ESSAYS ON FACTOR MOBILITY AND ECONOMIC OUTCOMES

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Oleg Firsin, Ph.D.

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Abstract

This dissertation studies economic consequences of factor mobility in three different contexts. The first chapter deepens understanding of the effect of immigration on native wages in a setting where offshoring also takes place. It does so by relaxing restrictive assumptions of existing theoretical models on the nature of offshoring and showing that if a greater share of native than immigrant tasks is offshored, offshoring reinforces the impact of immigration. Using spatial approach and instrumental variable strategy to address potential endogeneity, empirical results show that the U.S. commuting zones that experienced greater offshoring change featured a magnified effect of immigration on wages of low-skilled native workers.

The second chapter contributes to the economics literature by showing that immigrants can affect trade not just with their countries of origin, but also certain third party countries, and investigating which immigrants do that as well as the likely channels. The study provides evidence of both an inter-ethnic spillover effect (through the role of common spoken non-native language and geographic proximity) as a well as a special role of ethnic ties (through additional effect of common native language). The magnitude of the trade promotion effect of some third party country immigrant groups is comparable to the trade promotion effect of immigrants from

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trading partner country.

The third chapter examines determinants of international differences in agricultural labor productivity. In particular, it focuses on the role of farm size distribution and policy distortions. The findings suggest that international labor productivity differences are mostly explained by differential input quantity and quality, while average farm size and farm size distribution do not have a significant effect conditional on input quantity and quality. This suggests that even if there is misallocation in the form of suboptimal input use in poor countries with smaller farms, it is likely less severe than suggested by hypotheses that assume larger farms are overall more productive. Additionally, price distortions analyzed do not appear to explain either farm size or productivity differences.

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BIOGRAPHICAL SKETCH

Oleg Firsin was born in Vilnius, Lithuania. He grew up in the Baltic country in the 1990s, attending "Ateities" Secondary School. It was there that the first steps towards becoming an economist were taken, mainly through the efforts of his mathematics teacher Laima Tyncenko. A teacher and vice principal, Ms. Tyncenko was notorious for converting all canceled and rescheduled classes into math classes, because, in her words, "matematika eto kruto!" (math is awesome!). The range of questions addressed in math classes seemed too limited to abstract problems, however, so Oleg did not have the desire to pursue it in college (if only there had been a discipline that combined mathematics and social science!). Oleg decided to pursue higher education abroad (before it became cool)–specifically, in the United States, having learned about the liberal arts education system, with its emphasis on free inquiry and critical thinking, which contrasted with the more rigid and vocation-oriented education system at home. At Saint Anselm College, Oleg studied international relations, as the major required a sampling of courses from a variety of social science and humanities disciplines, offering broad knowledge of multiple subject matters. After college, Oleg went on to complete a master's degree in public policy at the College of William and Mary. It was there that he read his first economics journal article and discovered economics as an academic discipline that offered rigorous tools for answering important questions in social science. After William and Mary, Oleg worked as a research assistant at the Brookings Institution, where he was inspired by an environment of rigorous pursuit of policy-relevant knowledge generation and dissemination by a group of talented and committed individuals. A particularly no-

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table part of the experience was witnessing rigorous and objective work on highly partisan but vital subjects, such as immigration and trade. Finally, after working for a year, Oleg enrolled at Cornell University to pursue a Ph.D. in Applied Economics and Management. At Cornell, he continued his quest for contribution to nonpartisan knowledge creation in the fields of immigration, trade, and development, a pursuit that has brought new appreciation for the favorite words of his high school math teacher.

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To my loving and supportive wife, Tanvi Rao.

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I am grateful to my family for their love and support before and during the years of my graduate education, especially my mother, father, brother and sister. I would also like to thank the friends that I met at Cornell, especially Leah, Vidhya, Shouvik, Gary and Rhiannon, who were a pleasure to be around both in academic setting and outside of it, from whom I learned a lot, and who inspired me by their examples.

Lastly, the most important person in my life at Cornell was my wife, Tanvi, whom I met during my first week in Ithaca. In addition to being sharp, eloquent, loving, and kind, she is also an economist. She has made me both a better researcher and a better person.

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INTRODUCTION

The impact of factor mobility on economic outcomes is a vital class of inquiry in the economics discipline. This dissertation studies economic consequences of factor mobility in the contexts of economic effects of immigration on native worker wages (Chapter 1) and trade (Chapter 2) as well as the effects of factor allocation across sectors and establishments on agricultural labor productivity (Chapter 3). In the three distinct chapters, this dissertation contributes to economic literature by providing novel insights by challenging some of the prevailing assumptions of existing studies–such as those on task intensity of offshoring in Chapter 1, on direction and causes of trade promotion in Chapter 2, and on causes of international agricultural productivity differences in Chapter 3–as well as by creating new datasets and employing appropriate identification strategies that allow asking new questions and answering old questions with more detail and rigor.

How does immigration affect labor market outcomes of American workers? How does offshoring–the relocation of parts of the production process overseas–change native wages? Is the effect of immigration affected by the extent of offshoring? These questions are of high significance for understanding domestic repercussions of globalization, including both economic and political economy aspects. Yet studies that consider both processes together are very scarce, and Chapter 1 of my dissertation is directed at filling this gap. Focusing on wage outcomes, Chapter 1 starts from recognizing a key insight from the canonical task-based model of the labor market (Peri and Sparber (2009))–an exogenous labor market shock can potentially impact

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native wages via two distinctive channels: (i) by changing aggregate labor supply and (ii) by impacting how natives are sorted into or out of tasks where they have comparative advantage.

By definition, both immigration and offshoring of tasks increase the supply of workers engaged in production. Immigration also pushes natives to perform tasks in which they have greater comparative advantage. More nuanced, however, is the question of how offshoring can impact the sorting of natives *and* immigrants into tasks. Specifically, by expanding on existing models (e.g., Ottaviano, Peri and Wright (2013)) and relaxing some of their restrictive assumptions (such as complete task offshoring and specific location on a spectrum with natives and immigrants), the paper shows that offshoring can push natives into tasks where they have greater comparative advantage if it is immigrant task intensive–offshores a greater share of immigrant tasks than native tasks. Conversely, it can push natives into tasks where they have less comparative advantage if it is native task intensive. The key novel insight is that if offshoring is native task intensive, by decreasing average native comparative advantage compared to immigrants, it raises immigrant wage share, which intensifies native wage response to immigration. In sum, whether immigration effect on wages is positive or negative, it is reinforced by offshoring if it is native task intensive and mitigated if offshoring is immigrant task intensive. Identifying the nature of offshoring and how it affects native wage response to immigration in the United States is left to the empirical analysis.

Empirically, Chapter 1 focuses on commuting zone outcomes and analyzes a pe-

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riod of high immigration and offshoring exposure growth, 1990 to 2000, in the U.S. The level of analysis choice reflects empirical evidence of strong cross-industry and cross-occupation mobility with limited mobility between commuting zones, which could confound wage effect with employment effect. Using plausibly exogenous Bartik-type instruments for immigration and offshoring based on historical settlement patterns and pre-existing industrial composition, respectively, the paper shows that greater levels of offshoring exposure change increase wage elasticity of competing natives in response to low-skilled immigration. Furthermore, empirical results provide evidence that the channel of influence is consistent with the theory, i.e., offshoring increases immigrant wage share. The study extends the literature by showing that immigration and offshoring affect native workers in a way that is interactive rather than mutually orthogonal, and by providing a theoretical model with relaxed assumptions on the nature of offshoring that helps understand the joint impact of the two processes.

Chapter 2 examines the role of immigration in trade promotion. The early works on immigration-trade connection analyzed it through the Heckscher-Ohlin Model, which featured production factor trade and commodity trade as substitute processes, implying movement in the opposite directions for migration and trade (Mundell (1957)). Since the seminal work of Gould (1994), however, the empirical evidence of positive effect of immigration on trade has prompted literature studying immigrants as trade facilitators. Yet this literature has virtually exclusively focused on trade with immigrant country of origin. In this chapter, I show that immigrants can also affect trade with other (third party) countries, and investigate which immigrants do

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that as well as the likely channels.

Specifically, I explore the role of geographic and linguistic proximity to trading partner country, as these factors reflect business networks, foreign market information and communication facilitation as potential mechanisms of trade facilitation. Geographic proximity measure for a country pair is based on sharing a common border. Linguistic proximity is operationalized through a number of measures, including sharing the same native, spoken and official languages as those in the trading partner country. Importantly, since the various linguistic and geographic proximity measures are correlated, I take steps to disentangle the role of each one–the effects of cross-country ethnic spillovers (through native language), the inter-ethnic spillovers through geographic proximity and the separate roles of the same spoken and official languages.

The analysis in the study indicates evidence of both ethnic (evidenced by special importance of native language) and inter-ethnic (evidenced by the importance of spoken non-native language) trade promotion spillover effects to third party countries. In fact, the magnitude of trade promotion for immigrants proximate by both geographic and linguistic measure is close to that of immigrants from trading partner country. By highlighting and thoroughly investigating a heretofore scarcely explored direction of immigration-trade link, this paper accentuates potential importance of immigration in trade promotion discussions and importance of trade-related effects in immigration policy considerations.

A different question of no lesser importance is addressed in Chapter 3. In-

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ternational differences in agricultural labor productivity are dramatic and significantly larger than non-agricultural productivity differences. In fact, the gap is about two times larger in agriculture (Caselli (2005), Gollin, Lagakos and Waugh (2014), Adamopoulos and Restuccia (2014)). Additionally, countries with larger average farm size tend to be characterized by higher labor productivity. Since more productive labor is key to higher standards of living and most of the world's poor working adults make a living through agriculture (Castaneda et al. (2016)), understanding the reasons for the productivity gaps and increasing agricultural labor productivity in low-income countries could have tremendous impact on poverty reduction.

Because factor mobility is expected to equalize factor returns absent market distortions, large gaps in returns to labor in agriculture and non-agriculture as well as between large and small farms may be caused by factor misallocation. Additionally, incentive-distorting government policies have been suggested as potential cause for misallocation. In fact, Adamopoulos and Restuccia (2014) argue that as much as 3 quarters of the differences in agricultural labor productivity between top and bottom quintiles of countries in terms of per capita income can be attrubited to policy interventions and market distortions that lead to fewer large, more productive farms and more small farms; in particular, one-quarter of the variation is explained by crop-specific price distortions in poor countries that favor small farms. Because smallholders constitute the majority of farmers and are poorer, and since the role of smallholders in structural transformation is a broader question in development economics, it is particularly important to investigate what role farm size plays in explaining international differences in agricultural productivity; I also examine whether

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incentive distortion indicators examined in the literature explain labor productivity differences, through farm size or directly.

Using panel data on agricultural output, input quantity and quality as well as proxies for total factor productivity for a large number of countries, I find that average farm size and farm size distribution do not affect labor productivity conditional on input quantity and quality. This suggests that even if there is misallocation in form of suboptimal input use in poor countries with smaller farms, it is likely less severe than suggested by hypotheses that assume larger farms are more productive TFP-wise. Additionally, policy distortions examined in the literature, such as nominal (NRA) and relative (RRA) rates of assistance to agriculture and price distortion/farm size correlation, do not appear to explain a non-trivial amount of variation in productivity. In particular, I find that is it is unlikely that low-income countries on the whole subsidize smaller farms, as there has not been a negative relationship between crop-level nominal rate of assistance and average farm size for the crop in lowest income quintile countries since the 1990 World Census of Agriculture. Additionally, it does not appear to be the case that agricultural productivity differences between high- and low-income countries are explained by poor countries misallocating labor to agriculture through subsidies, as both NRA and RRA tend to be higher in rich countries; also, neither NRA nor RRA have a direct effect on labor productivity. Most of the international variation in agricultural labor productivity is explained by input quantity and quality, which may or may not signal misallocaiton. Overall, Chapter 3 suggests need for caution in interpreting ostensible correlation between average farm size and agricultural labor productivity as evidence of innate

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advantages of larger farms that poor countries are unable to make use of due factor misallocation caused by policy and institutional barriers.

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CHAPTER 1

THE INTERACTIVE EFFECT OF IMMIGRATION AND OFFSHORING ON U.S. WAGES

1.1 Introduction

The effects of immigration[1] and, more recently, offshoring on the domestic labor market have been subject to growing academic and policy interest, which is likely to continue in the future. Part of the reason is the rapid increase in offshoring and immigration over the past three decades, combined with falling employment and wages of the low-skilled American workers in manufacturing. As of 2014, out of all workers employed by U.S. (multinational) manufacturing companies directly or through affiliates over 30% were located abroad, up from 18% in 1990 (Figure 1.1). During the same time period, the share of non-college educated workers in manufacturing who are of immigrant origin doubled, going from 9% to 18% (Figure 1.2). Contemporaneously, total manufacturing employment of native (non-immigrant) workers without college degree decreased from 15.4 million to less than 9 million (Figure 1.3), and wages of the same group decreased in real terms and relative to higher-educated workers (Figure 1.4). These rapid and significant changes spurred a rich and growing literature investigating the consequences of immigration and offshoring for American workers, particularly the low-skilled.

[1]Here, we focus on low-skilled immigration, and, for brevity, generally refer to low-skilled immigration when we say "immigration," unless otherwise specified.

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An important aspect of the growing knowledge about the consequences of these processes is the increasing understanding of the heterogeneity of impact depending on native and foreign worker characteristics, occupation and industry type, as well as other factors. In this study, we show that an important source of the heterogeneity of immigration effect on wages of low-skilled natives is the extent of offshoring exposure.[2] The key insight is that since the pattern of specialization between natives and immigrants affects immigration impact on native wages, offshoring, by differently affecting native and immigrant workers and thereby shifting the specialization pattern, can also affect the wage impact of immigration.

To understand why offshoring may influence immigration wage consequences for natives, it is instructive to first understand how immigration affects natives on its own. The two main channels through which immigration is found to impact wages of natives are factor supply (which operates in a similar fashion to what we term price effect here) and productivity, which operate differently in different specifications and can be individually or both at play. The extent to which an increase in immigrant labor affects native wages through factor supply channel depends, among other aspects, on the degree of substitutability between the two types of workers in question, with relatively more negative effect on wages of workers who are the closest substitutes in production. This proximity is most commonly empirically proxied by skill level of workers or task content of jobs performed. The degree of similarity between

[2] This is superficially similar but substantially different from a contemporaneous work by Burstein et al. (2017), who investigate the role of (potential) tradability within the U.S., as opposed to actual exposure to offshoring in affecting native wage response to immigration, and who examine a very different economic mechanism.

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skills workers possess is usually measured through the level of education completed (Card (2001)) or education-experience cells (Borjas (2003)); skill-wise more similar workers (who tend to be low-skilled given that immigrants tend to be disproportionately low-skilled) generally see negative, although small, wage effects (Altonji and Card (1991), Borjas (2003), Card (2001), Longhi, Nijkamp and Poot (2005)). Similarity between tasks performed is measured in terms of whether occupation entails heavy use of manual, routine, communication (or, relatedly, interpersonal) or abstract (or, relatedly, cognitive) tasks (Peri and Sparber (2009), Peri and Sparber (2011)). Among the low-skilled in the U.S., immigrant and native workers tend to concentrate in jobs requiring completion of different tasks. In particular, immigrants tend to work in more manual- and less communication-intensive occupations, and when the share of immigrants increases, natives tend to increase concentration in tasks in which they have comparative advantage, which limits the downward wage pressure (Peri and Sparber (2009)). Thus, native wages depend on both ratio of overall low-skilled factor input in production to high-skilled and task concentration of immigrants and natives (in particular, the extent of comparative advantage), both of which are affected by immigration. Importantly, in a way explained in detail in Section 1.2, the size of these two effects is positively related to immigrant wage share. Immigrant wage share, in turn, depends on both immigrant share in employment and immigrants' task specialization/comparative advantage.

The latter, namely average comparative advantage of immigrants (and natives),– and this is generally overlooked in the literature–can be altered by offshoring, as the latter affects immigrant in addition to native labor, potentially differentially and

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in an *a priori* unknown way. Conceptualizing offshoring as trade in tasks (along the lines of Grossman and Rossi-Hansberg (2008)(GRH)), we highlight the fact that the tasks offshored may be native- or immigrant-task intensive (proportionately more native vs. immigrant tasks may be offshored), which will shift comparative advantage patterns, potentially affecting immigrant wage share and native wage elasticity with respect to immigration.

Most of the existing offshoring literature considers only the effect of offshoring on natives. It stresses that since offshoring leads to some tasks[3] being performed abroad, while others are performed at home, workers who previously performed the tasks now done abroad switch to different tasks within the firm or switch firms/industries/locations/become unemployed. Other workers are forced to compete with workers whose task were offshored, putting downward pressure on wages. On the other hand, if (enough) of the gains from cheaper offshoring accrue to firms rather than foreign workers, the higher productivity of labor composite has a positive effect on wages, making the overall effect theoretically ambiguous.

The empirical literature is generally consistent with manufacturing offshoring generally having greater impact–sometimes positive, more often negative–on wages of low- or middle-skilled workers, those in most routine, least interactive occupations and those in the middle or at the low end of the wage distribution (Ebenstein et al. (2014), Oldenski (2014), Tempesti (2015), Olney (2012) in the U.S.; Baumgarten,

[3]Importantly, as we note later, a fraction of a task of a specific kind can be offshored, since there is no natural definition of a task, and it can be defined more broadly or more narrowly; additionally, a fraction of a task can be conceived of as a fraction of the number of repetitions of the same task.

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Geishecker and Görg (2013), Geishecker and Görg (2008) in Germany; Hummels et al. (2014) in Denmark), suggesting that these are the workers "whose jobs" are being offshored or competing workers. Interestingly, Ebenstein et al. (2014) find that offshoring to low-income countries decreases native wages and offshoring to high-income countries increases them, while Olney (2012) finds the opposite to be true, yet the effects are greatest on the competing workers (in most routine occupations in the former case and in lower wage percentiles in the latter) in both papers. Studies focused not on wages but employment also find greater, but mainly negative, effects on the competing workers (Harrison and McMillan (2011), Wright (2014)), as do studies that look at labor task composition, which is shifted to more skilled, non-routine and interactive occupations (Baumgarten (2015), Carluccio et al. (2015)).

Thus, taken separately, immigration and offshoring literatures suggest that both processes affect native wages through changes in the factor supply ratio and productivity/comparative advantage.[4] Due to the similarity of the effects of the two processes, it is natural to ask whether joint analysis leads to new insights. Three papers stand out as having looked at the effects of immigration and offshoring together, both theoretically and empirically. Barba Navaretti, Bertola and Sembenelli (2008) use Italian firm-level data and find that offshoring decreases the share of unskilled workers in domestic employment and immigrant share of employment, suggesting that offshore workers are closest substitutes for unskilled natives and immigrant workers. A more closely related study to ours is that by Ottaviano, Peri and Wright (2013),

[4]Additionally, (Grossman and Rossi-Hansberg (2008)) show that a price effect of offshoring (of a different kind than in this paper) can also take place.

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who extend Grossman and Rossi-Hansberg (2008) framework, modeling task allocation among natives, immigrants and offshore workers in such a way that immigrants specialize in low-complexity tasks, offshore workers perform the intermediate tasks, and natives specialize in most complex tasks. Due to this assumption, an increase in offshoring leads to an increase in native task complexity, a decrease in immigrant task complexity, and lower relative productivity of immigrant workers (this is a crucial assumption, and our results in large part depend on relaxing it). The empirical results obtained by examining industry-time variation suggest that offshoring decreases immigrant and native employment shares, but does not significantly affect wages. Immigration, on the other hand, decreases offshoring employment share, but does not affect native employment share or wages. This suggests that offshore workers, in a way, isolate natives from competition with immigrant workers; significantly, the empirical result considers all native workers together, without separating those most likely affected–the less-skilled natives. Another closely related study is by Olney (2012), who also extends GRH framework, but by modeling immigration as an increase in low-skilled labor supply in addition to offshoring. An important assumption in the latter paper is that offshoring increases effective labor supply of a given factor, with no difference in the extent of offshoring between native and immigrant jobs within the factor. Empirically, the paper exploits state-industry-year variation to simultaneously estimate the effects of low- and high-skilled immigration and of offshoring to low- and high-income countries on wages of natives along wage percentile spectrum, but not how/whether the two interact.

Additionally, in a purely theoretical paper with occupational choice between

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"worker" and "entrepreneur," Unel (2017) obtains a similar insight to Ottaviano, Peri and Wright (2013), in that lowering of offshoring costs "downgrades" tasks performed by immigrants and "upgrades" tasks performed by natives; the paper also predicts that immigration increases the number of entrepreneurs, firm productivity and welfare, while having no effect on entrepreneur/worker inequality. In a stochastic growth model, Mandelman and Zlate (2016) use structural estimation to show that in a general equilibrium context, offshoring increases job polarization by affecting mainly middle-skilled jobs, while low-skilled immigration decreases wages of the low-skilled workers.

Lastly, it is also useful to address a contemporaneous study that looks not at immigration and offshoring but immigration and tradability, and which we consider complementary to our work. The paper by Burstein et al. (2017) finds that "a local influx of immigrants crowds out employment of native-born workers in more relative to less immigrant-intensive nontradable jobs, but has no such effect within tradable occupations." The proposed mechanism is that within tradables, adjustment occurs more through output rather than prices. In particular, occupations are "traded" across regions within the U.S. In contrast, our paper asks what happens to the effect of immigration when offshoring increases. The different assumptions underlying each paper and the different questions asked provide rather different insights. Burstein et al. (2017) focus on immigration-induced native employment (and, to a lesser extent, wage) changes at the occupational level within region as affected by a more permanent characteristic of tradability. In contrast, we concentrate on how native wage response to immigration changes due to actual/imputed offshoring ex-

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posure, reflecting substantial occupational mobility among natives (Kambourov and Manovskii (2008)) in the spatial approach taken, but not investigating it as the subject of primary interest. Thus, the two papers provide somewhat different insights about somewhat different determinants of the way immigration affects native labor market outcomes.

In sum, existing literature does not directly consider the impact of offshoring on the wage response of natives to immigration. We tackle this hitherto unaddressed issue here. We theoretically formalize the approach by following the literature in using general GRH framework, but deviating from it in how we do it. In particular, we build on Grossman and Rossi-Hansberg (2008) and Ottaviano, Peri and Wright (2013) by modifying/extending them in two ways: in the full version of the model, we do not assume that tasks that are offshored are offshored completely and we do not posit the location[5] of tasks most affected by offshoring, letting the empirics speak to that instead.[6] The model in the paper has two factors of production, one being high-skilled labor[7] and the other–low-skilled labor composite, the production of which includes tasks performed by low-skilled native workers, low-skilled immigrants, whose comparative advantage differs across tasks, and offshore workers, with the share of offshore workers varying across tasks. By increasing the supply of low-skilled workers, and hence overall input of low-skilled worker tasks (with the

[5]Location, here, means the place on the spectrum of tasks that are performed by immigrants and natives. In case of Ottaviano, Peri and Wright (2013), the analog is task complexity spectrum.

