that the evidence in favor of H4 and H5 operates through these mechanisms versus competing alternatives.

4 Research Design

4.1 Populist Perot and the "Giant Sucking Sound"

During the second presidential debate held on October 15 1992, third party candidate Ross Perot famously described in his opening statement a "giant sucking sound" of American jobs being lost to Mexico (Times, 1992). I test this theory in the context of Perot's historically popular (but ultimately unsuccessful) runs for president in both the 1992 and 1996 elections. As the independent candidate in the 1992 election, and as the Reform Party candidate in 1996, Perot commanded 19 and 9 percent of the popular vote, respectively. This made him the most disruptive third party candidate in modern US history.[39] Perot is frequently described by his status as a billionaire businessman, a political outsider, and by his "East Texas populism"[40]. While these characteristics distinguished him in a field of establishment candidates, his popular success is just as importantly attributed to the singular focus of his campaign on economic nationalism—particularly the vilification of free-trade policies.

Using historically popular presidential debates, innovative infomercials[41], and frequent celebrity talk show appearances, Perot focused on issues of wages, unemployment, and the national debt, and saw in all of these a common cause that he proposed to resolve:

[39] The previously success story was Theodore Roosevelt's Progressive Party in 1912 which earned 27 percent of the vote.

[40] See Dunham, Richard S.; Douglas Harbrecht (April 6, 1992). "Is Perot after the Presidency, or the President?". Bloomberg Businessweek. Bloomberg"

[41] Perot released several 30 to 60 minute infomercials which attracted a very "respectable" 16.5 million viewers on CSPAN https://www.nytimes.com/1992/10/30/us/1992-campaign-independent-campaign-stalled-perot-team-tries-fix-it-fast.html?pagewanted=1. These featured him delivering power point style figures and data on the economy.

job-killing trade deals and the corrupt elites who signed them. A frequent point of emphasis was his view that "people leave the white house staff, become federal lobbyists, have access to the white house, and have enormous influence on trade negotiations."[42] His persistent calls to "re-industrialize America" and restore "Made in the U.S.A." echo to this day with Donald Trump. I discuss in the conclusion the extent to which we can infer lessons from Perot to more recent populist parties.

The laser focus of Perot's campaign on trade provides ideal conditions for evaluating the consequences of trade on populist voting and trade preferences. In terms of candidates for executive office under majoritarian electoral rules, his campaign is as close to a direct ballot proposition on trade as one gets in the American context. Relative to a Democrat or Republican, who each represent a large basket of social and economic issues, a vote for Perot can be more readily interpreted as a vote against trade openness—a proposition I defend with numerous robustness checks.

The Perot case also provides two benefits for causal identification. First, a public opinion survey carried out by the American National Elections Studies (ANES) tracked the policy attitudes and voting behaviors of a representative sample of Americans between the 1992 and 1996 elections during which Perot ran. The result is a rare panel of individuals that offers all of the identification benefits of panel-data estimators. This is an important contribution to the trade preferences literature where the lack of panel has have forced researchers to rely on lab or online experiments to achieve causally identified results (Kuo and Naoi, 2015).

Second, these elections and survey samples overlap with a rapid increase in net import competition from Mexico on January 1st 1994. On that date, the North American Free Trade Agreement (NAFTA) took effect, dropping a majority of US tariffs on imported Mexican goods immediately to zero. Figure 1 captures this fortuitous combination of a

[42] See (https://www.c-span.org/video/?34277-1/perot-campaign-commercial-1992.

sudden and rapid increase in the US trade deficit, the Perot elections, and a conveniently-timed panel dataset on American public opinion.

Within the ANES panel, I construct two key dependent variables: (1) an indicator equal to one if a respondent voted for the anti-trade populist Ross Perot, and (2) an indicator for supporting higher tariff policies. Since Perot represents a third party candidate, I operationalize voting with two alternative reference groups. The first compares votes for Perot versus the two establishment candidates. The second compares votes for Perot versus abstentions. This distinction reflects two alternative voting processes. The former invokes a more strategic calculus where voters must balance their choice between (a) which candidate lies closest to their ideal point, and (b) which candidate actually has a chance of winning in a two-party system. That is, Duverger's Law weighs heavily here (Riker, 1982). The second construction however offsets these strategic considerations, at least to a degree, by comparing votes for Perot to people who decided not to make a strategic choice at all—that is, a measure of who decided to show up to the polls for Perot versus stay at home.

My second key dependent variable measures individual preferences on tariff policy. The ANES asks "some people have suggested placing new limits on foreign imports in order to protect American jobs. Others say that such limits would raise consumer prices and hurt American exports. Do you FAVOR or OPPOSE placing new limits on imports, or haven't you thought much about this?" Consistent with prior literature, I adopt the binary coding for FAVOR or OPPOSE (Scheve and Slaughter (2001); Blonigen (2011); Blonigen and McGrew (2014); Ferrara (2022); Baccini and Weymouth (2021)).

4.2 The 1994-95 NAFTA Shock

I create a new dataset of annual US *net* imports from Mexico at the commuter zone level. Following the now standard procedure used in Autor, Dorn and Hanson (2013), I aggregate annual trade in manufacturing goods from the UN Comtrade Database (at the HS-6 product code level) to 3-digit Census industries.[43] I use these to measure industry j's change in *net*[44] imports per worker[45] from Mexico at time t:

$$NetImports_{j,t} = \Delta ln\left(\frac{Imports_{j,t}}{L_{j,t-1}}\right) - \Delta ln\left(\frac{Exports_{j,t}}{L_{j,t-1}}\right). \tag{1}$$

Specifically, $L_{j,t-1}$ is the one year lag of total employment in industry j while $Imports_{j,t}$ and $Exports_{j,t}$ are realized US imports and exports from Mexico in industry j in year t, adjusted to 1987 dollars. I take the one year difference to capture for the change in trade flows before and after NAFTA. A logarithmic transformation accounts for extreme skewness in trade values and industry employment.

Figure 1 plots this variable's average trend in dollar units to show the magnitude of the shift caused by the January 1 1994 drop in US tariffs on Mexican net imports. Within one year of this date, the US goes from a stable net exporter to Mexico to a large net importer at the magnitude of a little over 1000 dollars per worker, adjusted to 1987 values. This equates to an approximate 0.5 standard deviation increase in net imports compared to 1992 levels, as shown in Appendix Figure 12. Note that this rapid increase in exposure is unique to the 1994-1995 change, which occurred just prior to the 1996 presidential election.

$$NAFTA_{r,t} = \sum_j \frac{L_{r,j,t-1}}{L_{r,t-1}} NetImports_{j,t}. \tag{2}$$

Also following standard measurement practices, the $NAFTA_{r,t}$ shock in equation 4 attributes this industry shock to the level of commuting zones r[46] (CZONE) according to

[43] Unfortunately, the lack of data for trade in services and agricultural employment does not allow this analysis directly test hypotheses related to "core" regions that benefit from free trade.

[44] The importance of measuring net imports rather than the more standard unadjusted imports is shown in Appendix Figure 14, which shows that in the pre-NAFTA period, exports to Mexico were very high and tracked lock-step with imports. In contrast to trade with China, this means that exposure to trade with Mexico would be biased if we did not account for the fact that the average manufacturing industry saw large export gains to trade.

[45] Specifically, the one-year lag of industry j employment, $L_{j,t-1}$, uses Eckert et al. (2020)'s 1990-1996 imputed employment data from the US Census Bureau's County Business Patterns dataset.

[46] Commuting zones offer the ideal level of geographic aggregation to study economic geography because

r's lagged share of total employment in that industry ($L_{r,j,t-1}/L_{r,t-1}$). See Appendix B for the full description of the data collection and measurement procedure. Figure 3A maps the spatial distribution of shocked industries. This distribution is clearly non-random, with the most exposed regions concentrated in the manufacturing rust belt and sun belt, as well as manufacturing hubs in the Pacific Northwest. Figure 3B highlights the importance of conditioning on initial manufacturing specialization,[47] which results in a more random distribution of exposure to the 1994-1995 jump in import competition from Mexico. I show evidence below that the regions with heavy exposure to the 1994 shock had pre-treatment employment trends that were indistinguishable from those of non-shocked regions (i.e. parallel trends).

4.3 Measuring individual geographic mobility

Measuring an individual's empirical mobility across commuting zones presents a significant challenge: public opinion surveys generally lack direct or quality measures; and recalling from the theory, an individual's propensity to migrate reflects a complex combination of location-specific benefits and individual-specific costs to migration. Fortunately, the challenge of measuring complex latent variables is a key strength of machine learning.

I measure $\hat{MOBILE}_{i,t}$ as individual i's probability in year t of having migrated from a different CZONE at anytime between t and $t-5$.[48] These probabilities come from the output of a Random Forest classifier[49] which is trained on 9.7 million cases of observed internal migration from US Census microdata. Specifically, the measure uses 2000 Census

they were designed to map most clearly onto the concept of a local labor market where economic activity is spatially bounded by locations of work and locations of residence (Tolbert and Sizer, 1996). Also, unlike metropolitan statistical areas, commuter zones cover the entire landmass of the US.

[47] Conditional exposure is produced by taking the residuals of a regression of CZONE net exposure on lagged manufacturing specialization. All subsequent analyses account for this by controlling for lagged manufacturing specialization.

[48] Migration *between* CZONEs allows this measure to capture mobility due to labor market adjustment rather than *residential* housing adjustment—e.g., moving to a more suitable home down the street (Bell et al., 2002).

[49] This algorithm is known to perform well in binary classification problems like the one here (Wright, Wager and Probst, 2016).

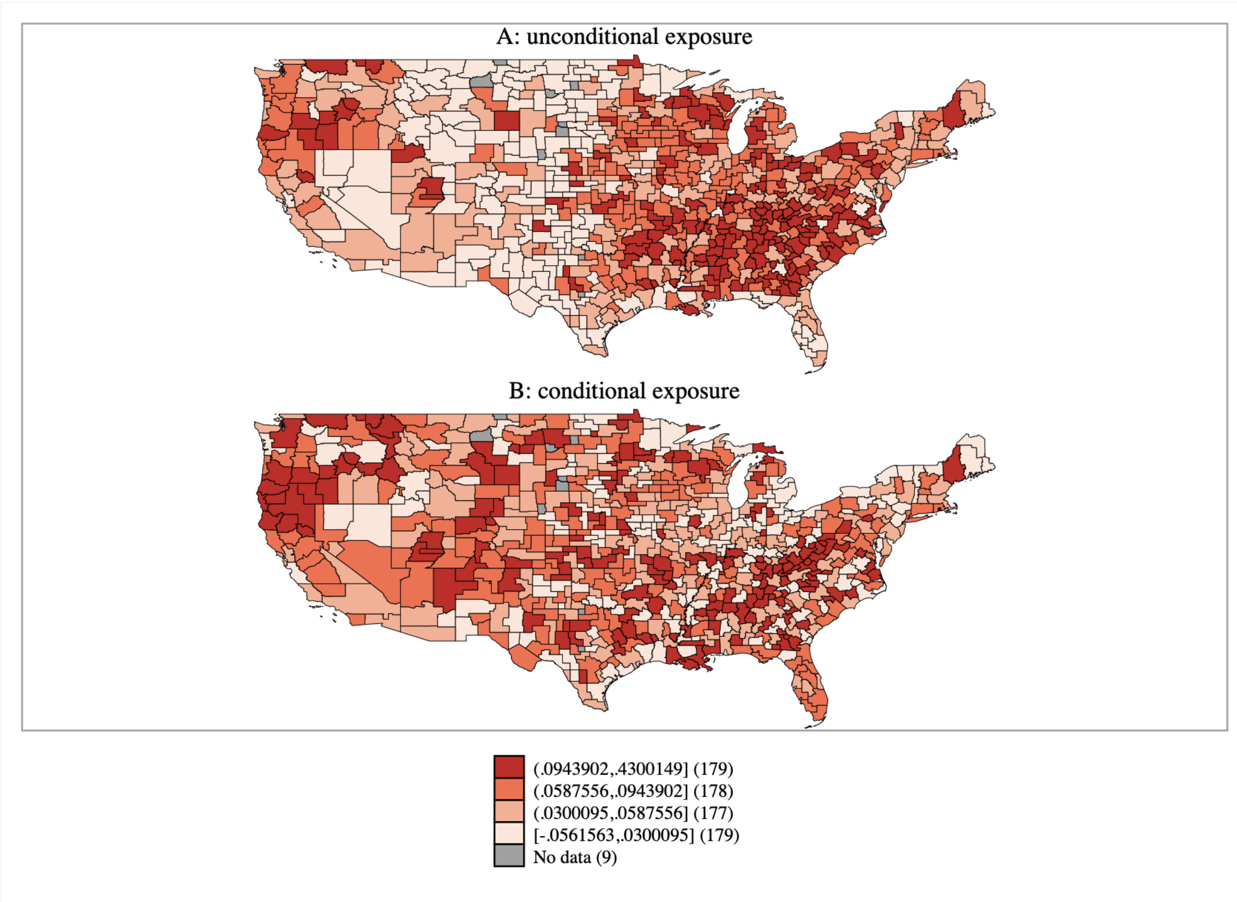

Figure 3: The spatial distribution of the NAFTA shock across 1990 Commuter Zones. Panel A maps the unconditional 4-year change in net imports from 1992 to 1996. Panel B plots the conditional exposure by mapping the residuals from a regression of CZONE net exposure on lagged manufacturing specialization.

data from the Integrated Public Use Microdata Series (IPUMS) database, which identifies each respondent's location at the time of the survey, as well as their prior location if they relocated in the prior five years.

This method incorporates the costs and benefits of migration in a much more flexible, and realistic, way than is theoretically possible in a simple cost-benefit framework. It does this by modeling real moves as a function of very high-order interactions between all migration-cost variables[50] and migration-benefit variables[51] found in the Census and ANES. This allows for the possibility that white renters at age 25, with 10 years of schooling, making between 15-20K a year, and employed in a high-growth region within California (to give a purely hypothetical example), can itself be a *single* classification category that is distinct from all other possible combinations.[52] The results is an individual's propensity to migrate based on a battery of underlying characteristics that are highly predictive of actual moves. The full procedure is documented in Appendix C.

I emphasize three advantages to measuring the *ability* to migrate as a function of underlying mobility factors, as opposed to observed migration. First, this paper's theory, as well as the large literature on migration models, deals with migration incentives, not migration itself, as the concept of theoretical interest (e.g., Bonin et al. (2008)). As the numerical intuition in Appendix A shows, it matters more whether voters know they can or cannot move rather than whether they actually end up moving. Second, changes in the incentives/propensity to migrate are observable for both those who actually move and those who do not, whereas actual moves suffer from greater self-selection effects. Third, the probability of moving, compared to a binary moved or not moved, yields rich continuous variation.

Two key assumptions uphold this supervised learning method. First, the random sample and joint distribution assumptions are largely satisfied by the nationally representative sampling procedures of the US Census Bureau and ANES.[53] Still, we might expect this

[50]These include the following individual socioeconomic variables: sex, age, marital status, 3-digit Census 1990 industry codes of the respondent and their partner, 2-digit Census 1980 occupation codes of the respondent and their partner, education level of the respondent and their partner, family total income, race, Hispanic, number of years lived in current dwelling, employment status of the respondent and their partner, and ownership status of dwelling.

[51]These include the following geographic indicators and variables: state of birth, current Census MIGPUMA (Migration Public Use Microdata Area), current Census region, and commuter zone industry specialization (Hirshman-Herfindahl Index). I drop other employment characteristics of regions because they worsen model performance due to the fact this information is already absorbed by the regional dummies.

[52]In other words, by the Universal Approximation Theorem, machine learners like Random Forests can accurately model (approximate) the complex functional form of geographic mobility whereas traditional regression techniques would quickly encounter the curse of dimensionality.

[53]Specifically, the joint distribution is assumed to be same in the training, validation, and unlabeled ANES

assumption to fail if the determinants of mobility in 1996 were different from those in 2000—i.e., "population drift" (Hopkins and King, 2010). I show evidence below that this measure is strongly predictive of alternative, although inferior, measures from 1992 and 1996. Second, the variables chosen to train the algorithm are sufficient to explain the outcome—i.e., geographic mobility. With the variables described above, the out-of-sample accuracy approaches 85 percent, compared to a coin flip—respectable for social science variables.[54] Still, the data lack information on certain variables that we would expect to predict mobility.[55]

Figure 4 plots the distributions of $MO\hat{B}ILE_{i,t}$ for the two ANES waves. As expected, most respondents have a low probability of changing CZONEs in either year. Additionally, the probability distribution shifts slightly toward zero by 1996, an approximate 4 point drop in average mobility which is consistent with the steady decline in rates of interstate migration over this period (Autor, 2021).

The individual-level correlates of $MO\hat{B}ILE_{i,t}$ in Figure 5 are strongly consistent with prior analyses of large scale surveys in the US and Europe, regardless of decade (Bonin et al., 2008; Molloy, Smith and Wozniak, 2011; Jia et al., 2022).[56] Among individual-level characteristics, age and homeownership have always—regardless of country, region, or time period—been strongly associated with very low geographic mobility. Here, owning a home and being 17 years older than the mean age is associated with 0.20 and 0.15 point decreases in the probability of migration, respectively. Income, gender, and race/ethnicity are unrelated to mobility with the exception of black individuals who, compared to those who are white, are marginally less mobile (0.04). Features that increase respondents' migration probabilities include being widowed compared to single (0.17), leaving the labor force compared to being

set.

[54]Additionally, the sensitivity equals 89 percent while specificity reaches 70 percent.

[55]First, we cannot directly observe respondents' social/familial networks. Second, the ANES only reports one's current region, meaning that we cannot incorporate information about regions of prior residence that are found in the Census. Fortunately, exclusion of this information has a negligible impact on classification accuracy.

[56]The coefficients come from a CZONE-year fixed effects regression of predicted migration probabilities with confidence intervals clustered at the state level.

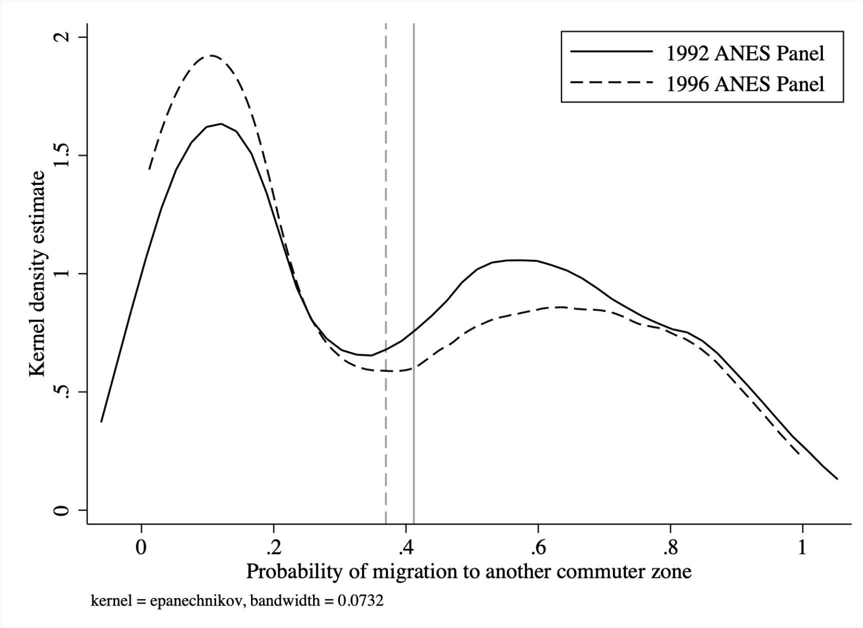

Figure 4: The distribution of predicted mobility for ANES 1992 and 1996 panel respondents. The solid (dashed) vertical line denotes the 1992 (1996) sample mean.

employed (0.10), and holding a bachelors degree (0.05). Also consistent with prior studies, a one standard deviation higher annual growth in employment is associated with a marginally higher probability (0.05) of having migrated in the last five years. On a more novel finding, living in a region with a highly specialized economy—e.g., factory towns where most workers concentrate in just a few industries—makes respondents very unlikely to move (a decline of 0.22), although this relationship dissipates, even reversing, at extreme levels of specialization. Finally, respondents who lean Republican are only slightly more mobile than moderates (0.02), which is consistent with small levels of partisan sorting (Abrams and Fiorina, 2012).

Several construct validity tests fortify confidence in this measure. A strong test would show that these migration probabilities, which rely on Census 2000 data, predict actual moves within the ANES sample between 1992 and 1996.[57] One reason we might expect this to fail is population drift, discussed above. Appendix Table 4 alleviates this

[57]While fine for validity tests, these alternative ANES measures lack several key qualities: first, they do not identify moves at the CZONE level, which therefore confound them with theoretically irrelevant, yet numerically dominant, residential migration (Bell et al., 2002); second, the attrition-based measures are only identified for one, but never both, ANES waves, which would prevent the use of panel-data-estimators; third, all ANES alternatives are necessarily binary, which masks important heterogeneity.

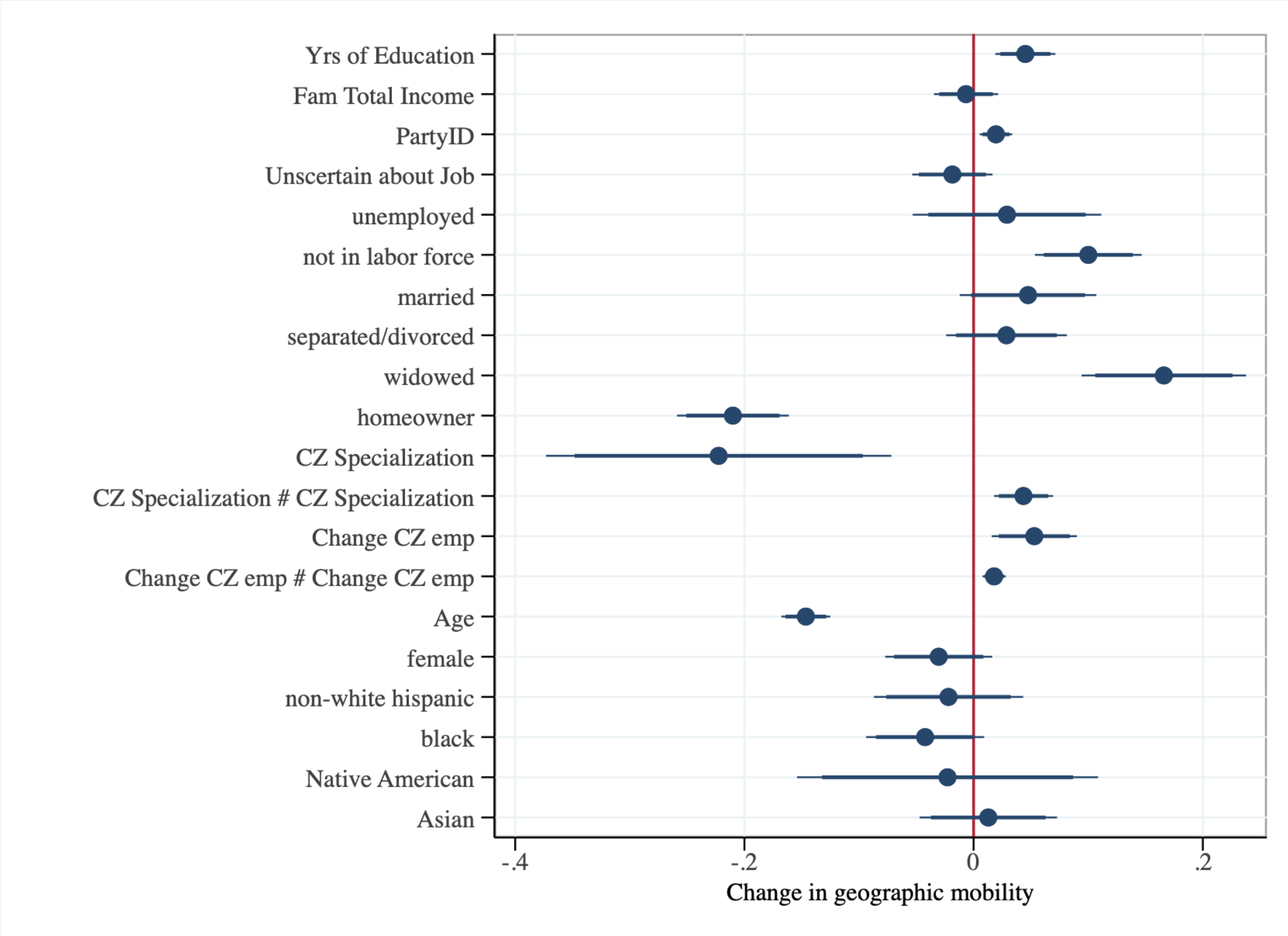

Figure 5: Correlates of geographic mobility, ANES 1992-1996 sample. The coefficients come from a CZONE-year fixed effects regression of migration probabilities with confidence intervals clustered at the state level. All non-binary coefficients are standardized. The # operator denotes an interaction.

concern by showing that $\hat{MOBILE}_{i,t}$ is the most important predictor of two types of panel attrition found in the ANES: one predicting whether a respondent drops in a *future* survey wave; and, another predicting whether a respondent changed address in a *past* wave. This is consistent with the finding that internal migrants grew significantly detached from their communities within the five years before and after their actual move date (Lueders, 2021), and also consistent with the finding that past mobility is a powerful predictor of future mobility (Vandenbrande, Coppin and Van der Hallen, 2006; Bonin et al., 2008).[58]

[58]To further alleviate population drift concerns, Molloy, Smith and Wozniak (2011) find that the relation-

Variable	Mean P(Mobility)	Std. dev.
Some college or greater	0.436	0.309
High school or less	0.327	0.271
Manufacturing sector	0.416	0.300
Non-Manufacturing sector	0.391	0.294

Table 1: Geographic Mobility Cuts Across Standard Cleavages. Data come from the 1992-1996 ANES panels. The first two rows report mobility summary statistics by educational cleavage predicted by the factor endowment model (Rogowski, 1989). The last two rows explore the same statistics across the sectoral cleavage predicted by the specific factors model Gourevitch (1986); Frieden (1991).