[6]In the appendix with a special case of the model, we follow the literature in assuming full offshoring of tasks and utilizing the location of offshored jobs. This simpler setting produces similar insights to the main model.

[7]We are primarily interested in the other factor.

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other factor being fixed), immigration decreases marginal product of low-skilled labor composite, which has a negative effect on native wages. On the hand, with more tasks completed by immigrants, natives specialize in tasks where they have greater comparative advantage, which positively affects wages. Both effects are reinforced by immigrant wage share, as is the net effect–native wage elasticity with respect to immigration (assuming one channel sufficiently dominates the other). Offshoring can either increase or decrease immigrant wage share by affecting native tasks relatively more or less than immigrant ones; it may, thus, increase or decrease native wage elasticity with respect to immigration.

To address the question empirically, we use geographic and time variation across U.S. commuting zones in exposure to immigration and offshoring to investigate the presence, extent and nature of the potential interactive effect of the two processes on native wages. We primarily focus on the manufacturing sector, because it has experienced far greater offshoring exposure and exposure increase than non-manufacturing, while seeing similar levels of immigration exposure change. Plausibly exogenous Bartik-type (Bartik (1991)) instruments, based on past settlement patterns for immigration (Card (2001)) and on pre-existing industrial composition for offshoring, address the problem of immigration and/or offshoring being potentially related to local labor demand shocks that affect wages. We find that low-skilled immigration, on average, decreases wages of low-skilled natives, while offshoring increases them. The results also reveal a robust negative interactive effect of low-skilled immigration and offshoring on the wages of low-skilled natives in manufacturing, whereby a negative effect of immigration is reinforced by offshoring. Additionally, offshoring is

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associated with an increase in immigrant wage share, providing support for native task intensive offshoring as the channel for the interactive effect. The results hold for wages of the middle- and low-skilled workers, and those in most routine, most manual, least abstract, less cognitive and less communication-intensive occupations. They are robust to including controls for local labor demand shocks, import competition, demographic variables, and using alternative definitions of immigration and offshoring.

The findings here provide first evidence that offshoring may be exacerbating the negative effect of low-skilled immigrants on low-skilled natives in local labor markets, with supporting theory and evidence. They suggest that since immigration and offshoring effects are not independent, they are more accurately understood when studied together. The rest of the paper proceeds as follows: Section 1.2 provides a theoretical model, Section 1.3 discusses empirical specification and data, while Section 1.4 presents results and Section 1.5 concludes.

1.2 Theoretical model

1.2.1 Part A: Immigration

We propose a simple task-based model of the labor market and investigate the role of offshoring as a determinant of the native wage impact of low-skill immigration. To illustrate the intuition of the model in a simple setting and because there is

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relatively little offshoring outside of the manufacturing sector, there is only one sector in the model. We begin with a setting in which there is only native and low-skilled immigrant employment. Specifically, let aggregate output Q be a function of a composite low-skilled labor input Y,[8] and an exogenously given level of high-skilled labor input H,[9] henceforth normalized to unity:

$$\ln Q = \alpha \ln Y + (1-\alpha) \ln H,$$

where one unit of the composite low-skilled labor input Y is the result of the completion of a unit each of a continuum of tasks $y(i), \quad i \in [0,1]$. We assume that $\alpha (\in (0,1))$ is the share of low-skilled labor input in aggregate output.

Task $y(i)$ can be completed either by native low-skilled workers n, or immigrant low-skilled workers m:

$$y(i) = n(i)/a_n + m(i)/a_m(i).$$

Thus, each low-skilled task can be accomplished by a_n units of native work or $a_m(i)$ units of immigrant work. We assume that the ratio $B(i) = a_n/a_m(i)$ is continuously differentiable and monotonically increasing in i, and consequently natives have comparative advantage in low index tasks, while immigrant workers have comparative advantage in high index tasks.

[8]We can also think of tasks in Y as those that are more manual and routine and less abstract, communication-intensive and cognitive-intensive–tasks more likely performed by low-skilled immigrants and offshore workers, and by natives that compete with the two latter types of labor.

[9]We are primarily interested in the low-skilled labor in the Y factor, and H plays little role. It could also be conceived of as a composite of exogenous inputs.

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Let w_n and w_m denote native and immigrant wages. Define the threshold task I as:

$$I \equiv \{i | w_m a_m(i) = w_n a_n\}.$$

It follows that the unit cost of task i is minimized for $i \leq I$ by hiring only native workers, and for $i > I$ by hiring only immigrant workers. Summing across all tasks $i \in [0, 1]$, the unit cost of the composite low-skilled labor input is thus:

$$\begin{aligned} c(w_n, w_m) &= w_n a_n I + w_m \int_I^1 a_m(i) di \\ &= w_n \left(a_n I + B(I) \int_I^1 a_m(i) di \right) \equiv w_n \phi(I) \end{aligned} \tag{A1}$$

Note that $\phi(I) < a_n$ whenever $I < 1$. Thus $\phi(I)$ denotes the cost savings achieved by hiring immigrant workers. This is analogous to the productivity effect of offshoring defined in Grossman and Rossi-Hansberg (2008).

Let M and N denote the exogenously given supply of low-skilled immigrants and native workers. Given the threshold task i, it follows that total labor supply is equal demand for immigrants and natives if and only if

$$N = Y a_n I, \quad M = Y \int_I^1 a_m(i) di.$$

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Define σ as the share of low-skilled immigrant workers:

$$\sigma = \frac{M}{M+N}.$$

It follows, therefore, that the threshold value task is determined whenever σ is given, since by definition

$$\frac{\sigma}{1-\sigma} = \frac{\int_I^1 a_m(i)di}{a_n I}.$$

Now let θ denote the immigrant wage share:

$$\theta \equiv w_m \int_I^1 a_m(i)di/c(w_n, w_m) = \frac{w_m M}{w_m M + w_n N} = \frac{B(I)M}{B(I)M + N}. \tag{A2}$$

Since $B(I)$ is monotonically increasing in I, the wage share of immigrants is strictly increasing in the threshold I, at constant supply of immigrant and native workers. All else equal, as I increases, immigrant workers become more specialized in tasks in which they have comparative advantage, while native workers spread out and begin to take on some high index tasks in which they have less comparative advantage. As the relative wage of immigrant workers

$$\frac{w_m}{w_n} = B(I)$$

increases with I, the relative wage share of immigrant workers also increases.

The immigrant wage share, or equivalently, one minus the native wage share, plays

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an important role in what follows. First, the threshold task elasticity of immigration, where $\hat{x} = dx/x$ denotes proportionate change, can be expressed as:

$$\frac{\hat{I}}{\hat{\sigma}} = -\frac{\theta}{1-\sigma}. \tag{A3}$$

Thus, the threshold task is more responsive to changes in immigrant supply when the immigrant wage share is high. The same is true for the output elasticity of immigrant supply:

$$\frac{\hat{Y}}{\hat{\sigma}} = \frac{\theta}{1-\sigma}. \tag{A4}$$

Intuitively, the more immigrant workers specialize in tasks where they have comparative advantage, the higher their impact on the allocation of tasks as well as the supply of composite labor input Y.

These intuitions carry over to the responsiveness of the native wage with respect to immigrant inflow as well. To see this, note that with competitive input markets, workers are hired until the marginal product of the composite labor input equals marginal cost:

$$p \equiv \alpha Y^{\alpha-1} = w_n \phi(I), \tag{A5}$$

where p denotes the competitively determined price of the composite labor input. Making use of (A1) and (A5),

$$\frac{\hat{w_n}}{\hat{\sigma}} = \frac{\hat{p}}{\hat{\sigma}} - \frac{\phi(\hat{I})}{\hat{\sigma}}.$$

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Simply put, the native wage impact of an increase in the share of immigrant workers depends on the interplay between the price effect, $\hat{p}/\hat{\sigma}$, and productivity effect $\phi(\hat{I})/\hat{\sigma}$. Naturally, the former depends on the output impact of immigration, since from (A5)

$$\frac{\hat{p}}{\hat{\sigma}} = (\alpha - 1)\frac{\hat{Y}}{\hat{\sigma}}.$$

The productivity effect can also be derived using (A3), giving

$$\frac{\hat{\phi}(I)}{\hat{\sigma}} = \varepsilon\frac{\theta^2}{1-\sigma},$$

where $\varepsilon = dlnB(I)/dlog(I) > 0$ parameterizes the size of the productivity effect of immigration. Taken together, we have:

$$\frac{\hat{w_n}}{\hat{\sigma}} = (\alpha - 1 + \varepsilon\theta)\frac{\theta}{1-\sigma}. \tag{A6}$$

We summarize these findings as follows:

Proposition 1. *The native wage impact of low-skilled immigration is negative (positive) if (and only if) the productivity effect is small (large) relative to the price effect. In both cases, if the difference between productivity parameter ε and price effect $\alpha - 1$ is sufficiently large, a higher immigrant wage share magnifies the native*

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wage impact of low-skilled immigration, all else equal.

Thus, low-skilled immigration may increase or decrease the native wage, depending on the relative size of the price and productivity effects. A higher immigrant wage share magnifies both the price effect and the productivity impact of immigration. The balance of the two depends on whether the price or the productivity effect dominates.

Of course, the immigrant wage share itself is endogenous. Our next task is to demonstrate that the nature of offshoring, in the sense of whether offshoring is native or immigrant tasks intensive, is a key determinant of the immigrant wage share.

1.2.2 Part B: Immigration and Offshoring

We next assume that a fraction of any task in Y can be offshored. Unlike most models (notably, (Ottaviano, Peri and Wright, 2013)), we do not assume full offshoring of tasks and do not posit a particular location of the "more offshorable" tasks along the i spectrum; instead, we allow for the possibility of full offshoring and for any particular location of offshored tasks with respect to natives and immigrants. This makes the model more flexible and generalizable (as well as, arguably, more realistic), with some of the assumptions of the existing models being special cases of this one (we provide one illustrative example of a special case of the theoretical model in Appendix). Specifically, the production of task i can be written as

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$$(1 - \beta s(i))y(i) = n(i)/a_n + m(i)/a_m(i), \tag{B1}$$

where $\beta s(i)$ - fraction of task offshored, $\beta <= 1$ is common to all tasks and $s(i)$ indicates heterogeneity in offshorability across different tasks. The threshold task between natives and immigrants is still given by

$$w_m a_m(I) = w_n a_n, \tag{B2}$$

which, again, means $\frac{w_m}{w_n} = \frac{a_n}{a_m(I)} = B(I)$.

A potential division of tasks between natives, immigrants and offshore workers is graphically illustrated in Figure 1.5. The right axis represents the unit cost of performing a task. The cost of producing any task with native labor is $w_n a_n$, represented by the flat solid line. The cost of producing with immigrant labor decreases with i, as immigrant labor becomes more productive ($a_m(i)$ decreases). The intersection represents the threshold task above which immigrant labor is used domestically and below which native labor is used. The left axis measures the share of task offshored. For the sake of example, the share of task offshored, $\beta s(i)$, is represented by the parabola-like line bounding shaded areas. The figure is deliberately drawn to have a greater share of tasks offshored in the middle of the i spectrum. If we think of i spectrum as equal but reverse of the "complexity" scale in Ottaviano, Peri and Wright (2013), the figure is consistent with greater offshorability of middle-complexity jobs. As drawn, the figure also features a greater extent of offshoring of native tasks than

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immigrant ones. However, this need not be the case, as offshoring function $\beta s(i)$ can take any form (with values between 0 and 1). Figure 1.6 represents another possibility, where offshorability increases with i index and a greater fraction of immigrant tasks (than native) is offshored.

Let now the unit labor cost abroad $w_o a_o$ be a fraction (1-γ) of the local labor cost (e.g., as a result of Nash bargaining). Then the unit cost of task i, c(i), is

$$(1-\gamma)min\{w_n a_n, w_m a_m(i)\} = w_o a_o(i), \gamma \in (0,1) \tag{B3}$$

The unit cost of Y, summing across all tasks, is

$$P = C_y = \int_0^1 c(i)di = w_n[\int_0^I (1-\beta s(i)\gamma)a_n di + B(I)\int_I^1 (1-\beta s(i)\gamma)a_m(i)di] \equiv w_n\phi(I,\beta) \tag{B4}$$

Note that $\phi(I,\beta) < a_n$ whenever there is some immigration and/or offshoring. Thus $\phi(I,\beta)$ denotes the cost savings achieved by hiring immigrant and offshore workers.

Labor market clearing is now given by

$$N = Y\int_0^I (1-\beta s(i))a_n di, \;\; Y\int_I^1 (1-\beta s(i))a_m(i)di = M, \tag{B5}$$

which can also be written as

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$$\frac{N}{\int_0^I (1-\beta s(i))a_n di} = Y, \quad \frac{M}{\int_I^1 (1-\beta s(i))a_m(i) di} = Y,$$

from which it is evident that both effective labor supply of natives and effective labor supply of immigrants, as well as the labor composite, are expanded with greater offshoring.

The ratio of immigrant to native labor is now given by

$$\frac{\sigma}{1-\sigma} = \frac{\int_I^1 (1-\beta s(i))a_m(i) di}{\int_0^I (1-\beta s(i))a_n di} = \frac{M}{N}. \tag{B6}$$

From (B6), we can obtain the relationship between proportionate changes in immigrant wage share, threshold task and offshoring :

$$\frac{\hat{\sigma}}{(1-\sigma)} = -(\frac{1}{\zeta\theta})\hat{I} + (\frac{O_n}{N} - \frac{O_m}{M})\hat{\beta}, \tag{B7}$$

where $\hat{I} = dI/I$, $\hat{\beta} = d\beta/\beta$, $O_m = Y\int_I^1 \beta s(i)a_m(i)di$, $O_n = Y\int_0^I \beta s(i))a_n di$, and $\zeta = \frac{\int_0^I (1-\beta(s(i)))di}{I(1-\beta s(I))}$, the share of native tasks offshored divided by the share of the threshold task offshored. Note that without offshoring, $\zeta = 1$, the second term above is 0 and we are back to the result with just immigration. Alternatively expressed, (B7) gives

$$-\hat{I} = \theta\zeta[\frac{\hat{\sigma}}{1-\sigma} - (\frac{O_n}{N} - \frac{O_m}{M})\hat{\beta}],$$

which can be used to assess the effect of offshoring on threshold task:

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$$\frac{\hat{I}}{\hat{\beta}} = (\frac{O_n}{N} - \frac{O_m}{M})\zeta\theta.$$

Thus, threshold task is increasing in offshoring exposure if a relatively larger share of native than immigrant tasks is offshored. Using (B5), proportionate change in Y can be expressed as

$$\hat{Y} = -1/\zeta\hat{I} + \frac{O_n}{N}\hat{\beta} \tag{B8}$$

or, using (B7),

$$\hat{Y} = \theta\frac{\hat{\sigma}}{1-\sigma} + (1-\theta)\frac{O_n}{N}\hat{\beta} + \theta\frac{O_n}{M}\hat{\beta}. \tag{B9}$$

Thus, both higher immigrant and offshore shares increase the labor composite. Proportionate change in productivity term, using (B4), can be epxressed as

$$\hat{\phi} = \varepsilon\tilde{\theta}\hat{I} - \Omega\hat{\beta}, \tag{B10}$$

where $\varepsilon = \frac{B'(I)I}{B(I)}$, $\tilde{\theta} = \frac{B(I)\int_I^1(1-\beta s(i)\gamma)a_m(i)di}{\phi(I,\beta)}$, $\Omega = \frac{1}{\phi(I,\beta)}[\int_0^I(\beta s(i)\gamma)a_n di + B(I)\int_I^1(\beta s(i)\gamma)a_m(i)di]$. Turning to the change in native wages, since $p \equiv \alpha Y^{\alpha-1} = w_n\phi(I,\beta)$, change in native wages depends on both the change in the labor composite and the productivity term:

$$\hat{w_n} = (\alpha-1)\hat{Y} - \hat{\phi}$$

$$= [(\alpha-1)\theta + \varepsilon\tilde{\theta}\theta\zeta]\frac{\hat{\sigma}}{1-\sigma} + [\Omega + (\alpha-1)((1-\theta)\frac{O_n}{N} + \theta\frac{O_m}{M})]\hat{\beta}. \tag{B11}$$

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Consequently, change in native wages in response to greater immigrant labor share is

$$\frac{\hat{w}_n}{\hat{\sigma}} = [(\alpha - 1) + \varepsilon\zeta\tilde{\theta}]\frac{\theta}{1 - \sigma}. \tag{B12}$$

The first term in the square brackets represents the price/labor supply effect and the second–productivity effect. The native wage response to immigration can again be summarized by restating Proposition 1, except whether the productivity effect is sufficiently large or small now also takes into account ζ and $\tilde{\theta}$:

Proposition 1. *The native wage impact of low-skilled immigration is negative (positive) if (and only if) the productivity effect is small (large) relative to the price effect. In both cases, if the difference between productivity parameter ε and price effect $\alpha - 1$ is* ***sufficiently*** *large, a higher immigrant wage share magnifies the native wage impact of low-skilled immigration, all else equal.*

Turning to the effect of offshoring, from (B11) we have

$$\frac{\hat{w}_n}{\hat{\beta}} = \Omega + (\alpha - 1)((1 - \theta)\frac{O_n}{N} + \theta\frac{O_m}{M}), \tag{B13}$$

where Ω represents the positive productivity effect and the rest – negative labor supply effect. Similar to the effect of offshoring, the direct effect of offshoring is summarized below:

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Proposition 2. *The native wage impact of offshoring is negative (positive) if (and only if) the productivity effect is small (large) relative to the price effect.*

Lastly, the potential interactive effect is slightly more complicated. First, we point out that the relationship between immigrant cost share and offshoring (based on the definition of θ) can be expressed as

$$\frac{\partial\theta}{\partial\beta} = \theta(1-\theta)\varepsilon\frac{1}{B(I)}\frac{\hat{I}}{\hat{\beta}}$$

$$= (\frac{O_n}{N} - \frac{O_m}{M})[\zeta\theta^2(1-\theta)\varepsilon\frac{1}{B(I)}].$$

The term in square brackets is positive, while the sign of the first term depends on whether offshoring is more native or immigrant task intensive. Thus, offshoring increases immigrant wage share if it offshores a relatively larger fraction of native jobs than immigrant ones, leading us to the following inference:

Proposition 3. *If offshoring is native (immigrant) task intensive, it increases (decreases) the immigrant wage share. This reinforces (mitigates) the negative wage impact of immigration if the productivity effect is sufficiently small relative to the price effect; alternatively, this reinforces (mitigates) the positive wage impact of immigration if the productivity effect is sufficiently large relative to the price effect.*

We represent the main elements of the proposition in the table below:

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Immigration Effect		
Dominating Effect	Price ($\alpha - 1 \gg \varepsilon$)	Productivity ($\alpha - 1 \ll \varepsilon$)
Immigration Effect	Negative	Positive
Interactive Effect		
Native task intensive ($\frac{O_n}{N} > \frac{O_m}{M}$)	Reinforcing (-)	Reinforcing (+)
Migrant task intensive ($\frac{O_n}{N} > \frac{O_m}{M}$)	Mitigating (+)	Mitigating (-)

The table shows that whether the effect of immigration is (sufficiently) positive or (sufficiently) negative, native task intensive offshoring reinforces it. However, the sign of the interactive effect (also the sign of the coefficient on the interaction term in the empirical results) will be negative if it reinforces the negative effect and positive if it reinforces the positive effect. Analogously, it will be positive if it mitigates the negative effect and negative if it mitigates the positive effect. In what follows, we take this question to the data.

1.3 Empirical Methodology

In the previous section we provided an explanation for how immigration and offshoring may affect native wages within the context of a single-sector economy where natives, immigrants and offshore workers can all perform tasks in one of the two composite labor inputs (where low-skilled workers are concentrated). In particular, the insights from the previous section suggest that the wage consequences of immigration and offshoring depend on the relative sizes of respective productivity and

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price[10] effects. Additionally, Proposition 2 suggests that native wage elasticity of immigration increases in offshoring exposure if offshoring is native task intensive. In the empirical analysis, we want to estimate the effects of changes in immigration and offshoring on wages of native workers likely to be in Y, as well as whether greater offshoring has an effect on native wage elasticity of immigration. Additionally, since the effects derived in the theory section apply to the low-skilled labor, we are interested in seeing whether immigration and offshoring change the ratio of high- to low-skilled labor wages. Lastly, since the channel through which the interactive effect is posited to take place in the model is the effect on immigrant wage share, we estimate whether offshoring increases or decreases immigrant wage share.

Spatial Approach

There are several decisions that need to be made when choosing the empirical methodology for estimating the wage effects of immigration and offshoring on natives. One important decision is the level of analysis. The observation levels that have been used in either immigration or offshoring literature include individual worker, occupation, industry, geographic area (spatial approach), and a combination of geographic area and worker category (by skill/education).

Of the papers that jointly analyze the effects of immigration and offshoring, Olney

[10]which, here, works in a similar fashion to a labor supply effect in other setting, in that it is generated by lower marginal productivity of the low-skilled labor composite.

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(2012) is the one that focuses on wage outcomes. It uses BEA[11] 2-digit level NAICS[12] industry data across both manufacturing and non-manufacturing to construct state-industry offshoring exposure measure.[13] It then combines it with low- and high-skilled immigrant shares in state-industries and uses annual data (2000-2006) to test the separate effects of high- and low-skilled immigration and offshoring to high- and low-income countries on wage outcomes for natives at different wage percentiles.

The second paper closest to this one, Ottaviano, Peri and Wright (2013), also uses annual BEA industry-level employment data (4-digit manufacturing-only industries) and combines it with immigrant share data, but does not incorporate a geographic component. It is primarily interested in employment outcomes, but does test for wage effect, of which it finds none for either offshoring or immigration (using annual 2000-2007 data).

The third most relevant study is Burstein et al. (2017). Their level of analysis is commuting zone-occupation, with decadal changes in the outcomes of interest and immigration. However, instead of estimating the effects of offshoring in addition to immigration, they investigate the importance of "tradability," which is a more permanent characteristic and does not reflect actual trade or offshoring.

In contrast to these studies, here we take the spatial approach, looking at the effect of changes within a labor market (commuting zone) in immigrant share and

[11] Bureau of Economic Analysis.

[12] North American Industrial Classification System.

[13] using proportionality assumption that state's share of (national) industry GDP translates to the corresponding share of offshore employment in the industry. It is a similar assumption to what we use here, except for the geography level and aggregation across industries.

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offshoring exposure, as well as the interaction of the two on wage changes for native workers in manufacturing. Spatial approach is arguably more suited for studying wage effects after labor reallocation than either industry or state-industry (as well as occupation) approaches. To some extent, labor adjustment in response to labor demand shocks includes some switching of occupation, industry and work location, but, in practice, mostly the former two dimensions. In the United States, mobility responses to labor demand shocks are very limited spatially (Blanchard et al. (1992), Glaeser and Gyourko (2005)), especially for the less-skilled workers (Bound and Holzer (2000), Notowidigdo et al. (2011)). On the other hand, mobility between narrowly defined industries and occupations has been relatively high and rising, particularly for low-skilled workers, who switch at much higher and slightly higher rates than high-skilled between occupations and industries, respectively (Kambourov and Manovskii (2008)). The broader the industry definition, the less the inter-industry mobility (Kambourov and Manovskii (2008)), and mobility between large sectors is especially difficult (Artuç, Chaudhuri and McLaren (2010)). Thus, for analysis of wage outcomes, industry-level analysis (as in Ottaviano, Peri and Wright (2013)) is likely both too narrow, in that the wage effect of immigration and offshoring would likely be mitigated by employment response out of and into the industry, and too broad, in that reallocation within industry but between geographic areas is limited. State-industry (as in Olney (2012)) and CZ-occupation (as in Burstein et al. (2017)) analysis would help the latter problem but still be subject to the former. In the spatial approach we take, labor mobility response is mitigated to better identify the wage effect. This is also consistent with the model of one large sector in a closed

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labor market with large occupational mobility that we posited in the model.

To minimize potential labor mobility effect even further, we choose commuting zones as the geographic area of analysis because they have the advantage of being defined in way that tries to capture the local labor market, rather than being merely an administrative unit, such as a state or a county. They are large enough that most competition among workers happens within CZs, but small and plentiful enough that there is enough of them to exploit inter-area variation and to exclude many non-competing workers. In this way, CZs are preferable to other areas that are frequently used—states, cities, metropolitan areas and counties.

In additional to concerns about the employment effect, an important aspect of estimation is whether the estimated effect is relative or absolute. Dustmann, Schönberg and Stuhler (2016) discuss three main types of empirical specifications to estimate the effect of immigration on native workers—pure spatial approach, national skill-level approach, and mixture approach. While the latter two estimate relative wage effects (compared to other native education-experience groups), the pure spatial approach estimates the total wage effect on a particular native skill group. Since, we are interested in the absolute wage effect (with relative wage effect as a secondary question), the spatial approach is the most appropriate from this point of view also.

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1.3.1 Specification

For the reasons outlined above, empirical specification follows the pure spatial approach, similar to that discussed in Dustmann, Schönberg and Stuhler (2016). Here, low-skilled immigrant share is out of low-skilled, rather than total labor, as the theoretical model studies the importance of immigrant labor within the low-skilled labor composite,[14] and first difference of offshoring is added.[15] The specification has the following form:

$$\Delta ln(\text{wage}_{zg}) = b_g + b_g^{imm}\Delta\text{immshare}_z + b_g^{off}\Delta\text{offexp}_z + \epsilon_{zg},$$

where Δ is decadal change (1990-2000), $ln(wage_{zg})$ is the average manufacturing (log)wage of natives of group g (skill group, task intensity group, etc.) in commuting zone z, immshare_z[16] is the immigrant share of domestic (low-skilled) labor in the commuting zone, offexp_z is the offshoring exposure in CZ (defined further below), and ϵ_{zg} are potentially heteroskedastic errors.[17]

To estimate the potential interactive effect, we modify the equation above, ob-

[14]It is worth mentioning that the results are similar with either measure.

[15]Additionally, since there are only two periods and first difference is taken, there is no additional group-specific time trend (other than the constant).

[16]defined as $\frac{M_z}{M_z+N_z}$, where M and N are immigrant and native numbers in CZ, respectively.

[17]Errors could also be potentially correlated–for example, within state. In practice clustering errors within state led to lower standard errors, suggesting a potentially negative correlation within clusters. On the other hand, the number of census divisions, at 9, is too small. We, therefore, do not cluster standard errors.

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$$\Delta ln(wage_{zg}) = \tilde{b}_g + \tilde{b}_g^{imm}\Delta\text{immshare}_z + \tilde{b}_g^{off}\Delta\text{offexp}_z + \eta_g(\Delta\text{immshare}_z * \Delta\text{offexp}_z) + \tilde{\epsilon}_{zg}.$$

Thus, b_g^{imm}, b_g^{off}, and η_g are the main coefficients of interest in analyzing the joint effect of immigration and offshoring on native wages. There is a number of estimation concerns to address, including measurement, potential endogeneity, and robustness.

Measurement

The measures of immigration and offshoring should be such that they adequately estimate the effects of the two processes on native wages and are sufficiently close to the relevant expressions in the theory section. Equation (B12) expresses native wage elasticity with respect to immigrant share. In the empirical specification above, wage is still estimated as a percent change, while the change in immigrant share is in the form of percentage points, thus making it not identical but similar to the relevant expression in the theory section. This way of defining immigrant share change is more in line with the literature, and the alternative of a "percent change" in the share would be subject to a severe scale effect. Potential endogeneity of this measure is addressed further below.

Measuring offshoring is more challenging. Offshoring refers to conducting part of the production process abroad. This has normally been done either through ana-

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lyzing intermediate imports or employment of affiliates of multinational enterprises, or, in some cases, by defining occupational "offshorability"–job characteristics that make it easier or more feasible to perform abroad without significant loss of quality. We use a measure of offshoring rather than offshorability here, as using information on actual offshoring employment changes arguably brings one closer to measuring what we understand as offshoring than characteristics that suggest potential offshoring, or "offshorability." Perhaps the most common measure of the latter is an index by Blinder and Krueger (2013), and the evidence for presence of any labor market consequences of it is mixed: Blinder and Krueger (2013) do not find evidence that any of the measures of offshorability they consider affect wages or probability of layoff, while Goos, Manning and Salomons (2014) look at the effects of routiness and offshorability on labor demand, and find that the former decreases labor demand but the latter has no independent effect when controlling for routiness; in contrast, Burstein et al. (2017) find that tradability of occupations affects how natives respond to immigration. Relatedly to the latter, it has been shown that certain job characteristics that are associated with offshorability (such as routiness and interactivity) influence the effect of imputed/actual measures of offshoring exposure on labor market outcomes; that is, rather than being used as measures of offshoring themselves, they are used as measures of vulnerability to offshoring (or other shocks) in addition to other offshoring measures, which is similar to what we do here (when we measure effects for occupations with varying task intensities).

Since change in β in the theoretical model represents increase in the share of employment offshored, we operationalize this by using employment by affiliates of

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multinational enterprises to find offshoring exposure measure (as do Ottaviano, Peri and Wright (2013) and Olney (2012)), rather than intermediate input share. Except for a few firm-level studies and "offshorability" measures, offshoring exposure is usually derived from industry-level data, which is then proportionally allocated either to occupation or region. In our case, CZ level offshoring exposure is calculated as a the sum of national industry-level offshoring exposure weighted by local industry share in manufacturing; specifically, offshoring exposure is defined as

$$Offexp_{zt} = \sum_{u} [\frac{D_{uz,t}}{D_{z,t}} * \frac{O_{ut}}{D_{dt} + O_{ut}}],$$

where O_{ut} is offshore employment in industry u in year t, and $D_{uz,t}$ is domestic employment in industry u and commuting zone z.[18]

Endogeneity

Immigrant choice of location may not be exogenous to labor market conditions, as low-skilled immigrants, unlike natives, are quite mobile (Cadena and Kovak (2016)), and move to locations of positive labor demand shock. To address this problem, we use the shift-share instrument throughout, which allocates immigrant flow to specific CZs based on preexisting immigrant enclaves and national level immigrant flows by

[18]This is an imperfect measure and relies on the proportionality assumption (similar rates of offshoring for industries in different locations), but is common in the literature (including Olney (2012)).

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origin group, before aggregating over origin groups (similar to Ottaviano, Peri and Wright (2013)). Specifically, the instrument for immigrant share is constructed as follows (for those without bachelor's degree). First, predicted number of immigrants from each large region of origin (out of 10 regions) in year t (we end up mostly looking at 1990-2000, partly because the instrument is no longer strong after 2000) is calculated based on the share of all immigrants from region r in CZ z in 1980 and growth rate in the group r in the rest of the country (z^{-}); these numbers are then summed over all regions, i.e.

$$\hat{M}_{zt} = \sum_r \hat{M}_{rzt} = \sum_r [M_{rz,1980} + (M_{rtz^-} - M_{r,1980,z^-})\frac{M_{rz,1980}}{M_{r,1980}}].$$

Validity of this instruments relies on it affecting immigrant share change, but not other factors that may affect wages.

Because offshoring exposure change may be both due to national industry-level offshoring change as well as CZ industrial composition change, endogeneity concerns of a different kind than in the case of immigration may arise. Here, a productivity shock at the CZ level that is not industry-specific is not expected to be correlated with offshoring exposure change. However, if a negative CZ-industry productivity shock 1) leads to lower industry employment, 2) happens in a low-offshoring (high-offshoring) industry and 3) the industry is large enough to affect overall wages, then higher (lower) offshoring would be spuriously correlated with lower (higher) wages. Because of the above concern, we instrument for offshoring using initial period industrial distribution, so offshoring exposure change is only driven by national industry-level

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offshoring exposure change. Specifically,

$$\widehat{Offexp}_{zt} = \sum_u [\frac{D_{uz,t=1990}}{D_{z,t=1990}} * \frac{O_{ut}}{D_{dt} + O_{ut}}].$$

Consequently, instrumented offshoring exposure change is driven by national industry-level offshoring changes, which are likely uncorrelated with local labor market area demand shocks.[19] They can, however, be potentially correlated with industry-level import competition change or productivity shocks, which we address below. Lastly, to instrument for the product of immigration and offshoring, we use the product of their instruments.

Robustness Checks

In addition to the main specficiation, we conduct several robustness checks, including additional controls and alternative definitions of immigration and offshoriong. First, since cheaper offshoring may be a result of tariff reduction or industry specific shock, it may be correlated with increased imports of final goods, and greater offshoring exposure change may be associated with greater import penetration; for this reason, in robustness checks, we include import penetration controls. Specifically, we focus on imports from China, and use import penetration change estimates from

[19]Because after 1999 offshore employment is provided using NAICS classification and before–SIC (Standard Industrial Classification), we convert NAICS-based estimates into SIC industries.

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Acemoglu et al. (2016), defined as $\Delta IP_{zt}^{CZ} = \sum_d \frac{L_{zd,1991}}{L_{z,1991}} \Delta IP_{dt}$, where $\frac{L_{zd,1991}}{L_{z,1991}}$ is the industry d share of CZ employment in 1991, IP_{dt} is industry d import competition change (1991-1999), $\Delta IP_{dt} = \frac{\Delta M_{d,t}^{UC}}{Y_{d,91}+M_{d,91}-E_{d,91}}$, where $\Delta M_{d,t}^{UC}$ is change in imports from China over the period (1991-1999) in industry d, and the denominator is the initial absorption measure (ind. shipments+imports-exports). We use the instrument based on imports from China on the part of 8 other high income countries (from Acemoglu et al. (2016)).

Next, in case CZ offshoring exposure change is correlated with local labor demand shocks because of initial industrial composition,[20] we include control for labor demand shocks using a "Bartik" instrument (from Basso and Peri (2015)). Bartik control for growth in labor productivity (labor demand) predicts productivity growth based on national industry-level growth and initial composition; it is defined as $Bartik_{zt} = \sum_d (share_{zd,1970}^{empl} \Delta lnwage_{dt})$, where $share_{zd,1970}^{empl}$ is the initial employment share of industry d in commuting zone z and $\Delta lnwage_{dt}$ is the national wage growth from 1970. Lastly, we control for a number of demographic factors, although they may be endogenous due to push factors out of manufacturing being correlated with wages and also demographic characteristics, which is why we do not include them in the main specification.

Another robustness check entails using an alternative definition of offshoring–employment of majority-owned enterprises, since employment of all affiliate (including arm's-length) enterprises may be overestimating the total change in offshoring

[20]For example, if industries that experience large offshoring increase also experience large labor productivity shocks.

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exposure (although, in practice, the two measures are very close), and trade with arm's-length affiliates may be different than with majority-owned ones. Additionally and relatedly, additional robustness check uses parent-based industry classification (instead of affiliate-based used in the main specification) to measure offshoring. The effect may be different if the local industry is engaging in offshoring rather than being offshored; in practice, a lot of offshoring is intra-industry when industry definition is sufficiently coarse and parent- and affiliate-based measures are very close.

The next robustness check uses an alternative definition of immigration change, one standardized by initial employment. One potential criticism of using immigrant share change is that it includes native worker number in the denominator, which may be affected by local demand shocks that also affect wages. An alternative approach is to define $\Delta imm\_stand_z = \Delta M_z/(M_{z,t-1} + N_{z,t-1})$, a change in the number of immigrants divided by lagged employment to take out potential native outflow, although, as mentioned, native mobility response to labor demand shocks is generally limited.

We primarily focus on manufacturing since it is subject to overwhelmingly greater extent of offshoring, but we broaden the scope in the last three variations on the main specification. Specifically, we use immigrant share change with respect to the entire CZ employment, rather than manufacturing, as this way movement (of natives) out of manufacturing is less likely to confound interpretation. Additionally, we investigate whether wage effects for the entire CZ are different compared to just manufacturing. Lastly, we incorporate non-manufacturing offshoring in measuring offshoring expo-

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sure to see if the results are robust to a much broader measure of offshoring.

Native vs. Immigrant Task Intensity of Offshoring

Since the channel through which offshoring may enhance the effect of immigration on native wages is through increasing immigrant wage share, we test for this explicitly. Specifically, we look at whether offshoring impacts change in immigrant wage share:

$$\Delta \text{immwageshare}_z = b^o + \gamma^o \Delta \text{offexp}_z + \epsilon_z^o, \tag{B14}$$

where immwageshare_z is the immigrant share of all low-skilled labor payments.

1.3.2 Data

Measures of wages by education level, wage percentiles, and by task characteristics are calculated based on data from the U.S. Census and American Community Survey (from IPUMS). We focus on hourly wages, imputed by dividing annual earnings by the product of the number of weeks worked and usual weekly hours of work, to avoid capturing employment intensity effect. The universe of individuals includes workers with positive income, aged between 18 and 65, not in group quarters and working at least 30 hours a week (not part-time workers). Cognitive and communica-

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tion task intensity of occupations is from O*NET. "Communication" task intensity score for the occupation is the average population-weighted percentile (among occupations) of the importance of "Oral Comprehension," "Written Comprehension," "Oral Expression," and "Written Expression" in the occupation; the analog for "cognitive" score is calculated using questions on several cognitive abilities.[21] Manual, routine, and abstract task intensity of occupations is from Autor and Dorn (2013), who use Dictionary of Occupational Titles to calculate relevant occupational scores. Specifically, data on EYEHAND (eye, hand, foot coordination) requirements operationalizes manual task intensity score, STS (adaptability to work requiring set limits, tolerances, or standards) and FINGDEX (finger dexterity)–routine score, and DCP (direction, control, and planning of activities) and GED-MATH (quantitative reasoning requirements)–abstract score.

The time frame for empirical estimation is 1990-2000, because this was a period of large growth in immigration and offshoring, both in terms of employment shares and numbers, driven by macro-level, locally-exogenous factors (additionally, instrument for immigration, crucial for analysis, is no longer strong after 2000). Immigrant shares within CZs are calculated using data from U.S. Census for 1980, 1990, and 2000, and from American Community Survey beyond 2000 (all from IPUMS). Public Use Microdata Area-based data is aggregated up to CZs.[22]

Raw offshoring data comes from Bureau of Economic Analysis Activities of Multi-

[21]"Fluency of Ideas," "Originality," "Problem Sensitivity," "Deductive Reasoning," "Inductive Reasoning," "Information Ordering," "Category Flexibility," "Mathematical Reasoning," "Number Facility" and "Memorization."

[22]using crosswalk from Autor and Dorn (2013).

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national Enterprises, [23] where it is provided at the industry level. The main specification uses all non-bank affiliate employment based on affiliate industry; the latter is provided at the most disaggregated level, which allows most accurate crosswalk between NAICS-based offshoring in 2000 and SIC-based industries used in 1990, employed for the instrument. In robustness checks we also use offshoring calculated from parent-based industries and only majority-owned (which tends to not change results).

1.3.3 Descriptive Statistics

Figure 1.1 shows change in manufacturing, non-manufacturing and overall offshoring exposure over time. The graph makes it clear that at a sectoral level offshoring is mainly a manufacturing sector phenomenon. Whereas manufacturing offshoring exposure grew from 18% in 1990 to 22% in 2000 and over 30% in 2014, non-manufacturing offshoring share at the same points was 3%, 5% and 9%. The difference is likely because a lot of services are local and/or require interpersonal contact, and thus cannot be offshored without severe loss of quality. This is the main reason for focusing on the manufacturing sector. Importantly, while many manufacturing industries saw offshoring exposure growth between 1990 and 2000, this process did not affect all industries equally, and there is a large variation in offshoring change over this period (Table 1.1), with some industries even seeing a decline (petroleum,

[23]BEA data before 2009 is for non-bank majority owned affiliates; all vs non-bank makes a trivial or no difference in employment values in large manufacturing industries.

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for example, which saw new sources opening in the U.S.). This varying change in offshoring exposure growth across industries is likely unrelated to CZ-level shocks, creating an opportunity to exploit it for identification purposes.

Figure 1.2 shows that immigrant share for all skill levels is higher in manufacturing, and while it increased overall, it did so slightly more in manufacturing. Table 1.2 shows native and immigrant shares of domestic employment. Total immigrant share of employment increased from 0.07 in 1980 to 0.09 in 1990, 0.13 in 2000 and 0.17 in 2014. Meanwhile, the share of foreign-born in manufacturing increased from 0.08 in 1980 to 0.11 in 1990, 0.16 in 2000 and 0.19 in 2014. Thus, immigration increase was largest in the 1990s and especially in manufacturing, further motivating the selection of the sector and time frame for analysis.

Figure 1.3 illustrates the change in employment numbers rather than shares. Total domestic manufacturing employment went from 20.6 million in 1990 to 19.2 in 2000 and 14.9 in 2014. On the other hand, offshore employment increased from 4.6 million in 1990 to 5.5 in 2000 and over 6.6 in 2014. A somewhat different pattern is observed for immigration. Total (low-skilled) immigrant employment in manufacturing increased from less than 2 million to 2.5 between 1990 to 2000, but decreased to 2 million by 2014, despite the fact that immigrant share of domestic manufacturing labor grew throughout. Since we are interesting in studying the effect of *immigration*-driven increase in immigrant share in manufacturing, this reinforces the idea that studying increase in immigration in manufacturing is more appropriate before 2000 than after. The likely reason behind the decrease in immigrant employment

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in manufacturing after 2000 is that despite continued immigration into the country (as shown in the growth in immigrant number in non-manufacturing), trade shocks, primarily driven by China entering the WTO, together with technological change decreased demand for both domestic and immigrant workers. Interestingly, while the number of workers in manufacturing without college education decreased since 1990, the number of those with college education remained virtually the same (Figure 1.3) suggesting jobs losses primarily affected the low-skilled, decreasing native low-skilled share of all manufacturing jobs even further (Figure 1.7).

We next illustrate the geographic distribution of offshoring and immigration exposure in 2000 as well as change in exposure from 1990 to 2000 (Figure 1.8, Figure 1.9, Figure 1.10, Figure 1.11). The figures show that there is a great deal of variation in geographic exposure to immigration and offshoring: whereas Appalachia saw large increases in both immigration and offshoring, Southwest mainly experienced large increase in immigration and several commuting zones in the Northwest and Rockies were subject to growing offshoring exposure only,[24] while some CZs throughout the country saw little change in either. This provides useful geographic variation to exploit for empirical analysis. It is also useful to note that the geographic correlation between immigrant share change and offshoring exposure change is close to 0, at -0.007, which is consistent with the two processes being driven by different non-local factors, and one process not significantly affecting the other.

[24]Offshoring exposure change was much more decentralized than what was documented for trade by Autor, Dorn and Hanson (2013) who showed trade exposure grew above the median rate mainly in the Midwest and areas east and northeast of Midwest.

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Lastly, Table 1.3 shows average decadal changes in the variables used for regressions. It is notable that the wages of those with at most high-school education increased by only about 1% (the table shows changes in log wages), while wages of those with some college education increased by 4% and those with completed college education–9%. Most cognitive-intensive, communication-intensive and abstract occupations also saw slightly higher wage growth than others. We should also note that wages of those in most manual intensive occupations–those at greatest competition with low-skilled immigrants–increased at less than half the rate of those in least manual intensive ones (4% compared to 9%). On the other hand, workers who are seen as closest substitutes for offshore workers–those in the most routine occupations–saw changes generally not different from those in least routine occupations (at 7%). We approach the question of causal effect of immigration and offshoring on wages of various native groups in the next section.

1.4 Results

We begin by presenting OLS and 2SLS results for the main specification without and with the interaction term. Column 1 in Table 1.5 shows that without instrumenting immigration exposure change does not have a statistically significant effect on wages of natives with less than bachelor's degree, while offshoring tends to increase them. In column 2 we show 2SLS results, using the instruments for immigration and offshoring described earlier (with the first stage shown in Table 1.4). First stage Wald

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F-statistic of 152.5 suggests rejecting weak instrument hypothesis. As is common in the literature, 2SLS results suggest that OLS estimate of immigration effect on native wages is likely upward-biased, as it decreases from statistically insignificant -0.05 to significant -.5 in 2SLS. This means a 0.5 percent decrease in low-skilled native wages for a 0.01 point increase in low-skilled immigrant labor share (or 1 percentage point increase in percent of immigrants in low-skilled labor). This also suggests that price or labor supply effect, which is negative, dominates the positive productivity effect. To compare our estimated result to the literature, the average elasticity of native low-skilled workers with respect change in immigrant share in Longhi, Nijkamp and Poot (2005) meta-analysis is -0.2; it does, however, include all immigrants (inclusive of high-skilled) and incorporates studies using small geographic areas, which tend to see lower impact due to outmigration, as well as studies that do not instrument for immigrant share. In any case, the estimated coefficient here is not statistically different from -0.2, suggesting that the average estimated effect is in line with general findings in the literature of a negative but small effect of immigration on wages of low-skilled natives.