A related concern to population drift is that reliance on a Census 2000 measure to analyze voting and attitudes in 1992-1996 exposes the analyses to post-treatment bias. To guard against this, I conduct two analyses found in Appendix E.2. I first show that $MOB\hat{I}LE_{i,t}$ highly predicts a proxy for geographic mobility measured by the ANES—the number of years a respondent lived in their current dwelling. Second, while this proxy has significant drawbacks, I show that the main results of the paper are robust to its use.

Table 4 further demonstrates construct validity by showing that geographic mobility predicts weaker social and economic linkages to one's current CZONE. Specifically, those with a high probability of migration are, as expected, much less likely to: own a home, talk to their neighbors, or serve as a member in a community organization. Taken together, the consistency of the individual-level variation with prior research, five validation tests, and the robustness of the analysis to alternative measures, all strongly point to the same conclusion: $MOB\hat{I}LE_{i,t}$ is a highly reliable measure of geographic mobility.

Finally, the variation in mobility cuts across standard political cleavages. While it is true that mobility increases with education, large variation exists within these groups, as shown in the last column of Table 1. Similarly, sector based cleavages feature similar average levels of mobility, but with high within-group spread. This alleviates concerns that mobility is primarily explained by previously theorized political cleavages.

ships between mobility and socio-demographic characteristics are very stable across annual and decennial waves of the Current Population Survey and Census between 1981 and 2010.

5 Analysis

5.1 Spillover effects of the '94-'95 NAFTA Shock

I first establish evidence that the NAFTA trade shock serves as an appropriate test case for the theory. That is, the shock created regional variation in net import competition that, in the context of US-Mexico trade, causes losses in manufacturing employment (H1), and that these losses spillover into the community via agglomeration externalities (H2). This analysis also serves the purpose of establishing that the community-level effects of NAFTA are similar to those of the more heavily studied China trade shock.

Figure 6 presents the results of an event-study differences-in-differences model that regresses the 1-year change in CZONE manufacturing employment on the lag of CZONE net import exposure from NAFTA[59] interacted with year indicators. I control for lagged manufacturing specialization interacted with years to account for differential trends in specialization. Standard errors are clustered at the CZONE level. Following convention, I select the year before the treatment period (1993) as the baseline. The results show that a one standard deviation increase in net import exposure causes an average loss of approximately 800 manufacturing jobs within a CZONE over three years. CZONEs with and without exposure demonstrated similar pre-treatment trends in manufacturing employment. Evidence of parallel trends validates the argument that the rapid increase in Mexican net imports within one year of NAFTA's implementation was exogenous to US regions.

Figure 7 illustrates how local shocks to traded sectors spillover into non-traded sectors.[60] Consistent with H2, shocks to the manufacturing industry also cause employment losses in closely linked industries like transportation, communication and utilities as well

[59]The one period lag accounts for "stickiness" in the labor market. That is, it often takes time for firms to hire or fire employees.

[60]This figure is produced by running separate differences-in-differences regressions on sector-specific dependent variables—one for each major sector. For robustness, I also report coefficients from a comparable random effects model.

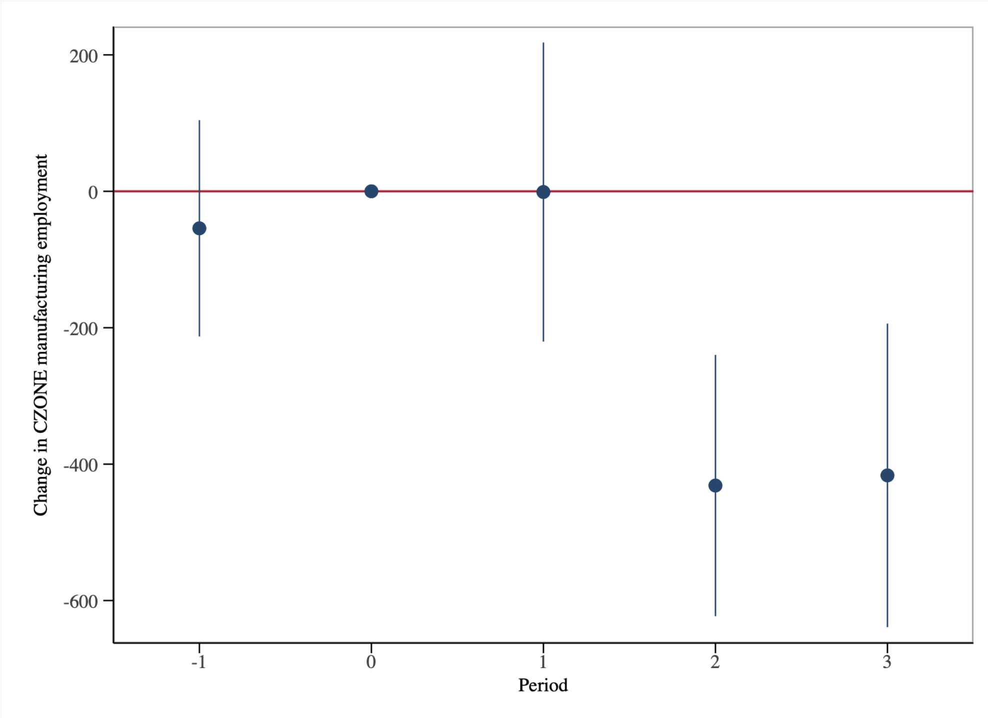

Figure 6: Parallel trends plot. Coefficients come from an event study specification that regresses the one-year change in CZONE manufacturing employment on the interaction of year indicators and the lag of the one-year change in net imports from Mexico. I control for the lag of CZONE manufacturing specialization interacted with a time trend to account for differential trends in specialization, as well as year and CZONE fixed effects. Coefficients are standardized and standard errors clustered at the CZONE level. Following convention, coefficients on net imports are benchmarked to the period before the event—in this case January 1st, 1994 when NAFTA took effect. Time periods reflect the availability of trade data starting in 1991, and the end of employment data using the SIC 1987 classification scheme in 1997.

as construction. These industries all depend on robust demand from locally traded manufacturing. Mixed support is found for spillovers in the service and wholesale trade sectors. Promisingly, these results are largely consistent with Hakobyan and McLaren (2016) despite using a different measures of NAFTA exposure and different employment and wage data.[61] Furthermore, industries that most likely benefit from NAFTA, agriculture and finance, either insignificantly decline or even grow.[62] Overall, the coefficient on trade exposure from

[61] By using annual BLS data, I extend their decadal analysis of Census data by showing that the local spillovers occurred as early as the 1996 election.

[62] This is consistent with labor reallocation from declining industries to those that benefit from trade. However, these meager employment gains fail to offset the total magnitude of losses across other sectors.

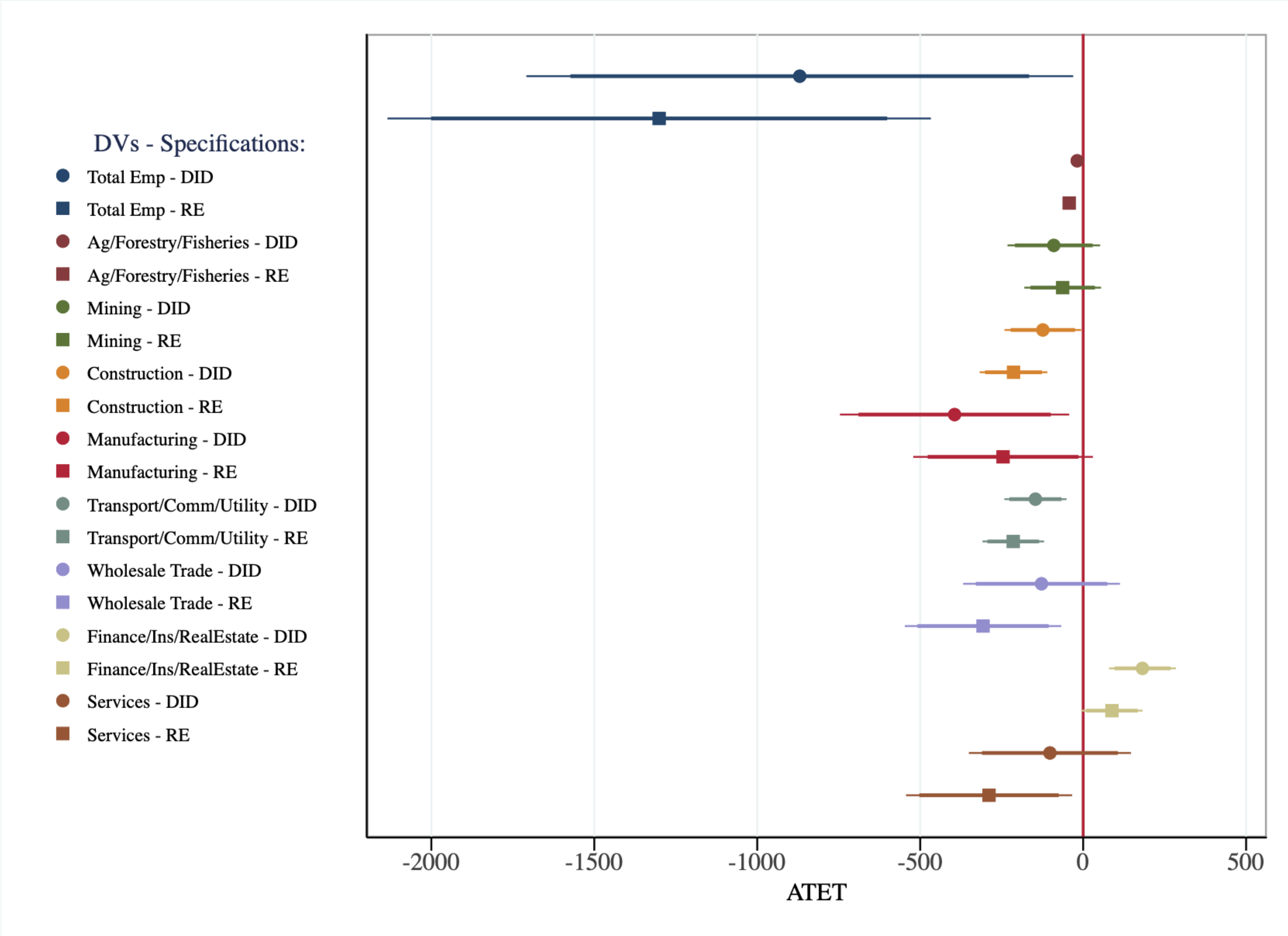

Figure 7: The Effect of NAFTA on CZONE employment, by sector. This coefficient plot shows evidence that trade exposure spills over from manufacturing to other sectors. Each color denotes a separate empirical model that differs by dependent variable: one for CZONE changes in employment for each major sector, including total employment. Coefficient labels marked with circles denote differences-in-differences specifications on the NAFTA shock with CZONE and year fixed effects and a control for manufacturing specialization interacted with a time trend. Specifications marked with squares denote alternative random effect specifications with random CZONE intercepts and year fixed effects. All standard errors are clustered at the CZONE level.

the regression on *total* CZONE employment summarizes the key result that trade exposure does indeed depress entire regional economies.

5.2 Divergent Support for Perot within NAFTA-Shocked CZONEs

I now explore how these regional disruptions affect individual votes for the populist anti-trade candidate Ross Perot. To test the primary hypotheses (H4 and H5), I estimate the following logistic regression:

$$Y_{i,r,t} = Logit^{-1}(\alpha_1 NAFTA_{r,t} + \alpha_2 \hat{MOBILE}_{i,t} + \alpha_3(\hat{MOBILE}_{i,t} \times NAFTA_{r,t}) + \boldsymbol{X'\beta}) + \epsilon_{i,r,t}$$

where i, r, and t index the individual, regional CZONE, and survey year, respectively. $Y_{i,r,t}$ includes the binary dependent variables: $VotedPerot_{i,j,t}$ and $AntiTrade_{i,j,t}$. These are regressed on the interaction of the NAFTA shock $NAFTA_{r,t}$, predicted geographic mobility $\hat{MOBILE}_{i,t}$, and their interaction. Under hypotheses H4 and H5, we should expect a statistically significant negative slope on the interaction term α_3. This would suggest that local exposure to net imports from NAFTA caused a relative increase in support for protective tariffs and votes for Ross Perot among geographically *im*mobile voters compared to mobile voters. Additionally, a significantly negative coefficient on α_2 would support H4.1 that greater mobility on average makes voters opposed to trade barriers and to anti-trade populists, regardless of local conditions.

To control for endogeneity from alternative cultural factors, the vector $\boldsymbol{X'}$ adds: a 7-point party ID scale, and binary indicators for race, ethnicity, and gender. I also include lagged CZONE specialization in manufacturing as well as education and an industry-level measure of exposure to Mexican imports ($\%\Delta NetImports_{i,t}$) to account for alternative factor-endowments and industry-based trade theories. Fixed effects for CZONE or respondents (when sample size permits) help to account for fixed characteristics of regions and individuals. Finally, year fixed effects account for national shifts in party campaigns, survey changes, and other unit-invariant shocks. All standard errors are clustered at the US state level to address spatial autocorrelation between CZONES and the voters within them.

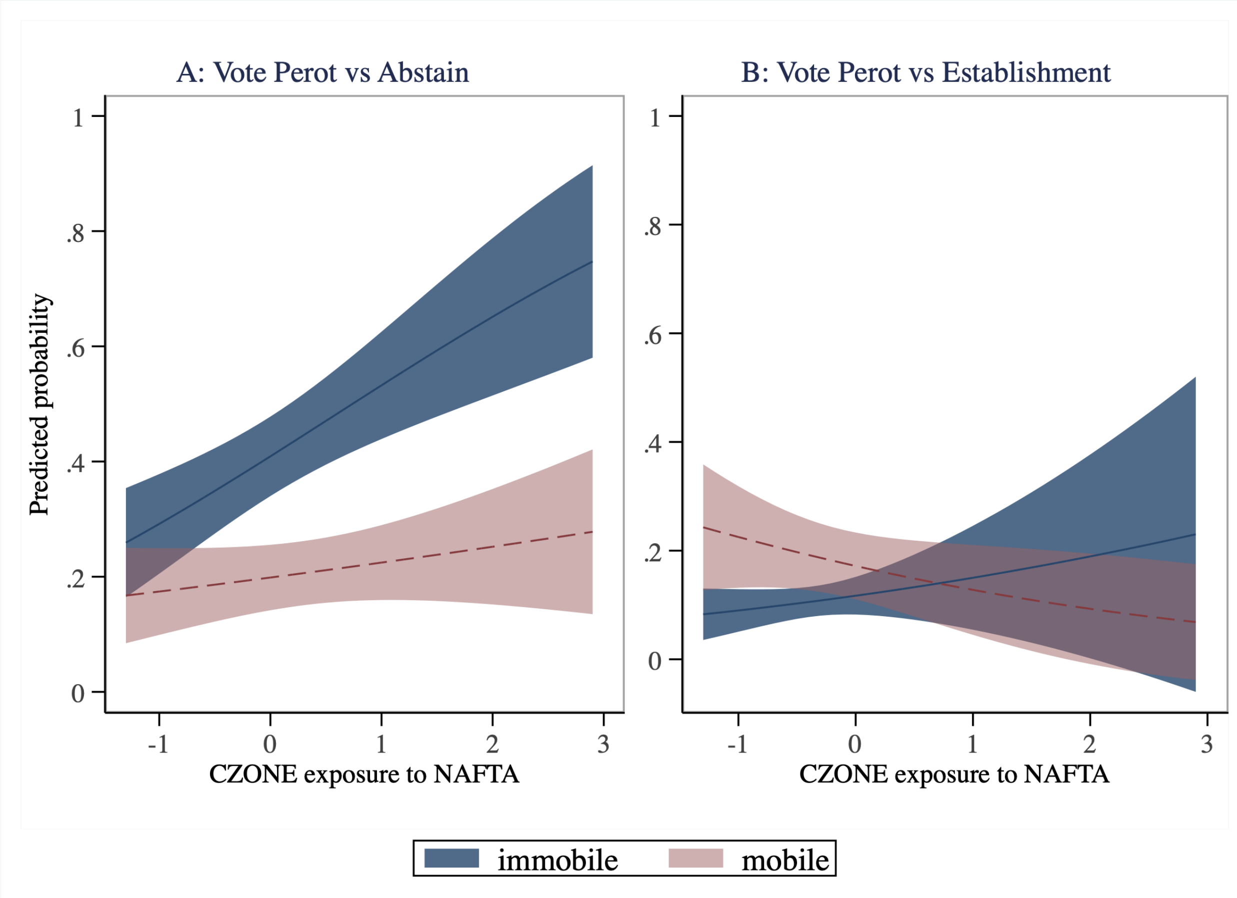

Figure 8: The mobile-immobile divergence in support for Ross Perot. The y-axis measures the predicted probability of voting for Ross Perot compared to those who abstain from voting (Panel A) and compared to establishment candidates (Panel B). The x-axes show the change in CZONE exposure to net imports from Mexico in standardized units, particularity within the range in which we have observations. Predicted probabilities are provided for respondents where the probability of migration equals one and zero—mobile and immobile, respectively. Control variables include education, industry-level NAFTA exposure, lagged manufacturing specialization, party ID, sex, indicators for race and ethnicity, and year and CZONE fixed effects. Estimates come from m2 and m5 from Table 6 in Appendix F.1. 90 percent CIs are clustered at the state level.

Figure 8A shows the primary finding that the NAFTA shock significantly contributed to anti-trade populism among geographically immobility voters. I plot the predicted probability of voting for Ross Perot among mobile and immobile voters.[63] Within each mobility group, changes in the predicted probability of voting for Perot can be interpreted

[63] I define immobiles and mobiles as having a predicted probability of migration that equals zero and 100 percent, respectively. Figure 25 shows that the results are highly robust to alternative migration probabilities above and below 80 percent.

as the effect of different NAFTA shock levels. The quantified effect is linear and large in magnitude. Within the most exposed regions, a geographically immobile voter's predicted probability of voting for Perot (versus abstaining) increases on average to 0.75 from 0.25.[64] In contrast, mobile respondents report an average 0.20 probability of voting for Perot, which does not change with local trade conditions. Therefore, the average *divergence* in probabilities between groups equals 0.45 within the most exposed regions.[65] This provides strong confirmatory evidence in favor of the mobile-immobile political cleavage within adversely affected regions (H5). Mobility constrained voters within trade shocked regions significantly drive anti-trade populism.

The results from Figure 8B also confirm H5 (α_3 = -0.743, se = 0.36 from Table 6); however, the relatively weak magnitude and precision likely reflect strategic voting under majoritarian electoral rules. Recall that Perot's status as a third-party candidate means that he is very unlikely to win. By Duverger's Law, winner-take-all electoral rules discourage voters from throwing away their votes by choosing Perot (Riker, 1982). Panel B emphasizes this strategic choice by comparing votes for Perot versus the Republican and Democratic candidates who have a realistic chance of winning. By contrast, Panel A's construction makes a less strategic comparison between votes for Perot versus staying home. In this way, the powerful results from B suggest that much of the electoral effect of NAFTA was to motivate otherwise non-voters to turn-out for an anti-trade candidate. This contrasts with the more common political narrative of trade shocks causing voters to abandon liberal parties for ones on the right (Milner, 2021; Ferrara, 2022). In fact, these data find that the effect of NAFTA on Republican voting is limited to geographically immobile voters who self-identify as Independents, and who would have otherwise abstained (see Figure 28 in Appendix F.1).[66] The overall results are robust to analyses of candidate and party feeling

[64] Figure 25 in Appendix F.1 shows that the positive effect of NAFTA on Perot voting decays at a linear rate as predictive mobility increases, becoming statistically indistinguishable from zero after 80 percent. That is, only the most mobile respondents are statistically *in*sensitive to local trade shocks.

[65] This effect applies especially to respondents who self-identify as Democrats and Independents (see Appendix Figure 26).

[66] This likely reflects, in part, data limitations that restrict these analyses to the *immediate* political

thermometers (see Appendix E.1). I discuss in the conclusion how this result might differ in alternative electoral contexts.

Finally, I provide suggestive evidence that voters in "core" regions did not vote for Perot in response to local trade variation. However, the lack of trade data outside of manufacturing makes identifying such regions challenging. The use of net imports helps to identify some regional "winners," but we ideally want a measure of knowledge economy exports. Figure 30 provides evidence from a proxy measure based on CZONE population size. It relies on the plausible assumption that regions that specialize in the knowledge economy have, like San Fransisco, Los Angeles, and New York City, population sizes above the 75 percentile. Based on this definition of core-periphery, we see the hypothesized results that the marginal effect of the NAFTA shock to lower-population regions produces the same result as before while exposure within large population regions causes no pro-Perot backlash. Appendix F.4 repeats this analysis on Perot feeling thermometers and trade attitudes.

5.3 Divergent Trade Attitudes within NAFTA-Shocked CZONEs

This section shows that anti-trade policy preferences underpin the populist backlash at the polls. Figure 9A plots the predicted probabilities of opposing free trade (i.e., preferring protective tariff barriers). Within a mobility group, changes in the predicted probability of opposing trade can be interpreted as the effect of different NAFTA shock levels. The plot illustrates support for H4 that geographically immobile respondents grew more opposed to trade in response to local trade exposure while mobile respondents grew more supportive of trade. Considering the hardest hit regions in the ANES sample, the probability of opposing trade among immobile respondents increases on average to 0.8 from 0.6 while opposition among mobile respondents decreases to 0.1 from 0.5. This divergence in trade attitudes in

aftermath of NAFTA (within two years of the shock), during which a disruptive 3rd-party candidate saw much success. In contrast, most studies observe long run electoral effects—the effect of the 1990-2007 China trade shock on voting in 2016.

response to the same shock equals about 70 percentage points.[67]

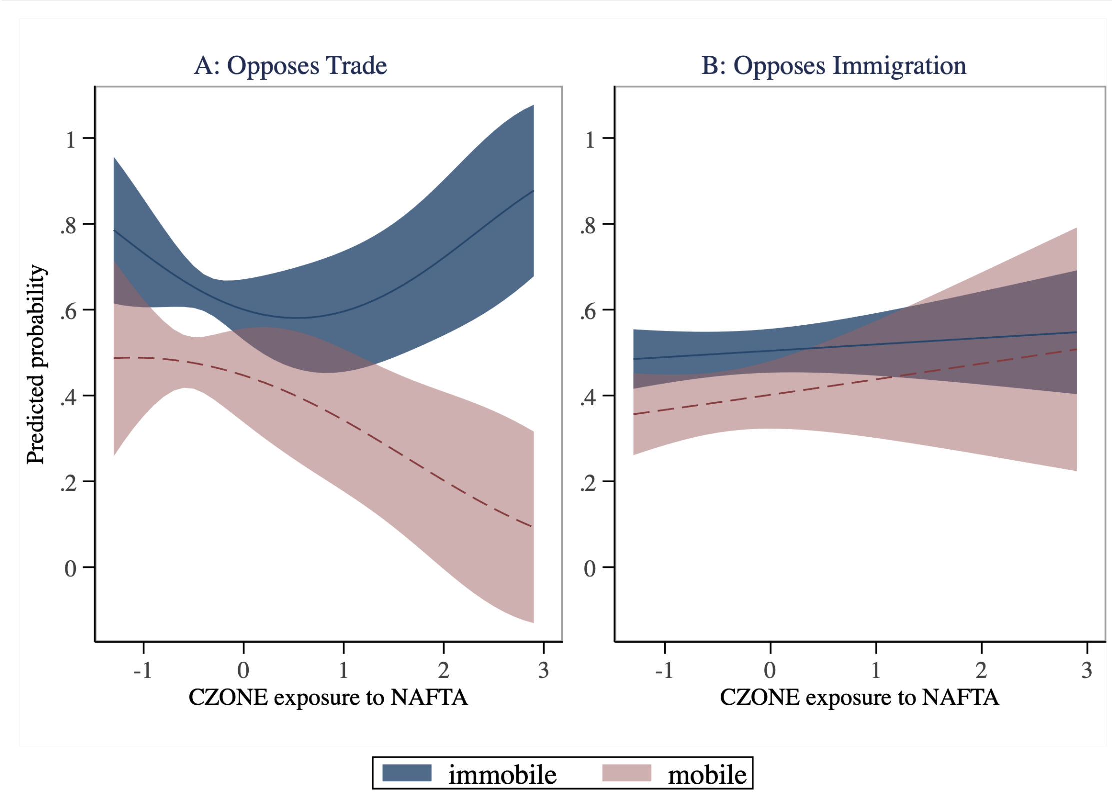

Figure 9: The mobile-immobile divergence in trade preferences. The y-axis in panel A measures the predicted probability of reporting opposition to trade (i.e., support "placing new limits on foreign imports in order to protect American jobs"). The y-axis on Panel B measures the predicted probability that the respondent prefers to reduce the number of foreign immigrants. The x-axes show the change in CZONE exposure to net imports from Mexico in standardized units. Predicted probabilities are provided for respondents where the probability of migration equals one and zero—mobile and immobile, respectively. Results are robust to controls for education, industry-level NAFTA exposure, lagged manufacturing specialization, party ID, sex, indicators for race and ethnicity, and fixed effects for year, CZONE, and individuals. Estimates come from Table 8 in Appendix F.2. 90 percent CIs are clustered at the state level.