In contrast to immigration, the coefficient on offshoring becomes more positive, increasing from 0.3 to 0.6 (the difference not being statistically significant), further testifying to the productivity effect being dominant for offshoring. Given that Olney (2012) is the closest study to this one in terms of estimating the effect of offshoring on native wages, and it finds a positive effect of offshoring to low-income countries but the opposite for high-income, the result is consistent with change in offshoring to low-income countries dominating change in offshoring to high-income countries,

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which is possible, given that offshoring to low-income countries increased by more than offshoring to high-income during the period.

Column 3 in Table 1.5 adds the interaction term. Interpreting OLS specification results with caution, since we know at least immigration share variable is likely endogenous, we observe that the coefficients on both immigration and offshoring are positive. The coefficients on the level terms are not very informative, however, since they estimate the effect of one variable, when the other is 0, which rarely happens, since per descriptive statistics in Table 1.3, 0 is almost 2 standard deviations below offshoring change mean and 1.5 standard deviations below immigration mean. In this specification we are primarily interested in the interaction term, which is negative, at -9, and highly statistically significant. Column 4 provides 2SLS results with the interaction term. First stage Wald F-statistic of 52.7 suggests the instruments are strong. The coefficient on the interaction term is again negative, but is larger in magnitude, at -32, which means that with a 0.01 increase in offshoring exposure change the effect of immigration on native wages decreases by 0.32. Given the negative effect of immigration found in the specification without the interaction term, offshoring making the effect of immigration more negative is consistent with model scenario of offshoring reinforcing the effect of immigration via increasing immigrant wage share.

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1.4.1 Independent Immigration and Offshoring Effects

We next look at the effects of immigration and offshoring at the same time on wages of natives by education level. The upper panel of Table 1.6 illustrates that the effect of immigration is most negative on those least educated. Wages of natives with at most high school education decrease by around 0.7 percent for a 1 percentage point increase in low-skilled immigrant share of CZ employment. For the same immigrant share change, wages of natives with less than college education decrease by 0.5. The effect becomes progressively less negative with higher education, and the coefficient is negative and insignificant for those with some college education and positive and insignificant for those with completed college education. This is consistent with immigrants with less than college education being closest substitutes for natives with high school education or less and price effect dominating. In contrast, offshoring increases wages of those with less than college education by 0.6 and those with high-school or less by 0.7, with no effect on those with some college or above. This, again, indicates that productivity effect dominates for offshoring, and that offshoring more strongly affects the low- and medium-skilled workers, who are likely to be part of the factor whose tasks are being offshored.

In panel B of Table 1.6 we turn to a different proxy for skill–wage percentiles. A one percentage point increase in low-skilled immigrants' share of employment leads to 1.2 percent reduction in 10th wage percentile, 0.6 percent reduction in the 25th percentile and an increase of 0.4 and 0.5 in 75th and 90th percentiles, indicating, again, negative price effect dominating for the low-skilled. The positive effect on

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the high-skilled is likely a result of the favorable change in factor ratio. Offshoring exposure increases wages of 25th and 50th percentiles among natives, corroborating the presence of the strongest effect on the low- and medium-skilled.

We next look at how task characteristics of workers affect the way they are impacted by immigration and offshoring. Panels C and D present results by routine, manual, abstract, cognitive and communication intensity. It is evident that the most routine, most manual, least abstract, cognitive and communication-intensive occupations are more strongly affected by immigration, seeing an effect size of about -0.6 to -0.7. This is consistent with workers in jobs with these task intensities being more likely to compete with immigrant workers, which echoes findings from Peri and Sparber (2009) and Peri and Sparber (2011). These task intensity groups are also most subject to the effect of offshoring, which ranges from 0.6 to almost 1, which is consistent with the literature findings of greatest offshoring effect on the most routine, although the sign in other empirical works varies and is often negative at least on some types of offshoring (Ebenstein et al. (2014),Baumgarten, Geishecker and Görg (2013)), suggesting productivity effect on most routine workers dominates in the context studied here but maybe not in others.[25]

[25]It is worth pointing out that part of the reason for the positive effect of offshoring may also be the selective employment effect, where the least productive workers (hence, the lowest paid) drop out of manufacturing.

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1.4.2 Interactive Effect

Table 1.7 tests whether offshoring reinforces, mitigates or has no impact on how immigration impacts native wages for different native groups. The upper panel shows estimation results for the specification with the interaction term by education level. It reveals that there is a negative interactive effect on groups with less than college education. The point estimate of the interaction term is -0.32 on average for those with less than college education. This means that for a 1 percentage point increase in offshoring the effect of immigration becomes more negative by 0.32; consequently, going from offshoring exposure change of 1 standard deviation below mean (0.02) to 1 standard deviation above (0.08) decreases immigration effect from 0.55 (1.19-0.32*2) to -1.37 (1.19-0.32*8). Figure 1.12 illustrates the extent to which interactive effect matters graphically–it shows the effect of immigration on wages of natives with less than college education ranges from positive and significant to negative and significant, depending on the extent of offshoring. The interaction term is even more negative for those with less than high school education and less negative for those with some college, but is not statistically significant for college graduates.

Similar estimates arise from using wage percentiles as skill proxies, with the interaction term coefficient decreasing from -40 to -24 going from 10th to 90th. These results are further reinforced in the lower two panels. The interactive effect of offshoring and immigration on the most manual occupations is almost twice that on the least (-42 compared to -23). Slightly smaller but still large differences are present between least and most routine and least and most abstract occupations. Similarly,

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the most cognitive and communication-intensive occupations are least subject to the negative interactive effect.

Table 1.8 demonstrates one consequence of the unequal effects across skill/wage spectrum. The table presents effects on the ratio of 90th to 10th wage percentile, the spread between the tails; 75th to 25th percentile, the spread in the middle; 90th to 50th, upper-tail spread; and 50th to 10th, lower tail spread. A monotonically more negative effect of immigration on lowest skilled workers as measured by wage percentile leads to increasing polarization. Additionally, the last column shows the effect on the ratio of wages of college-educated to non-college-educated workers. By all 5 measures, (low-skilled) immigration increases polarization, the lower tail more so than the upper tail. Offshoring, on the other hand, slightly decreases the spread, particularly in the upper tail. When it comes to the interactive effect of immigration and offshoring (lower panel), offshoring tends to increase the extent to which immigration increases polarization, but mainly for the upper tail, as well as the wage difference between those with college education and those without.

1.4.3 Additional Results and Robustness Checks

Offshoring Task Intensity

The negative interactive effect suggests that offshoring reinforces the negative effect of immigration, consistent with the model prediction in case of offshoring in-

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creasing immigrant wage share. It is useful, then, to asses whether offshoring does seem to increase immigrant wage share, which we address in Table 1.9. The results tend to confirm this prediction. We look at the effects of offshoring on immigrant wage share in manufacturing and all industries (since later on we also use immigrant share with respect to all CZ employment) separately, and estimate the impact for different subgroups of the low-skilled, with and without instruments for offshoring. The results suggest that a 1 percentage increase in offshoring exposure increases immigrant wage share by between 0.2-0.5 percentage points, depending on specification.

Additional Controls

Since the instrument for offshoring is based on local industrial composition and national industry-level changes in offshoring exposure, it may fail the exclusion restriction if national industry-level offshoring exposure is correlated with factors that may also affect wages. In particular, there is a potential problem if greater import exposure/competition change is positively correlated with offshoring change and also affects native wage changes. This may happen, since factors that promote import competition such as lower wages or higher productivity in a given industry abroad or lower tariffs within and industry may stimulate both offshoring and non-offshoring imports. On the other hand, improvements in communication technology may be more important for trade in tasks, where control over the production process abroad and coordination with tasks performed at home are paramount, than for trade in final goods. Since import competition tends to decrease native wages (Ebenstein et al.

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(2014)), if instrumented offshoring were to be positively correlated with it, it would be downward biased, and the actual positive wage effect of offshoring would be even greater. More importantly, the interaction term estimate would also be biased. To test this, we explicitly control for import competition in the first panel of Table 1.10. The results are very similar to those in Table 1.7, which does not control for imports, suggesting that it is unlikely that omitting import competition biases results. The cofficient on the import competition variable itself is generally negative and small, and is only marginally statistically significant for the college-educated natives.

Another possible reason for the instrument exclusion restriction to fail is if national industry-level offshoring exposure change is correlated with industry-level productivity shocks. If the correlation is positive, since higher labor productivity leads to higher wages, offshoring estimate from previous specifications would be biased upwards, and the interaction term would be biased as well. To address this issue, we add labor demand shocks proxy, a "Bartik" instrument as in Basso and Peri (2015), in the second panel of Table 1.10. The results, again, remain similar. The coefficient on Bartik instrument itself is positive and highly statistically significant, as is expected from a positive demand shock instrument.

Next, in Table 1.11, we include a variety of demographic controls, including average age, share male, black, single, college educated, Asian and Hispanic. Higher average age, share male, college educated, and Asian tend to increase wages, while share Hispanic tends to decrease them. The main outcome of interest, becomes slightly smaller, but is comparable in magnitude and is still highly statistically sig-

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nificant. However, since manufacturing saw a significant labor exit during the period of analysis, and exit out of manufacturing is non-random, with demographic incidence that also has repercussions for wages, it is possible that one or more of these variables are endogenous, which is why we do not include them in the main specification. This point also highlights the fact that even though mobility between large sectors and between CZs is limited, it does exist, as does movement into unemployment, so the estimated wage effects are after these potential adjustments.

Alternative Offshoring Measures

Next, since there is more than one way to define relevant offshore employment, we explore whether using alternative measures makes a difference for the results. In particular, since we include all non-bank employment, inclusive of arm's length affiliates, this may not be most representative of more narrowly defined offshoring–imports from majority-owned affiliates (although in practice the two measures produce similar offshoring exposure estimates). We use the latter definition and present results in the first column of the upper panel in Table 1.12; the estimates indicate that the interactive effect is similar to that in the main specification.

Another possibility is that attributing affiliates to industries based on affiliate industry classification may be different from parent-based industry classification. In theory, parent-based classification is more closely associated with industry engaging in offshoring rather than being offshored. In practice, at the 2-digit SIC level of analysis, a lot of offshoring is intra-industry, and affiliate-based measures are very

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close to parent-based. Column 2 of Table 1.12 shows that this alternative definition produces similar results to the main specification.

Alternative Immigration Measure

The next robustness check entails using an alternative definition of immigration, one that uses a change in the number of immigrants divided by lagged employment to take out potential native outflow; the issue with the latter is that it may be correlated with negative labor demand shocks that also affect wages (and not are not fully captured by "Bartik" labor demand shock instrument), although native outflow is generally limited at the level of analysis used here (and was more limited before 2000 than after). The third column of the upper panel of Table 1.12 shows that the sign of the coefficient on the interaction term is maintained, and it increases in magnitude, but the estimates become more noisy and are not statistically significant; part of the reason for the latter may be that it is the immigrant labor share change that interacts with offshoring (per the theoretical model), rather than the standardized change calculated here. Nevertheless, although noisy, this result is generally in line with the other results.

Lastly, in the lower panel of Table 1.12 we use measures of immigration, wages and offshoring that include non-manufacturing industries. The first column of the lower panel uses immigrant labor share in the entire commuting zone. While mobility between non-manufacturing and manufacturing is limited, it may still play a role in equilibrating local labor markets, and immigrant labor share in the entire CZ

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may matter differently (than within manufacturing); additionally, immigrant share in manufacturing is generally very similar to overall CZ share, and using the latter may decrease the problem of native outflow from manufacturing. The coefficient on the interaction term is slightly larger in size than in the main specification and is statistically significant, corroborating the main results. In column 2 of the lower panel we look at the effects on overall commuting zone wages, not just those in manufacturing, since part of the adjustment to trade and immigration shocks is switching to jobs outside of manufacturing. Here, too, the main results are echoed. Lastly, we calculate a measure of offshoring based on all the industries, not just manufacturing. While the results become very noisy, perhaps because non-manufacturing offshoring cannot be measured as accurately, the sign and size of the coefficient remains comparable to previous results.

1.5 Conclusion

The labor market implications of immigration and offshoring have been of interest to researchers, policy makers and the public for quite some time. While there is a fair amount that is known about the impacts of the two and is not controversial, other consequences are debated and some important questions have not been addressed at all. An important aspect of the growing knowledge about the consequences of these processes is the increasing understanding of the heterogeneity of impact depending on native an foreign worker characteristics, occupation and indus-

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try type, and other factors. In this study, we show that an important source of the heterogeneity of immigration effect is the extent of offshoring exposure. By analyzing the effects of immigration and offshoring jointly, looking at the effects on workers of different skill levels and task specialization, and focusing on local labor market area effects, we provide a novel contribution to the literature–we find a negative interactive effect between immigration and offshoring, whereby greater levels of offshoring exposure reinforce the negative effects of low-skilled immigration on the low-skilled native workers. The effect is especially salient for those least educated, those in the lowest wage percentiles, and those in the most routine, most manual, least abstract, less cognitive and less communication-intensive occupations in manufacturing (and, tentatively, across all industries within the commuting zone). In estimation, we use plausibly exogenous instruments that rely on pre-period immigrant settlement patterns and industrial composition, and the main results are robust to controlling for local labor demand shocks and import competition as well as to alternative definitions of immigration and offshoring shocks. In addition to evidence of negative interactive effect of immigration and offshoring on native wages, we provide estimates of the average immigration and offshoring effects on native wages, with immigration decreasing and offshoring increasing low-skilled wages, but with little effect on the high-skilled. Potential economic mechanisms behind the empirical results can be understood using the theoretical model developed.

The theoretical model developed provides potential explanation for both the average effects of immigration and offshoring and the interactive effect. Specifically, a task-based model that allows complete and incomplete offshoring of native and im-

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migrant tasks provides several insights. First, increase in low-skilled immigrant labor increases the composite low-skilled labor input, which, in turn, decreases marginal product of labor and composite labor price, and consequently, native wages. At the same time, immigrant labor leads to more specialization on the part of natives in tasks in which they have comparative advantage, increasing wages. The balance of the latter productivity effect and the former price effect determines the net effect of immigration on low-skilled native wages. Empirical results suggest the price effect dominates. This net effect is reinforced by higher immigrant wage share. Labor wage share, in turn, is a function of relative wages, which depend on average comparative advantage of immigrants compared to natives. If offshoring affects natives relatively more than immigrants, it reduces average comparative advantage of natives and increases average immigrant comparative advantage and wage share. This way, offshoring increases the elasticity of native wage response to immigration, producing the negative interactive effect estimated. As an additional confirmation of this mechanism, we find empirically that offshoring is likely native task intensive, as it increases immigrant wage share. Lastly, the model shows that the average effect of offshoring on native wages depends on the relative magnitudes of its price and productivity effects, with the empirical results suggesting the latter dominates.

These findings suggest that there are reasons for researchers and policy makers alike to analyze the effects of immigration and offshoring together, rather than separately, and to take into account the extent of one when predicting the impact of the other. In particular, whereas we find that immigration reduces wages of low-skilled natives and offshoring reinforces this effect on average across all commuting zones,

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the theoretical model suggests that this need not be the case for every individual commuting zone: immigration can increase native wages in a given CZ if the productivity effect dominates there, while offshoring may mitigate immigration effect in some CZs if it is more immigrant task intensive there. This study provides both the estimates of how immigration and offshoring interact in affecting wages of low-skilled workers on average across all commuting zones, and the rational for why this effect may be heterogeneous across different labor markets.

It is worth acknowledging one of limitations of the model and empirical analysis, which is that while we assume no employment effect, it is part of the adjustment to labor supply and trade shocks. Hence, the role employment (especially, exit out of labor force) plays in the interactive effect of offshoring and immigration on native labor market outcomes warrants being part of a more comprehensive analysis. We leave this question for future research.

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Figures and Tables for Chapter 1

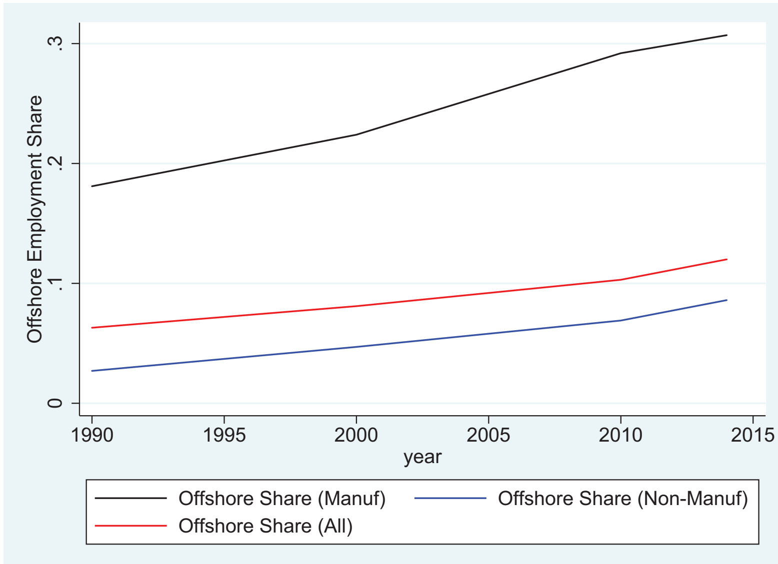

Figure 1.1: Offshore Share of Employment

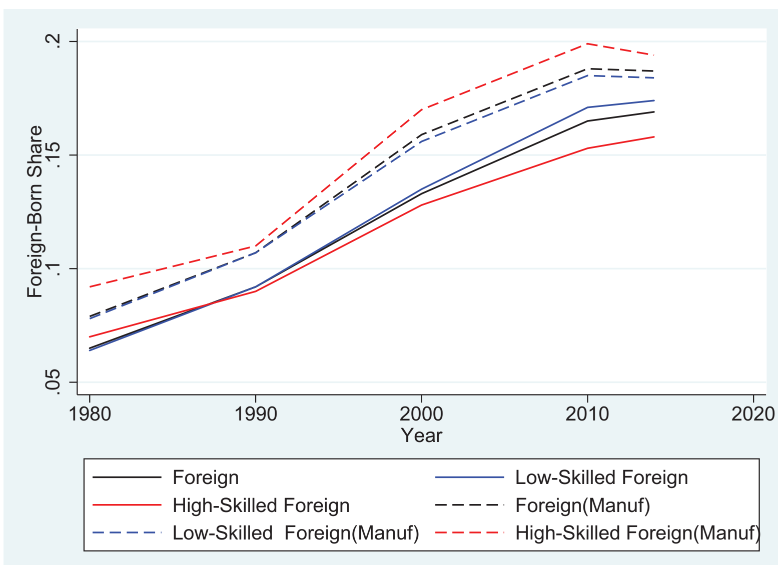

Figure 1.2: Foreign-born share of employment (Within Skill Group)

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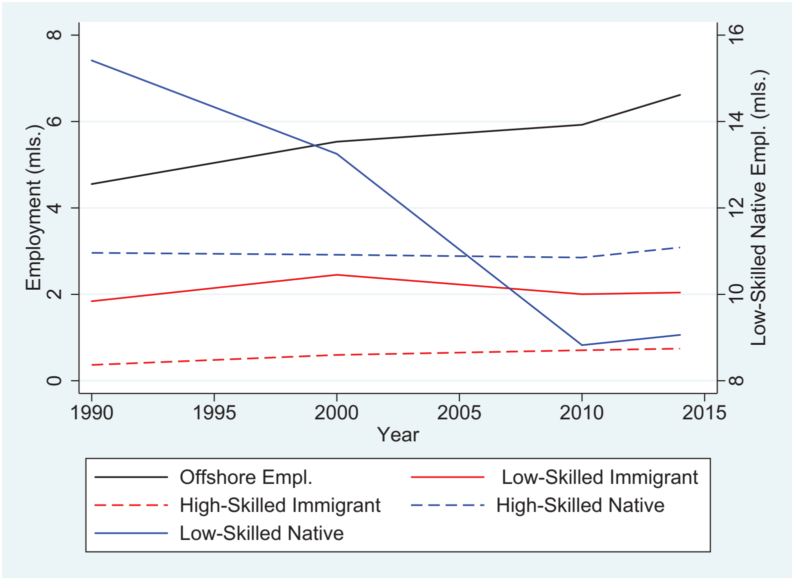

Figure 1.3: Manufacturing Employment Change

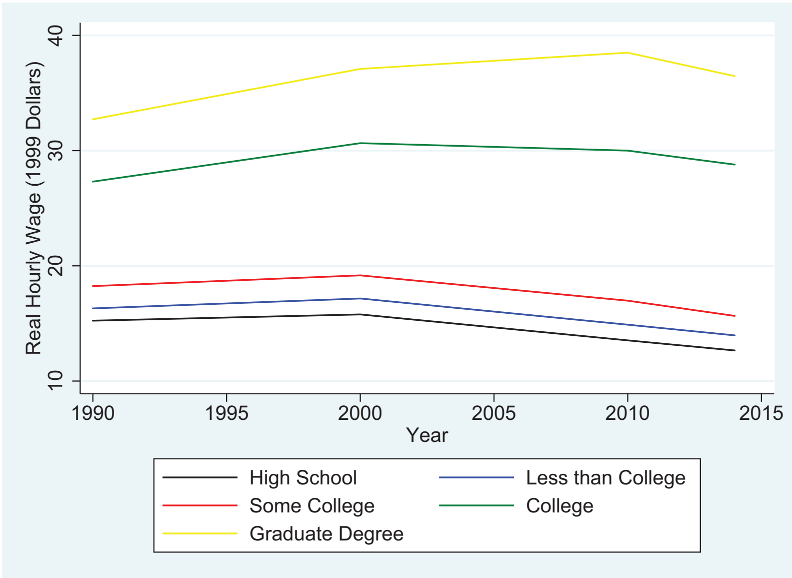

Figure 1.4: Hourly Wage in Manufacturing

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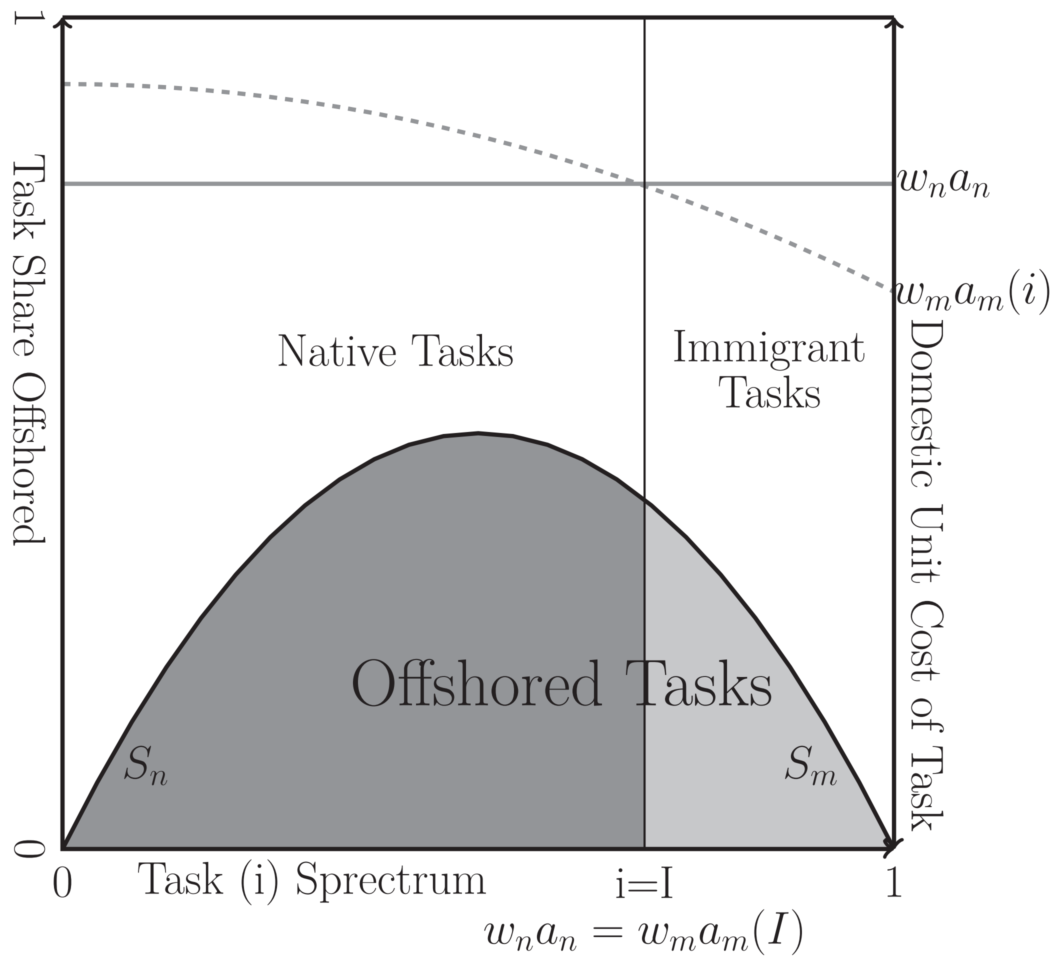

Figure 1.5: Task Division: Native Task Intensive Offshoring

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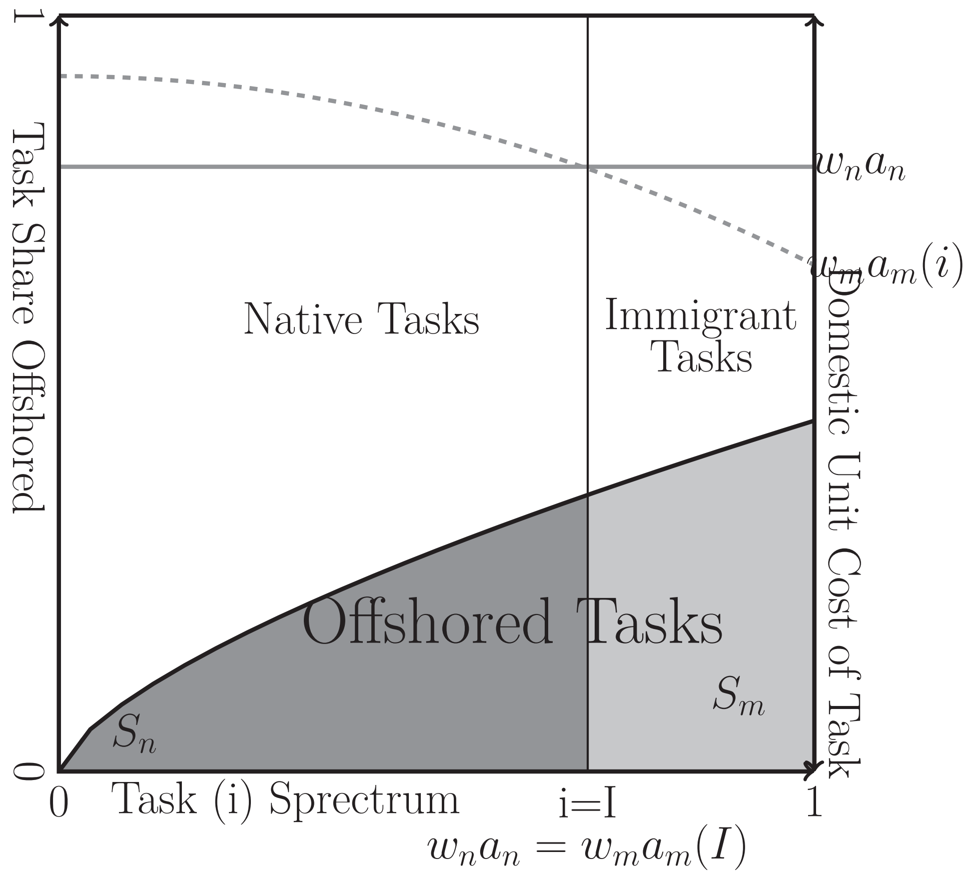

Figure 1.6: Task Division: Migrant Task Intensive Offshoring

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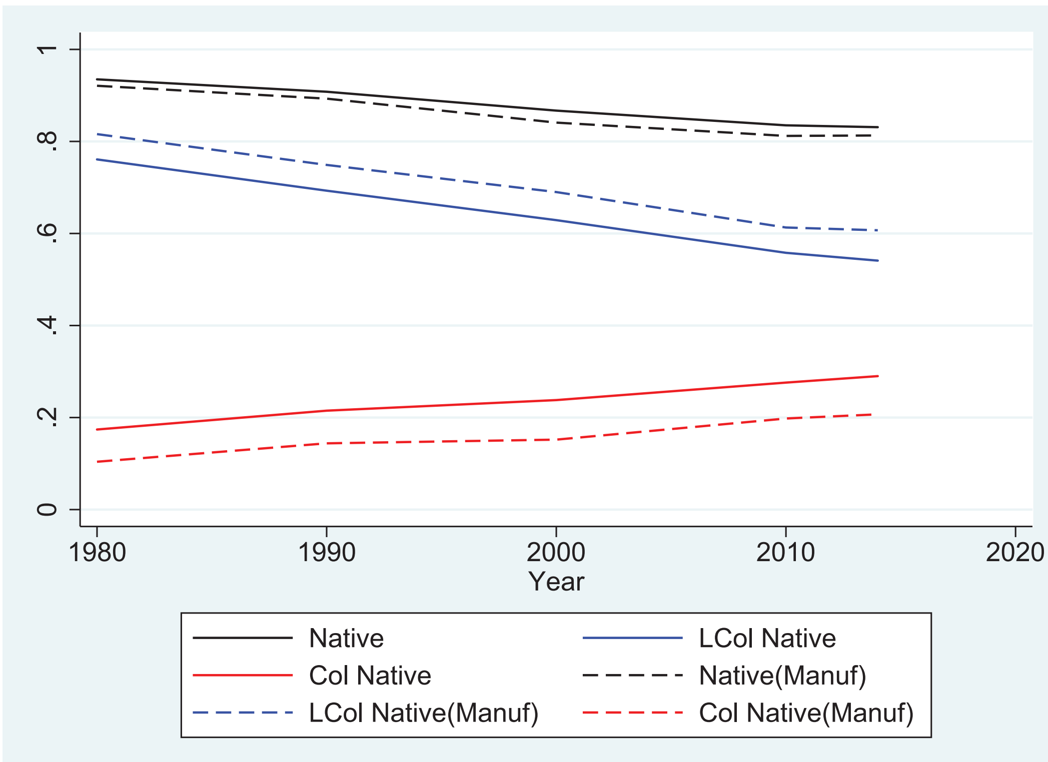

Figure 1.7: Native share of employment

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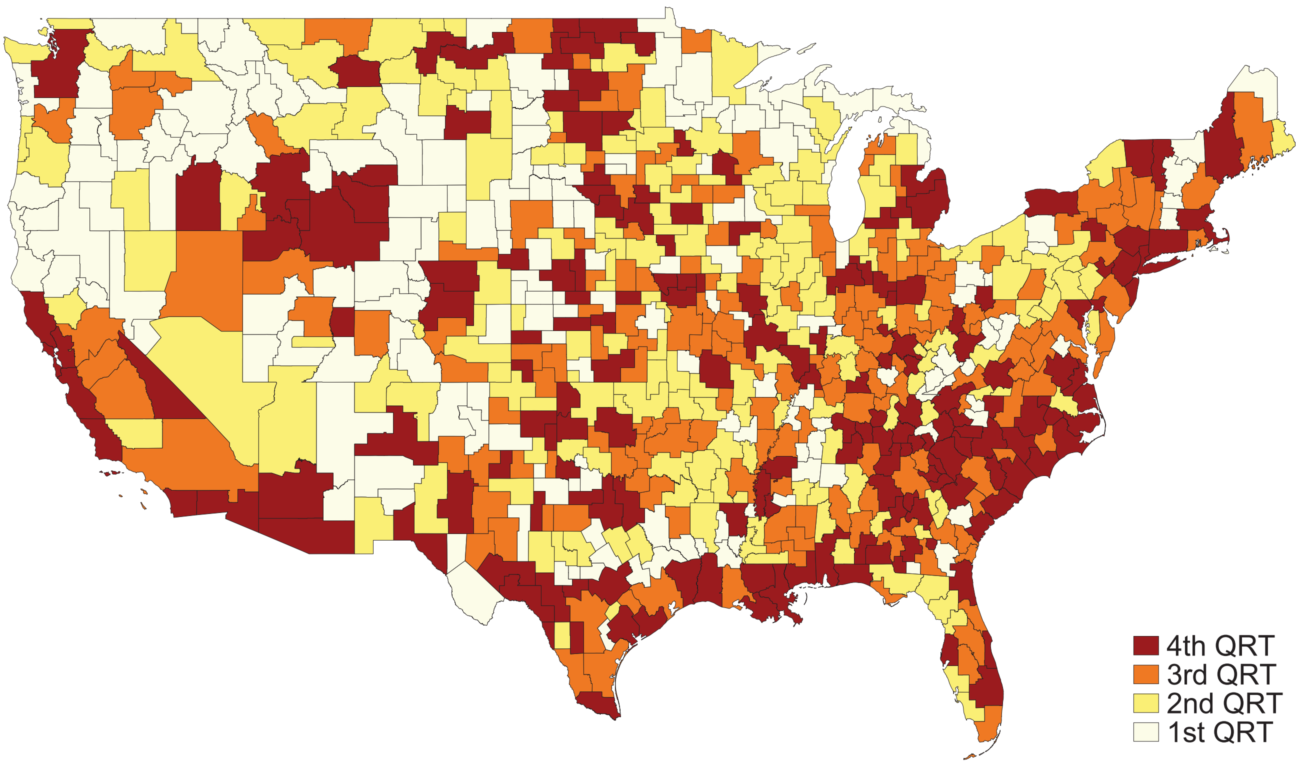

Figure 1.8: Offshoring Exposure (2000)

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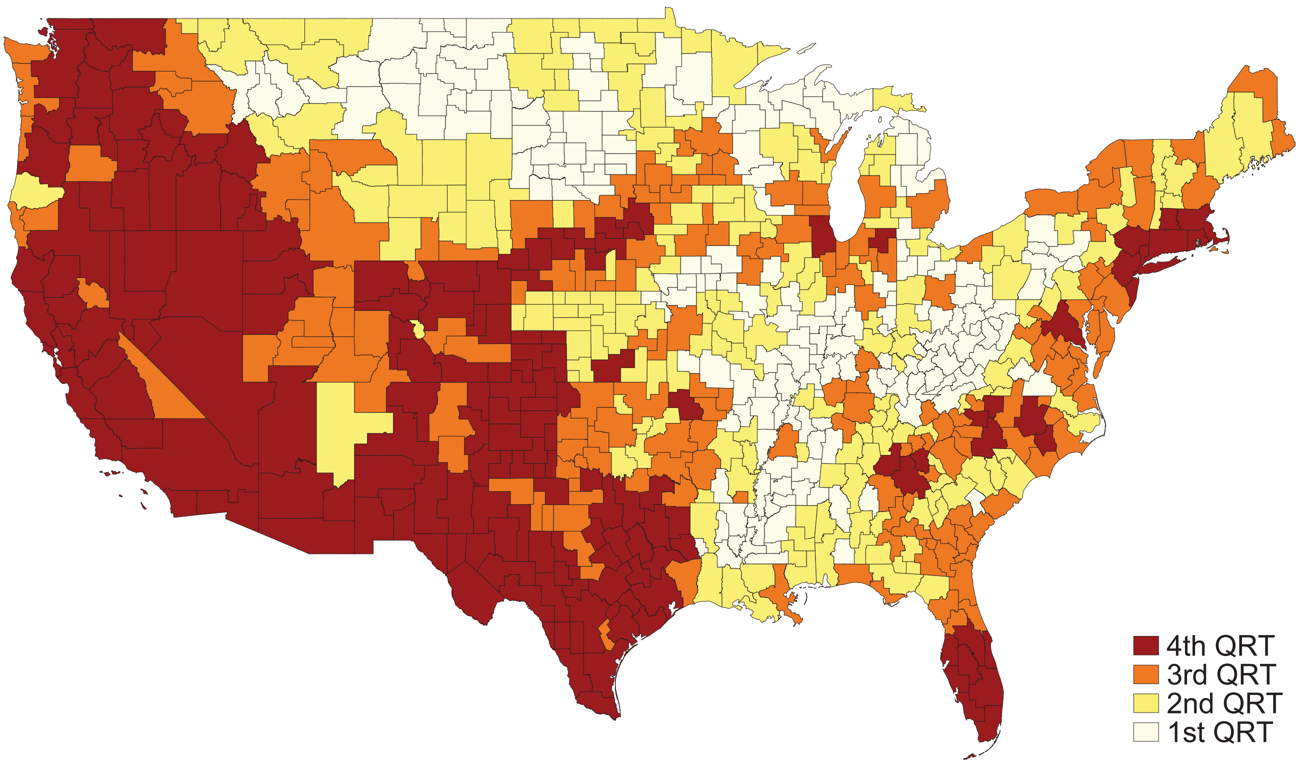

Figure 1.9: Low-Skilled Immigration Exposure (2000)

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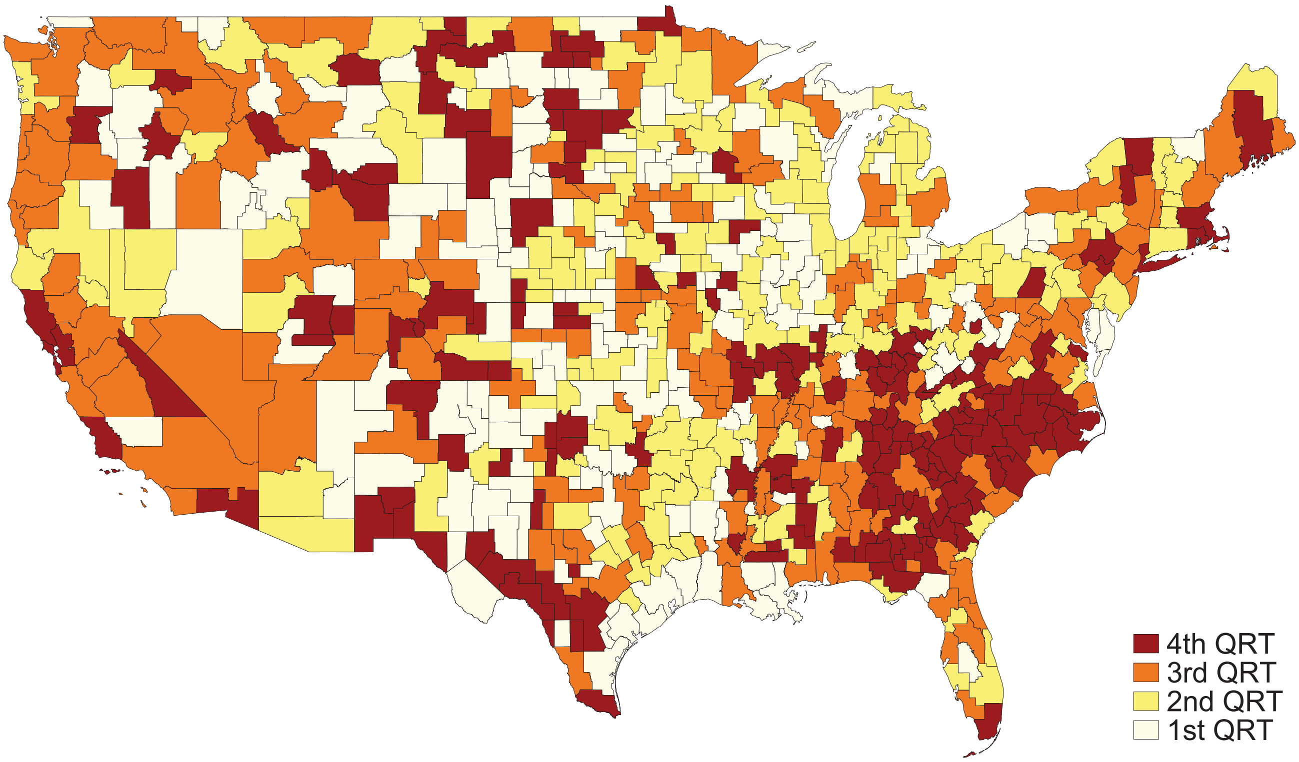

Figure 1.10: Offshoring Exposure Change (1990-2000)

(Correlation with immigration change = -0.007.)

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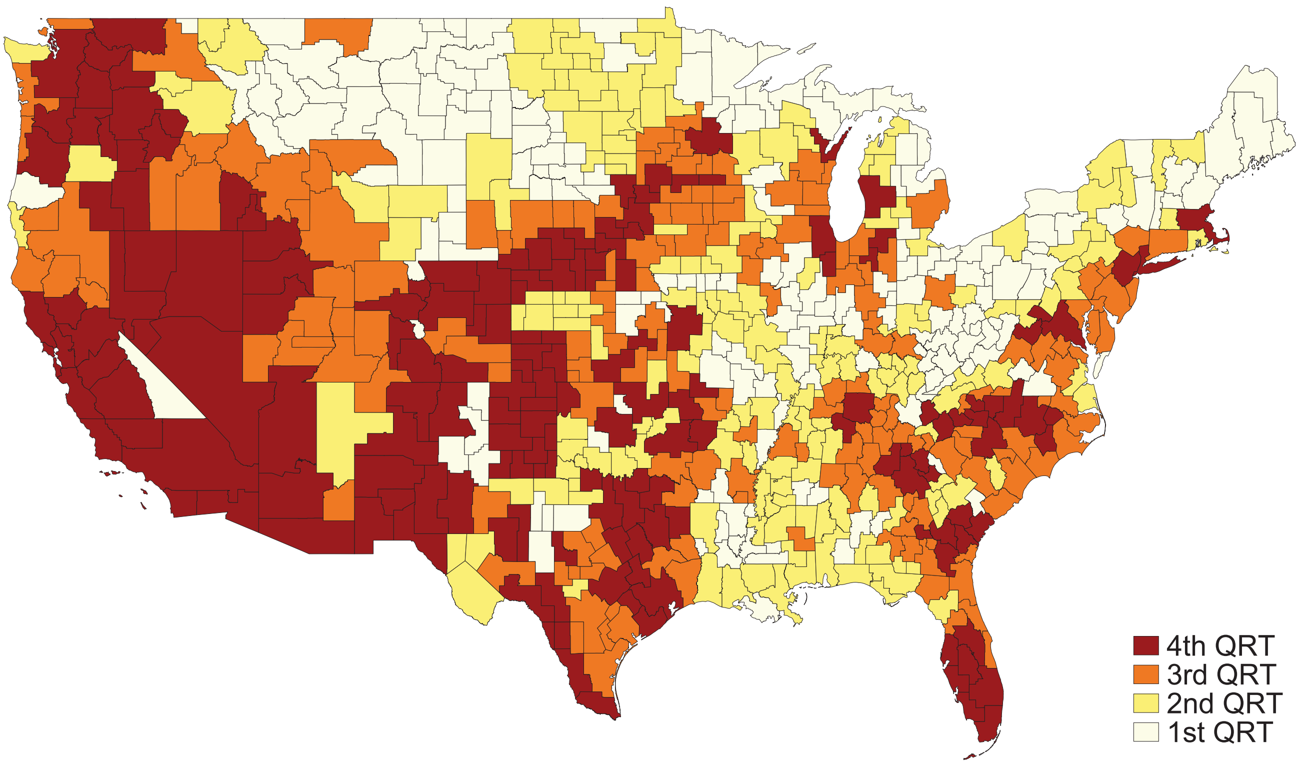

Figure 1.11: Low-Skilled Immigration Exposure Change (1990-2000)