Another important result is that geographically mobile voters are on average 15 percentage points more likely to favor free trade regardless of the level of the NAFTA shock (also see Table 8). This supports the auxiliary hypothesis H4.1 that mobile voters benefit from trade regardless of local conditions. Significantly, the magnitude of this *independent*

[67]Similar to the vote dynamics for Perot, this effect applies especially to self-reported Democrats and Independents (see Appendix F.2).

effect of mobility is as large or larger than that of education and gender—the two variables most consistently related to trade attitudes in the broader literature (Scheve and Slaughter, 2001; Mayda and Rodrik, 2005; Guisinger, 2016).

Overall, the significance of these results is twofold. First, they show substantively large effects of trade exposure when few other studies of individual preferences do (see Hafner-Burton et al. (2017); Margalit (2019)). Second, they reveal conditions under which we can expect trade attitudes to affect vote choice (Guisinger, 2009).

The common alternative argument is that cultural concerns drive trade preferences and votes for anti-trade populists (Inglehart and Norris, 2017; Mutz, 2018). While I control for respondents' race, ethnicity, gender, and party ID, Mexican imports could also affect attitudes toward immigration. Immigration is relatively more salient and ideological in American politics (Hajnal and Rivera, 2014), and voters may respond to economic uncertainty by scapegoating immigrants (Cerrato, Ferrara and Ruggieri, 2018). An anti-immigration response to trade would reliably reflect cultural or ideological concerns rather than economic ones because regional exposure to Mexican imports is uncorrelated with the border regions exposed to Mexican immigration. To test this alternative, Figure 9B shows the results of a placebo outcomes test. This uses the same empirical model to predict the probability of opposing immigration. If the NAFTA results are confounded by ideological or racial concerns, those biases would show up here. In short, I finds no evidence for this. Local exposure to NAFTA did not make either immobiles or mobiles sensitive to immigration.

Instead, the evidence favors the theory's underlying economic mechanisms. Specifically, NAFTA exposure causes a divergence in the economic welfare of mobiles and immobiles (H3). Figure 10 plots this divergence in terms of changes in family total income and the probability that either the respondent or their partner becomes unemployed. In panel A, the NAFTA shock causes an average 0.75 standard deviation gap in wages between mobiles and immobiles within the most exposed regions. Respondent fixed effect mean that these effects can be interpreted as within respondent changes between 1992 and 1996. Overall, NAFTA exposure causes stagnant wages among immobiles, and an income boost among mobiles. This income boost is consistent with theory: the wages of workers with high geographic mobility are competed up by wages in the "core."

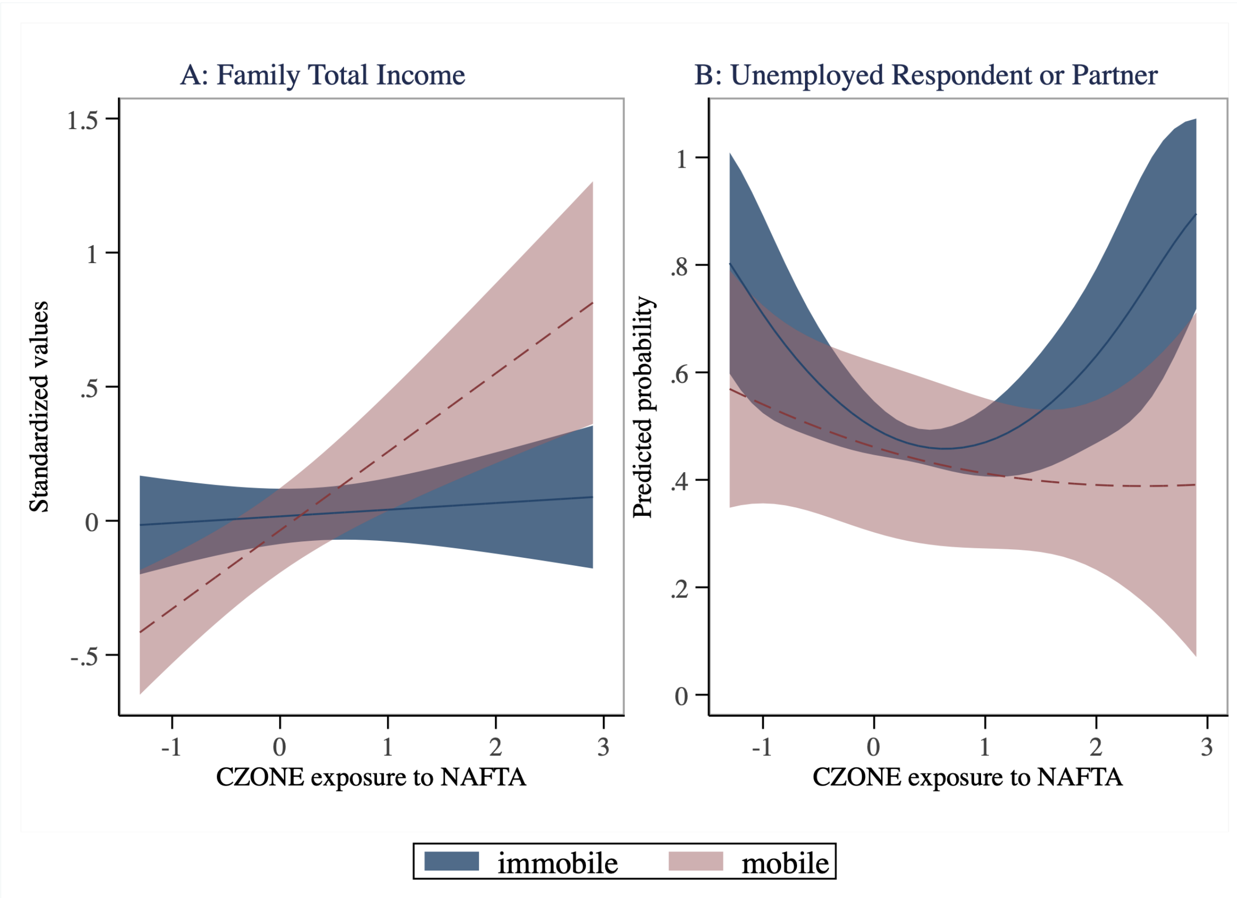

Figure 10: The mobile-immobile divergence in private fortunes. These marginal effects plots demonstrate evidence of the underlying theoretical assumption that mobiles benefit from trade while immobiles lose. Panel A measures this, along the y-axis, in terms of standardized family total income from a linear regression. Panel B uses a logistic regression for the predicted probability that either the respondent or their partner is unemployed. Both specifications control for the lag of manufacturing specialization, education, industry exposure to NAFTA, and year and individual fixed effects. Estimates come from Table 10 in Appendix F.3. 90 percent CIs are clustered at the CZONE level.

Since wages are sticky, we might expect the negative pocketbook effects of NAFTA to show up in unemployment. When faced with the economically-equivalent options of lowering workers' wages or laying off workers, firms may choose the latter.[68] Figure 10B confirms this intuition. Rather than reduced wages, immobile workers face a significantly higher probability of unemployment in absolute terms (an increase to 0.9 from 0.5) and in relative terms compared to mobile voters.[69]

Overall, these results confirm the economic mechanisms of the political results. CZONE exposure to NAFTA causes a significant divergence in voters' economic welfare, which in turn creates a political cleavage in trade attitudes and voting for anti-trade populists.

5.4 Alternative Hypotheses

Given the enormous scholarly debate on the economic versus cultural nature of trade attitudes, I take on this alternative more thoroughly here. At the core of the cultural backlash thesis lies the role of racial/ethnic out-groups, particularly from the perspective of white respondents who are hypothesized to respond to globalization with increased anxiety about their declining social and economic status relative to "competing" racial groups. A simple test of this runs the same regressions as before on feeling thermometers for different racial groups. The ANES asks respondents to rate how warmly they feel toward three racial/ethnic categories: Hispanic, white, and black, each shown in panels A, B, and C respectively from Figure 11. In short, I find no evidence that changes in trade exposure affect voters' racial attitudes. In fact, the only result of statistical importance is that immobile voters feel on average 7 out of 100 points warmer toward white individuals than mobiles—a result which remains constant across NAFTA exposure. In addition to the placebo outcome test on im-

[68] Across the board wage cuts are controversial. Also, wages sometimes reflect sticky contractual agreements that are difficult to break.

[69] Unexpectedly, the probability of unemployment among immobiles also rises within regions that experienced stable net exports. I speculate that this is consistent with greater labor market competition in these higher growth areas, particularly in response to an influx of geographically mobile voters looking to arbitrage the higher wage and employment opportunities. If so, immobile voters may experience negative effects of trade in both winning regions and losing regions. This might also explain why, in Figure 9A, immobile voters report very high baseline opposition to trade across all levels of exposure to NAFTA.

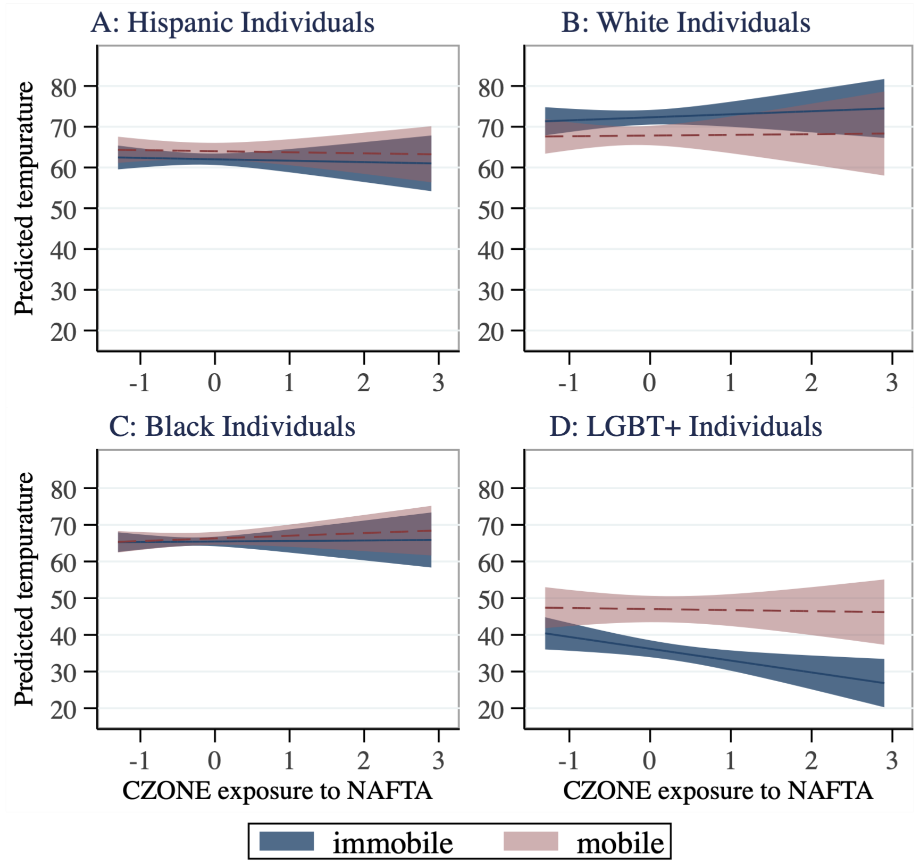

Figure 11: Effect of NAFTA on feelings toward non-economic identities. All panels feature marginal effects from linear regressions with four different dependent variables: feelings toward non-white Hispanics, whites non-Hispanics, and members of the black and LGBT+ community. Feeling thermometers range from 0-100 with 100 meaning that the respondent feels very warm (positive) toward that group. The x-axes measure the change in net-imports from Mexico in standardized units. Overall, exposure to NAFTA does not affect how either mobiles or immobiles feel about major racial or ethnic groups. However, NAFTA does create a cleavage between mobiles and immobiles on feelings toward the gay community. All specifications include family total income and unemployment to control for alternative economic mechanisms. They also include the lag of manufacturing specialization, party ID, sex, indicators for the respondent's own race of ethnicity, and year and CZONE fixed effects. Estimates come from Table 9 in Appendix F.2. Standard errors clustered at the state level.

migration, this evidence suggests a robust rejection of the cultural backlash hypothesis, at least in this empirical context.[70]

[70] These differences in findings may reflect differences in elite messaging between populists like Trump, who conflated economic and cultural messages on NAFTA, versus Perot who focused on its economic effects. The Trump messaging likely brought many more voters into the anti-trade coalition than were present in 1992 and 1996.

More interestingly, Panel C in Figure 11 shows a significant divergence between mobility groups on attitudes toward sexual orientation. Immobiles respond to the NAFTA shock with a significant drop in feelings toward the LGBT+ community.[71] This drop equates to a 20 point decline relative to neighboring mobile respondents. This suggests that immobiles exhibit more homophobic attitudes, relative to mobiles, in response to community decline from NAFTA. Appendix F.5 finds that these attitudinal shifts likely emerge from immobile voters' growing reliance on local church organizations for economic support. Specifically, Figure 33 shows how trade drives immobile voters to increase their probability of identifying as religious, as well as the number of times they pray. I argue that this turn towards church support—and its socially conservative views—cannot explain voting patterns for Perot, whose platform ran staunchly against religious conservatism.[72]

6 Discussion

At the NAFTA signing ceremony on December 9th, 1993, President Bill Clinton remarked that by creating the world's largest trade zone, NAFTA would displace the jobs of many, concluding that "we must see to it that our citizens have the personal security to confidently participate in this new era. Every worker must receive the education and training he or she needs to reap the rewards of international competition rather than the to bear its burdens." What Clinton did not realize is that re-education would provide little solace to voters stuck in afflicted communities, particularly when the new jobs created by global trade located in distant cities far from their reach. Clinton was not alone: political science has long relied on theories that emphasize individual education and skills at the expense of location, and the ability to move locations.

[71] Specifically, the 1992 and 1996 ANES questionnaires ask respondents how they would rate "gay men, and lesbians, homosexuals."

[72] He came out as pro-choice on abortion and promised to allow members of the LGBT+ community to serve in the military and in his cabinet.

By reconciling standard (OEP) models of trade with two empirical anomalies—geographic mobility and local spillovers—this paper contributes to our understanding of the electoral effects of international trade. It predicts that a voter's geographic mobility determines their exposure to trade's regional effects, and thus their positions on relevant policies and candidates. In import-shocked regions, immobile voters support barriers to globalization and the populist candidates that propose them; in contrast, those who can migrate to globalizing cities, along with those who already reside there, stand opposed to such barriers.

These predictions are borne out by a differences-in-differences analysis of a representative panel that tracked voters attitudes and votes across the 1992 and 1996 elections. Within the regions that suffered the highest 1994-95 increases in Mexican import competition, geographically immobile voters became on average 75 percent likely to vote for the anti-NAFTA populist Ross Perot, compared to an average 25 percent likelihood among mobile respondents. These voting patterns correspond to a large divergence in trade attitudes, with immobile voters growing at most 70 percentage points more likely than mobile voters to support job-protecting barriers against foreign imports. These results are robust to: alternative measures of geographic mobility, alternative political outcomes, endogeneity to time-invariant heterogeneity and unit-invariant shocks, and alternative economic and cultural backlash mechanisms.

This analysis overcomes two empirical challenges. First, unobservable features of geographic mobility are measured with a machine learning technique that predicts complex latent variables. This uses the observed internal migration patterns from US Census microdata to train an algorithm to predict the migration probabilities of respondents in public opinion surveys. Second, I address endogeneity concerns by leveraging the research design benefits of the NAFTA trade shock.

These results, and the theory they support, help address a significant issue in the literature. Many studies recognize a systematic gap in the conclusions of individual-level surveys and aggregate-level analyses of election outcomes (Hafner-Burton et al., 2017; Kuo and Naoi, 2015; Margalit, 2019; Frieden, 2022). While aggregate results generally support standard trade models, surveys rarely find that individuals vote or think in ways consistent with material self-interest. This commonly referenced "aggregation problem" challenges core theories of democratic behavior within American and International Political Economy, and has impeded scientific progress. Without a means to reconcile contradictory evidence, scholars have engaged in "fruitless rounds of debates, with political economists and survey researchers flinging results at each other" (Frieden, 2022, p.8). This paper suggests one mechanism for reconciling these differences: heterogeneity in geographic mobility determines how individuals are affected by globalization's region-level externalities.[73] By accounting for voters' differential exposure to aggregate shocks, the analysis recovers significant effects of self-interest on trade policy attitudes and voting.

More broadly, this paper contributes to our understanding of anti-trade populism. While racial prejudice and culture wars play an undeniable role, recent literature questions the independent role of economic dislocations, particularly from trade (Inglehart and Norris, 2017; Mutz, 2018; Ballard-Rosa, Jensen and Scheve, 2022; Margalit, 2019). This paper shows that some voters support populists because of trade's independent effects on their pocketbooks. Perhaps more importantly, it illuminates largely underutilized tools for the prevention of populism: "place-based" redistributive programs (e.g.; Kline and Moretti (2014); Austin, Glaeser and Summers (2018); Gaubert, Kline and Yagan (2021)) and relocation vouchers (Moretti, 2012, 161). While a growing number of economists see this type of redistribution as key to reducing regional inequalities, its political feasibility may depend largely on whether the public believes the grievances of those stuck in declining regions to be a product of prejudice or economic dislocations from global forces (Frieden, 2022).

[73] Another mechanism proposed by the literature is that individuals have non-economic reactions to the economic insecurity caused by aggregate shocks (Baccini and Weymouth, 2021; Ferrara, 2022). This paper however finds little evidence for this between 1992 and 1996.

7 Generalizability and Future Research

I offer a general framework for analyzing how voters depend on their local economies—a political economy of place. This framework contributes to the growing interests in how local conditions affect democratic accountability (Ebeid and Rodden, 2006; Healy and Lenz, 2017; de Benedictis-Kessner and Warshaw, 2020). Importantly, the insights from geographic mobility and local externalities extend beyond the realm of trade politics. Location-specific consequences to technological change (David, Dorn and Hanson, 2013), immigration (Tabellini, 2020), and climate change (Kaufmann et al., 2017; Hazlett and Mildenberger, 2020; Gazmararian and Milner, 2021), for instance, suggest an important role for geographic mobility in the politics of these issues.[74]

In the area of trade, the evidence here should generalize to the more recent American context. While I focus on NAFTA for causal identification, the empirical context shares more similarities than differences to that of the Trump elections. Both contexts featured political outsiders running strongly on an anti-trade platform.[75] Furthermore, two empirical trends make this theory especially relevant today. First, geographic mobility, measured by aggregate rates of interstate migration, has declined by approximately 30 percent between 1996 and 2018.[76] Second, regional trade shocks have intensified following China's 2001 ascension to the WTO. These trends imply a powder keg of rising economic pressure with fewer opportunities for adjustment—conditions which likely contributed to the explosion of populism within the last decade.

Of course, extensions of this analysis would need to account for significant institutional variation. Ross Perot's status as a third party candidate in a majoritarian electoral

[74] Indeed, a recent paper points to the central role of migration in climate politics (Draper, 2021).

[75] In fact their similarities extend much further. The two candidates shared many similar identities and appeals: billionaire businessman, political outsider, populist, and even authoritarian. See Dunham, Richard S.; Douglas Harbrecht (April 6, 1992). "Is Perot after the Presidency, or the President?". Bloomberg Businessweek. Bloomberg.

[76] Source: Third Way and U.S. Census Bureau, Current Population Survey, Historical Migration/Geographic Mobility Tables.

system significantly affects how the results might generalize. First, these results likely apply more strongly when the anti-trade candidate, or the referenda in the case of Brexit, has a reasonable chance of winning. By comparison, Perot represents a hard case since many voters were dis-incentivized to throw away their vote even if they preferred his anti-trade message. The results should therefore extend to cases like Donald Trump, Brexit, and anti-trade parties under proportional representation rules. Second, these results may understate the importance of cultural factors. Even though the 1990s saw some of the country's most intense race relations following the Rodney King verdict, Perot's 3rd party status likely downplayed these forces. Unlike major parties that must sell trade broadly within a large basket of other policies, third party candidates can present a more narrowly focused trade platform. By implication, voting for Trump likely features a more diverse set of voter motives than for Perot.

Finally, an account of geographic mobility is not complete without considering how the results aggregate over the long run. The results here speak primarily to short-run political responses to trade. That is, adversely shocked regions will initially feature stark divisions between immobiles who have every incentive to fight for policies that reverse local decline, and the mobiles who see economic opportunity in the new status quo. In the long-run[77] however, migration selection alters the politics of regions. In the rapidly globalizing core cities, migrant inflows will eventually drive up the cost of living. If these costs rise higher than positive externalities can compensate, then the pro-trade coalition in the core might begin to unravel. Within the periphery, immobile voters will be increasingly left behind, resulting in an homogeneous chorus of anti-trade voices. These aggregate processes would take years if not decades to unfold, consequentially delaying the time between a region's initial trade shock and its *aggregate* political backlash.

[77]While Autor, Dorn and Hanson (2013) document the puzzling absence of migration out of areas hit by Chinese imports, others find that out-migration concentrates at a seven-to-ten year lag(Hakobyan and McLaren, 2016; Greenland, Lopresti and McHenry, 2019).

References

Abrams, Samuel J and Morris P Fiorina. 2012. ""The big sort" that wasn't: A skeptical reexamination." *PS: Political Science & Politics* 45(2):203–210.

Acemoglu, Daron, David Autor, David Dorn, Gordon H Hanson and Brendan Price. 2016. "Import competition and the great US employment sag of the 2000s." *Journal of Labor Economics* 34(S1):S141–S198.

Ansell, Ben, Frederik Hjorth, Jacob Nyrup and Martin Vinæs Larsen. 2022. "Sheltering Populists? House Prices and the Support for Populist Parties." *The Journal of Politics* 84(3):000–000.

Austin, B, E Glaeser and LH Summers. 2018. "Saving the heartland: Place-based policies in 21st century America. Washington, DC: The Brookings Institution.".

Autor, David. 2019. Work of the Past, Work of the Future. Technical report National Bureau of Economic Research.

Autor, David. 2020. "The faltering escalator of urban opportunity." *Securing Our Economic Future* pp. 108–36.

Autor, David. 2021. "Why Was the 'China Shock'So Shocking—and What Does This Mean for Policy?" *Combating Inequality: Rethinking Government's Role* pp. 109–116.

Autor, David, David Dorn and Gordon Hanson. 2013. "The China syndrome: Local labor market effects of import competition in the United States." *American Economic Review* 103(6):2121–68.

Autor, David, David Dorn, Gordon Hanson and Kaveh Majlesi. 2016. "Importing political polarization? The electoral consequences of rising trade exposure." *NBER Working Paper* .

Baccini, Leonardo and Stephen Weymouth. 2021. "Gone for Good: Deindustrialization, white voter backlash, and US presidential voting." *American Political Science Review* pp. 1–18.

Bailey, Michael A, Judith Goldstein and Barry R Weingast. 1997. "The institutional roots of American trade policy: Politics, coalitions, and international trade." *World Politics* 49(3):309–338.

Baker, Andy. 2005. "Who wants to globalize? Consumer tastes and labor markets in a theory of trade policy beliefs." *American Journal of Political Science* 49(4):924–938.

Ballard-Rosa, Cameron, Amalie Jensen and Kenneth Scheve. 2022. "Economic decline, social identity, and authoritarian values in the United States." *International Studies Quarterly* 66(1):sqab027.

Ballard-Rosa, Cameron, Mashail Malik, Stephanie Rickard and Kenneth Scheve. 2017. The economic origins of authoritarian values: evidence from local trade shocks in the United Kingdom. In *Annual Meeting of the International Political Economy Society, Nov.* pp. 17–18.

Barone, Guglielmo and Helena Kreuter. 2020. "Low-wage import competition and populist backlash: The case of Italy." *European Journal of Political Economy* p. 101970.

Bartik, Timothy J. 1991. "Who benefits from state and local economic development policies?".

BEA. 2020.
URL: *www.bea.gov*

Bell, Martin, Marcus Blake, Paul Boyle, Oliver Duke-Williams, Philip Rees, John Stillwell and Graeme Hugo. 2002. "Cross-national comparison of internal migration: Issues and measures." *Journal of the Royal Statistical Society: Series A (Statistics in Society)* 165(3):435–464.

Berry, Christopher R and Edward L Glaeser. 2005. "The divergence of human capital levels across cities." *Papers in regional science* 84(3):407–444.

Bisbee, James. 2018. "What is out your back door: How policy preferences respond to local trade shocks." *Unpublished Manuscript, Department of Political Science, New York University* .

Black, Dan, Terra McKinnish and Seth Sanders. 2005. "The economic impact of the coal boom and bust." *The Economic Journal* 115(503):449–476.

Blanchard, Olivier Jean, Lawrence F Katz, Robert E Hall and Barry Eichengreen. 1992. "Regional evolutions." *Brookings papers on economic activity* 1992(1):1–75.

Blonigen, Bruce A. 2011. "Revisiting the evidence on trade policy preferences." *Journal of International Economics* 85(1):129–135.

Blonigen, Bruce A and Jacob McGrew. 2014. "Task routineness and trade policy preferences." *Economics & Politics* 26(3):505–518.

Bonin, Holger, Werner Eichhorst, Christer Florman, Mette Okkels Hansen, Lena Skiöld, Jan Stuhler, Konstantinos Tatsiramos, Henrik Thomasen, Klaus F Zimmermann et al. 2008. *Geographic mobility in the European Union: Optimising its economic and social benefits*. Vol. 19 IZA Bonn.

Borjas, George J, Stephen G Bronars and Stephen J Trejo. 1992. "Self-selection and internal migration in the United States." *Journal of urban Economics* 32(2):159–185.

Bourguignon, François. 2015. The globalization of inequality. In *The Globalization of Inequality*. Princeton University Press.

Broz, J Lawrence, Jeffry Frieden and Stephen Weymouth. 2021. "Populism in place: the economic geography of the globalization backlash." *International Organization* .