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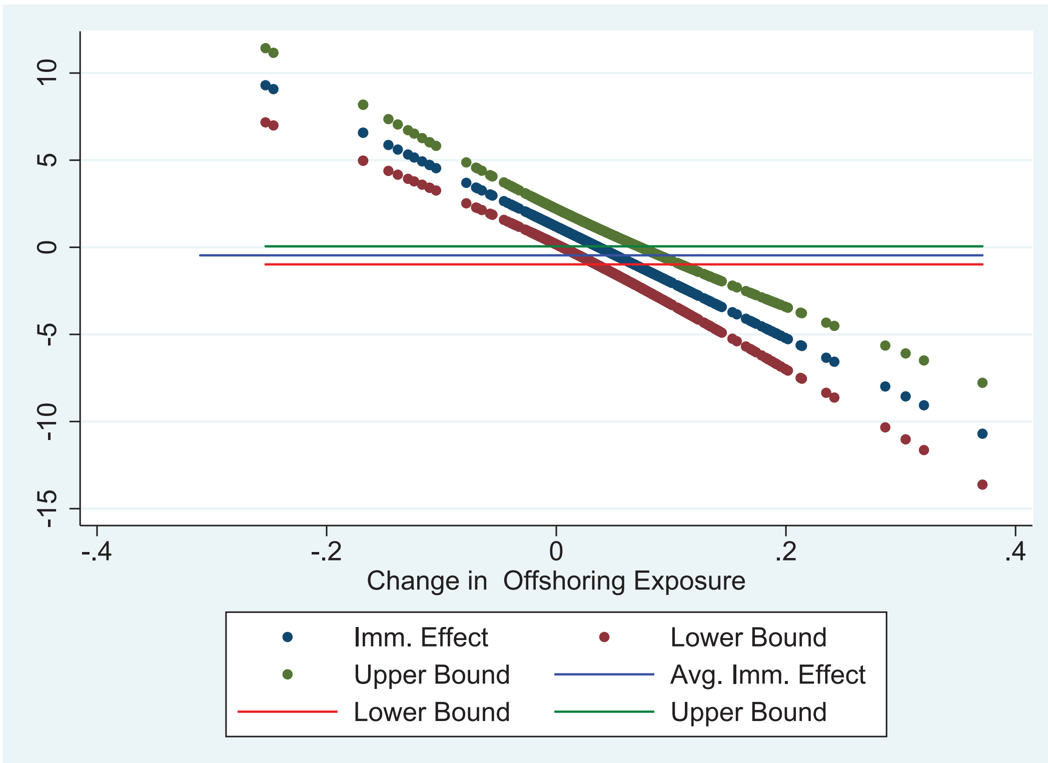

Figure 1.12: Immigration Effect on those with less than BA by Off. Exp

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Table 1.1: Offshoring Exposure: Affiliate Industry-Based (Emp. in '000)

Indname	SIC	Domestic Employment		Offshoring Exposure	
		1990	2000	1990	2000
Food	200	1453	1546	0.24	0.32
Tobacco	210	40	29	0.59	0.68
Textiles and Apparel	220.5	1682	1114	0.05	0.12
Wood & Furniture	240.5	1217	1336	0.04	0.05
Paper and Allied	260	631	603	0.21	0.22
Printing and Publishing	270	1552	1521	0.02	0.07
Chemicals	280	864	830	0.4	0.43
Petroleum	290	113	108	0.68	0.35
Rubber and Plastics	300	883	1060	0.15	0.14
Glass and Stone	320	523	525	0.16	0.14
Primary Metals	330	723	689	0.09	0.13
Fabricated Metals	340	1483	1568	0.1	0.09
Industrial Machinery	350	1922	1893	0.23	0.23
Electrical Equipment	360	1557	1508	0.31	0.36
Transportation Equipment	370	1798	1568	0.33	0.39
Instruments and Related	380	966	792	0.17	0.18
Other Manuf.	390	512	448	0.11	0.14

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Table 1.2: Employment Number and Share by Education and Origin

		All Industries					Manufacturing				
		1980	1990	2000	2010	2014	1980	1990	2000	2010	2014
		Share									
All	Native	0.935	0.908	0.867	0.835	0.831	0.921	0.893	0.841	0.812	0.813
	Foreign	0.065	0.092	0.133	0.165	0.169	0.079	0.107	0.159	0.188	0.187
HS	Native	0.932	0.891	0.826	0.765	0.760	0.918	0.878	0.809	0.763	0.763
	Foreign	0.068	0.109	0.174	0.235	0.240	0.082	0.122	0.191	0.237	0.237
Lcol	Native	0.761	0.693	0.629	0.558	0.541	0.816	0.749	0.69	0.613	0.607
	Foreign	0.052	0.07	0.098	0.116	0.114	0.069	0.089	0.128	0.139	0.137
Scol	Native	0.946	0.935	0.916	0.895	0.894	0.937	0.928	0.908	0.894	0.894
	Foreign	0.054	0.065	0.084	0.105	0.106	0.063	0.072	0.092	0.106	0.106
Col	Native	0.174	0.215	0.238	0.276	0.29	0.104	0.144	0.152	0.198	0.207
	Foreign	0.013	0.021	0.035	0.05	0.054	0.011	0.018	0.031	0.049	0.05
MA	Native	0.871	0.888	0.842	0.821	0.810	0.818	0.825	0.745	0.689	0.701
	Foreign	0.129	0.113	0.158	0.179	0.190	0.182	0.175	0.255	0.311	0.299
		Employment (millions)									
All	All	84.7	101.2	116.3	117.7	122.9	22.0	20.6	19.2	14.4	14.9
	Native	79.2	91.9	100.8	98.3	102.1	20.3	18.4	16.2	11.7	12.1
	Foreign	5.5	9.3	15.5	19.4	20.8	1.7	2.2	3.1	2.7	2.8
Lcol	Native	64.4	70.2	73.2	65.7	66.5	18.0	15.4	13.3	8.8	9.1
	Foreign	4.4	7.1	11.4	13.7	14.0	1.5	1.8	2.5	2.0	2.0
Col	Native	14.7	21.8	27.7	32.5	35.6	2.3	3.0	2.9	2.8	3.1
	Foreign	1.1	2.1	4.1	5.9	6.6	0.2	0.4	0.6	0.7	0.7

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Table 1.3: Regression Tables Descriptive Statistics (1990-2000 Change)

Variable	N	Mean	SD	Min	Max
Wages (HS or Less)	741	0.01	0.07	-0.5	0.24
Wages (Some College)	741	0.04	0.06	-0.66	0.59
Wages (Less than College)	741	0.03	0.06	-0.55	0.35
Wages (College)	741	0.09	0.09	-0.47	1.04
Wages (Masters)	710	0.08	0.2	-1.95	2.38
Wages (10th percentile)	741	0.05	0.09	-0.47	0.73
Wages (25th percentile)	741	0.04	0.07	-0.63	0.51
Wages (50th percentile)	741	0.03	0.07	-0.63	0.35
Wages (75th percentile)	741	0.05	0.07	-0.64	0.46
Wages (90th percentile)	741	0.09	0.08	-0.54	0.56
Wage Polarization (90th/10th)	741	0	0.08	-0.68	0.53
Wage Polarization (75th/25th)	741	0	0.04	-0.3	0.15
Wage Polarization (90th/50th)	741	0.02	0.03	-0.14	0.18
Wage Polarization (50th/10th)	741	-0.02	0.05	-0.41	0.38
Wages (College/No College)	741	0.02	0.04	-0.25	0.37
Wages (Least Manual)	741	0.09	0.07	-0.45	0.69
Wages (Most Manual)	741	0.04	0.08	-0.84	0.42
Wages (Least Routine)	741	0.07	0.08	-0.52	0.57
Wages (Most Routine)	741	0.07	0.08	-0.61	0.84
Wages (Least Abstract)	741	0.04	0.07	-0.68	0.44
Wages (Most Abstract)	741	0.05	0.07	-0.39	0.47
Wages (Least Cognitive)	741	0.01	0.08	-0.75	0.32
Wages (Most Cognitive)	741	0.03	0.08	-0.54	0.69
Wages (Least Communication)	741	0.04	0.08	-0.68	0.45
Wages (Most Communication)	741	0.05	0.1	-0.55	1.16
Immigrant Share (Below College)	741	0.03	0.02	-0.02	0.11
Immigration Instrument	741	0.04	0.05	-0.01	0.21
Offshoring Exposure	734	0.05	0.03	-0.25	0.37
Offshoring Instrument	737	0.03	0.02	-0.24	0.23
Offshoring (Majority Owned)	734	0.06	0.03	-0.23	0.32
Offshoring (Parent-Based)	734	0.04	0.03	-0.29	0.40

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(continued)

Import Competion	722	0.43	0.38	-0.02	7.62
Import Competition Isntrument	722	0.33	0.28	-0.47	4.28
Stardardized Immigration	722	0.07	0.06	-0.02	0.30
Bartik Labor Demand Shocks	722	0.06	0.01	0.04	0.08
Average Age	741	1.69	0.47	-0.56	4.47
Share Male	741	-0.01	0.01	-0.08	0.04
Share Black	741	0.01	0.01	-0.08	0.15
Share Single	741	0.01	0.02	-0.07	0.16
Share College Educated	741	0.04	0.02	-0.10	0.15
Manufacturing Empl. Share	741	-0.04	0.02	-0.24	0.09
Share Asian	741	0.02	0.01	-0.07	0.09
Share Hispanic	741	0.03	0.03	-0.20	0.31

Note: all wages are for native workers.

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Table 1.4: First Stage

	Dimmshare	Doffexp	Dimmshare	Doffexp	Dinteraction
Immigration Instrument	0.661***	0.0191	0.898***	-0.0470	0.00658
	(0.0556)	(0.0480)	(0.109)	(0.0965)	(0.0188)
Offshoring Instrument	0.331*	1.300***	0.598***	1.226***	0.0548**
	(0.130)	(0.127)	(0.156)	(0.140)	(0.0195)
Immigration Inst. # Offshoring Inst.			-6.418*	1.786	0.771
			(3.192)	(3.357)	(0.691)
N	737	734	737	734	734
R-sq	0.65	0.31	0.66	0.31	0.46
F-Stat, Inst.	71.6	58.5	74.0	44.8	15.7

Standard errors in parentheses
Weighted by lagged CZ employment.
+ p<0.10, * p<0.05, ** p<0.010, *** p<0.001

Table 1.5: Immigration and Offshoring Effects of Native Wages (< BA)

	OLS	2SLS	OLS	2SLS
Immigration Change	-0.0509	-0.462+	0.388	1.189*
	(0.191)	(0.250)	(0.256)	(0.521)
Offshoring Change	0.319*	0.576*	0.873***	2.295***
	(0.160)	(0.265)	(0.131)	(0.691)
Immigration Ch.#Offshoring Ch.			-8.894***	-32.03*
			(2.651)	(14.16)
N	734	734	734	734
R-sq	0.03		0.10	
Wald F-stat of 1st Stage		152.5		52.7

Standard errors in parentheses
Weighted by lagged CZ employment.
The dependent variable is change in wages of natives with less than Bachelor's
+ p<0.10, * p<0.05, ** p<0.010, *** p<0.001

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Table 1.6: Offshoring and Immigration Effects on Native Wages

	Panel A. By Education					
	High School or Below	Below College	Some Col.	College		
Immigration Change	-0.663*	-0.462+	-0.185	0.384		
	(0.288)	(0.251)	(0.186)	(0.241)		
Offshoring Change	0.698*	0.576*	0.164	-0.0412		
	(0.290)	(0.265)	(0.215)	(0.234)		
	Panel B. By Wage Percentile					
	p10	p25	p50	p75	p90	
Immigration Change	-1.172**	-0.579+	-0.0314	0.398+	0.504+	
	(0.430)	(0.321)	(0.280)	(0.232)	(0.287)	
Offshoring Change	0.481	0.686*	0.602*	0.258	-0.0552	
	(0.385)	(0.299)	(0.288)	(0.250)	(0.200)	
	Panel C. By Task Intensity					
	Manual		Routine		Abstract	
	Least	Most	Least	Most	Least	Most
Immigration Change	-0.0427	-0.632*	0.0376	-0.715*	-0.639*	0.266
	(0.272)	(0.271)	(0.268)	(0.333)	(0.275)	(0.222)
Offshoring Change	0.215	0.814**	-0.120	0.950*	0.622*	-0.0868
	(0.281)	(0.300)	(0.191)	(0.369)	(0.313)	(0.177)
	Panel D. By Task Intensity (Least/Most Intensive Third)					
	Communication			Cognitive		
	Least	Middle	Most	Least	Middle	Most
Immigration Change	-0.709*	0.238	0.187	-0.676*	-0.127	0.146
	(0.311)	(0.255)	(0.246)	(0.293)	(0.252)	(0.214)
Offshoring Change	0.785**	-0.161	-0.195	0.804**	0.302	0.114
	(0.300)	(0.272)	(0.159)	(0.298)	(0.236)	(0.182)

Standard errors in parenthesis

Weighed by lagged CZ employment. The number of observations is 734 in each regression.

+ p<0.10 * p<0.05 ** p<0.010 *** p<0.001

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Table 1.7: Offshoring and Immigration Effects on Native Wages (with Interaction)

Panel A. By Education

	High School or Below	Below College	Some Col.	College
Immigration Change	1.136+	1.189*	0.968**	1.119**
	(0.626)	(0.521)	(0.354)	(0.418)
Offshoring Change	2.573**	2.295***	1.365**	0.725
	(0.815)	(0.691)	(0.475)	(0.524)
Immigration Ch.#Offshoring Ch.	-34.92*	-32.03*	-22.38*	-14.28
	(17.16)	(14.16)	(9.163)	(9.971)

Panel B. By Wage Percentile

	p10	p25	p50	p75	p90
Immigration Ch.#Offshoring Ch.	-40.12+	-35.96*	-35.72*	-29.93**	-24.44*
	(22.18)	(17.73)	(15.38)	(11.48)	(10.87)
Immigration Change	0.896	1.274*	1.809**	1.940***	1.764***
	(0.787)	(0.646)	(0.579)	(0.518)	(0.437)
Offshoring Change	2.634*	2.616**	2.519***	1.865***	1.257*
	(1.054)	(0.845)	(0.736)	(0.561)	(0.524)

Panel C. By Task Intensity (Least/Most Intensive Third)

	Manual		Routine		Abstract	
	Least	Most	Least	Most	Least	Most
Immigration Ch.#Offshoring Ch.	-23.23*	-41.96*	-29.57+	-39.80*	-31.34+	-20.68+
	(11.06)	(19.69)	(15.88)	(16.39)	(16.66)	(11.01)
Immigration Change	1.234**	1.447*	1.481*	1.419*	0.975	1.331**
	(0.427)	(0.724)	(0.617)	(0.634)	(0.611)	(0.435)
Offshoring Change	1.126*	3.202***	1.803*	2.950***	2.304**	1.023*
	(0.535)	(0.933)	(0.768)	(0.795)	(0.803)	(0.515)

Panel D. By Task Intensity (Least/Most Intensive Third)

	Communication			Cognitive		
	Least	Middle	Most	Least	Middle	Most
Immigration Ch.#Offshoring Ch.	-26.99	-29.55**	-18.11	-28.72+	-30.47*	-21.24+
	(18.36)	(9.281)	(11.36)	(16.91)	(12.32)	(10.97)
Immigration Change	0.682	1.761***	1.120**	0.804	1.443**	1.240**
	(0.676)	(0.411)	(0.429)	(0.620)	(0.509)	(0.429)
Offshoring Change	2.234**	1.425**	0.777	2.346**	1.937**	1.254*
	(0.852)	(0.496)	(0.526)	(0.804)	(0.612)	(0.521)

Standard errors in parenthesis

Weighed by lagged CZ employment. The number of observations is 734 in each regression.

+ p<0.10 * p<0.05 ** p<0.010 *** p<0.001

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Table 1.8: Offshoring/Immigration Effects on Wage Polarization

	9010	7525	9050	5010	Col/Lcol
Immigration Change	1.264***	0.490***	0.194***	0.795***	0.354***
	(0.242)	(0.100)	(0.0397)	(0.171)	(0.0617)
Offshoring Change	-0.530+	-0.311*	-0.320***	-0.0615	-0.309**
	(0.288)	(0.121)	(0.0910)	(0.157)	(0.109)

With Interaction

	9010	7525	9050	5010	Col/Lcol
Immigration Ch.#Offshoring Ch.	24.46+	7.890	7.655*	10.54	9.778*
	(14.55)	(5.757)	(3.620)	(8.079)	(3.845)
Immigration Change	0.00315	0.0833	-0.200	0.252	-0.150
	(0.540)	(0.234)	(0.148)	(0.304)	(0.161)
Offshoring Change	-1.843**	-0.735**	-0.731***	-0.627	-0.834***
	(0.711)	(0.280)	(0.190)	(0.391)	(0.209)

Standard errors in parenthesis

Weighed by lagged CZ employment. The number of observations is 734 in each regression.

+ $p<0.10$ * $p<0.05$ ** $p<0.010$ *** $p<0.001$

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Table 1.9: Offshoring Effect on Migrant Wage Share

	OLS			2SLS		
	HS	SCOL	LCOL	HS	SCOL	LCOL
	Overall Across CZone					
Offshoring Change	0.265+	0.208*	0.227*	0.342	0.252*	0.293+
	(0.146)	(0.0854)	(0.112)	(0.235)	(0.107)	(0.167)
N	734	734	734	734	734	734
R-sq	0.03	0.10	0.05	0.03	0.09	0.05
	Manufacturing Sector Only					
Offshoring Change	0.230	0.187+	0.234	0.485	0.301*	0.485+
	(0.180)	(0.0983)	(0.151)	(0.307)	(0.152)	(0.248)
N	734	734	734	734	734	734
R-sq	0.02	0.03	0.03	.	0.02	.

Standard errors in parenthesis

Weighed by lagged CZ employment.

+ p<0.10 * p<0.05 ** p<0.010 *** p<0.001

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Table 1.10:
Robustness Checks: Import Competition, Labor Demand Shocks

	With Imp. Competition				
	High School or Below	Below College	Some Col.	College	Col/LCol
Immigration Change	1.115+	1.144*	0.892**	1.042**	-0.160
	(0.604)	(0.487)	(0.308)	(0.380)	(0.155)
Offshoring Change	2.543**	2.255**	1.316**	0.772	-0.793***
	(0.852)	(0.710)	(0.471)	(0.534)	(0.223)
Immigration Ch.#Offshoring Ch.	-34.33*	-31.01*	-20.86*	-13.67	9.503*
	(16.97)	(13.61)	(8.185)	(9.128)	(3.868)
Import Competition	0.00226	-0.00136	-0.00805	-0.0363+	-0.0149+
	(0.0269)	(0.0236)	(0.0183)	(0.0206)	(0.00892)
	With Bartik Labor Demand Shocks				
	High School or Below	Below College	Some Col.	College	Col/LCol
Immigration Change	0.813	0.886+	0.705*	0.884*	-0.104
	(0.583)	(0.462)	(0.280)	(0.346)	(0.159)
Offshoring Change	2.200**	1.962**	1.103**	0.593	-0.729***
	(0.772)	(0.641)	(0.426)	(0.509)	(0.221)
Immigration Ch.#Offshoring Ch.	-31.80*	-28.85*	-19.29**	-12.35	9.035*
	(15.76)	(12.52)	(7.314)	(8.302)	(3.744)
Bartik LD Shocks	3.615***	3.084***	2.244***	1.889+	-0.669
	(0.949)	(0.815)	(0.655)	(1.072)	(0.430)
Import Competition	-0.00440	-0.00705	-0.0122	-0.0398+	-0.0137+
	(0.0250)	(0.0223)	(0.0175)	(0.0213)	(0.00794)

Standard errors in parenthesis

Weighed by lagged CZ employment. The number of observations is 734 in each regression.

+ p<0.10 * p<0.05 ** p<0.010 *** p<0.001

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Table 1.11: Robustness Checks: Demographic Controls

	With Imp. Competition				
	High School or Below	Below College	Some Col.	College	Col/LCol
Immigration Change	0.731*	0.627*	0.285	0.447	-0.156
	(0.318)	(0.257)	(0.235)	(0.396)	(0.156)
Offshoring Change	1.310*	1.117*	0.430	0.149	-0.499*
	(0.546)	(0.457)	(0.371)	(0.460)	(0.232)
Immigration Ch.#Offshoring Ch.	-19.95**	-16.46**	-8.243+	-2.485	7.030*
	(7.746)	(5.989)	(4.399)	(6.603)	(3.165)
Change in Avg. Age	0.0100+	0.0157**	0.0226***	0.0162*	-0.000776
	(0.00568)	(0.00501)	(0.00557)	(0.00683)	(0.00327)
Change in Share Male	0.795***	0.695***	0.535***	0.117	-0.282**
	(0.174)	(0.152)	(0.145)	(0.181)	(0.0937)
Change in Share Black	0.298+	0.154	-0.0285	0.301	0.0414
	(0.165)	(0.131)	(0.121)	(0.184)	(0.0864)
Change in Share Single	-0.301	-0.123	-0.0164	-0.365	-0.0993
	(0.257)	(0.212)	(0.184)	(0.252)	(0.120)
Change in Share College Grad.	0.269	0.398*	0.449**	0.626**	0.0625
	(0.215)	(0.168)	(0.140)	(0.209)	(0.111)
Change in Share Asian	0.677	1.054*	1.260**	2.116***	0.325
	(0.551)	(0.469)	(0.480)	(0.606)	(0.230)
Change in Share Hispanic	-1.120***	-0.882***	-0.439**	-0.558*	0.213+
	(0.273)	(0.211)	(0.169)	(0.264)	(0.113)

Standard errors in parenthesis

Weighed by lagged CZ employment. The number of observations is 734 in each regression.

+ p<0.10 * p<0.05 ** p<0.010 *** p<0.001

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Table 1.12: Robustness Checks: Other

	Majority-Owned	Parent-Based	Standard. Imm.
Immigration Ch.#Offshoring Ch.	-27.27+	-24.69+	-45.52
	(15.12)	(12.95)	(41.27)
Immigration Change	1.344+	0.573	0.512
	(0.741)	(0.520)	(1.089)
Offshoring Change	1.685*	1.663*	1.816
	(0.772)	(0.821)	(1.504)

	CZ Immig. Share	CZ-Wide Wages	W/ Services Offshoring
Immigration Ch.#Offshoring Ch.	-38.38**	-24.98**	-45.77
	(13.12)	(9.600)	(49.51)
Immigration Change	1.241*	0.525	-0.132
	(0.483)	(0.354)	(0.693)
Offshoring Change	1.894***	1.498***	2.650
	(0.529)	(0.384)	(1.721)
Change in Share in Manuf.		0.511**	0.361*
		(0.170)	(0.150)

Standard errors in parenthesis

Weighed by lagged CZ employment. The number of observations is 734 in each regression.

+ p<0.10 * p<0.05 ** p<0.010 *** p<0.001

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Appendix: Theoretical Model Special Case

Whereas the model in the main part of the paper is more flexible and generalizable, we can also obtain analogous insights from modeling offshoring in a more standard way–where an offshored task is offshored completely. To see this in the most transparent way, consider the offshoring of a range of tasks performed originally by native workers $[0, \Delta_n]$, and a range of tasks originally performed by immigrants $[1-\Delta_m, 1]$, where $\Delta_m > 0$, and $\Delta_n > 0$. We show that whether native or immigrant tasks are offshored directly determines whether native workers adjust to offshoring by shifting to tasks that have relative comparative advantage in. In particular, the analogue of (A2) with offshoring is:

$$\frac{\sigma}{1-\sigma} = \frac{\int_I^{1-\Delta_m} a_m(i)di}{a_n(I-\Delta_n)}, \quad \theta = \frac{w_m \int_I^{1-\Delta_m} a_m(i)di}{w_m \int_I^{1-\Delta_m} a_m(i)di + w_n a_n(i)(I-\Delta_n)}. \tag{C1}$$

By inspection, the offshoring of native tasks shifts the threshold task to the right, allowing immigrants to specialize more in tasks where they have comparative advantage, while the opposite applies to native workers. The offshoring of immigrant tasks is analogous. Furthermore, offshoring allows more native workers and immigrants to be devoted to the remaining tasks. The result is an increase in the employment of the composite labor input, as can be seen below:

$$Y = N/(a_n(I-\Delta_n)), \quad Y = M/\int_I^{1-\Delta_m} a_m(i)di. \tag{C2}$$

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Finally, let $w_o(i)$ denote the wage cost of a unit of task i selected to be offshored. To account for the cost savings of offshoring, assume henceforth that

$$w_o(i) = (1-\gamma_i)\min\{w_n a_n, w_m a_m(i)\}, \quad \gamma_i \in (0,1),$$

where γ_i denotes proportional cost savings.[26] Introducing these changes into the model, the native wage can be express as

$$w_n = P/\phi(I, \Delta_n, \Delta_m), \tag{C3}$$

where

$$\phi(I,\Delta_n,\Delta_m) \equiv \left(a_n I + B(I)\int_0^1 a_m(i)di - \int_0^{\Delta_n}\gamma_i a_n di - B(I)\int_{1-\Delta_m}^1 \gamma_i a_m(i)di\right), \tag{C4}$$

where, once again, the native wage depends on a price effect and a productivity effect. The price effect depends directly on the employment of the composite labor input Y in (C2). The productivity effect $\phi(I,\Delta_n,\Delta_m)$ depends on the range of tasks natives specialize in, I, as well as the cost savings of offshoring Δ_n and Δ_m that spill over to benefit native workers (Grossman and Rossi-Hansberg (2008)). Using the results above and following the steps from the offshoring model in the main part of

[26]Note, cost savings are allowed to differ by taks, but the main results are the same if they are constant across tasks.

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the paper, it can be shown that

$$\frac{\hat{w}_n}{\hat{\sigma}} = \left(\alpha - 1 + \varepsilon\tilde{\theta}\right)\frac{\theta}{1-\sigma} \tag{C5}$$

$$\frac{\hat{w}_n}{\hat{\Delta}_n} = (\alpha-1)(1-\theta)\frac{\Delta_n}{I-\Delta_n} - \varepsilon\tilde{\theta}\frac{\Delta_n}{I-\Delta_n}\theta + \Omega_n\Delta_n \tag{C6}$$

$$\frac{\hat{w}_n}{\hat{\Delta}_m} = [(\alpha - 1 + \varepsilon\tilde{\theta})\theta\zeta_m\Delta_m + \Omega_m\Delta_m] \tag{C7}$$

where, $\varepsilon = \frac{dB(I)/B(I)}{dI/(I-\Delta_n)}$, $\tilde{\theta} = \frac{\int_I^1 a_m(i)di - \int_{1-\Delta_m}^1 \gamma_i a_m(i)di}{\phi(I,\Delta_n,\Delta_m)}$, $\Omega_n = \frac{\gamma(\Delta_n)a_n}{\phi(I,\Delta_n,\Delta_m)}$,$\Omega_m = \frac{B(I)\gamma(1-\Delta_m)a_m(1-\Delta_m)}{\phi(I,\Delta_n,\Delta_m)}$, and $\zeta_m = \frac{a_m(1-\Delta_m)}{\int_I^{1-\Delta_m} a_m(i)di}$]

From (C5), we can observe that the expression for the native wage impact of immigration is very similar to (B12), except for the absence of ζ in the productivity term and slightly different expressions for $\tilde{\theta}$ and ε. Proposition 1 can be expressed as before:

Proposition A1. *The native wage impact of low-skilled immigration is negative (positive) if (and only if) the productivity effect is small (large) relative to the price effect. In both cases, if the difference between productivity parameter ε and price effect $\alpha-1$ is* ***sufficiently*** *large, a higher immigrant wage share magnifies the native wage impact of low-skilled immigration, all else equal.*

With regards to the effect of offshoring, as long as offshoring gives rise to wages savings, $\gamma_i > 0$, the possibility of a native wage gain subsequent to either type of offshoring exists. For native task offshoring, the price effect $(\alpha-1)(1-\theta)\frac{\Delta_n}{I-\Delta_n}$ and comparative advantage effect $\varepsilon\tilde{\theta}\frac{\Delta_n}{I-\Delta_n}\theta$ are negative (for the latter–because na-

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tives are pushed to perform tasks in which they have less comparative advantage), while the productivity effect is positive. On the other hand, for offshoring of immigrant tasks, price effect $(\alpha - 1)\theta\zeta_m\Delta_m$ is negative, and comparative advantage effect $\varepsilon\tilde{\theta}\theta\zeta_m\Delta_m$ (because natives now perform tasks in which they have greater comparative advantage) and productivity effect $\Omega_m\Delta_m$ are positive. Naturally, the overall effect depends on the balance of the three effects.

Proposition A2. *Offshoring of native tasks increases native wages if the productivity effect dominates the price/labor supply and comparative advantage effects, and decreases them otherwise.*
Analogously, offshoring of immigrant tasks increases native wages if the productivity effect and comparative advantage effect dominate the price/labor supply effect, and decreases them otherwise.

In summary, the overall native wage effects of offshoring are ambiguous, depending on the relative magnitudes of the price, comparative advantage and productivity effects. Furthermore, since offshoring impacts the immigrant wage share, the precise nature of offshoring has very nuanced implications on the native wage impact of *immigration*:

Proposition A3. *Offshoring of native tasks increases the immigrant wage share, and reinforces the negative (positive) wage impact of immigration if the productivity parameter ϵ is sufficiently small (large) relative to the price effect $(1 - \alpha)$.*

Offshoring of immigrant tasks decreases the immigrant wage share, and mitigates

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against the negative (positive) wage impact of immigration if the productivity parameter ϵ is sufficiently small (large) relative to the price effect $1 - \alpha$.

Proposition 3A is more direct and less ambiguous than Proposition 3, as it is certain that native task offshoring increases immigrant wage share and offshoring of immigrant tasks–decreases it. On the other hand, the environment producing the above result is more stylized than that in the general model in the main part of the paper. This more stylized environment, by modeling offshored tasks as offshored completely and exploiting assumptions about their location, is more similar to what is commonly featured in the literature (Ottaviano, Peri and Wright (2013)). This simpler environment produces insights very similar to those in the main theory section, with the same mechanisms at work, and also helps understand factors likely behind the empirical results.

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CHAPTER 2

DO IMMIGRANTS PROMOTE TRADE WITH THIRD PARTY COUNTRIES? ON THE ROLE OF GEOGRAPHIC AND LINGUISTIC PROXIMITY

2.1 Introduction

Since the seminal work of Gould (1994), there has been an explosion of interest in studying the connection between immigration and trade. Previously, it had been analyzed through the Heckscher-Ohlin Model, which treated production factor trade and commodity trade as substitute processes (Mundell (1957)), implying movement in the opposite directions for migration and trade. This meant that easier immigration would likely reduce trade. Gould's work, which found that immigration increases trade with immigrant countries of origin, helped shift the analysis framework to that of immigrants as trade facilitators. Despite a large amount of additional evidence supporting the finding that immigration promotes trade, with economically significant average elasticity of about 0.16 (Genc et al. (2011)), trade promotion role of immigrants has not yet become a significant part of immigration or trade policy discussions. Yet proper accounting for immigrants' trade promotion effect is potentially important for policy formulation, since it is needed for more accurate understanding of benefits and costs of immigration, of ways to reduce trade barriers, and in case of the U.S., due to uneven public support for immigration and trade. Over 70% of Americans think of trade positively–as an opportunity for growth–while only 25%

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think of it as a threat (Jones (2018)). In contrast, only 45% of Americans think of immigrants as good for the economy, whereas 52% think they make it worse or make no difference (McCarthy (2017)). A broader understanding among the public of how immigration affects a process that is widely seen as economically beneficial could potentially elicit more support for local and national policies that reflect trade promotion effect of immigration. This paper accentuates potential importance of immigration in trade promotion discussions and importance of trade-related effects in immigration policy considerations by highlighting and thoroughly investigating a heretofore scarcely explored direction of immigration-trade link–to countries geographically and linguistically proximate and distant from immigrant country of origin.

The two main channels that are generally used to explain the immigrant trade facilitation effect are networks and knowledge/information. The former refers to reducing costs of searching for destination country business partners, negotiating and enforcing contracts by drawing on business relationships and contacts that immigrants may have in home countries; these informal relationships may be particularly important in countries with weak rule of law and institutions. The latter refers to information about legal, institutional and cultural aspects of export markets as well as language ability, as immigrants can improve communication and logistics and reduce search costs for foreign market information; this channel is especially important for trade among countries with different predominant languages and most dissimilar legal, institutional and cultural environment. While trade facilitation effect applies to both exports and imports, there is a lesser third channel, home preference effect

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(whereby immigrants prefer certain commodities from their home countries), which applies only to imports.

Although in nearly all empirical work to date the impact of immigrants has been assessed with regards to exports to or imports from their countries of origin, the above mechanisms suggest that the effect can also apply to trade with third party countries, as anyone with a network in or relevant legal, institutional, cultural or other knowledge of a given country c, or in possession of language skills that facilitate communication, can have a pro-trade effect with respect to that country. This idea overlaps with the motivation for the small number of papers in the literature that look specifically at cross-national ethnic networks. Rauch and Trindade (2002) and Giovannetti and Lanati (2015) show that, respectively, larger Chinese and Indian immigrant communities bolster trade between countries where they reside, and Felbermayr, Jung and Toubal (2010) show this also to be true for certain other large ethnic groups. What this leaves unaddressed, however, is the possibility of inter-ethnic networks or inter-ethnic spillover effect more generally.

To the best of our knowledge, Bratti, De Benedictis and Santoni (2014) is the only paper in the literature to raise the possibility of inter-ethnic spillovers. They do it mostly within the framework of discussion of omitted variable bias in estimation of the direct bilateral migration-trade elasticity, which may arise if migration decisions of "close" ethnic groups are correlated and spillover effect exists. They operationalize "close" by 1) aggregating over immigrants from countries where the same language

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is spoken by over 9% of the population and 2) trade affinity.[1] The findings suggest positive inter-ethnic spillovers for exports but not imports and that the language-based measure is significantly more important.[2] Since the inclusion of inter-ethnic spillovers is mainly to bolster confidence in the estimate of the own-country trade elasticity, little effort is made to examine the channels through which inter-ethnic spillovers operate (for example, whether the importance of linguistically proximate immigrants is due to them also being geographically close) or what is the influence of immigrants who are not a "close" ethnic group.

We address the above concerns, among others, and extend the analysis of immigration effect on trade to third party countries in a number of ways. First, there is a better way to find "close" ethnic groups than the "1)" above. To begin with, at the lower threshold, if 9% of people in two countries speak the same language, the probability of randomly chosen two people from the respective countries speaking the same language is less than 1% (0.81%, to be exact), but method "1)" would ascribe 100% of immigrants from respective countries as being ethnically close. Additionally, it is evident that speaking the same language is not indicative of belonging to the same or close ethnic group. The two problems together may lead to both misidentifying and overestimating ethnically close immigrants. Among many potential examples, consider Cameroon and Austria; both meet the threshold for share of population speaking French. The above methodology would suggest all Cameroonians are eth-

[1]Refer to Bratti, De Benedictis and Santoni (2014) for more details.

[2]The latter result is rationalized using the observation that countries with more "close" immigrants through language tend to be developing countries, for which migration-trade elasticity tends to be higher.

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nically close to all Austrians. A more reasonable proxy is the percent of the population who share a given *native* language, a criteria by which neither Cameroonians nor Austrians are assigned French ethnicity, but by which, for example, 20% of Swiss are ethnically close to 36% of Belgians on account of being native French speakers, with 7.2% probability of randomly chosen two people from the two countries being ethnically close. Second, the effect of ethnically close immigrants may be thought of as "intra-ethnic" cross-country spillover rather than "inter-ethnic." After all, "ethnic networks" is how the literature treats the role of Chinese immigrants living in different countries. What we may more accurately call "inter-ethnic" effect is the impact of those who *do not* share the same native language. This includes, among others, those who share the same spoken language but for whom it is not native. It also includes those who are from geographically close countries and from countries with the same official language but who do not have the same native language. Third, when it comes to estimation of either "intra-ethnic" or "inter-ethnic" spillover trade promotion effects, the fact that proximate immigrant groups defined by different measures overlap is a serous threat to identifying whether it is the overlapping or the non-overlapping segment or both that matter for trade.

To motivate the choice of proximity measures considered, we go back to the channels through which immigrants are thought to affect trade. It is straightforward enough to point out that immigrants are more likely to have business networks in their home countries. It was insightful on the part of the studies mentioned above to identify the trade effects of international networks of large ethnic groups, which is related to the fact that diasporas may also form business networks. But certain

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immigrants may also be more likely than host country natives to have business connections to entrepeneurs who are not part of their ethnic network and do not live in their country of origin. In particular, trade tends to be greater between countries that are closer (distance being the main barrier in the gravity equation) and share a common border, which also means more business connections between people in geographically close countries and those sharing a common border. Additionally, countries that are closer and/or share a common border may be more likely to share similar cultures, legal systems, institutional and market peculiarities that may make immigrants from country j in host country h valuable for trade with country c that borders country j–that is, the information channel may also be at work for geographically proximate countries.

The motivation for considering language as the source of immigrant proximity is also strong. Same official language has been found to increase trade between a country pair (Rauch and Trindade (2002), Aleksynska and Peri (2014), Egger, Von Ehrlich and Nelson (2012), Blanes-Cristóbal (2008)). More importantly, same official language between a country pair reduces trade promotion effect of immigrants, as immigrant language skills become less relevant. This is consistent with empirical findings from Aleksynska and Peri (2014) and Kandogan (2005), which suggests that the immigration-trade elasticity is higher for linguistically more dissimilar countries. This highlights the role of immigrant linguistic/cultural capital in trade promotion, which may carry over to trade with countries other than the immigrant country of origin. For example, if Ecuadorians can promote exports to Ecuador through facilitation in negotiations and logistics/communication due to speaking Spanish,

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potentially so can the Colombians, the Spaniards, and so on. It is also possible that native speakers of the same language from different countries develop relationships that are closer to ethnic than inter-ethnic, in which case the effect of the same native language spoken would be higher than non-native, which is consistent with Melitz and Toubal (2014) finding that a higher probability of speaking the same native language increases bilateral trade between two countries even when controlling for the probability of speaking the same (native or non-native) language.

Of course, in a given host country there are both immigrants who are linguistically or geographically proximate and those who are not. We term those who are not proximate "distant." The natural question is do they have any effect on trade with country j and, if the do, what is the effect? We should expect them to promote trade to some countries, at least their own countries of origin. This may or may not mean that some of the trade is diverted from j in favor of other countries. To understand the fuller picture of third country immigration effects on trade, we consider both different proximate immigrant groups as well as distant.

Methodologically, the large degree of overlap between geographically and linguistically proximate immigrants, as well as between those linguistically proximate due to the same official, spoken or native language makes it difficult to identify the effect of each group; to tackle this problem, we identify groups that are proximate by one measure but not others in addition to explicitly controlling for the number of immigrants from other groups. Furthermore, we use shift-share instruments to address other threats to identification, such as those related to reverse causality or

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omitted variables related both to immigration and trade. Because the data used for the analysis is U.S. state exports and imports by industry to and from a large number of foreign countries, annually over a number of years (2002-2016 for exports and 2008-2016 for imports), we are also able to control for state-country trading pair fixed effects. In addition to using the most recent data, we go beyond what other studies with the U.S. as the host country have done methodologically, by both including trading pair fixed effects and instrumenting for immigration, as Table 2.1 shows.

Empirically, we find that immigrants who are linguistically proximate to trading partner country increase both exports and imports, even if they come from countries that are not geographically proximate. Focusing only on linguistically but not geographically proximate immigrants, the role of spoken language appears to be important even when it is not spoken as a native language and when it includes only immigrants from countries with different official languages. This finding extends the literature by showing that third party (spillover) trade promotion effect of immigrants is not limited to ethnic diasporas, but also arises from immigrants across different countries speaking the same language, the most likely mechanism for which is communication/logistics facilitation and easier information acquisition. It also means that part of the reason for the cross-country ethnic network trade promotion effect is the same spoken language. In contrast, immigrants from countries with the same official language but who do not speak the same language do not increase trade. The implication of this finding is that the positive effect of common official language on bilateral trade may be largely due to correlation with shared spoken and/or native

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language.

When it comes to the role of native language over and above spoken, it tends to have an additional export- but not import-promotion effect. This finding provides further evidence for ethnic network effect found in Rauch and Trindade (2002), Giovannetti and Lanati (2015), and Felbermayr, Jung and Toubal (2010); at the same time, it hints that the trade promotion effect found in these three papers that constitute cross-country ethnic network trade promotion literature may be due to the export part of trade, as they rely on bilateral stocks and are not positioned to distinguish between export-promotion and import-promotion effects.

In terms of geographic proximity, immigrants who come from countries that border trading partner country increase exports, but only if they are also linguistically proximate, and they do not increase imports. Additionally, geographic proximity increases the magnitude of export promotion effect of linguistically proximate immigrants. Lastly, we find that distant immigrants, those neither geographically nor linguistically proximate, tend to decrease trade, which raises the issue of trade diversion, which has been, for the most part, neglected by migration-trade link literature.

The paper proceeds as follows: Section 2.2 describes data and presents descriptive statistics, while Section 2.3 outlines empirical methodology, Section 2.4 presents results, and Section 2.5 concludes.

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2.2 Data and Descriptive Statistics

To investigate how immigration affects trade, this study utilizes data on U.S. state-industry level trade with foreign countries[3] over the time period from 2002 to 2016 (from USA Trade Online), with export figures being available throughout and imports–from 2008. The benefit of this data is that we can control for state-country trading pair–which is fundamental to causal inference in this context, as trading pairs can have unobserved characteristics affecting both trade and migration–and still have enough variation in trade data left to explain by variables of interest. One downside is that the trade data source has missing data for small volumes of trade; rather than imputing exports based on missing values, we restrict analysis only to state-country-industry-year observations with positive trade values, and focus on the intensive margin of trade. We address the issue of zero trade in the results section.

State aggregate- and industry-level GDP[4] is taken from County Business Patterns of the U.S. Census Bureau, while country GDP and population are from World Development Indicators of the World Bank. All GDP and trade data are in 2009 U.S. dollars, using GDP deflator from St. Louis Federal Reserve Bank.

The main source of individual-level data is American Community Survey Public Use Microdata Sample (ACS PUMS) from the University of Minnesota Population Center. As is common in the immigration-trade literature, we designate as immi-

[3]The total number of countries used in the analysis is 152.

[4]Some of the industries are grouped together in CBP GDP data, so we aggregate other data across the same industries; the industries that were aggregated are presented as a range in Table 2.5 (ex., Agriculture and Livestock, NAICS 111-112).

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grants anyone who was born abroad not to American parents (not citizen at birth). It is worth noting that country of origin is identified as country of birth, rather than place of residence previously to arriving to the United States. We only consider adult population with positive income, who are more likely to be involved in commercial activity.

2.2.1 Immigrant Proximity

2.2.1.1 Geography

Since we want to explore how immigrants from third party countries affect trade and the corresponding channels, we want to be able to calculate the number of immigrants proximate to those from export destination/import origin country by the relevant measures, and also distinguish those that are proximate by one measure but not the other(s). The geographic proximity measure of choice should reflect the likelihood of having business networks and knowledge of country characteristics helpful for trade promotion. For geographic proximity measure we use a simple rule of assigning a value of 1 to a pair of countries that share a border and 0 to those that do not, and denote the value as B. It is certainly the case that there may be some country j that does not border country c, but is closer to its economic center than a country h that does border c, especially in the case of large countries; most of the time, however, a common border is a good measure of proximity to economically important areas, and is a more straightforward measure than arbitrary

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distance zone measures; furthermore, trade literature suggests that trade is greater among bordering countries even controlling for distance.

The estimated number of immigrants in state s, at time t, in industry i, who are from countries bordering country c, is calculated as follows:

$$I_{scti}^{bord} = \sum_{j \in C; j \neq c} I_{sjti} * B_{cj}, \tag{1}$$

where $B_{cj} = 1$ if c and j share a common border and 0 otherwise, I_{sjti} is the number of immigrants from country j, and summation is over all the countries other than c.

2.2.1.2 Language

Language proximity measure is more nuanced. It is an intuitive argument that common spoken language is vital for communication associated with international trade transactions, be it marketing, logistics or more informal communication. Yet language usually enters the estimated gravity equation as a dummy for common official language between trading countries and interacted with immigrant stock variable. One limitation of this strategy is that it is not accurate to assume that people from countries with the same official language will be able to communicate or that people from countries with different official languages will not be able to communicate. Realizing this, Melitz and Toubal (2014) further network specificity literature by showing that not only common official language, but also all common native and spoken languages, when spoken by a substantial portion of the population in each

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country, matter for trade. Their results indicate that all relevant languages together have double the effect of just the official language and that native language is especially important, since in addition to basic ability to communicate it allows more nuanced communication and potentially ensures more trust. To obtain most inclusive measure of linguistic proximity, we create a measure that captures both common spoken language (which is greater or equal to native) probability and common official language, but also provide separate analysis for each measure to better identify the underlying mechanisms through which linguistic proximity operates. The combined measure calculates the number of immigrants in state s, at time t, in industry i, who are linguistically proximate to immigrants from country c. This is done by first calculating the number of linguistically proximate immigrants for each country j and then summing over all the countries in the following manner:

$$I_{scti}^{lang} = \sum_{j \in C; j \neq c} I_{sjti} * max(COL_{cj}, CSL_{cj}), \tag{2}$$

where COL takes the value of 1 if j and c share the same official language and CSL is the estimated probability that a randomly taken person from country j speaks the same language as a randomly taken person from country c. In constructing CSL we use the methodology and data of Melitz and Toubal (2014) study. For a large number of countries, they compiled data on the official language, the share of people who report each language as native (with a 4% threshold) and the same measure for spoken language. To create a value for linguistic proximity, we follow their methodology to create a value for common spoken language (CSL) for each

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country pair, a measure that should be highly correlated with the true (unknown) probability that any two randomly taken people from two countries would share the same spoken language.[5] Common spoken language score for a pair of countries is calculated as

$$CSL_{cj} = \max_k(L_{kc}L_{kj}) + (\alpha - \max_k(L_{kc}L_{kj}))(1 - \max_k(L_{kc}L_{kj})), \tag{3}$$

where L_{kc} is the share of people in country c that speak language k, $\alpha = \sum_{k=1}^{n} L_{kc}L_{kj}$ is the sumproduct of shares of people who report speaking each language, and $max(L_{kc}L_{kj})$ is the maximum product of the two shares.[6] For example, if in country 1, 90 percent of people report speaking French, 50 percent report German, and 0 percent name Spanish, while in country 2, 80 percent report French, 90 percent report German, and 10 percent report Spanish, then $\alpha = 0.72 + 0.45 + 0 = 1.17$ and $CSL = 0.72 + (1.17 - 0.72)(1 - 0.72) = 0.09 + 0.05 * 0.91 = 0.846$; in practice, this ensures CSL is always between 0 and 1. CNL_{cj} calculation is analogous.