Campbell, Angus, Philip E Converse, Warren E Miller and Donald E Stokes. 1980. *The american voter*. University of Chicago Press.

Caselli, Mauro, Andrea Fracasso and Silvio Traverso. 2020. "Globalization and electoral outcomes: Evidence from Italy." *Economics & Politics* 32(1):68–103.

Cerrato, Andrea, Federico Maria Ferrara and Francesco Ruggieri. 2018. "Why Does Import Competition Favor Republicans? Localized Trade Shocks, Voting Behavior, and Scapegoating in the US.".

Colantone, Italo and Piero Stanig. 2018*a*. "Global competition and Brexit." *American Political Science Review* 112(2):201–218.

Colantone, Italo and Piero Stanig. 2018*b*. "The trade origins of economic nationalism: Import competition and voting behavior in Western Europe." *American Journal of Political Science* 62(4):936–953.

Cooke, Thomas J. 2011. "It is not just the economy: Declining migration and the rise of secular rootedness." *Population, space and place* 17(3):193–203.

Daniels, Joseph P and Marc Von Der Ruhr. 2005. "God and the global economy: Religion and attitudes towards trade and immigration in the United States." *Socio-Economic Review* 3(3):467–489.

David, H and David Dorn. 2013. "The growth of low-skill service jobs and the polarization of the US labor market." *American economic review* 103(5):1553–97.

David, H, David Dorn and Gordon H Hanson. 2013. "The geography of trade and technology shocks in the United States." *American Economic Review* 103(3):220–25.

Davies, Paul S, Michael J Greenwood and Haizheng Li. 2001. "A conditional logit approach to US state-to-state migration." *Journal of Regional Science* 41(2):337–360.

de Benedictis-Kessner, Justin and Christopher Warshaw. 2020. "Accountability for the local economy at all levels of government in United States elections." *American Political Science Review* 114(3):660–676.

DESA/UNSD. 2020. "United Nations Comtrade database.".
URL: *http://comtrade.un.org/*

Dippel, Christian, Robert Gold and Stephan Heblich. 2015. "Globalization and its (dis-) content: Trade shocks and voting behavior." *National Bureau of Economic Research* .

Dippel, Christian, Robert Gold, Stephan Heblich and Rodrigo Pinto. 2017. Instrumental variables and causal mechanisms: Unpacking the effect of trade on workers and voters. Technical report National Bureau of Economic Research.

Dorn, David, Gordon Hanson et al. 2019. "When work disappears: Manufacturing decline and the falling marriage market value of young men." *American Economic Review: Insights* 1(2):161–78.

Draper, Jamie. 2021. "Labor migration and climate change adaptation." *American Political Science Review* pp. 1–13.

Ebeid, Michael and Jonathan Rodden. 2006. "Economic geography and economic voting: Evidence from the US states." *British Journal of Political Science* 36(3):527–547.

Eckert, Fabian, Teresa C Fort, Peter K Schott and Natalie J Yang. 2020. Imputing Missing Values in the US Census Bureau's County Business Patterns. Technical report National Bureau of Economic Research.

Elhorst, J Paul. 2003. "The mystery of regional unemployment differentials: Theoretical and empirical explanations." *Journal of economic surveys* 17(5):709–748.

Ellison, Glenn, Edward L Glaeser and William R Kerr. 2010. "What causes industry agglomeration? Evidence from coagglomeration patterns." *American Economic Review* 100(3):1195–1213.

Feigenbaum, James J and Andrew B Hall. 2015. "How Legislators Respond to Localized Economic Shocks: Evidence from Chinese Import Competition." *The Journal of Politics* 77(4):1012–1030.

Feler, Leo and Mine Z Senses. 2017. "Trade shocks and the provision of local public goods." *American Economic Journal: Economic Policy* 9(4):101–43.

Ferrara, Federico Maria. 2022. "Why does import competition favor republicans? Localized trade shocks and cultural backlash in the US." *Review of International Political Economy* pp. 1–24.

Flaherty, Thomas and Ron Rogowski. 2021. "Rising Inequality as a threat to the liberal international order." *International Organization* .

Foote, Andrew, Michel Grosz and Ann Stevens. 2019. "Locate your nearest exit: Mass layoffs and local labor market response." *ILR Review* 72(1):101–126.

Freitas, Guilherme Vergara Marçal de et al. 2020. How are elections affected by an external shock? The 'China shock'in Brazilian politics PhD thesis.

Frieden, Jeffry. 2022. "Attitudes, Interests, and the Politics of Trade." *forthcoming, Political Science Quarterly* .

Frieden, Jeffry A. 1991. "Invested interests: the politics of national economic policies in a world of global finance." *International Organization* 45(4):425–451.

Fujita, Masahisa, Paul R Krugman and Anthony J Venables. 2001. *The spatial economy: Cities, regions, and international trade.* MIT press.

Fujita, Shigeru, Garey Ramey et al. 2006. The cyclicality of job loss and hiring. Technical report Federal Reserve Bank of Philadelphia.

Gabriel, Stuart A, Janice Shack-Marquez and William L Wascher. 1993. "The effects of regional house price and labor market variability on interregional migration: Evidence from the 1980s." *Housing markets and residential mobility* 31:51.

Ganong, Peter and Daniel Shoag. 2017. "Why has regional income convergence in the US declined?" *Journal of Urban Economics* 102:76–90.

Gaubert, Cecile, Patrick M Kline and Danny Yagan. 2021. Place-based redistribution. Technical report National Bureau of Economic Research.

Gazmararian, Alexander F and Helen V Milner. 2021. "Political Cleavages and Changing Exposure to Global Warming.".

Glaeser, Edward L, Hedi D Kallal, Jose A Scheinkman and Andrei Shleifer. 1992. "Growth in cities." *Journal of political economy* 100(6):1126–1152.

Gourevitch, Peter Alexis. 1986. *Politics in hard times: comparative responses to international economic crises.* Cornell University Press.

Greenland, Andrew, John Lopresti and Peter McHenry. 2019. "Import competition and internal migration." *Review of Economics and Statistics* 101(1):44–59.

Guisinger, Alexandra. 2009. "Determining trade policy: Do voters hold politicians accountable?" *International Organization* 63(3):533–557.

Guisinger, Alexandra. 2016. "Information, gender, and differences in individual preferences for trade." *Journal of Women, Politics & Policy* 37(4):538–561.

Guisinger, Alexandra and Elizabeth N Saunders. 2017. "Mapping the boundaries of elite cues: How elites shape mass opinion across international issues." *International Studies Quarterly* .

Hafner-Burton, Emilie M, Stephan Haggard, David A Lake and David G Victor. 2017. "The behavioral revolution and international relations." *International Organization* 71(S1):S1–S31.

Hainmueller, Jens and Michael J Hiscox. 2006. "Learning to love globalization: Education and individual attitudes toward international trade." *International Organization* 60(2):469–498.

Hajnal, Zoltan and Michael U Rivera. 2014. "Immigration, Latinos, and white partisan politics: The new democratic defection." *American Journal of Political Science* 58(4):773–789.

Hakobyan, Shushanik and John McLaren. 2016. "Looking for local labor market effects of NAFTA." *Review of Economics and Statistics* 98(4):728–741.

Hazlett, Chad and Matto Mildenberger. 2020. "Wildfire exposure increases pro-environment voting within democratic but not republican areas." *American Political Science Review* 114(4):1359–1365.

Healy, Andrew and Gabriel S Lenz. 2017. "Presidential voting and the local economy: Evidence from two population-based data sets." *The Journal of Politics* 79(4):1419–1432.

Hirschman, Albert O. 1970. *Exit, voice, and loyalty: Responses to decline in firms, organizations, and states.* Vol. 25 Harvard university press.

Hiscox, Michael J. 1999. "The magic bullet? The RTAA, institutional reform, and trade liberalization." *International Organization* 53(4):669–698.

Hiscox, Michael J. 2002. *International trade and political conflict: commerce, coalitions, and mobility.* Princeton University Press.

Hopkins, Daniel J and Gary King. 2010. "A method of automated nonparametric content analysis for social science." *American Journal of Political Science* 54(1):229–247.

Hothorn, Torsten, Kurt Hornik and Achim Zeileis. 2015. "ctree: Conditional inference trees." *The comprehensive R archive network* 8.

Inglehart, Ronald and Pippa Norris. 2017. "Trump and the Populist Authoritarian Parties: The Silent Revolution in Reverse." *Perspectives on Politics* 15(2):443–454.

Jia, Ning, Raven Molloy, Christopher L Smith and Abigail Wozniak. 2022. "The Economics of Internal Migration: Advances and Policy Questions.".

Johnson, Kenneth M, Katherine J Curtis and David Egan-Robertson. 2017. "Frozen in place: net migration in sub-national areas of the United States in the era of the great recession." *Population and development review* 43(4):599.

Kaufmann, Robert K, Michael L Mann, Sucharita Gopal, Jackie A Liederman, Peter D Howe, Felix Pretis, Xiaojing Tang and Michelle Gilmore. 2017. "Spatial heterogeneity of climate change as an experiential basis for skepticism." *Proceedings of the National Academy of Sciences* 114(1):67–71.

Kennan, John and James R Walker. 2011. "The effect of expected income on individual migration decisions." *Econometrica* 79(1):211–251.

Kim, In Song. 2017. "Political cleavages within industry: firm-level lobbying for trade liberalization." *American Political Science Review* 111(1):1–20.

Kim, In Song and Iain Osgood. 2019. "Firms in trade and trade politics." *Annual Review of Political Science* 22:399–417.

Kline, Patrick and Enrico Moretti. 2013. "Place based policies with unemployment." *American Economic Review* 103(3):238–43.

Kline, Patrick and Enrico Moretti. 2014. "Local economic development, agglomeration economies, and the big push: 100 years of evidence from the Tennessee Valley Authority." *The Quarterly journal of economics* 129(1):275–331.

Krugman, Paul R. 1993. *Geography and trade.* MIT press.

Kuo, Jason and Megumi Naoi. 2015. "Individual Attitudes." *The Oxford Handbook of the Political Economy of International Trade* p. 99.

Lake, David A. 2009. "Open economy politics: A critical review." *The Review of International Organizations* 4(3):219–244.

Larsen, Martin Vinæs, Frederik Hjorth, Peter Thisted Dinesen and Kim Mannemar Sønderskov.

2019. "When Do Citizens Respond Politically to the Local Economy? Evidence from Registry Data on Local Housing Markets." *American Political Science Review* pp. 1–18.

Lueders, Hans. 2021. "Rooted at Home: How Domestic Migration Separates Voters into National and Local Electorates." *Available at SSRN 3891174* .

Malamud, Ofer and Abigail Wozniak. 2012. "The impact of college on migration evidence from the Vietnam generation." *Journal of Human resources* 47(4):913–950.

Malgouyres, Clément. 2017. "The impact of Chinese import competition on the local structure of employment and wages: evidence from France." *Journal of Regional Science* 57(3):411–441.

Mansfield, Edward D and Diana C Mutz. 2009. "Support for free trade: Self-interest, sociotropic politics, and out-group anxiety." *International Organization* 63(03):425–457.

Margalit, Yotam. 2019. "Economic insecurity and the causes of populism, reconsidered." *Journal of Economic Perspectives* 33(4):152–70.

Marshall, Alfred and Mary Paley Marshall. 1920. *The economics of industry.* Macmillan and Company.

Mayda, Anna Maria. 2006. "Who is against immigration? A cross-country investigation of individual attitudes toward immigrants." *The review of Economics and Statistics* 88(3):510–530.

Mayda, Anna Maria and Dani Rodrik. 2005. "Why are some people (and countries) more protectionist than others?" *European Economic Review* 49(6):1393–1430.

Milner, Helen V. 2021. "Voting for populism in Europe: Globalization, technological change, and the extreme right." *Comparative Political Studies* 54(13):2286–2320.

Molloy, Raven, Christopher L Smith and Abigail Wozniak. 2011. "Internal migration in the United States." *Journal of Economic perspectives* 25(3):173–96.

Moretti, Enrico. 2012. *The new geography of jobs.* Houghton Mifflin Harcourt.

Mosley, Layna. 2000. "Room to move: International financial markets and national welfare states." *International organization* 54(4):737–773.

MSDC. 2021. "Geocorr 2000.".

URL: *https://mcdc.missouri.edu/geography/*

Mutz, Diana C. 2018. "Status threat, not economic hardship, explains the 2016 presidential vote." *Proceedings of the National Academy of Sciences* 115(19):E4330–E4339.

Owen, Erica and Noel P Johnston. 2017. "Occupation and the political economy of trade: Job routineness, offshorability, and protectionist sentiment." *International Organization* 71(4):665–699.

Przeworski, Adam and Michael Wallerstein. 1988. "Structural dependence of the state on capital." *American Political Science Review* 82(1):11–29.

Redding, Stephen J. 2020. "Trade and geography.".

Rho, Sungmin and Michael Tomz. 2017. "Why Don't Trade Preferences Reflect Economic Self-Interest?" *International Organization* 71(S1):S85–S108.

Riker, William H. 1982. "The two-party system and Duverger's law: an essay on the history of political science." *American political science review* 76(4):753–766.

Rogowski, Ronald. 1987. "Trade and the variety of democratic institutions." *International organization* 41(02):203–223.

Rogowski, Ronald. 1989. "Commerce and coalitions: How trade affects domestic political alignments." *Princeton, NJ* .

Ruggles, Steven, Sarah Flood, Ronald Goeken, Megan Schouweiler and Matthew Sobek. 2021. "PUMS USA: Version 12.0 [dataset].".

URL: *https://doi.org/10.18128/D010.V12.0*

Saks, Raven E and Abigail Wozniak. 2011. "Labor reallocation over the business cycle: New evidence from internal migration." *Journal of Labor Economics* 29(4):697–739.

Scheve, Kenneth F and Matthew J Slaughter. 2001. "What determines individual trade-policy preferences?" *Journal of International Economics* 54(2):267–292.

Sjaastad, Larry A. 1962. "The costs and returns of human migration." *Journal of political Economy* 70(5, Part 2):80–93.

Tabellini, Marco. 2020. "Gifts of the immigrants, woes of the natives: Lessons from the age of mass migration." *The Review of Economic Studies* 87(1):454–486.

Times, The New York. 1992. "The 1992 Campaign; Transcript of 2d TV Debate Between Bush, Clinton and Perot." *The New York Times* .
URL: *https://www.nytimes.com/1992/10/16/us/the-1992-campaign-transcript-of-2d-tv-debate-between-bush-clinton-and-perot.html*

Tolbert, Charles M and Molly Sizer. 1996. US commuting zones and labor market areas: A 1990 update. Technical report.

Vandenbrande, Tom, Laura Coppin and Peter Van der Hallen. 2006. "Mobility in Europe. Analysis of the 2005 Eurobarometer survey on geographical and labour market mobility.".

Walter, Stefanie. 2021. "The backlash against globalization." *Annual Review of Political Science* 24(1):421–442.

Winkler, Hernan et al. 2010. "The effect of homeownership on geographic mobility and labor market outcomes." *SSRN eLibrary* .

Wright, Marvin N, S Wager and P Probst. 2016. "Ranger: A fast implementation of random forests." *R package version 0.5. 0, URL https://CRAN. R-project. org/package= ranger* .

A Geographic Mobility and Trade Politics: Numerical Examples

Consider our simple two-region story where trade causes positive externalities in the core while the opposite occurs in the periphery. An individual's migration calculus in this context is commonly conceptualized as an investment decision: i.e., an individual relocates if the benefits to doing so are greater than the costs (Sjaastad, 1962; Bonin et al., 2008). For a voter i in the periphery, this implies the following expected utility to moving:

$$NetBenefit = U_{i,core} - U_{i,periphery} - C_i$$

Here, voter i's expected net benefits to moving from the periphery to the core $NetBenefit$ equals her expected utilities to living in the core versus the periphery, $U_{i,core}$ and $U_{i,periphery}$, respectively, as well as her expected moving cost C_i. If the value of the function is positive, then periphery voters receive net benefits from moving to the core; otherwise, they stay and receive utility $U_{i,periphery}$.[78]

First assume a world without trade where factor prices are equalized across our two regions at \$2 in expected utility. To keep the story simple, we will focus only on unskilled workers. With regional utilities equalized, $U_{i,core} - U_{i,periphery}$ will equal zero for all unskilled workers, thus producing zero migration incentives for all positive moving costs.

Now consider a trade shock where large externalities simultaneously increase the expected utility to low-skill in the core to \$4 and decrease low-skill utility in the periphery to \$1. We now get the expected utilities found in Table 2.

Consider first the intuitive case of those in the core. No matter the value of their relocation cost C, as long as it is greater than zero, they will always find it optimal to stay in the suddenly more prosperous core.[79] Given that they expect to stay, trade increases their expected utility to

[78] Of course, migration itself creates its own externalities that are ignored here for simplicity. As more workers leave the periphery, the more attractive a place the periphery becomes since there are less workers competing for the same jobs (Moretti, 2012).

[79] That is, a core worker's net benefit to moving equals $U_{periphery} - U_{core} - C = 1 - 4 - C = -3 - C$, which is always less than the \$4 she get from staying $\forall$ C$\geq$0.

Table 2: Political Alignments on Trade, by Region and Individual Mobility

	Geographic Mobility	$NetBenefit$ / optimal choice	Pre-trade Expected Utility	Post-trade Expected Utility	Political Alignment
Periphery	High ($C_i = \$1$)	\$2 $\rightarrow$ move	\$2	\$3 (win)	pro-trade
	Medium ($C_i = \$2$)	\$1 $\rightarrow$ move	\$2	\$2 (indifferent)	indifferent
	Low ($C_i = \$3$)	\$0 $\rightarrow$ stay	\$2	\$1 (lose)	anti-trade
Core	$\forall\ C_i \geq 0$	-\$3-C $\rightarrow$ stay	\$2	\$4 (win)	pro-trade

\$4 from \$2.

For periphery voters, however, the result depends on individuals' mobility. During the pre-trade period, everyone in the periphery expected \$2; after trade, they all stand to lose \$1 if they stay. First consider how the high-mobility workers responds: given their low moving cost, they optimally expect to move since they net \$2 to doing so. Trade therefore allows them to expect \$4 in the core minus their moving cost for the total of \$3—a one dollar increase in utility over their pre-trade condition. Politically, they should increase their support of free trade.

Now lets consider another periphery voter with a medium level of mobility at $C = \$2$. They also expect to benefit from moving; however, their higher relocation cost means that this move only allows them to maintain their pre-trade utility. For them, trade does not make them better or worse off, but mobility allows them to avoid an expected loss of \$1 if they instead stay in the periphery. Their political response is therefore ambiguous.

Finally, we have a periphery voter with very low geographic mobility, with a relocation cost of \$3. This makes their expected utility to moving equal to zero, which means they optimally choose to stay, or at least flip a fair coin. No matter what they do, trade causes them to lose an expected \$1. In fact, any relocation cost $\geq$ \$3 produces this result. Expanded trade should thus make them more supportive of tariff barriers that protect their region from negative externalities.[80]

[80]Throughout this simple exercise, we have assumed no "direct" effect of trade on individual's skills (i.e., no Stolper-Samuelson effects). This allowed the intuition to focus on the trade's indirect agglomeration externalities. Since these calculations only consider low-skill workers, we can trivially add Stolper-Samuelson effects by adding an additional utility deduction in the last column that is constant across all four cases. Had we instead considered four high-skill workers, each would then receive an additional utility *boost*.

From this we can conclude that two spatial elements jointly affect individual's trade politics. First, greater agglomeration externalities to trade create winners and losers along regional cleavages (i.e., core versus periphery). Second, the higher one's migration costs within negatively afflicted regions, the greater the exposure to trade's negative externalities. However, if the regional divergence from externalities is sufficiently large, and migration costs sufficiently small, then periphery voters can actually experience expected net gains from trade.

B Measuring the NAFTA Shock

This section details the construction of the CZONE-year level NAFTA trade shock. The construction follows the procedure used in Autor, Dorn and Hanson (2013), but with some notable differences to account for particular features relevant to the analysis and context. The measurement procedure proceeds in the following steps.

1. Scrape raw bilateral trade data from UN Comtrade's bulk data API (DESA/UNSD, 2020). Bulk extraction includes annual 1991-1996 dollar value of imports and exports between the US and all trading partners at the 6-digit HS product code level. These years represent the earliest available year of data and the last year needed for the 1992-1996 ANES panel. The API was accessed on January 25, 2020 at 1:30pm Pacific.

2. Download raw Personal Consumption Expenditures data from the Bureau of Economic Analysis (BEA, 2020). Generate a price deflater to adjust UN Comtrade trade values to 1987 dollars.

3. Concord 6-digit HS product codes to 4-digit SIC 1987 industry codes using David Dorn's probabilistic crosswalk in Autor, Dorn and Hanson (2013). Then convert from 4-digit SIC 1987 industries to the 3-digit Census 1990 Industry codes used by the 1992-1996 ANES. This uses the probabilistic crosswalk used in Dorn, Hanson et al. (2019). This results in a year t and industry j level dataset with separate variables for imports ($Imports_{j,t}$) and exports ($Exports_{j,t}$) between the US and trading partners.

4. Download imputed employment data at the county-SIC87-year level. This data comes from the County Business Patterns Dataset, and is cleaned and imputed by Eckert et al. (2020). First apply the SIC87 aggregation program (cbp1990_impute.do) from Autor, Dorn and Hanson (2013) to ensure exact industry matches. Second, probabilistically match David Dorn's SIC 1987 industries to the 3-digit Census 1990 Industry codes in the cleaned trade dataset. Third, convert 1990 counties to 1990 Commuting Zones r using the crosswalk in David and Dorn (2013). Finally, generate variables for one-year lagged industry j total employment

($L_{j,t-1}$), lagged CZONE total employment ($L_{r,t-1}$) and lagged industry-CZONE employment ($L_{r,j,t-1}$).

5. Calculate the percentage change in US industry exposure to net imports from Mexico:

$$NetImports_{j,t} = \Delta ln\left(\frac{Imports_{j,t}}{L_{j,t-1}}\right) - \Delta ln\left(\frac{Exports_{j,t}}{L_{j,t-1}}\right). \tag{3}$$

6. This industry level exposure is attributed to CZONEs by calculating:

$$NAFTA_{r,t} = \sum_{j} \frac{L_{r,j,t-1}}{L_{r,t-1}} NetImports_{j,t}. \tag{4}$$

Logarithmic transformations account for the extreme skewness while first-differences accounts for trending in trade flows. An additional change is made when these variables are merged to the 1992-1996 ANES: to capture the 1994-1995 rapid jump in net-imports, it is necessary to lag the measure by one year (reference Figure 1). However, no trade data exist to construct a 1990-1992 change. To avoid missing trade data for the ANES 1992 panel, the lag is only applied to the 1996 panel. This asymmetric lag is permitted by the temporal stability of pre-NAFTA trade flows seen in Figure 1.

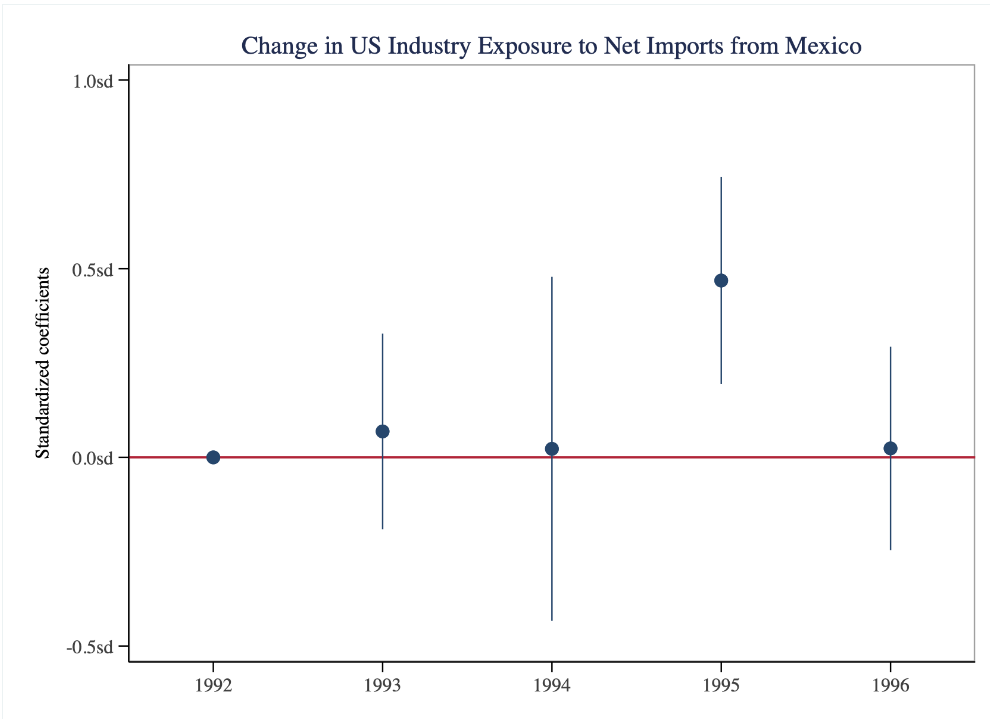

Figure 12: Magnitude of the NAFTA shock, in terms of standard deviation changes in net imports to the US from Mexico. These estimates come from a regression of industry net exposure on year indicator variables controlling for the log of industry employment and industry fixed effects. Standard errors are clustered at the industry level.

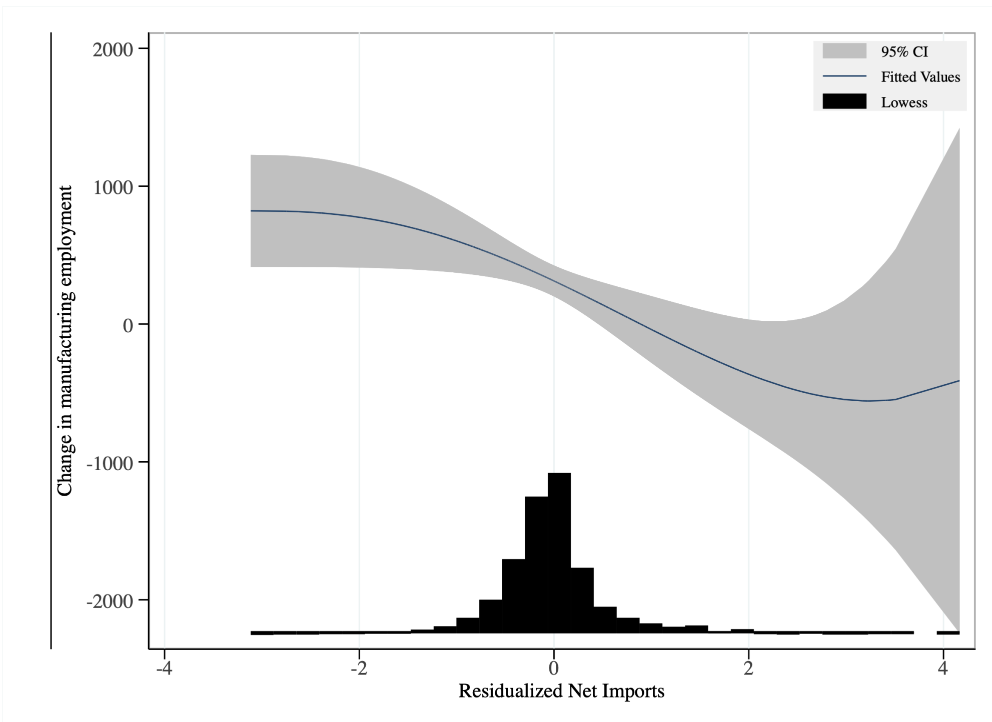

Figure 13: Bivariate relationship between the one year change in CZONE manufacturing employment and the residualized change in net imports from Mexico, which is created by taking the residuals of a regression of CZONE net exposure on lagged manufacturing specialization.

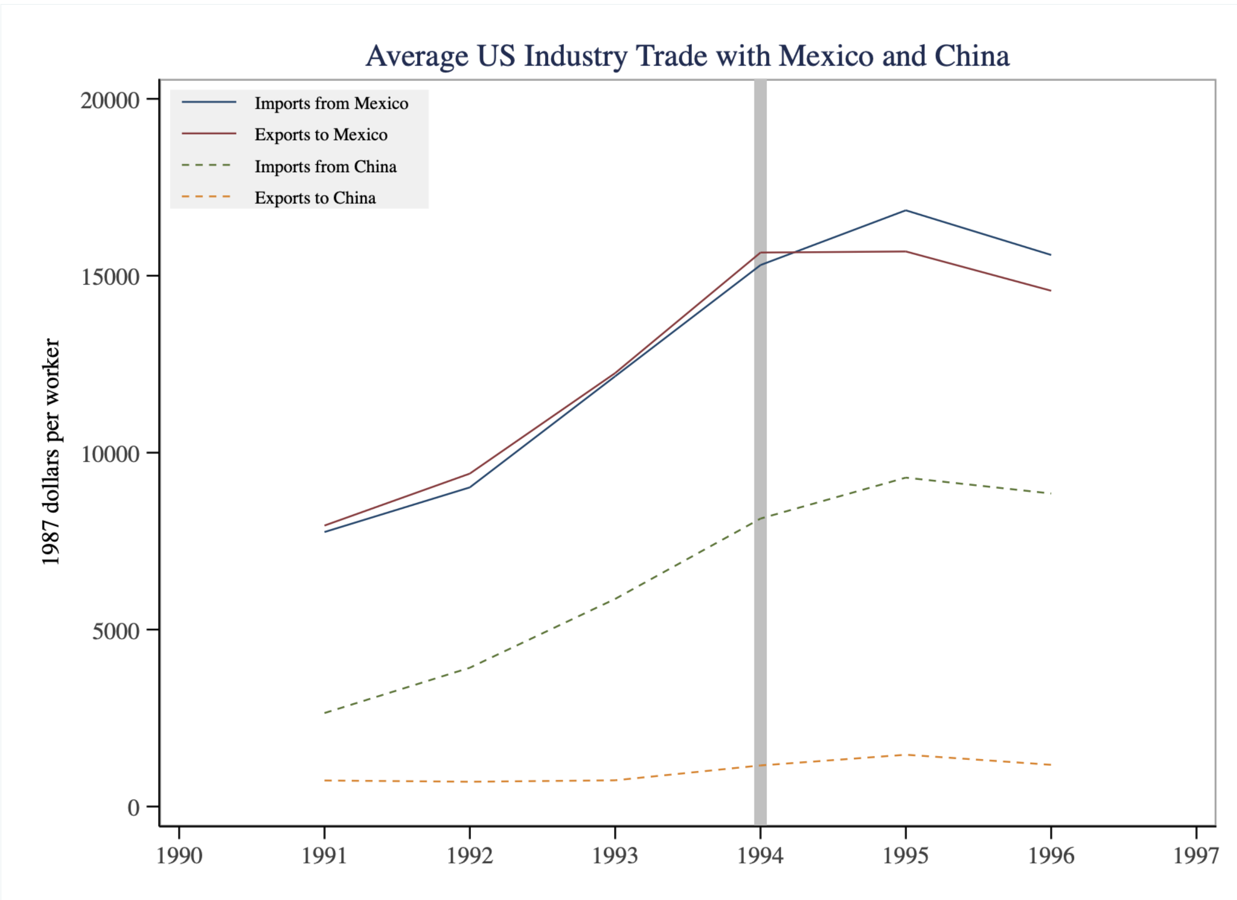
Average US Industry Trade with Mexico and China
Imports from Mexico
Exports to Mexico
Imports from China
Exports to China
1987 dollars per worker
20000
15000
10000
5000
0
1990
1991
1992
1993
1994
1995
1996
1997

Figure 14

C Measuring Geographic Mobility

The measurement procedure proceeds in the steps shown in Figure 15. I first collect data from the US Census American Community Survey microdata, specifically from the 2000 5% sample, collected by IPUMS USA (Ruggles et al., 2021). This data was access on January 18th, 2021.

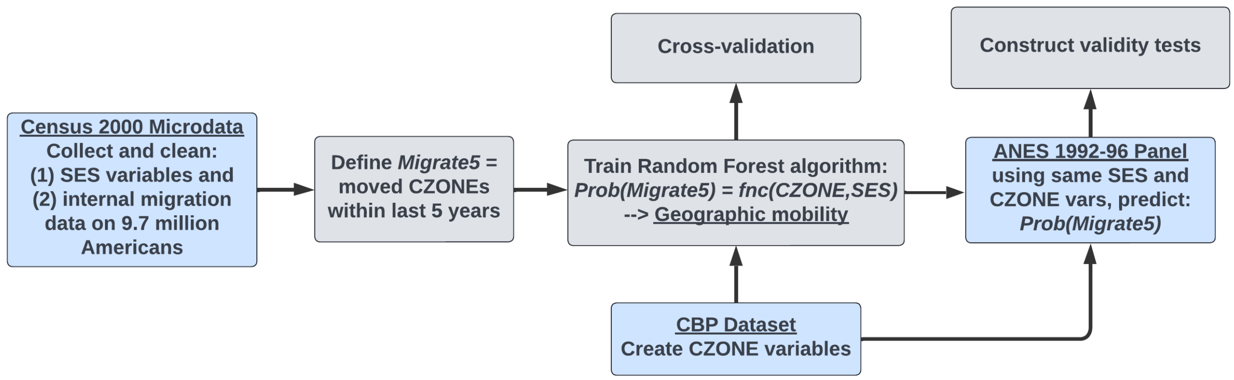

Figure 15: Geographic mobility measurement procedure. Blue boxes denote steps with data sources while gray boxes denote procedural steps.

On sample selection, I use the Census 2000 5% sample to train the classifier for three reasons. First, this wave measures respondents' migration patterns between 1995 and 2000, which is the most relevant to the period under investigation. The second advantage deals with the time period over which migration is measured. Depending on the wave, the Census offers three alternative ways of measuring migration: by comparing an individual's current location to: (i) their birth location (lifetime moves), (ii) their location five years ago, or (iii) their location 12 months ago. The 2000 wave marked the last time the Census used the preferred five-year interval. This minimizes two sources of measurement error. The first is from intervals that are too long that cannot detect moves that occurred between the end points. For example, someone born in Massachusetts who spend their whole life moving around, but eventually retired near family back in Massachusetts would be inevitably mis-labeled a non-mover. The second source of measurement errors arises from intervals that are too short, which can mis-classify true movers who moved just outside the reference window. A third and final advantage to the Census 2000 wave is that it codes locations using Public Use Microdata Areas (PUMA) which are more standard and user-ready compared to the country block groups used in earlier waves.

To clean this dataset, I re-scaled respondents' socio-economic variables to concord with the scales of the ANES versions of the variables. For instance, the occupational codes in the Census sample must correspond 1-to-1 to the occupational codes use in the ANES sample. The scales were created according to two decision tules. First, the less-granular of the two scalings was adopted. Second, error codes retained their own category. Common error codes are "DK," and "NA." These are combined and retained for classification only since they likely are predictive of mobility status. Identical scales are necessary in order for the Census-trained algorithm to identify patterns in the ANES sample.

I pruned the Census 2000 sample to ensure that the training set contained the same representative group of Americans as the ANES test set. Following ANES sampling procedures, I dropped all Census observations under the age of 18, and all those living in "ineligible group dwelling units." This category describes places owned or managed by an organization that provides services to unrelated people who live there in a group setting. Examples include the residents of convalescent hospitals, college dormitories, prisons, barracks, and homeless shelters. This pruning is necessary to ensure that the algorithm is trained on the same distribution of cases on the ANES. I also drop cases who moved to or recently from Hawaii, Alaska, and Puerto Rico since CZONEs only cover the continental US.[81] Finally, to focus on internal mobility, I drop migrants who moved from abroad. This results in a sample size of 9,722,725.

Since migration incentives theoretically reflect local economic conditions in addition to SES variables, I generate CZONE employment growth and industry specialization indices from the County Business Patterns dataset used to construct the NAFTA trade shock variable. I merge this CZONE level data in to the Census and ANES samples.

Define $Migrate5 = 1$ if respondent i lived in a different Commuter Zone[82] on April 1, 1995 (i.e., moved labor markets within the five years prior to the time of interview.)

[81] To be clear, this does not drop immigrants who came to the US greater than five years prior to the survey.

[82] Consistent with the NAFTA shock measure, Commuting Zones offer the ideal unit of geography since they delineate economically meaningful boundaries between places of work and residence. Therefore, the use of CZONEs allows this measure to capture migrations due to labor market adjustment rather than residential housing adjustment (Bell et al., 2002).

Identifying respondents' current and prior CZONEs requires two types of geographic merges. To preserve respondent confidentiality, IPUMS USA constructs two types of regions. First, Public Use Microdata Areas (PUMAs) denote one's current residence, and demarcate geographies that generally follow the boundaries of county groups and Census-defined "places." Also, because they are limited to population sizes between 100,000 and 200,000, they can sometimes cover larger or smaller areas than CZONEs, which are instead demarcated by commute-to-work patterns. Second, migration PUMAs (MIGPUMAs) are aggregations of PUMAs used to identify respondent's prior region of residence. Therefore, to get comparable pre-move and post-move geographies, I first construct a PUMA to MIGPUMA crosswalk. I then construct a second crosswalk that probabilistically matches MIGPUMA 2000 codes to CZONE 1990 codes based on population weights constructed using data from the Missouri Census Data Center (MSDC, 2021).

These probabilistic merges create duplicate observations for about 7% of sample that changed CZONEs with some uncertainty. That is, they moved between MIGPUMAs that may or may not have correspondent with a move between CZONEs. This situation can arise when the reported arrival and destination MIGPUMAs overlap with the same CZONE. The following procedure reconciles these duplicate cases. Each duplicate receives their own binary migration status, which differs by their probability-weighted MIGPUMA-CZONE pair. I then recover a single migration status by taking a weighted average across duplicates and assigning binary moved or non-moved status by a 50 percent threshold rule. If the weighted average migration status is greater than 0.5, then respondent i is coded as having migrated between CZONEs. The weight is defined by the population allocation factors from MIGPUMAs to CZONEs. The result is a single internal migration status for each observation.

Out of the 9,722,725 cases 15 percent were found to have moved CZONEs within the prior five years, compared to 16.5 percent between MIGPUMAs. To put these moves into perspective, 41.2 percent made any kind of move while 25 percent moved only within their PUMA. About 8 percent of all moves were across state boundaries.

Once both the Census and ANES datasets are cleaned, I randomly split the Census dataset into a training set (75 percent of the sample) and validation set (25 percent). Each are balanced on the two categories (moved versus not-moved) in order to help the algorithm learn both categories with identical sample sizes, and to ensures that the performance statistics are compared to a benchmark of 50 percent accuracy.

Using the training set, train a Random Forest classifier using the `ranger` package in R to predict what makes a respondent move or not move as a function of standard socio-economic status variables and region level indicators and economic conditions. Comprehensively, these include: sex, age, marital status, state of birth, current MIGPUMA, current Census region, 3-digit Census 1990 industry codes, 2-digit Census 1980 occupation codes, education, family total income, race, Hispanic, number of years lived in current dwelling, employment status, ownership status of dwelling, and commuter zone industry specialization (Hirshman-Herfindahl Index). I dropped CZONE change in employment since collinearity with region indicator variables result in worse predictive performance.

While many alternative algorithms are possible, Random Forest classifiers are less prone to outliers and over-fitting. Since the algorithm uses decision trees as its base learner, atypical observations get isolated into small leaves (i.e., small subspaces of the original space). Furthermore, while a single decision tree suffers from notoriously over-fitting, a random forest avoids this by randomly considering subsamples of the data (bootstrapping) combined with randomly selected features to learn from. The ensemble of these individual trees performs well out of sample.

I tune the Random Forest's two hyperparameters—the number of trees (*ntree*) and the number of randomly selected features used to grow each tree (*mtry*)—using a grid search that explores predictive performance across every combination of hyperparameters provided. In this case I set the number of trees to *ntree*=500, 1000, 2500. The value of *mtry* must lie between one and the number of features, with that default being the square root of the features (about 4). I thus explore all *mtry* values in the 1:15 interval. This results in a total of 45 combinations. The results are depicted in Figure 16. The combination of hyperparameters that achieves the lowest out of bag (OOB) error has an *mtry* of 3 and *ntree* of 2500. However, since we prefer simpler models to complex ones, we opt for the *ntree* of 500 since its performance was indistinguishable.

This allows for an optimal Random Forest classifier whose out-of-sample performance on the 25 percent withheld validation set is summarized by the confusion matrix in Table 3. From this table, the accuracy can be calculated as 0.84 while the sensitivity and specificity are 0.89 and 0.78, respectively.

Once validated, I deploy the classifier on the unlabeled ANES dataset. For prediction to work, the equivalent ANES variables that were used for Census classification must also be cleaned using the same procedures, and also purged of missingness. I apply the `missRanger` package in R to impute missing values for age and marital status.

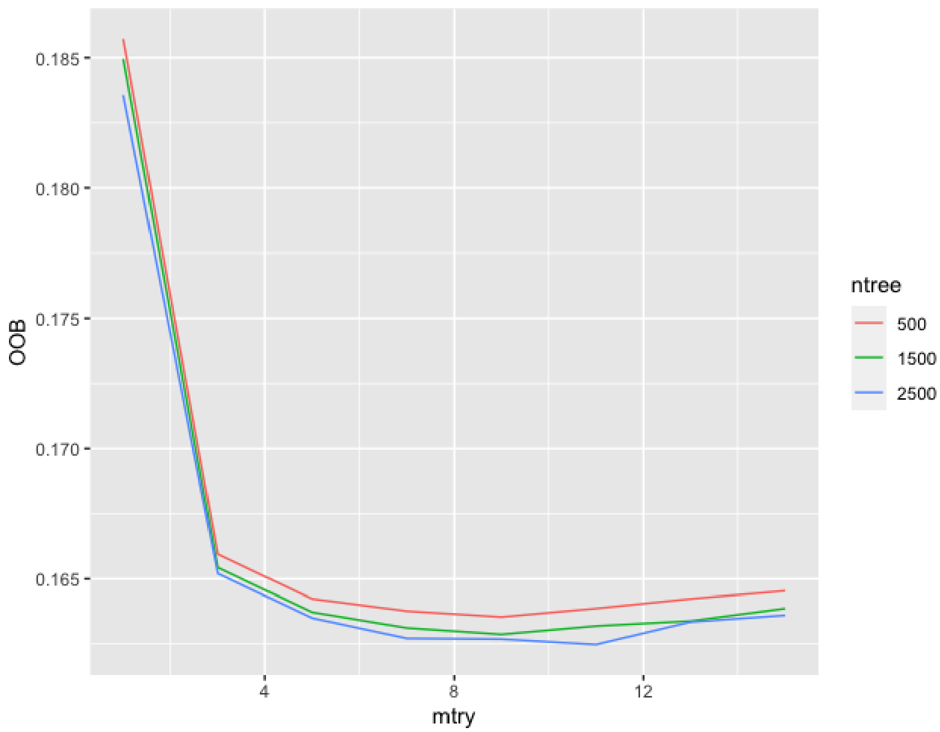

Figure 16: Random Forest Hyper-parameter Grid-search

Table 3: Confusion Matrix

		Predicted	
		immobile	mobile
True	immobile	24246	3550
	mobile	6697	27730

Variable importance metrics provide us a sense of how much each variable contributed to the model's accuracy. Figure 17 reports the average drop in model accuracy that would result from omitting a feature. These rely on unbiased conditional inference trees that are less biased than standard metrics in the presence of continuous variables and variables with many categories (Hothorn, Hornik and Zeileis, 2015). Features with higher values can be interpreted as more important. Clearly, the number of years a respondent has lived in their current dwelling holds the most sway over model accuracy–about 18 percent to be precise. I interpret this as evidence of persistence in mobility's data generating process. As latter evidence will confirm, the longer someone stays at a location, the less likely they are to move. Equivalently, residents who have recently moved into their current dwelling are likely to move again. This is consistent with findings elsewhere that past moves predict future moves (Bonin et al., 2008). This has important empirical implications, namely that we should not expect much temporal variation in mobility within individuals. Rather, as will become clearer, we should expect more variation between individuals and between locations.

The second and fourth most important variables in Figure 17 demonstrate the importance of place. Which region someone currently lives in tells us a lot about their geographic mobility. This is hardly surprising given the abundance of literature documenting enormous divergences in welfare, many measures of which are not included here, across space. However, the extent of mobility's dependence on location has not been documented in this way before. Related to the power of place, a region's industry specialization (Hirshman-Herfindahl Index) also consistently lands in the top five most predictive variables, with birthplace not too far behind. If we sum the total contribution of three regional variables, they contribute about 10 percent to the overall accuracy of the model.

Three other top variables whose importance has been documented elsewhere include age, education (of respondents plus their partners), and homeowner status (Bonin et al., 2008; Winkler et al., 2010). This consistency with prior studies builds confidence in the measure. In total, these contribute a little over five percent to overall accuracy.

In contrast, model performance is least impacted by respondents' race and ethnicity, sex, and employment status. In the middle lie features related to family income and the industry and occupational statuses of respondents and their partners, each of which contributes about a percentage point to overall predictive accuracy.

Caveats to this metric are that we cannot tell direction of relationship, and that this metric does not imply an independent contribution of each feature. In other words, the mean decrease in accuracy reflects the direct effect of a feature, as well as all of it's higher order interactions with other features.

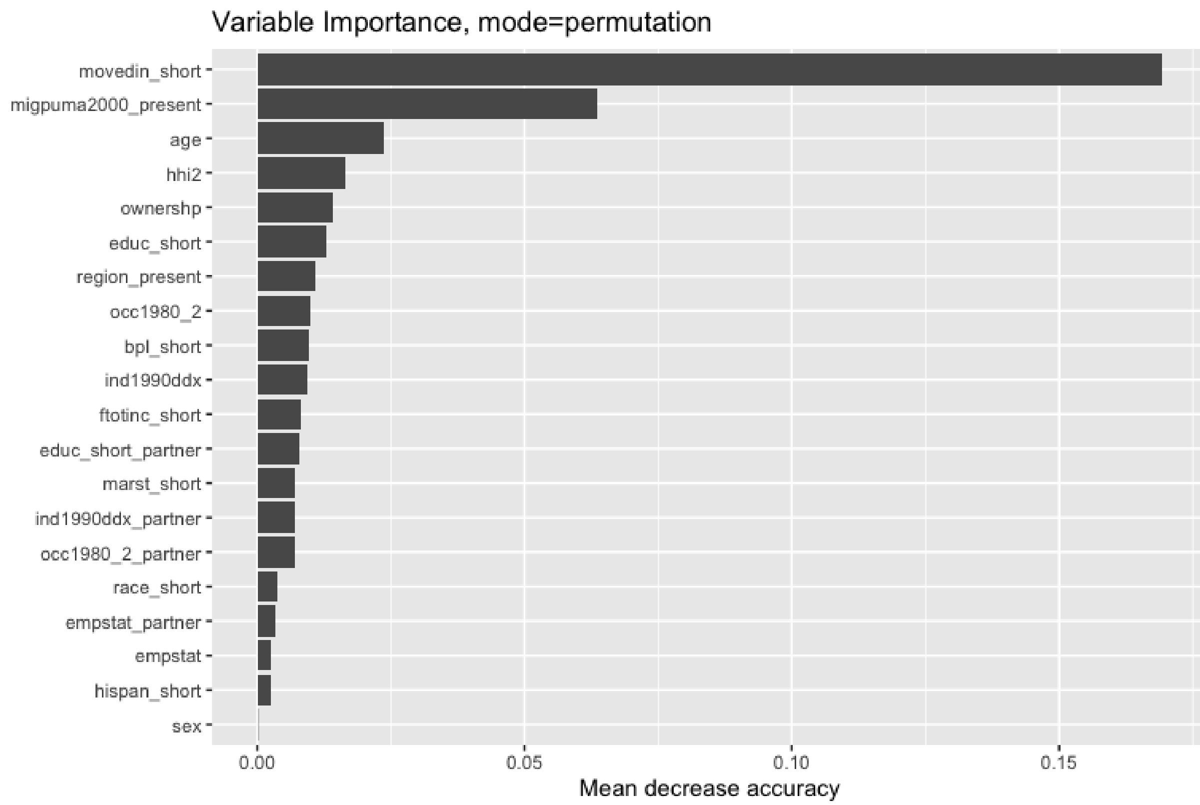

Figure 17: Variable Importance

D Supplemental Mobility Validation

Table 4 alleviates "population drift" concerns—the the determinants of mobility in the 2000 training set differ from the determinants in the 1992-1996 ANES—by showing that the predicted probability of migrating between CZONEs ($Mobile_i$) is the most important predictor of two types of panel attrition found in the 1992-1996 ANES. The first, an indicator for whether a respondent enters the study in 1992 but drops, and is never recovered, for unknown reasons by 1996,[83] shows that respondents with a high predicted probability of moving were very likely to drop out of the study.[84] The second measure of attrition is even more informative because it identifies, in 1996, relocation as the reason for dropping.[85] In both cases, predicted geographic mobility stands as the most important predictor of panel attrition, even relative to commonly cited causes like education and age. Figure 18 does justice to this by plotting the remarkably strong and precisely estimated relationship with a non-linear model.

Table 4 further demonstrates construct validity by showing that $Mobile_i$ predicts weaker social and economic linkages to one's current CZONE. ANES respondents who are very mobile are *much* less likely to invest in homes—the most important location-specific asset an average person can invest in. Socially, mobile respondents are also much less likely to report talking to their neighbors or report membership in a local community organization like church.[86] Taken together, I show a variety of evidence that all point to the same conclusion: $Mobile_i$ is a highly reliable measure of the theorized concept geographic mobility.

[83]Of the 757 respondents entering study in 1992, 160 drop out for unknown reasons.

[84]This specification includes a significant non-linear term which reflect that a positive but diminishing effect of mobility on attrition. Specifically, As mobility increases, the probability of dropping rises rapidly before leveling off.

[85]In an effort to fight off attrition, the ANES tracked down as many respondents as possible who dropped between 1994 and 1996. This indicator equals one if the respondent was successfully tracked down to their new address. Of the 596 who made it to 1996, 139 had to be tracked down due to an address change. However, to reduce survey costs, these respondents were given a shortened version of the survey over the phone rather than in-person.

[86]The ANES specifically asks respondents if they have in the last 12 months joined a community organization to solve a community problem.

	Panel Attrition		Community Linkages		
	Drops Study	Changes Address	Homeowner	Talks Neighbors	Community Org Member
Mobile	0.408*	0.750***	-0.581***	-0.205***	-0.126*
	(0.23)	(0.07)	(0.06)	(0.05)	(0.07)
Mobile^2	-0.448*				
	(0.24)				
Age	-0.033*	-0.065***	0.064***	0.018	0.001
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
Yrs of Education	-0.052**	0.010	-0.005	0.013	0.082***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Fam Total Income	0.011	-0.090***	0.176***	0.038*	0.028
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
PartyID	0.001	0.002	0.019*	0.008	-0.005
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Female	0.022	0.003	0.012	0.001	0.020
	(0.03)	(0.03)	(0.03)	(0.02)	(0.04)
N	673	539	1042	1021	1021
Year FE	n/a	n/a	x	x	x
CZONE FE	x	x	x	x	x
R-sqr	0.117	0.398	0.388	0.117	0.111

Note: *$p<0.1$; **$p<0.05$; ***$p<0.01$.

Table 4: Construct validity tests of geographic mobility. Specification are all linear OLS models with either CZONE or CZONE and year fixed effects, depending on whether the sample includes one or two panels. All standard errors adjusted for heteroscedasticity.

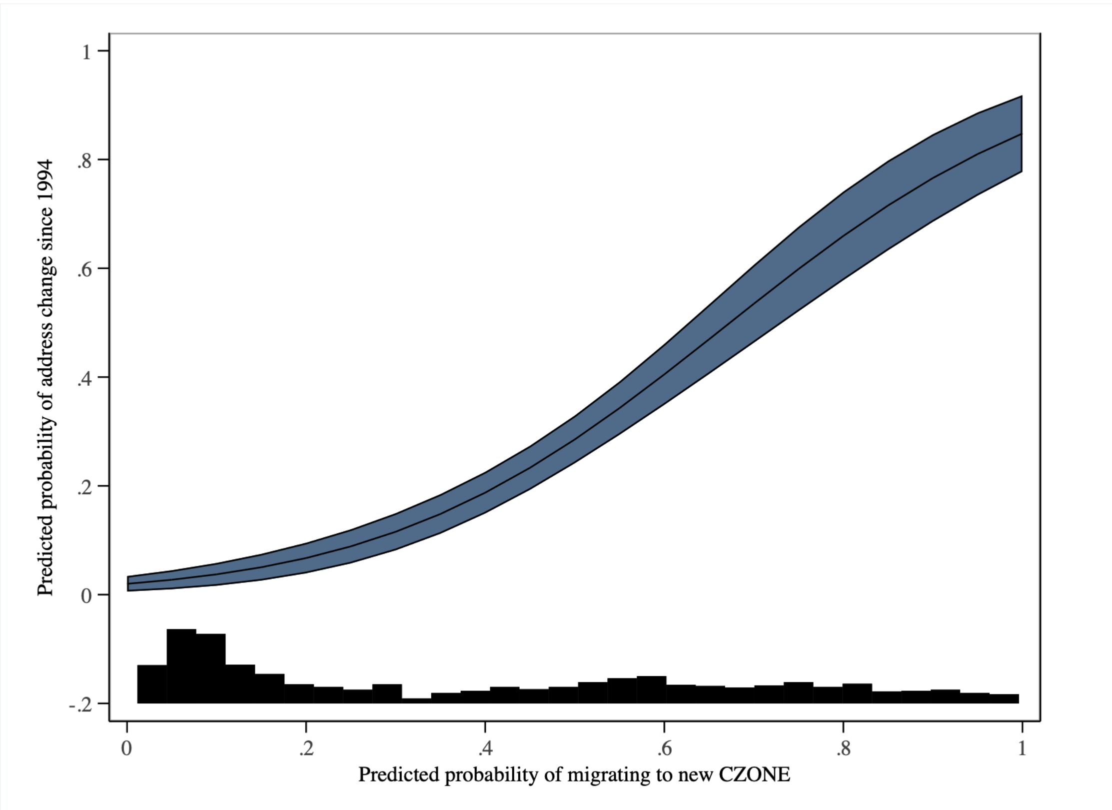

Figure 18: Construct validity tests of geographic mobility. Estimates come from a logistic version of the OLS model in specification 2 of Table 4.

E Robustness Checks:

E.1 Feeling Thermometers

To further guard against bias from strategic voting or bias from other candidate-to-candidate comparisons,[87] Figure 19 shows that the findings are robust to how voters rate Perot on a 0-100 feeling thermometer. As exposure to NAFTA increases across the range, immobile voters flip from a dislike of Perot with a temperature rating of 35/100 to a "warmth" of 60/100. Conversely, mobiles' relationship is unresponsive to local exposure to NAFTA, and this difference is statistically significant. This result is robust to the standard demographic and political controls as well as year, CZONE, and respondent fixed effects.

The effect of NAFTA and geographic motility do not seem to matter for feelings toward Clinton or either political party. In model m4 however, there is some evidence that the NAFTA shock caused a divergence between mobiles and immobiles with the former growing less favorable to Clinton.

[87] While comparing votes for Perot versus abstentions helps offset this concern, the act of voting itself my be strategic or biased for other reasons.

Candidate/Party feeling thermometers (0-100)

	Perot		Clinton		Democrats		Republicans	
	m1	m2	m3	m4	m5	m6	m7	m8
NAFTA	6.234***	5.346**	-1.140	0.934	-2.064	-1.221	0.079	0.565
	(1.49)	(2.49)	(1.78)	(2.18)	(1.36)	(1.61)	(1.05)	(1.72)
Mobile	0.143	-1.682	-2.140	-3.161	-3.951	-1.316	-4.386*	-0.008
	(2.28)	(6.86)	(2.72)	(4.29)	(2.75)	(3.63)	(2.31)	(4.29)
NAFTA × Mobile	-8.094***	-6.771*	-1.383	-5.311***	-1.084	-2.315	-1.215	-2.222
	(2.90)	(3.95)	(1.83)	(1.80)	(2.08)	(2.82)	(1.50)	(2.37)
N	1111	1159	1129	1177	1130	1179	1130	1179
Controls	x	x	x	x	x	x	x	x
CZONE FE	x		x		x		x	
Respondent FE		x		x		x		x
Year FE	x	x	x	x	x	x	x	x
Adj. R2	0.02	0.29	0.45	0.65	0.51	0.68	0.36	0.58
F-stat	6.39	1.59	132.71	10.05	155.55	10.23	70.62	7.03

Note: *p<0.1; **p<0.05; ***p<0.01.

Table 5: Feeling thermometer (0-100) regressions. Specification are all linear two-way FE models. Constants suppressed. Measures of trade exposure, manufacturing specialization, and party ID are standardized for comparison. The race/ethnicity reference group is white non-Hispanic. All standard errors clustered at the state levels.

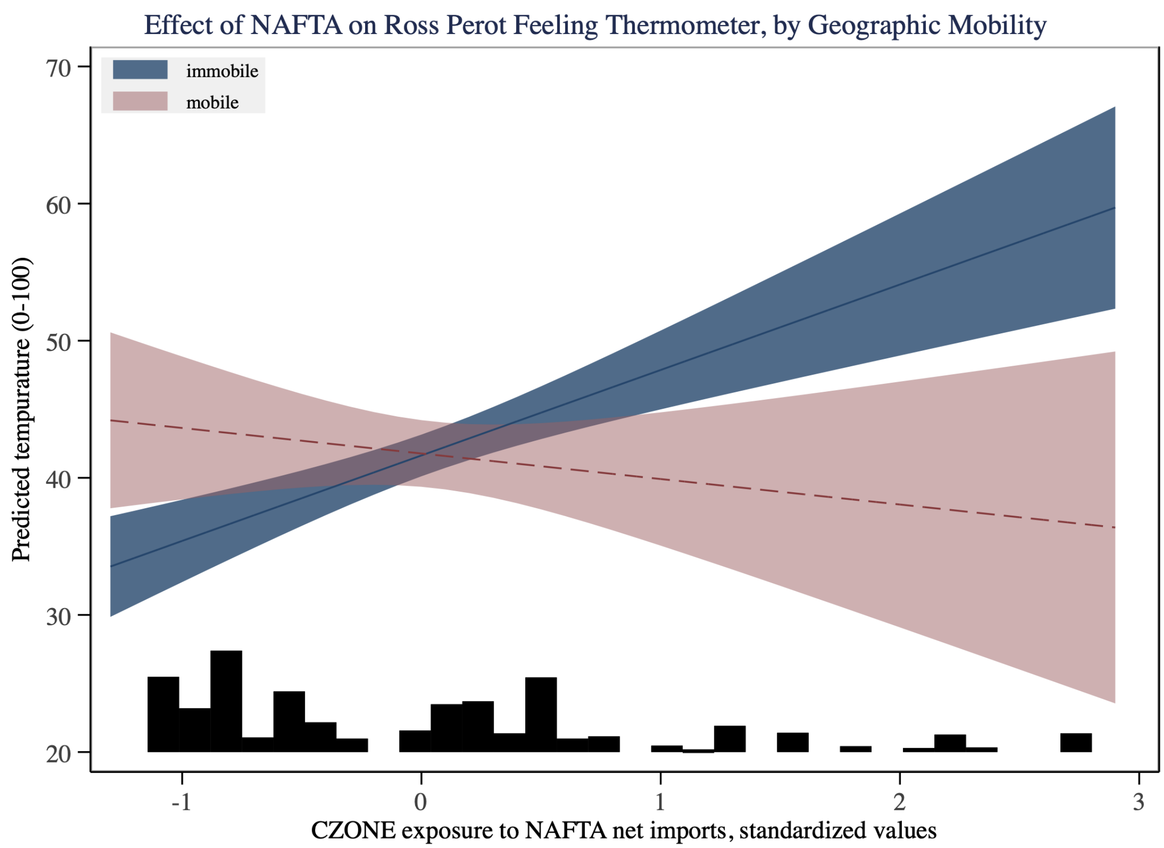

Figure 19: Robustness of mobile-immobile divergence using Ross Perot feeling thermometer. Histogram of net import exposure from NAFTA demonstrates that the marginal effects are not vulnerable to out-of-sample extrapolation. Estimates are based on the regression in Table 5.

E.2 Robustness to Alternative Geographic Mobility Measure

This section explores the robustness of the results to an alternative measure of geographic mobility: the number of years and ANES respondent reports to have lived in their current apartment or home. The analysis shows that, while the alternative measure carried significant drawbacks, it is in fact highly predictive of geographic mobility. Furthermore, the results are largely consistent with those that use the preferred measure. This helps to alleviate concerns that the machine-learning measure suffers from population drift or post-treatment bias.

The correlation between the predicted probability of between-CZONE migration within five years, and the number of years a respondent has lived in their current apartment or house, is -0.61, which is visualized in Figure 20, separately for owners of apartments versus homes. As expected, the average correlation is negative: the longer a respondents lives in their dwelling, the lower their migration probability between CZONEs. However, much of this depends on the type of dwelling.

For homeowners, the relationship is nearly linear: the longer one lives in a house, the less likely they are to move CZONEs, converging to zero probability at around 30 years. Renters however are very different. Very recent tenants (rented a unit for less than five years) have a much higher between CZONE migration probability. After about 20 years, tenants quickly become the most mobile group—a U-shaped curve. This could reflect life-cycle migration where renters reach retirement age seek to lower costs by migrating to cheap regions for retirement. It could also reflect young workers becoming more likely to leave their birth region for opportunities elsewhere. Overall, while these results make intuitive sense, they reveal the first significant problem with this measure. We ideally desire of measure that increases monotonically with geographic mobility. In contrast, mobility both increase and decrease to extreme levels as the years in current dwelling rise. By implication, use of this alternative measure must allow for non-linear effects and interpret results with care.

The reliance on dwellings, rather than regions, to indicate mobility presents the second significant problem with this measure. By focusing on dwellings, this measure is biased by residen-

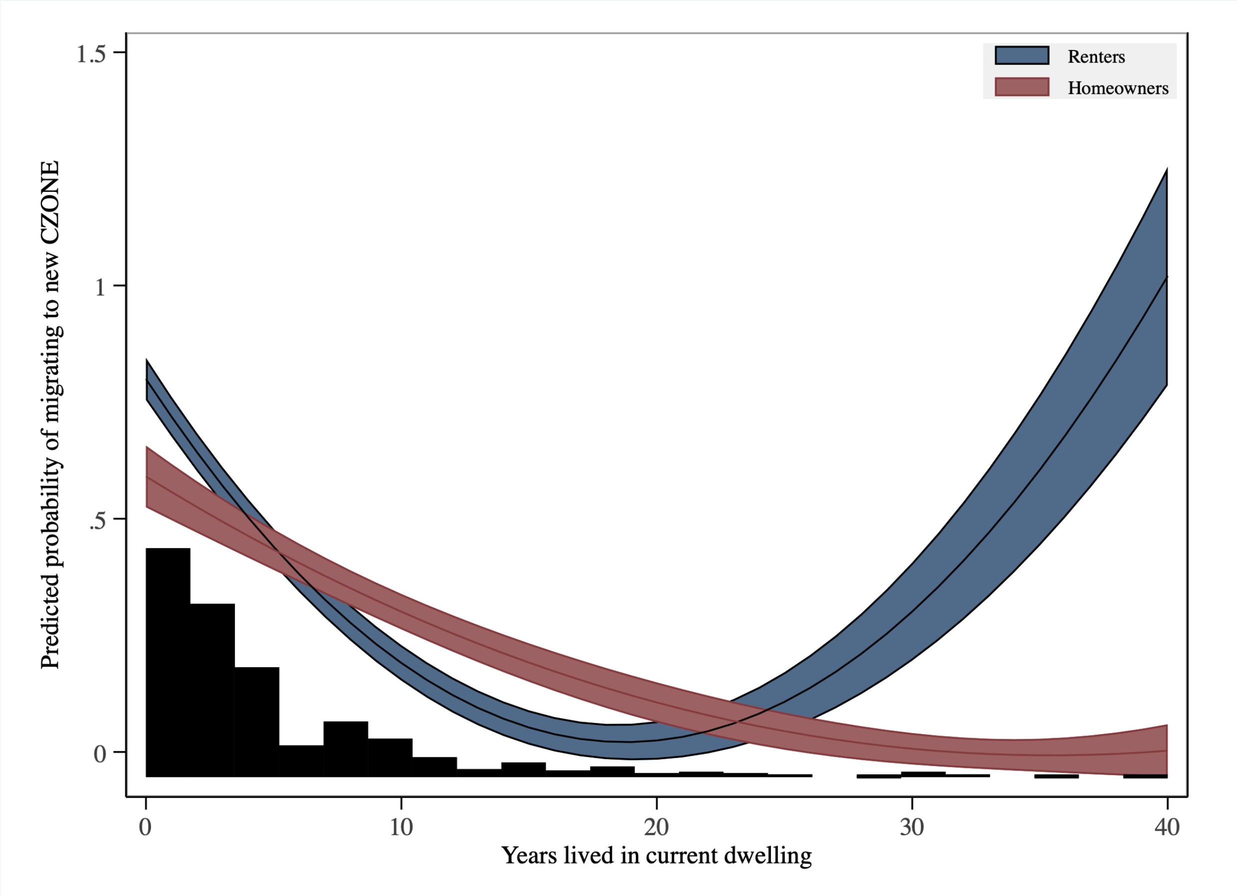

Figure 20: Predicted Migration Probability by the Number of Years in Dwelling. Estimates come from an OLS regression of predicted migration probabilities on the number of years a respondent has lived in their current apartment or home, interacted with whether a respondent owned a home versus apartment. Standard errors are clustered at the state level. The red dashed line denotes the 50 percent probability threshold above which a respondent is predicted to more likely be a migrant than not.

tial migration—movements between dwellings within the *same* CZONE. Residential migration is theoretically less relevant to labor market adjustment, and it is also more common than between labor market moves. As a result, we see that even for respondents who have lived in their current house of apartment less than five years, the probability of between-CZONE migration never reaches higher than 60 percent for homeowners and 75 percent for renters—i.e., many movers simply changed address across town.

These problems aside, the analysis below suggests that the primary results are robust to this measure of mobility. To show robustness of the Perot voting results, Figure 21 plots the marginal effects of a 10th-90th percentile increase in NAFTA exposure at different number of years in current dwelling. Consistent with the main results, higher mobility voters—those who have lived at their address for less than about 3 years, did not significantly increase their probability of voting for Perot. However, as the number of years in residence increases, so does the probability of backlash. At a peak of around 20 years of living in at the same address, residents respond to NAFTA with an average 50 percent increase in the probability of voting for Perot. The magnitude of this response is similar to the results using the machine learning measure of mobility. However, we start to see the non-linear effects of this measure invert the predicts after around 30 years of living at the same address. Past this point, the there is no effect of NAFTA, or NAFTA significantly decreases Perot voting. This makes sense given that the predicted mobility rose most dramatically within this range in Figure 20. These results control for age, education, industry-level NAFTA exposure, lagged manufacturing specialization, party ID, sex, indicators for race and ethnicity, and year and CZONE fixed effects. Figure 22 shows that this result is robust to an analysis of Ross Perot feeling thermometers. The same non-linearity emerges: high mobility voters at the extremes of this measure are unlikely to change how they feel towards Perot in response to NAFTA. However, low mobility voters grow on average 20 points "warmer" toward Perot.

On trade attitudes, Figure 24 similarly shows the consistency in the results with this alternative measure. Panel B defines low mobility as having moved to one's address within the year versus 15 years ago. While the coefficients are unsurprisingly noisier, given confounding from residential moves and nonlinearity, they broadly confirm the same story. Mobile voters are on average more supportive of free trade, regardless of local conditions. Also, in response to very high levels of local exposure to NAFTA, the two groups' attitudes diverge in opposite directions.

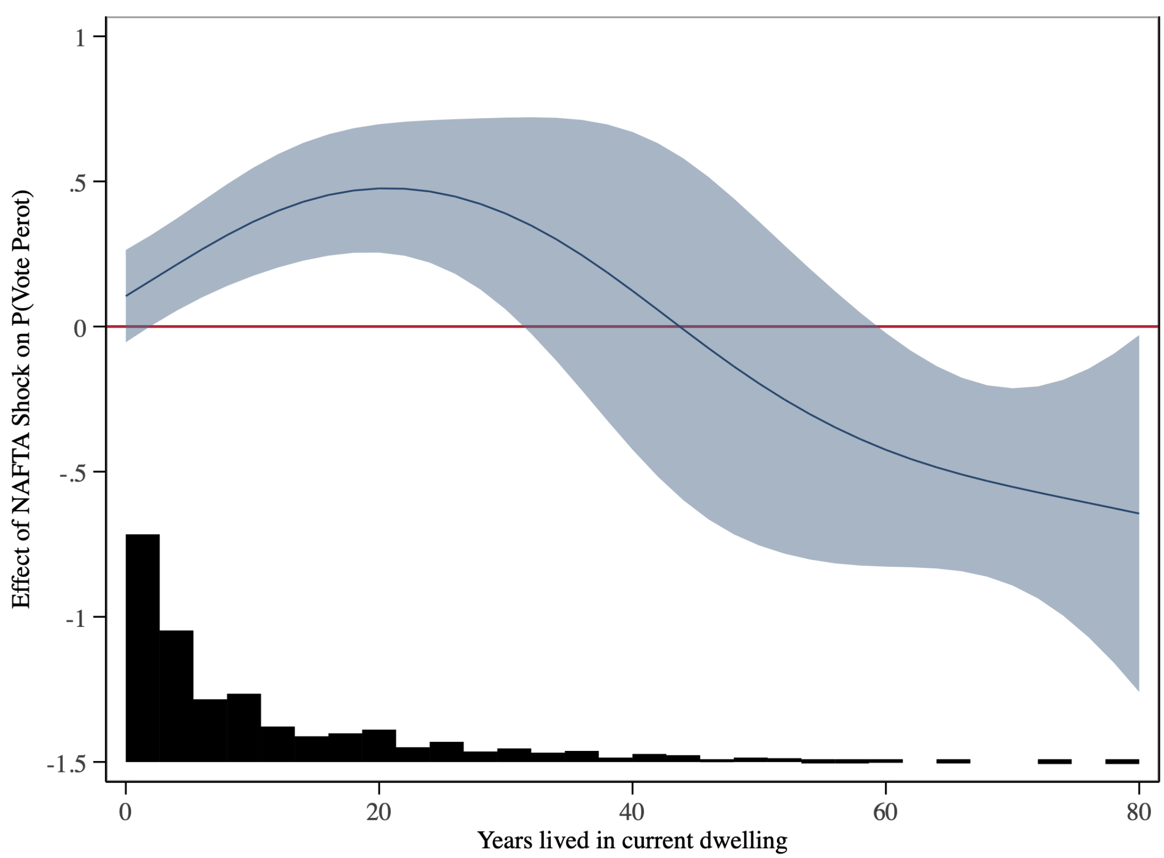

Figure 21: Marginal effect of the NAFTA trade shock on the 0-100 feeling thermometer for Ross Perot, by the number of years a respondents has lived in their current dwelling. The histogram of mobility demonstrates support for these estimates across its range. The marginal effects reflect a 10th-90th percentile increase in exposure to Mexican net imports. Control variables include age, education, industry-level NAFTA exposure, lagged manufacturing specialization, party ID, sex, indicators for race and ethnicity, and year and CZONE fixed effects. 90 percent CIs are clustered at the state level.

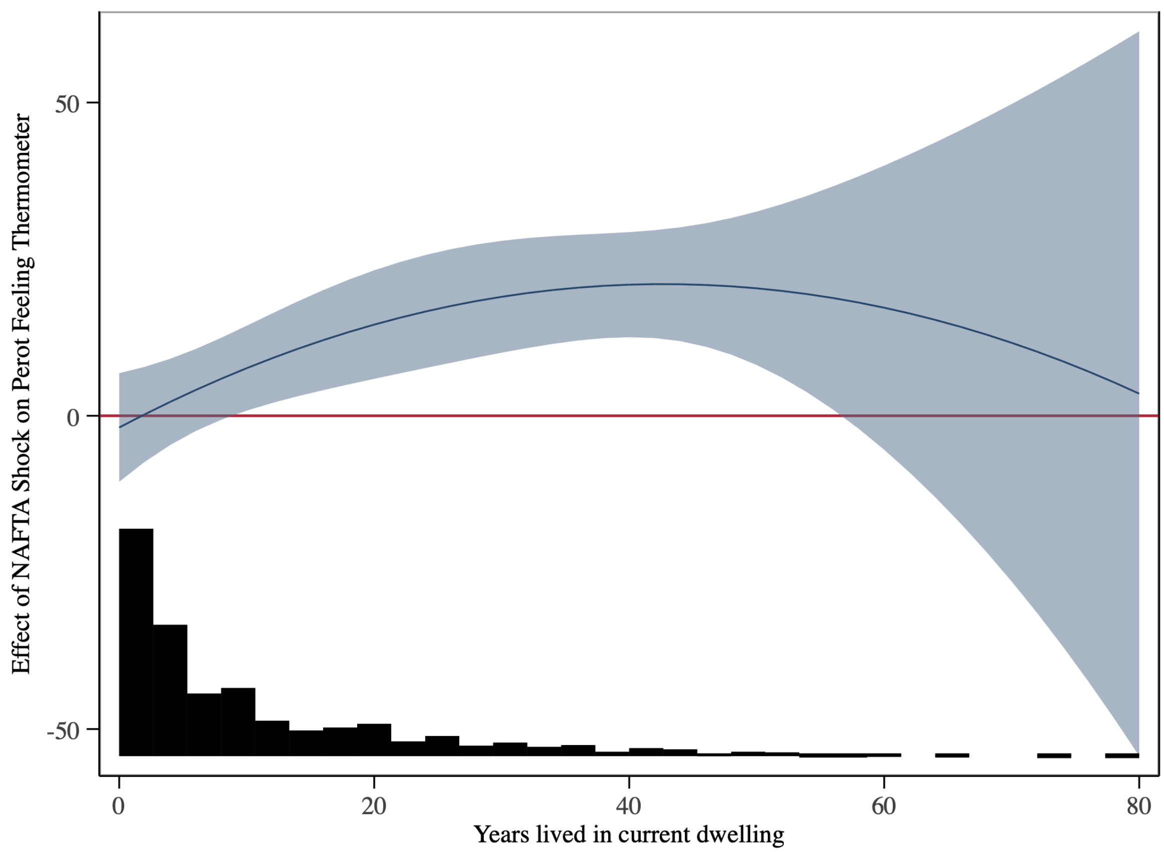

Figure 22: Marginal effect of the NAFTA trade shock on the probability of voting for Ross Perot (versus abstention), by the number of years a respondents has lived in their current dwelling. The histogram of mobility demonstrates support for these estimates across its range. The marginal effects reflect a 10th-90th percentile increase in exposure to Mexican net imports. Control variables include age, education, industry-level NAFTA exposure, lagged manufacturing specialization, party ID, sex, indicators for race and ethnicity, and year and CZONE fixed effects. 90 percent CIs are clustered at the state level.

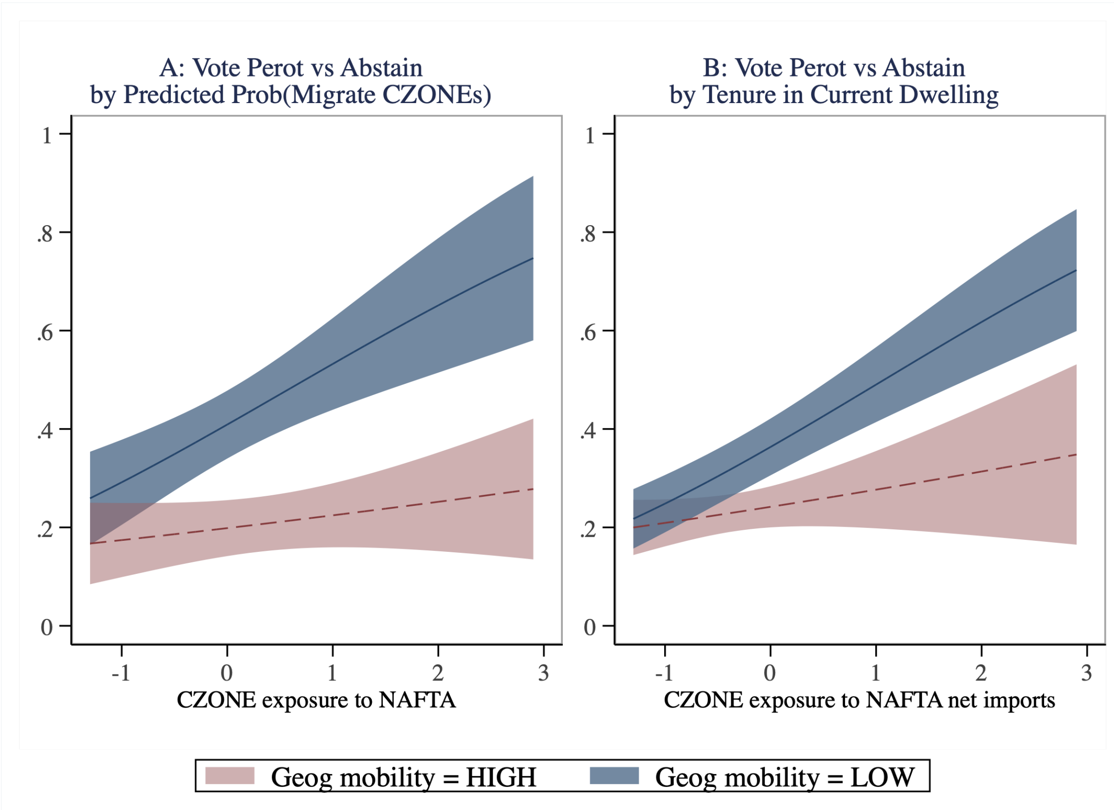

Figure 23: Robustness to Alternative Measures of Geographic Mobility. Panel A is identical to that of Panel B in Figure 8, which shows the effect of NAFTA exposure on the predicted probability of voting for Ross Perot, versus abstention, among voters with high and low predicted probabilities of migration. Panel B shows that this results is robust to an alternative measure of geographic mobility that relies on an ANES survey question which asks how long respondents have lived in their current apartment or house. This fixes mobile voters as those who just moved into their dwelling while immobiles voters are set to 15 years (the 75th percentile). The x-axes show the change in CZONE exposure to net imports from Mexico in standardized units. Results are robust to controls for age, education, industry-level NAFTA exposure, lagged manufacturing specialization, party ID, sex, indicators for race and ethnicity, and year and CZONE fixed effects. 90 percent CIs are clustered at the state level.

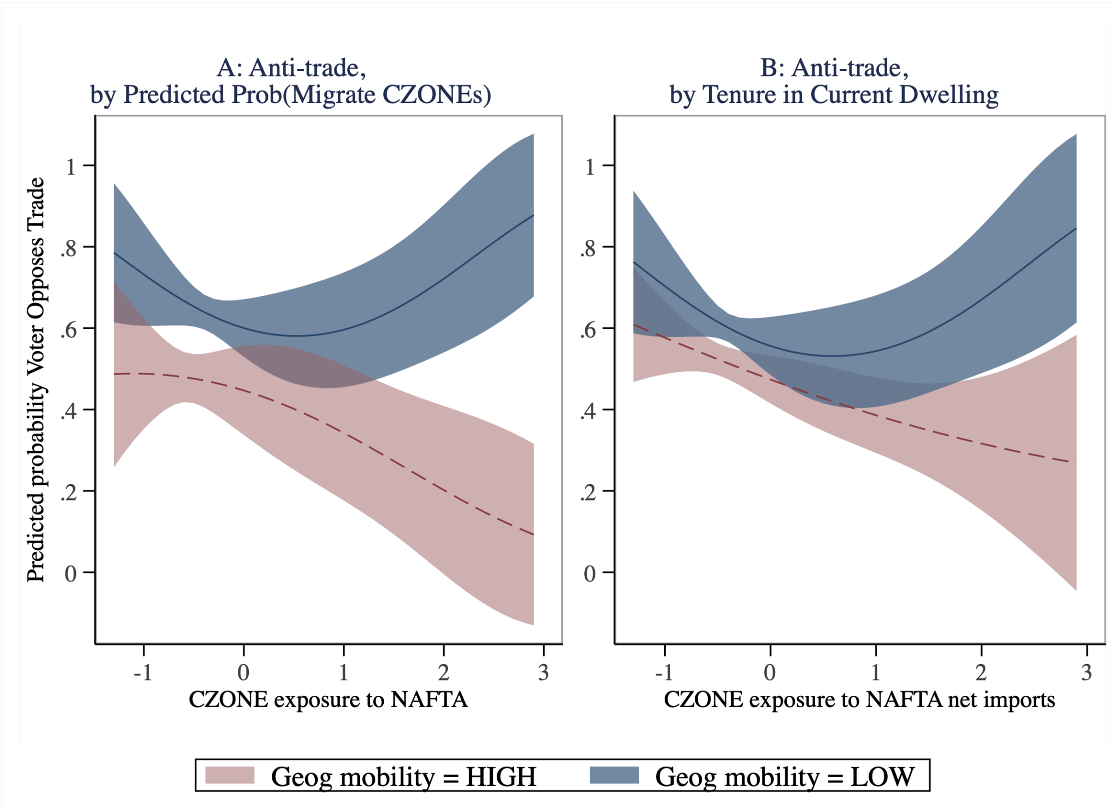

Figure 24: Robustness to Alternative Measures of Geographic Mobility. Panel A is identical to that of Panel A in Figure 9, which shows the effect of NAFTA exposure on the predicted probability of opposing free trade policies for voters with high and low predicted probabilities of migration. Panel B shows that this results is robust to an alternative measure of geographic mobility that relies on an ANES survey question which asks how long respondents have lived in their current apartment or house. This fixes mobile voters as those who just moved into their dwelling while immobiles voters are set to 15 years (the 75th percentile). The x-axes show the change in CZONE exposure to net imports from Mexico in standardized units. Results are robust to controls for age, lagged manufacturing specialization and fixed effects for year, CZONE. 90 percent CIs are clustered at the state level.

F A6: Supplementary Analysis

F.1 Voting patterns

Care must be taken when interpreting the coefficients in Table 6. Due to the interaction, the coefficient on the NAFTA shock is actually the effect of a one standard deviation increase in NAFTA exposure for individuals with low levels of mobility, while the interaction coefficient reports the *additional* effect of NAFTA for individuals with high levels of mobility. Additionally, these coefficients do not contextualize group heterogeneity in terms of each group's baseline levels of support or opposition. Due to these well-known interpretive difficulties, the marginal effects and predicted probability plots are far more informative.

The interactive effects of the NAFTA shock on voting for Ross Perot in Table 6 are in the expected directions, but especially apparent when the baseline voter abstains. Model 5 shows the primary result: NAFTA causes a large increase in the probability of turning out for Perot among low mobility voters while high-mobility voters have little to no reaction, and are unlikely to ever turnout for Perot. See Figure 8 for a visualization. This regional effect of NAFTA persists until voters become sufficiently mobile across regions (past a predicted migration probability of 0.8, shown in Figure 25).

Model 6 replaces CZONE fixed effects for individual-level fixed effects to further account for time-invariant voter-specific unobservables. While the direction of the coefficients in model 6 are in the correct direction, none achieve statistical significance. However, with only 44 observations, and only 18 commuter zones, these results are uninformative since there is too little within-case variation in Perot voting within this sample. Larger panel datasets of Perot voting are required to fully guard against endogeneity from time-invariant individual characteristics. Still, the inclusion in model 5 of many of these as controls—race, party ID, education, gender, as well as those absorbed by CZONE fixed effects—likely accounts for the most serious identification threats. To increase confidence the Perot findings, the regressions in Table 5 show that, when we analyze feeling thermometers for Ross Perot, we get the same results in support of the theory with either CZONE or respondent fixed effects.

The same results, but for individual votes for Republican candidates, are reported in Table 7. The most robust finding is the geographically mobile respondents are significantly less likely to vote for Republicans, all else equal. Mobile voters sometimes vote for republicans at the same rate as immobile voters in response to the NAFTA shock; however this result is not robust to controls for party ID, education, and gender.

I now explore party heterogeneity in these results. The NAFTA shock has no effect on how respondents voted for Republicans versus other candidates. Each self-reported Democrat, Independent, and Republican continued to vote along partisan lines. The same is not true for abstainers in Figure 28.

In Figure 28, NAFTA made immobile Democrats move from sometimes turning out for Republican candidates to very unlikely to turn out for Republicans. Much of this is driven by this group reliably voting for Ross Perot, as seen in Figure 26. Immobile Independents responded to NAFTA by turning out for both Republicans and Perot. Immobile Republicans on the other hand proved immovable in their partisan leanings—either not changing their voting patterns or growing even more likely to vote along partisan lines. Consistent with the theory and overall results, mobile respondents across all partisan groups, and across all vote choices, were relatively unresponsive to location-specific trade shocks. Immobile voters on the other hand were on average more sensitive, especially Democrats and Independents, in the expected directions.

	Voted Perot vs establishment			Voted Perot vs abstention		
	m1	m2	m3	m4	m5	m6
NAFTA	0.300	0.334	-2.413**	0.689**	0.953***	0.202
	(0.31)	(0.39)	(1.22)	(0.31)	(0.35)	(2.73)
Mobile	0.272	0.524	-2.639	-1.038	-1.817**	-1.148
	(0.41)	(0.50)	(5.38)	(0.74)	(0.72)	(1.75)
NAFTA × Mobile	-0.770***	-0.743**	2.259	-0.339	-0.696*	-5.658
	(0.29)	(0.33)	(2.36)	(0.35)	(0.39)	(6.44)
lag mfg specialization	2.875***	2.675***	13.166	3.423***	3.563***	
	(0.80)	(0.93)	(8.31)	(1.11)	(1.23)	
industry net imports		0.116	0.009		0.057	
		(0.16)	(0.53)		(0.18)	
>12yrs education		-0.348	-27.208***		1.266**	
		(0.29)	(3.04)		(0.52)	
Party ID		0.007	2.190		0.585***	
		(0.12)	(1.71)		(0.21)	
female		-0.719***			-0.261	
		(0.25)			(0.29)	
non-white hispanic		-0.729			-0.646	
		(0.77)			(0.82)	
black		-2.161***			-2.653***	
		(0.79)			(1.01)	
Native American		2.092***			2.076**	
		(0.71)			(1.05)	
Asian		0.000			0.000	
		(.)			(.)	
N	783	724	82	355	323	44
CZONE FE	x	x		x	x	
Respondent FE			x			x
Year FE	x	x	x	x	x	x
Pseudo R2	0.1258	0.1686	0.6438	0.2183	0.3192	0.5989
Log likelihood	-273.019	-240.165	-20.243	-165.706	-132.006	-12.231

Note: *p<0.1; **p<0.05; ***p<0.01.

Table 6: Logistic Regressions of Individual Votes for Ross Perot, ANES 1992-1996 panel. Specification are all nonlinear Logistic regressions with constants suppressed Measures of trade exposure, manufacturing specialization, and party ID are standardized for comparison. The race/ethnicity reference group is white non-Hispanic. All standard errors clustered at the state levels.

	Voted Republican vs establishment			Voted Republican vs abstention		
	m1	m2	m3	m4	m5	m6
NAFTA	-0.255*	-0.171	-0.903	0.031	0.137	-1.219
	(0.15)	(0.33)	(1.01)	(0.16)	(0.32)	(2.03)
Mobile	-0.089	-1.151**	3.267	-1.023*	-2.284***	-4.015
	(0.31)	(0.52)	(2.39)	(0.55)	(0.58)	(3.23)
NAFTA × Mobile	0.405*	0.261	3.439*	0.357*	0.145	2.117
	(0.22)	(0.32)	(1.86)	(0.19)	(0.27)	(2.33)
lag mfg specialization	-1.485*	-0.636	-4.173	0.510	2.582	
	(0.83)	(1.72)	(6.45)	(1.27)	(2.01)	
industry net imports		-0.034	1.084**		-0.175	
		(0.11)	(0.53)		(0.11)	
>12yrs education		0.395	-7.419**		1.554***	
		(0.25)	(3.38)		(0.27)	
Party ID		2.285***	5.045**		1.751***	
		(0.17)	(2.12)		(0.25)	
female		0.385*			0.374	
		(0.22)			(0.29)	
non-white hispanic		0.373			-0.453	
		(0.59)			(0.59)	
black		-0.429			-0.998	
		(0.66)			(1.00)	
Native American		0.609			0.338	
		(0.44)			(2.55)	
Asian		3.446***			0.213	
		(0.79)			(0.46)	
N	851	811	106	572	542	82
CZONE FE	x	x		x	x	
Respondent FE			x			x
Year FE	x	x	x	x	x	x
Pseudo R2	0.0725	0.4959	0.3012	0.1028	0.3981	0.1730
Log likelihood	-531.784	-276.069	-51.345	-354.464	-224.957	-47.006

Note: *p<0.1; **p<0.05; ***p<0.01.

Table 7: Logistic Regressions of Individual Votes for Republican Candidates, ANES 1992-1996 panel. Specification are all nonlinear Logistic regressions with constants suppressed. Measures of trade exposure, manufacturing specialization, and party ID are standardized for comparison. The race/ethnicity reference group is white non-Hispanic. All standard errors clustered at the state levels.

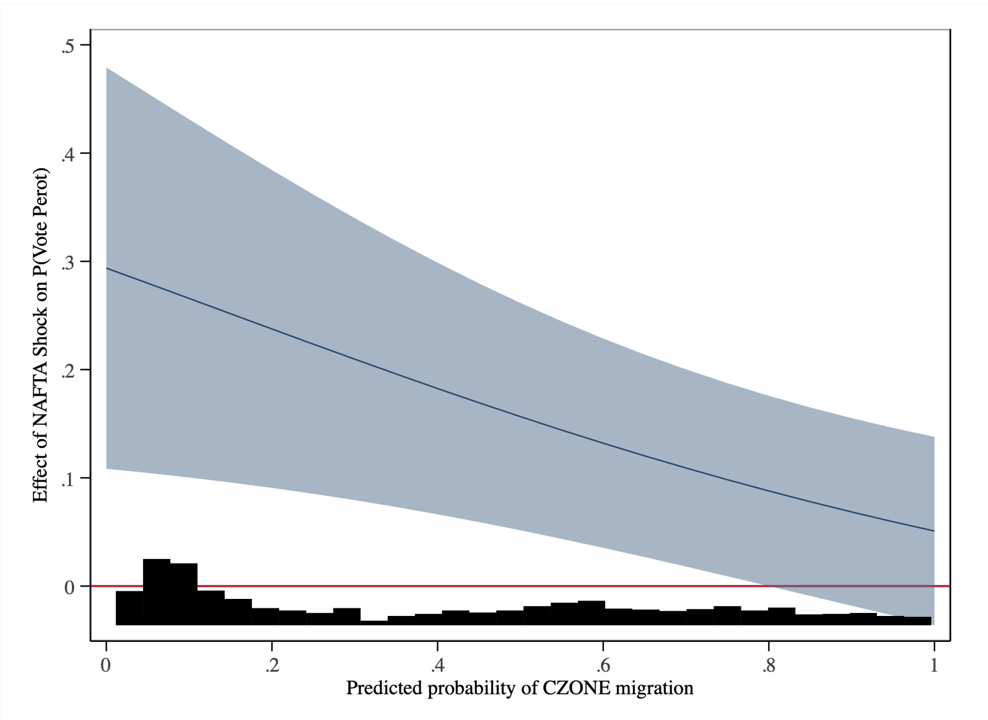

Figure 25: Marginal effect of the NAFTA trade shock on the probability of voting for Ross Perot (versus abstention), by predicted levels of geographic mobility. The x-axis shows the predicted probability that a respondent migrated between commuter zones within the past five years, as well as a histogram of mobility to demonstrate support for these estimates across its range. The marginal effects reflect a 10th-90th percentile increase in exposure to Mexican net imports. Control variables include education, industry-level NAFTA exposure, lagged manufacturing specialization, party ID, sex, indicators for race and ethnicity, and year and CZONE fixed effects. Estimates are from model 5 in Table 6. 90 percent CIs are clustered at the state level.

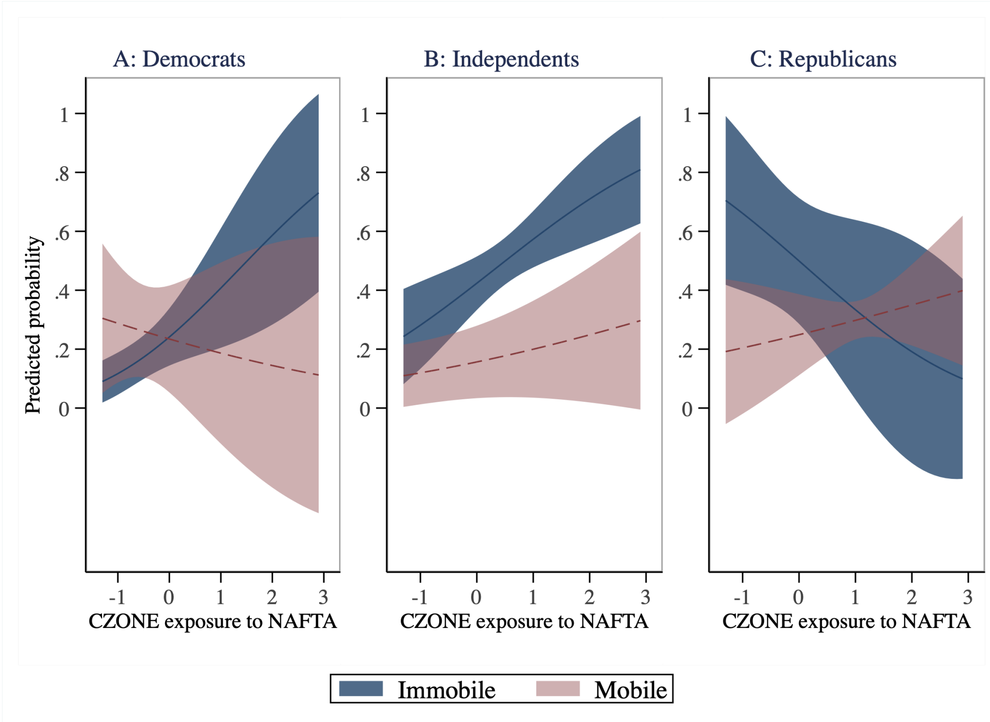

Figure 26: Predicted probability of voting for the Ross Perot, versus abstention, by party ID.

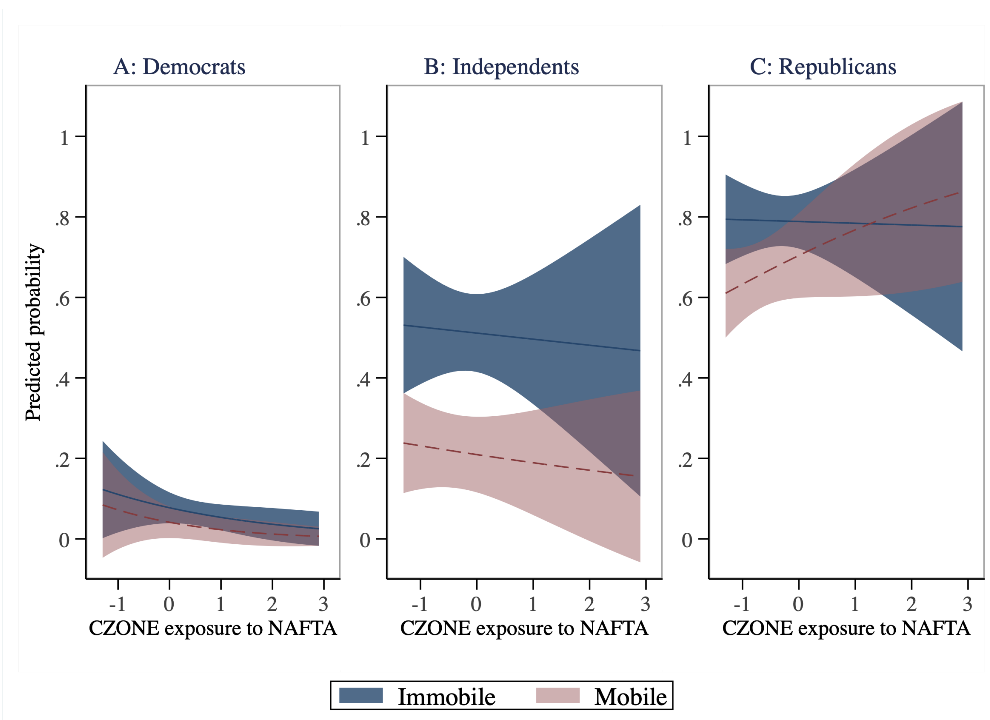

Figure 27: Predicted probability of voting for the Republican candidate, versus abstention, by party ID.

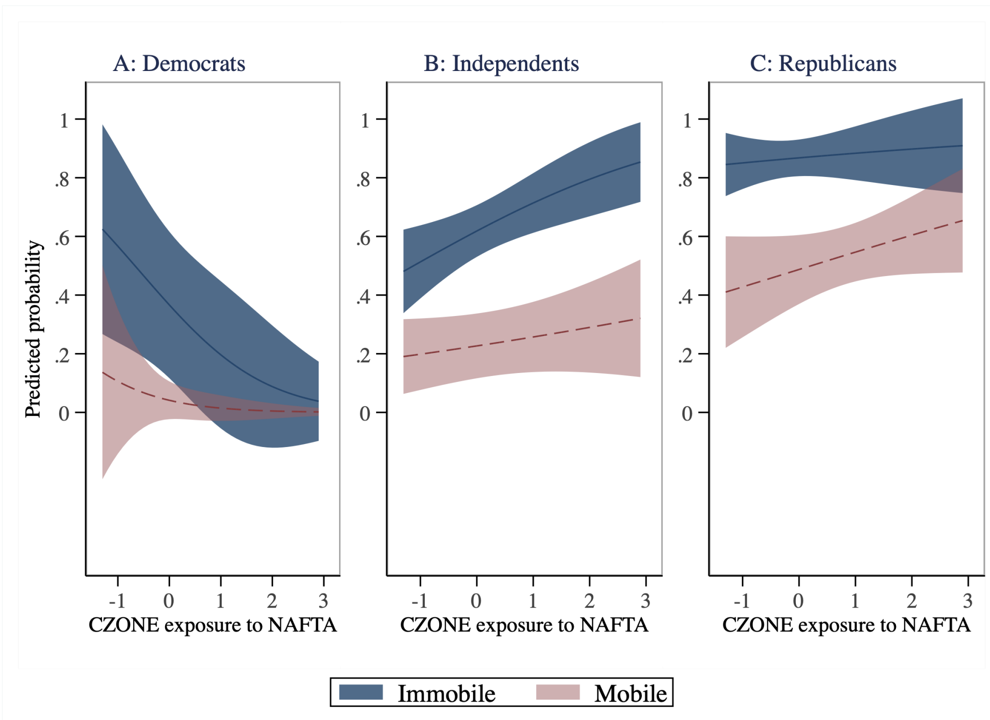

Figure 28: Predicted probability of voting for the Republican candidate, versus abstention, by party ID.

F.2 Attitudinal patterns

The interactive effects of the NAFTA shock on opposition toward free trade in Table 6 are in the expected directions. The most evident result from the table is the geographically mobile voters are, as expected, among the most supportive of free trade, next to whites, men, and those with greater than a high school degree. Reassuringly, the coefficients for the controls are consistent with prior literature (e.g., Scheve and Slaughter (2001); Mayda and Rodrik (2005)). Unfortunately, the interactive effects between mobility and the NAFTA shock are not obvious from the Table due to significant non-linearities shown in Figure 9A. Specifically, low and high mobility voters do not begin their expected divergence in trade attitudes until the NAFTA shock grows sufficiently large (greater than 0.5 standard deviations). In contrast, the interactive coefficients in the Table report the 0-1 standard deviation relationship. Results from m3 with respondent fixed effects maintain the expected relationships; however, these results are inconclusive due to the large drop in observations. Attitudes for immigration are relatively linear. Consistent with Mayda (2006), only high levels of education predict greater support for immigration.

Additionally, m0 reports the results of a standard cross-sectional Logistic regression commonly found in the literature (Kuo and Naoi, 2015). By ignoring the panel structure, it lacks a defense against biases that are corrected by unit and year fixed effects. While most results are consistent, we see that the effects of local exposure to NAFTA takes the wrong sign, with voters in NAFTA shocked regions growing, counter-intuitively, more supportive of trade. This suggests that analyses that rely on cross-sectional surveys to test region-level trade effects may suffer from significant bias.

Figure 29 explores partisan heterogeneity in anti-trade attitudes. Like with Perot voting, primary results are robust for Democrats and Independents. However, Republican respondents are consistently unresponsive, voting for co-partisans and maintaining their attitudes regardless of trade conditions.

	Anti-Trade				Anti-Immigrant		
	m0	m1	m2	m3	m4	m5	m6
NAFTA	-0.270***	0.074	0.081	0.460	0.064	0.055	0.500
	(0.09)	(0.25)	(0.28)	(0.68)	(0.11)	(0.13)	(0.36)
Mobile	-0.791**	-1.144***	-0.863**	-0.156	-0.449	-0.253	0.555
	(0.35)	(0.32)	(0.41)	(2.21)	(0.35)	(0.32)	(1.24)
NAFTA × Mobile		-0.414	-0.458	-0.677	0.095	0.048	-0.292
		(0.43)	(0.43)	(0.98)	(0.24)	(0.23)	(0.74)
industry net imports	0.150		0.130	0.027		0.007	-0.143
	(0.09)		(0.09)	(0.40)		(0.11)	(0.17)
>12yrs education	-0.720***		-0.828***			-0.306*	
	(0.20)		(0.25)			(0.18)	
lag mfg specialization	0.349***	1.501	1.859	5.348***	-0.101	0.048	0.089
	(0.11)	(1.07)	(1.21)	(1.91)	(0.68)	(0.61)	(1.40)
Party ID	-0.061		-0.051			-0.028	
	(0.10)		(0.12)			(0.08)	
female	0.625***		0.665***			0.089	
	(0.17)		(0.18)			(0.16)	
non-white hispanic	0.263		0.189			0.112	
	(0.40)		(0.52)			(0.36)	
black	0.678***		1.083***			-0.375	
	(0.25)		(0.38)			(0.29)	
Native American	-1.055*		-1.170			0.266	
	(0.57)		(1.06)			(0.61)	
Asian	0.721*		0.782*			-0.126	
	(0.40)		(0.46)			(0.44)	
N	721	727	689	166	1102	1043	340
CZONE FE		x	x		x	x	
Respondent FE				x			x
Year FE		x	x	x	x	x	x
Pseudo R2	0.0874	0.0898	0.1382	0.0539	0.0625	0.0735	0.0705
Log likelihood	-444.152	-452.246	-403.846	-108.863	-713.394	-667.594	-219.052

Note: *p<0.1; **p<0.05; ***p<0.01.

Table 8: Individual trade and immigration attitude regressions. Specification are all non-linear Logistic regressions with constants suppressed. Measures of trade exposure, manufacturing specialization, and party ID are standardized for comparison. The race/ethnicity reference group is white non-Hispanic. All standard errors clustered at the state levels.

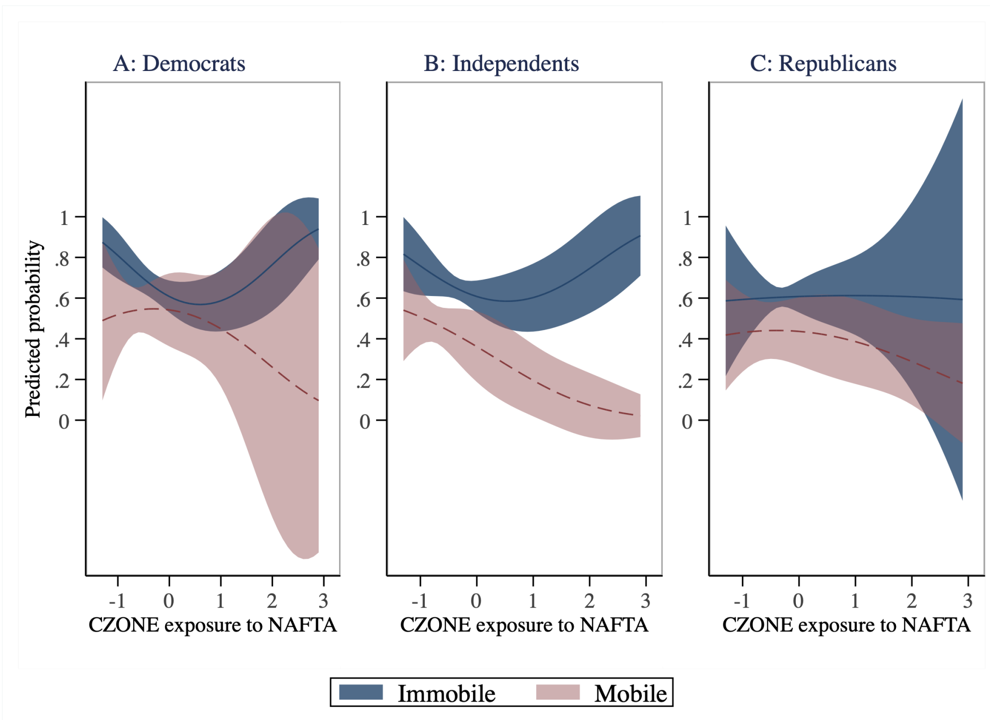

Figure 29: Predicted probability of supporting job-protecting tariffs on foreign imports, by party ID.

	Out-Group Feeling thermometers (0-100)				Religion	
	Hispanics	Whites	Blacks	Gays	Religious	Prays
NAFTA	-0.344	0.747	0.139	-3.229**	0.255	0.725*
	(1.31)	(1.41)	(1.39)	(1.41)	(0.23)	(0.43)
Mobile	1.990	-4.475*	0.850	10.824***	-1.272***	-1.667*
	(1.99)	(2.43)	(1.74)	(3.41)	(0.35)	(0.85)
NAFTA × Mobile	0.082	-0.578	0.585	2.942	-0.444*	-1.331***
	(1.84)	(2.02)	(1.59)	(2.36)	(0.24)	(0.50)
industry net imports	-0.312	0.470	0.021	-0.551	0.035	-0.216
	(0.45)	(0.53)	(0.43)	(0.67)	(0.08)	(0.14)
Family total income	1.148	-1.703**	0.161	0.878	0.199*	0.045
	(0.70)	(0.79)	(0.55)	(0.96)	(0.10)	(0.28)
unemployed	-3.437	-2.160	-1.108	0.585	0.052	0.681
	(2.32)	(2.99)	(3.04)	(2.50)	(0.33)	(0.65)
Party ID	-1.496***	0.491	-1.321*	-6.185***	0.252**	1.177***
	(0.53)	(0.67)	(0.66)	(1.15)	(0.10)	(0.31)
female	1.594	2.250	3.225***	6.699**	0.894***	2.572***
	(1.02)	(1.39)	(0.89)	(2.51)	(0.25)	(0.50)
non-white hispanic	16.625***	1.718	5.922***	3.883	0.803**	0.909
	(2.56)	(3.39)	(1.70)	(4.58)	(0.41)	(0.79)
black	10.890***	-0.117	23.932***	5.712*	1.781***	4.656***
	(2.17)	(2.85)	(1.68)	(3.31)	(0.47)	(1.02)
Native American	9.460**	3.649	5.769	2.487	-0.034	0.119
	(3.94)	(3.77)	(4.09)	(9.34)	(0.64)	(0.79)
Asian	-3.324	1.817	2.667	-2.433	2.357**	0.000
	(4.43)	(3.11)	(3.87)	(7.99)	(1.06)	(.)
lag mfg specialization	2.634	-15.278***	1.201	-2.007	0.398	-3.917**
	(4.73)	(5.06)	(5.09)	(10.22)	(1.09)	(1.63)
>12yrs education	-1.426	-3.675**	0.816	5.397***	-0.198	0.429
	(1.47)	(1.47)	(1.25)	(1.90)	(0.19)	(0.44)
N	991	991	992	992	958	338
CZONE FE	x	x	x	x	x	x
Year FE	x	x	x	x	x	x
Adj. R2	0.16	0.06	0.19	0.19	n/a	n/a
F-stat	13.86	13.02	62.02	16.52	n/a	n/a
Pseudo R2	n/a	n/a	n/a	n/a	0.1683	0.3836
Log likelihood	n/a	n/a	n/a	n/a	-440.134	-127.059

Note: *p<0.1; **p<0.05; ***p<0.01.

Table 9: Feeling thermometer (0-100) regressions. Specification are all linear two-way FE models with constants suppressed. Measures of trade exposure, manufacturing specialization, and party ID are standardized for comparison. The race/ethnicity reference group is white non-Hispanic. All standard errors clustered at the state levels.

F.3 Wage and emp outcomes

	Fam Tot Income	Unemployed	Unemployed
NAFTA	0.025	0.276	-1.492
	(0.06)	(0.56)	(1.17)
Mobile	-0.052	-0.864	-1.686
	(0.16)	(1.22)	(1.93)
NAFTA × Mobile	0.268***	-0.106	0.474
	(0.08)	(0.65)	(1.15)
lag mfg specialization	-0.031		
	(0.22)		
>12yrs education	0.109	1.073	0.946
	(0.20)	(1.48)	(1.17)
industry net imports	-0.051*	0.375*	0.337
	(0.03)	(0.22)	(0.34)
lag mfg specialization		2.014	7.152*
		(3.64)	(4.13)
NAFTA × NAFTA			0.894*
			(0.51)
NAFTA × NAFTA × Mobile			-0.261
			(0.71)
N	1055	116	116
Respondent FE	x	x	x
Year FE	x	x	x
Adj. R2	0.738	n/a	n/a
F-stat	9.63	n/a	n/a
Pseudo R2	n/a	0.1940	0.3118
Log likelihood	n/a	-32.401	-27.666

Note: *p<0.1; **p<0.05; ***p<0.01.

Table 10: Individual wage and family unemployment regressions. Unemployment regressions are nonlinear Logit models. Constants suppressed. Measures of trade exposure, manufacturing specialization, and party ID are standardized for comparison. The race/ethnicity reference group is white non-Hispanic. All standard errors clustered at the state levels.

F.4 Heterogeneity by CZONE Population

Define "core" CZONEs as those with populations over 1.8 million, which is about the 75th percentile in the ANES sample. CZONEs below this threshold fall into the periphery category. Based on the theory, we should expect the political divergence between mobiles and immobiles to be particularly pronounced in the periphery where imports work through agglomeration networks harm all but the mobile workers. In the core, trade exposure should work through agglomeration networks to create a virtuous cycle of prosperity that extends to both mobile and immobile voters.

These results are merely suggestive. Ideally, we would rely on a measure of trade exposure that accounts for the true winners of trade—regions specializing in the production of agriculture, services, and the so-called knowledge economy. Unfortunately, readily available data of trade flows rarely measure imports and exports in these sectors, which has encouraged trade studies, including this one, to study on variation on the losers—manufacturing regions. Additionally, population size is a poor proxy for regions that win from trade liberalization. Population sizes vary for reasons other than trade, and the winning industries concentrate in a diversity of cities, some of which overlap with the regions with concentrations of losing manufacturing. Incorporating these these winners into our standard measures of trade exposure will be a necessary step for future research interesting in analyzing the politics of the core.

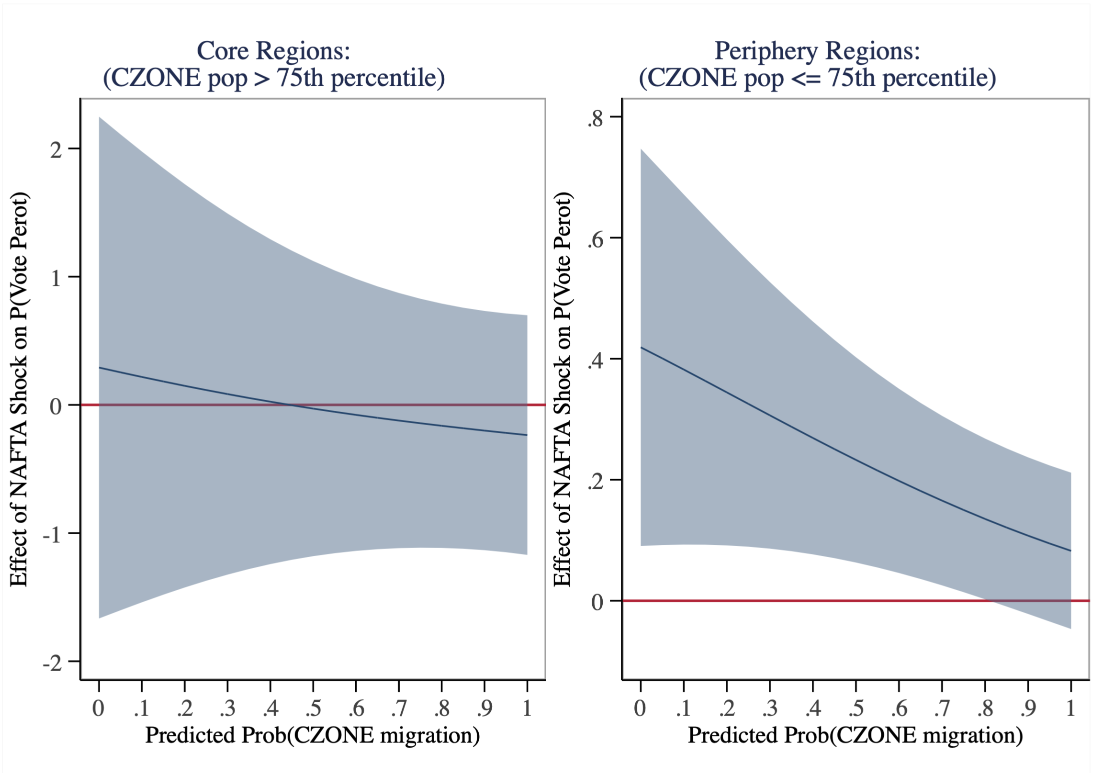

Figure 30: Marginal effect of the NAFTA trade shock on the probability of voting for Ross Perot (versus abstention), by predicted levels of geographic mobility. The x-axis shows the predicted probability that a respondent migrated between commuter zones within the past five years, as well as a histogram of mobility to demonstrate support for these estimates across its range. The marginal effects reflect a 10th-90th percentile increase in exposure to Mexican net imports. Panel A relies on a sample of CZONEs with population sizes above the 75 percentile. Panel B relies on a sample below the population threshold. Control variables include education, industry-level NAFTA exposure, lagged manufacturing specialization, party ID, sex, indicators for race and ethnicity, and year and CZONE fixed effects. Estimates are from model 5 in Table 6. 90 percent CIs are clustered at the state level.

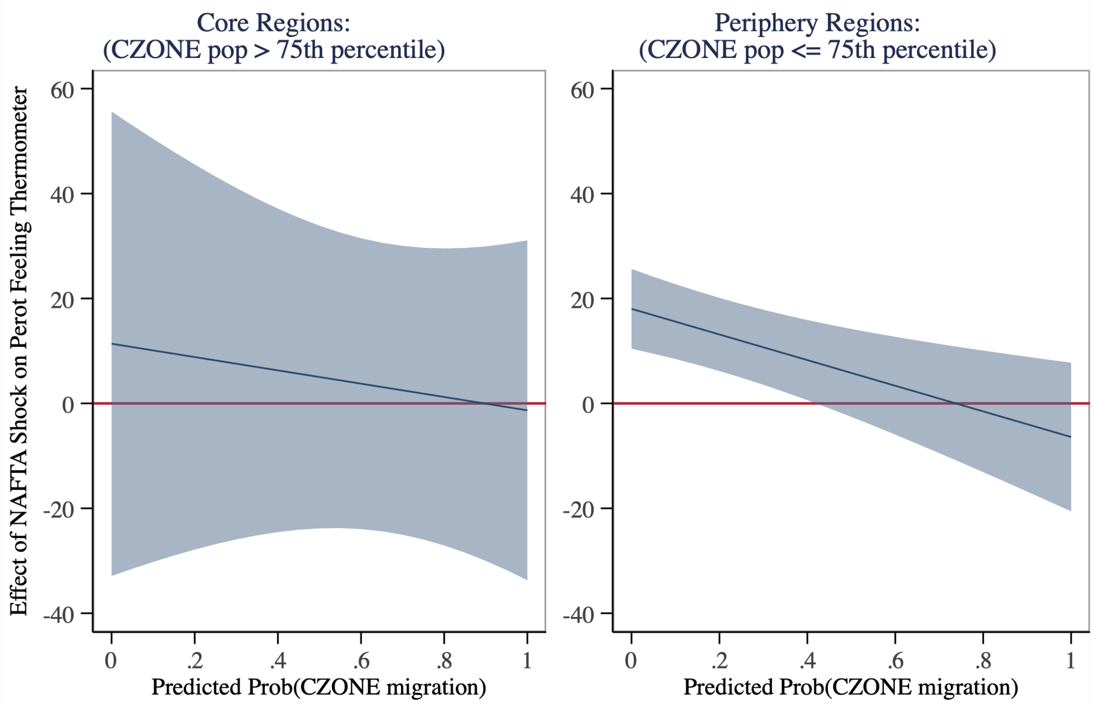

Figure 31: Marginal effect of the NAFTA trade shock on the (0-100) feeling thermometer for Ross Perot, by predicted levels of geographic mobility. The x-axis shows the predicted probability that a respondent migrated between commuter zones within the past five years, as well as a histogram of mobility to demonstrate support for these estimates across its range. The marginal effects reflect a 10th-90th percentile increase in exposure to Mexican net imports. Panel A relies on a sample of CZONEs with population sizes above the 75 percentile. Panel B relies on a sample below the population threshold. Control variables include education, industry-level NAFTA exposure, lagged manufacturing specialization, party ID, sex, indicators for race and ethnicity, and year and CZONE fixed effects. Estimates are from model 5 in Table 6. 90 percent CIs are clustered at the state level.

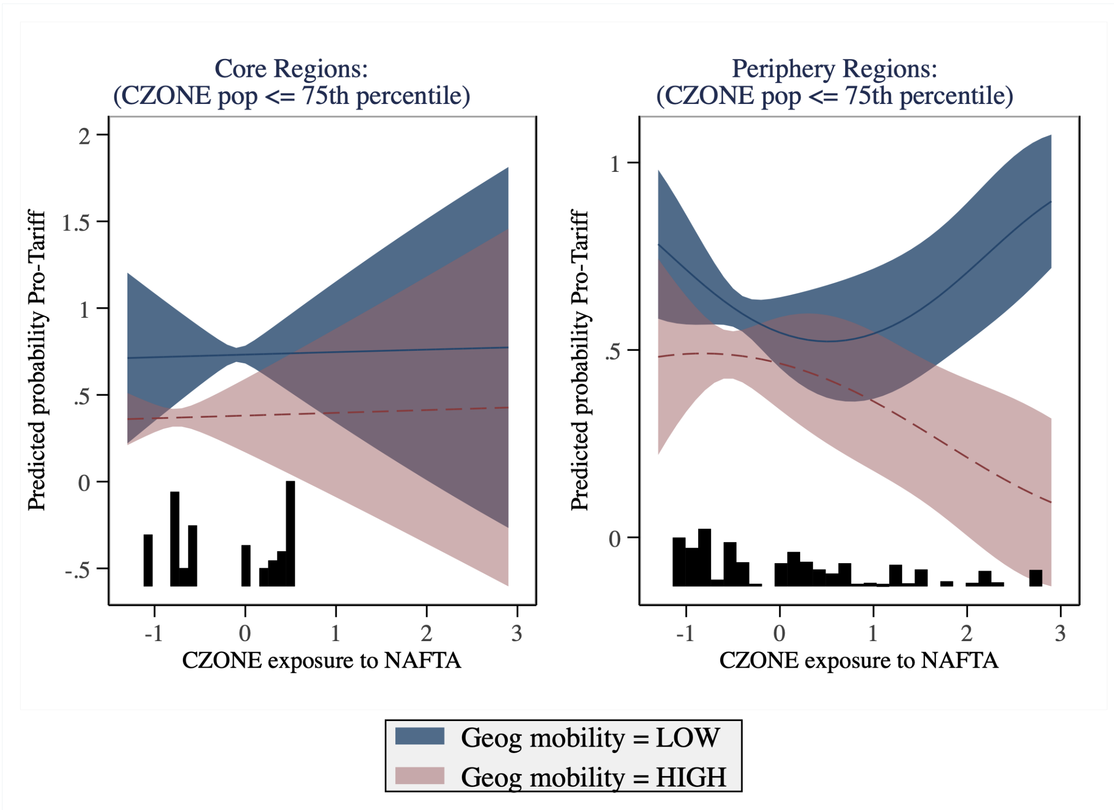

Figure 32: The mobile-immobile divergence in trade preferences. The y-axis measures the predicted probability of reporting opposition to trade (i.e., support "placing new limits on foreign imports in order to protect American jobs"). The x-axes show the change in CZONE exposure to net imports from Mexico in standardized units. Predicted probabilities are provided for respondents where the probability of migration equals one and zero—mobile and immobile, respectively. Panel A relies on a sample of CZONEs with population sizes above the 75 percentile. Panel B relies on a sample below the population threshold. Results are robust to controls for education, industry-level NAFTA exposure, lagged manufacturing specialization, party ID, sex, indicators for race and ethnicity, and fixed effects for year, CZONE, and individuals. Estimates come from Table 8 in Appendix F.2. 90 percent CIs are clustered at the state level.

F.5 God and Globalization

The divergence on sexual orientation attitudes from Figure 11 should be strange from the perspective of the cultural backlash hypothesis since, unlike race or ethnicity, global competition from foreign imports bears no obvious relation to the LGBT+ community. However, it turns out that this strong result has a surprisingly intuitive relation to this paper's theory. Religion and the church have always stood as institutions that supports the needy, and are heavily embedded in the social fabric of local communities. However, it is also the group most critical of LGBT+ rights. Could it be that economically distressed immobile voters within trade-shocked communities seek relief not only in populist politicians, but also in their local church organizations? If so, the immobiles who turn to their local church for help in hard times may be inadvertently exposed to the church's strong views on homosexuality. Indeed, I find evidence in Figure 33 that the NAFTA shock caused immobile respondents to increase their probability of identifying as religious (panel A) and praying several times a day (panel B). Consistent with the idea that economic suffering leads people to God, we see that the mobile respondents who greatly benefited from NAFTA no longer found a need, on average, to pray as much.

In sum, the response to NAFTA shows no sign of the racial/ethnic animus predicted by the cultural backlash thesis. However, I find compelling evidence for an unexpected alternative: a rise in *religious* conservatism. These findings are consistent with prior work on religion and globalization which suggest that empirical results commonly attributed to race are actually an effect of religion (Daniels and Von Der Ruhr, 2005). While future work would need to test this more thoroughly, it seems clear that this effect is incidental to (not an alternative to) the economic geography theory presented here. Trade exposes local communities to a downturn with the immobile populations absorbing all the pain. This pain drives two separate and distinct outcomes: (a) an opposition to free trade policies and increased support for anti-trade populists, and (b), increased religious conservatism driven by economically vulnerable immobiles seeking relief through prayer and/or local church organizations. I therefore view this effect on religion to be rationally consistent, but with potentially far-reaching and unanticipated consequences that demands further research.

Importantly, we can confidently conclude that this surprise phenomenon is indeed inci-

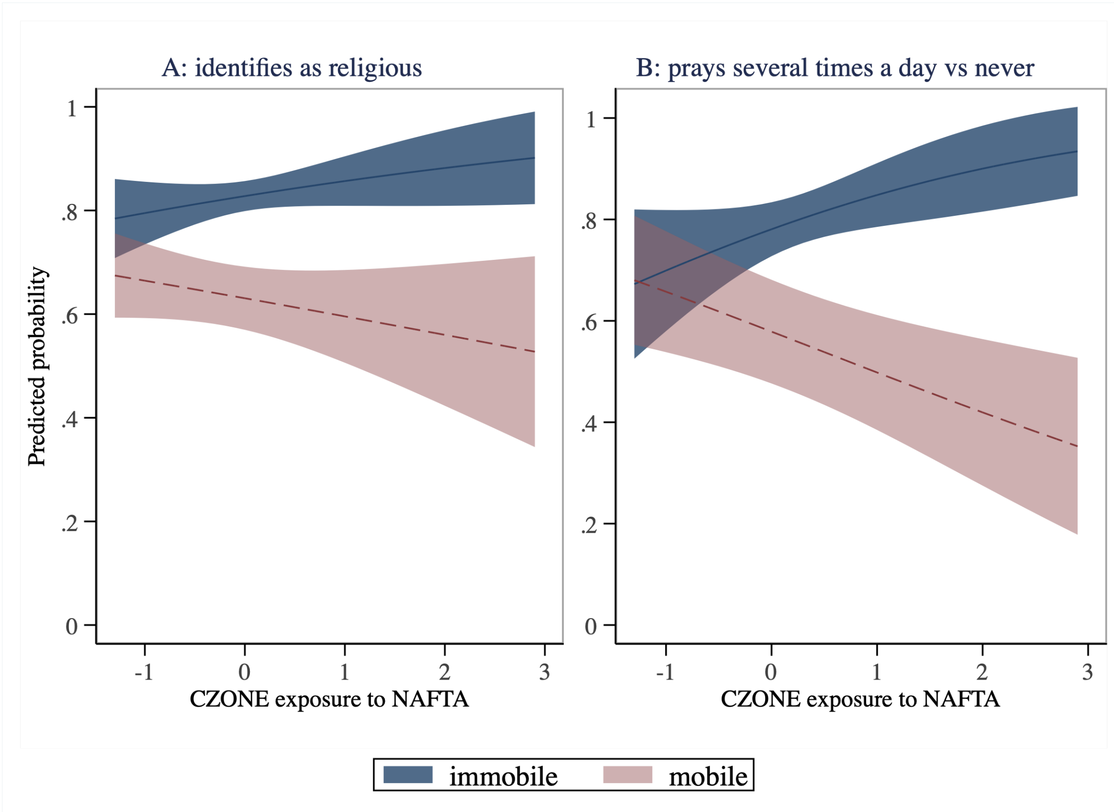

Figure 33: The mobile-immobile divergence in religiosity. The y-axis in panel A measures the predicted probability of self-identifying as religious while Panel B's y-axis measures the predicted probability that the respondent reports praying several times a day versus never. The x-axes show the change in CZONE exposure to net imports from Mexico in standardized units. Both come from logistic regressions that include family total income and unemployment to control for alternative economic mechanisms. They also control for the lag of manufacturing specialization, party ID, sex, indicators for the respondent's own race/ethnicity, and year and CZONE fixed effects. Estimates come from Table 9. Standard errors clustered at the state level.

dental to the Perot phenomenon because his candidacy would represent the last choice a religious conservative would make. Perot was by no means a candidate known for pro-religious stances. He came out as pro-choice on abortion[88] and stated that he would allow members of the LGBT+ community to serve in the military and in his cabinet[89] If religious conservatives did turn out for Perot, it was because of his position on trade and not his stances on religious issues. In fact, that these voters turned out for Perot despite religious cross-pressure speaks to the intensity of their

[88]Allison, Wick (April 28, 1992). "The Democrats Should Adopt Perot". The New York Times. New York. p. 23. Archived from the original on April 28, 2019. Retrieved May 27, 2010

[89].Kelly, Michael (July 10, 1992). "Undeclared Candidate; Perot Shifts on Homosexuals in Military". The New York Times. New York. p. 18. Archived from the original on April 19, 2015. Retrieved May 27, 2010.

anti-trade motivations.