[5] Standard errors are not available for the spoken and native languages data used, which does lead to underestimation of the true amount of noise.

[6] As in Melitz and Toubal (2014), English is included since as a common official language it may be reflective of common colonial power and legal origin, which may be important for international commerce, even though it may not be very valuable for communication in the U.S. as the host country. However, the main results are very similar with and without including English as one of the languages, so it does not drive any of the results.

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2.2.1.3 Geography versus Language

Since our measure of geographically proximate immigrants includes linguistically proximate and vice versa, it does not allow a clear inference about which feature matters (or matters more). Hence, we construct "exclusive" measures that only capture people that meet one criteria but not the other. To estimate the requisite number of linguistically proximate immigrants, we sum over only the non-bordering countries:

$$I_{scti}^{lang,excl} = \sum_{j \in C_{NB}; j \neq c;} I_{sjti} * max(COL_{cj}, CSL_{cj}). \tag{4}$$

For exclusive border-based measure, we first exclude countries that share the same official language; then, we subtract the estimated number of those able to speak the same language as immigrants from country c and sum over all countries j,

$$I_{scti}^{bord,excl} = \sum_{j \in C_{-o}; j \neq c} I_{sjti} * max(B_{cj} - CSL_{cj}, 0), \tag{5}$$

where C_{-o} refers to countries that do not have the same official language as c. Additionally, we construct a measure of distant immigrants, those neither linguistically nor geographically proximate,

$$I_{scti}^{NB,NL} = \sum_{j \in C; j \neq c;} I_{sjti} * (1 - (max(B_{cj}, COL_{cj}, CSL_{cj}))), \tag{6}$$

where NB and NL refer to neither sharing a common border nor linguistically proximate.

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2.2.1.4 Linguistic Channels

Because different measures of linguistic proximity are correlated, as shown in Table 2.3, we need to take steps to tease out the separate effects of each type of linguistic proximity. We focus on the exclusive measures to isolate the geographic proximity effect. Because the share of people speaking a given language generally is greater than or equal to the share speaking it as a native language, we can calculate the (expected) number of people who speak the same non-native language in countries with a different official language as

$$I_{scti}^{csl,-on,excl} = \sum_{j \in C_{NB} \cap C_{-o}; j \neq c;} I_{sjti} * (CSNNL_{cj}). \tag{7}$$

where $CSNNL_{cj}$ refers to common spoken non-native language probability between c and j, based on shares of people speaking the same language as non-native, which are the differences between the share speaking a given language and the share reporting it as native languages.

Additionally, because some of the people in countries with the same official language (as trading partner country c) do not speak the same language, we can try to get at the role of the same official language as separate from spoken,

$$I_{scti}^{col,-s,excl} = \sum_{j \in C_{NB} \cap C_{o}; j \neq c;} I_{sjti} * (1 - CSL_{cj}). \tag{8}$$

Unfortunately, we cannot identify people who share the same native but not spoken

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language (naturally), so the best we can do is control for the number of CSL speakers in the same regression equation.

2.2.2 Descriptive Statistics

Table 2.4 presents descriptive statistics for the main variables of interest. It is clear that data is not normally distributed, with many observations clustered at or near zero and overwhelmingly large values at the end of the right tail. This is mitigated by taking logs of all the variables as part of the estimation of the gravity equation (Table 2.2). One of the ways to account for the presence of zeros when taking logs is to add one, with resulting zeros for the zero observations, which is what we do here. This produces little distortion, especially for the trade and GDP numbers[7], as the lowest non-zero figures are at least in tens of thousands and more often in tens of millions. Most of the immigration numbers are also large enough that 1 has a negligible effect.

It is clear from Table 2.4 that the (unweighted) average import value is significantly larger than export value, partly reflecting trade imbalance and partly–the fact that import numbers start from 2008 and exports are from 2002. In terms of immigration numbers, we see that the average own-country immigrant number is more than 10 times smaller than the number of those proximate based on language and more than 2 times smaller that the number based on common border. Addi-

[7] GDP product numbers are zero in a few cases where industry-state production is 0, but imports are positive.

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tionally, we can observe that the number of proximate immigrants based on common spoken language is similar to that based on official language, but larger than that based on common native language. The number of immigrants proximate in terms of language but not geography is slightly lower than overall language-based estimate, while the number of immigrants based on geography and not language is about half that of the overall number based on geography. Immigrants proximate both in terms of language and geography number about as many as own-country immigrants. The number of distant immigrants, those neither proximate by language nor by geography, constitute more than half of all the state-industry immigrants. The last 5 variables in the table summarize more granulated measures of linguistic proximity, where it is notable that across countries with a different official language, there are quite a few immigrants who speak the same non-native language ($Imm_{scti}^{csl,-on,excl}$), but few who speak the same native language ($Imm_{scti}^{cnl,-o,excl}$).

Table 2.5 breaks down descriptive statistics by industry (sorted by immigrant employment share). The numbers presented are for the mid-point of the sample period, 2009. There is a great deal of heterogeneity both in terms of industry immigrant labor share and export and import intensity. Apparel and leather employment consists of 50% immigrant labor, while the number for Minerals and Ores is only 5%. Also notably, Apparel and Leather imports on average exceed state-industry GDP ten times, by far the largest ratio, implying the highest comparative advantage of trade partners in this industry. In contrast, the import (and export) GDP ratio in Newspapers, Books and Other is less than 0.01. The highest export intensity is observed for Transportation Equipment, at 3.25, which is similar to the import

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intensity of the industry. On average, import to GDP ratio is higher than export to GDP, reflecting trade imbalance in goods. Overall, there is no clear relationship between import and export intensity and immigrant employment share.

2.3 Empirical Strategy

2.3.1 Aggregate Exports

The foundation of the empirical strategy for estimating determinants of trade is the gravity equation, which has both strong empirical support and theoretical foundation (Anderson (1979), Bergstrand (1985)). It has the following general form:

$$X_{sct} = B_0 \frac{(Y_{st} Y_{ct})^{\beta_Y}}{(D_{sc})^{\beta_D}}, \tag{9}$$

where X_{sct} is trade flow between state[8] s and country c at time t, Y_{st} and Y_{ct} are, respectively, state and country GDP at time t, and D_{sc} is the distance between a state and a country. Since we are interested in examining determinants of trade flows at the industry level, we modify (9) to obtain the following:

$$X_{scti} = B_0 \frac{(Y_{sti} Y_{ct})^{\beta_Y}}{(D_{sc})^{\beta_D}}, \tag{10}$$

[8] Or country, in case of country-country trade.

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where i indicates industry. Taking the log of equation (10), we can obtain the following estimating equation:[9][10]

$$lnX_{scti} = \beta_0 + \beta_Y ln(Y_{sti}Y_{ct}) + \beta_D lnD_{sc} + \delta_{scti}, \tag{11}$$

where $\beta_0 = log(B_0)$. If we think of the denominator of (10) as not just distance but also other trade inhibitors and facilitators, which can vary over time, we can add log of immigrant stock (adding one to observations with zero immigrants) from country c in state s at time t (and appropriate fixed effects), and obtain

$$lnX_{scti} = \beta_0 + \beta_Y ln(Y_{sti}Y_{ct}) + \beta_D lnD_{sc} + \beta_I lnI_{scti} + FE + \delta_{scti}, \tag{12}$$

where the main $FE = [\psi_{st} + \psi_{ct} + \psi_{sc} + \psi_i]$, which accounts for trading pair fixed effects, state-year and country-year effects and industry time-invariant differences, although we do present results with other FE as well. Since we hypothesize that immigrants proximate to those from country c may matter for trade with country c, we include measures based on border, language and neither:

$$\begin{split} lnX_{scti} = \beta_0 + \beta_Y ln(Y_{sti}Y_{ct}) + \beta_D lnD_{sc} + \beta_I lnI_{scti} + I_{scti}^{lang} + I_{scti}^{bord} + \\ I_{scti}^{NB,NL} + FE + \delta_{scti}. \end{split} \tag{13}$$

[9] We look separately at determinants of export and import flows, for which the right-hand side of the estimating equation is analogous to that for total trade flows.

[10] As mentioned, 1 is added before taking the log, which we omit from equations for ease of notation.

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Additionally, since some of the geographically proximate immigrants are also proximate linguistically, we separate I_{scti}^{lang} and I_{scti}^{bord} into $I_{scti}^{lang,excl} + I_{scti}^{bord,excl} + I_{scti}^{bord,lang}$, with the latter being those both geographically and linguistically proximate. Lastly, we separate linguistic proximity into its components consisting of official, native and spoken language, as discussed earlier, and estimate their separate effects on trade.

2.3.2 2SLS

Although we already control for more potential time-invariant unobservables than any other study of the kind focused on the U.S. as the host country, we may still be concerned about time-variant unobservables that affect both immigrant flows between a country and a state and trade between the two. To address this potential concern, we use the shift-share instrument of the type previously used by Peri and Requena-Silvente (2010) and Bratti, De Benedictis and Santoni (2014) for immigration-trade link studies and many others used for predicting immigrant stock in other contexts (Altonji and Card (1991), Card (2001)). The instrument uses the assumed orthogonality between immigrant stock at a sufficiently distant period in the past–in our case, 1980–and shocks related to exports in the time period of interest. The instrument for the stock of immigrants from country c in year t in state s is the sum of the number of immigrants from that country in the state in 1980 plus the product of the national share of immigrants from country c in 1980 in the state and national change in the number of immigrants from c between 1980 and year t. The following expression captures the procedure:

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$$\hat{I_{sct}} = I_{sc80} + (I_{sct}/I_{ct}) * (I_{ct} - I_{c80}). \tag{14}$$

To obtain the predicted immigrant number at the industry level, we use the same general process as above, but modify it based on national industry composition of immigrants from country c in year t, and obtain

$$\hat{I_{scti}} = \hat{I_{sct}} * (I_{cti}/I_{ct}), \tag{15}$$

where I_{cti} is the total national number of immigrants from c in industry i in year t. Although proximate and distant immigrant groups are less likely to be endogenous in the equation, it may still happen if reverse causality of trade and immigration has cross-country spillovers, so we use instruments for other immigrant groups constructed analogously to the above to account for this possibility.

2.4 Results

2.4.1 Bilateral Trade Promotion

We begin by presenting regression results with just the main gravity equation variables (GDP product[11] and distance) and immigrants from the trading partner

[11]The coefficients on the main variables of interest are virtually the same if we separately include state and country GDPs

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country. Table 2.6 Panel A presents OLS results of export determinants for different, progressively more demanding, fixed effects. The first column shows results for the specification with state, country, year, and industry fixed effects. As expected, GDP product has a strongly positive effect, with elasticity of 0.5, while distance has a strongly negative effect, with elasticity of -1.3. The coefficient on the number of own-country immigrants[12] is 0.1, meaning a 10% increase in immigrants from country c increases exports to it by 1%. The next three columns feature almost identical coefficients on GDP product–0.53–and immigration–0.06–despite different fixed effects, which include trading pair (state-country), industry, and, respectively, year, country-year, or country-year and state-year (distance is omitted due to trading pair fixed effects). In columns 5 and 6 we also add state-industry fixed effects. The coefficient on immigration decreases to 0.04 and that on GDP product decreases about 10 times, to 0.05-0.07. Notably, the within variation explained by the model decreases from 8% to less than 0.1%. The last two specifications control for state-country-industry and year or country-year, so that only non-cross-section variation is left to be explained by the model. The coefficient on immigration decreases to about 0.005-0.006, but remains highly statistically significant.

To account for potential threats to identification not captured by the main gravity equation variables and fixed effects, we use instrumental variable strategy discussed previously and present results in panel B; as is evident from panel c, the instrument is very strong (the standard error is consistently smaller than the coefficient on IV by

[12] Imm_{scti} corresponds to the shorter notation of I_{scti} in the earlier sections; the same is true for other immigrant groups.

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about a factor of 100). Predictably, the coefficients on GDP product and distance do not change significantly. The coefficient on immigration increases to about 0.28-035 for columns 1-6 and to 0.04-0.05 in columns *7* and *8*. This may be because OLS result was downward biased, because instrument is not actually exogenous, or due to heterogenous effects, whereby the export-immigration elasticity on the observations that are affected by IV is higher than for those that are not. It is nevertheless reassuring that the sign and the level of statistical significance remain the same.

Table 2.7 presents the equivalent of Table 2.6 for import determinants. GDP product coefficient is positive and significant throughout, but about half the magnitude of the one for exports. Distance coefficient is negative and significant, but almost 50% smaller than for exports. On the other hand, immigration coefficient is larger by about 0.01-0.02 than for exports for the first 6 specifications; for the last two specifications–with state-country-industry fixed effects–it is not statistically significant, unlike for exports; notably, less than 0.1% of within variation is explained in these specifications. Panel B indicates that after instrumenting the effect of immigration becomes even larger than that for exports at between 0.45 and 0.66. Import elasticity being higher than export elasticity is consistent with the presence of the additional "preference" channel and also consistent with common literature findings. For specifications with state-country-industry fixed effects, the coefficient on immigration remains insignificant after instrumenting.

In the two results tables discussed, the estimated coefficients on immigration are very similar in the first 6 fixed effects specification. In what follows, we focus on

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the most demanding specification–column (4)–out of those that explain a non-trivial amount of within variation (columns (1)-(4)).

2.4.2 Proximate Immigrants

Table 2.8 columns (1)-(4) present OLS and IV results for the effects of proximate and non-proximate immigrants on exports for the specification with trading pair (state-country), state-year, country-year and industry fixed effects, as in column (4) in tables 2.6 and 2.7. The first thing to note is that the coefficient on own-country immigrants remains similar to that in column (4) of table 2.6, suggesting that own-country immigration is not endogenous due to correlation with proximate immigrants. With regards to proximate immigrants, OLS coefficients on both language-based and geography-based measures are positive and statistically significant, both at about 0.03 (column (1)); the coefficient on non-proximate immigrants is also positive, at 0.016. Column (2) uses exclusive measures of language- and geography-based proximate immigrants and includes the measure for those proximate by both criteria. The coefficients on border-based measure decreases to 0.008, while the language-based one increases to 0.032 and that on non-proximate decreases to 0.013. The largest elasticity is for immigrants proximate based both on language and geography, at 0.046. Column (3) presents results for the same specification as 1 after instrumenting for all the immigration variables. The coefficient on language-based proximate immigrants increases to 0.066, while that on geography-based measure remains at around 0.03. On the other hand, the coefficient on distant immigrants

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becomes negative, at -0.068, and statistically significant. Instrumenting for exclusive measures in column (4), we find that the coefficient on language-based proximate immigrants increases to 0.056, while that on border-based measure becomes negative, at -0.077, and statistically significant; the reason for the latter may be trade diversion effect of border-based immigrants from countries with different official language (than export destination country) and who do not speak the same language. The coefficient on distant immigrants becomes -0.084. Notably, the coefficient on immigrants proximate by both language- and border-based criteria increases to 0.21, almost equal to the own-country immigration effect. *Thus, linguistically proximate immigrants increase exports, especially if they are geographically close, whereas geographically proximate immigrants increase exports if they are also linguistically close.*

The estimated effects of proximate immigrants on import are presented in Table 2.8 columns (5)-(8). The coefficients on own-country immigrants again do not change much. Import elasticity with respect to language-based proximate immigrants is higher than in the case of exports, at 0.075, while that for border-based immigrants is lower, at 0.013, and the coefficient on distant immigrants is -0.026. Using exclusive measures, the coefficient on language-based proximate immigrants increases to 0.082 and that on border-based proximate immigrants becomes close to 0 and not statistically significant; the coefficient on those proximate based on both measures is not higher like in the case of exports, but in-between the language- and border-based measures, at 0.034. Instrumenting affects the estimates significantly, as the coefficient on language-based measure increases to 0.46, meaning a 10% increase in language-based proximate immigrants bolsters imports by almost 5%. On the

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other hand, the effect of border-based immigrants becomes -0.1. This again, may be due to trade diversion. The effect of distant immigrants becomes -0.4. When using exclusive measures, language-based immigration measure becomes 0.5, while the effect of border-based measure is -0.095, that of immigrants proximate by both measures is 0 and distant immigrant coefficient is -0.435. *Thus, like for exports, linguistically-proximate immigrants increase imports even if they are not geographically proximate, but geographically-proximate immigrants do not increase imports in either case.*

2.4.3 Linguistic Proximity Components

We next want to examine whether the parts that compose language-based measure have different effects. The export effects of proximate immigrants by different language measures are presented in Table 2.9. As before, using inclusive measure, geographically proximate immigrants increase exports, except when controlling for immigrants speaking the same native language. Both OLS and 2SLS coefficients on all three language-based measures are positive and statistically significant; when instrumenting, the largest effect is observed for the number speaking the same native language, followed by those from countries with the same official language as trading partner and the number sharing the same spoken language, at 0.012, 0.096 and 0.082, respectively. Table 2.10 presents the equivalent of Table 2.9 for imports. It shows that all three language-based measures are positive and significant in all specifications, but that the one on CSL measure dwarfs the CNL and COL ones.

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After instrumenting, CSL-based measure coefficient is 0.58, compared to 0.17-0.18 for CNL- and COL-based ones.

Table 2.11 uses exclusive measures to examine the effects of different language proximity measures on exports. In OLS specification, all the measures are positive and statistically significant for proximate immigrants, except geographically proximate immigrants when controlling for those speaking the same language. When instrumenting, all the linguistic proximity measures increase exports, with that based on native language being the largest. at 0.08. The largest overall effect, however, is that of CSL-based proximate immigrants who are also from geographically proximate countries, with elasticity of 0.29. Table 2.12 shows that all language proximity measures produce a positive effect on imports, but common spoken language-based one by far the largest, at 0.6, compared to 0.17 and 0.08 for CNL and COL ones (in IV specification). In sum, all three different linguistic proximity measures appear to increase both exports and imports, but native language is more important for exports and spoken language by far the most important for imports.

The previous results strongly suggest that linguistically proximate immigrants increase both imports and exports, and that they do so even if they are geographically distant. Separately, all measures of linguistic proximity appear to matter. However, we know that they are all highly correlated (Table 2.3), which makes inference about the role of any one more difficult. In what follows, we aim to identify the separate effects of the three measures. The first question we address is whether native and spoken languages matter if we isolate them from the role of the official language. In

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columns (1) and (2) (OLS) and (6) and (7) (2SLS) of Table 2.13 we test whether, respectively, CSN- and CSL-based proximate immigrants increase exports when they are not also from countries with the same official language (indicated by $^{-o}$). The results suggest that they do, with 2SLS coefficient of 0.24 for CSN and 0.36 for CSL. The next question is whether native language has an additional effect compared to spoken after isolating from official; again, we find that it does, with a 2SLS coefficient of 0.19 (column (8)). This is consistent with Melitz and Toubal (2014) findings that there is an additional effect of common native language for the level of trade between a country pair. Next, we examine whether the number of immigrants who are proximate based solely on spoken but not native or official language matters when controlling for the other two; column (9) shows that the resulting coefficient on this measure is 0.14 and statistically significant, suggesting that even if language spoken is not native, it still matters for exports. Lastly, we test whether immigrants from countries with the same official language but who do not speak the same language increase exports, and it appears that they do not, and, in fact, decrease exports, although only half as much as distant immigrants do, with a coefficient of -0.15 (column (10)).

We next examine the role of linguistic import determinants in Table 2.14. Similarly to the effects on exports, immigrants speaking the same language increase imports even if they do not speak it as a native language and if they are from countries with a different official language (with coefficient of 0.9 in column (9)). Also similar to the case of exports is that immigrants from countries with the same official language but who do not speak the same language tend to decrease imports (column

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(10)). Unlike in export results, however, native language does not seem to have an independent effect when controlling for official and spoken languages (column (8)), which indicates that the import promotion effect of native language identified earlier was likely due to people who speak the same native language also (by definition) speaking the same language, and not a special role of language as native.

Since import data is only available from 2008 while export from 2002, it may be that the difference in results for exports and imports is partly due to the time period; longer time period may make estimates more precise, allowing to identify statistically significant results, or it may be that the more recent effect of ethnic networks has been weaker. Nevertheless, Table 2.15 shows that export determinants results after 2008 are very similar to those for the entire time period available.

Finally, it is useful to recall that USA Trade Online does not record 0 values of trade, so the sample only includes observations with positive trade values. It is common to focus the analysis on the observations with positive trade numbers (Aleksynska and Peri (2014), Bratti, De Benedictis and Santoni (2014)). Yet it is important to recognize that omitting observations with no trade may introduce bias. The more traditional way to address this is to assign zeros to missing observations, add a small amount (usually 1) and take the log. Taking this approach (which is potentially compromised by the fact that we cannot be sure missing trade data is actually zeros) more than doubles the number of observations compared to exports specification and increases it 6 times compared to the imports specifications. The results (Table 2.16), however, are generally similar to those in the specification with

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positive trade, in the direction, scale and significance of the main coefficients of interest. Spoken non-native language increases both imports and exports, native language has and additional export- but not import-promotion effect, and those in countries with the same official language but who do not speak the same language decrease both exports and imports. Geographically proximate immigrants, again, increase exports but not imports. The coefficient on immigrants from trading partner country becomes a little smaller for imports and larger for exports, but remains highly statistically significant. Another approach, proposed by Silva and Tenreyro (2006), is to use Poisson maximum likelihood estimation (PMLE), which uses levels rather than logs. It would still require assigning zeros to observations with zero trade, however, which may not be accurate. More importantly, the large number of fixed effects employed here introduces numerical problems that preclude convergence, similarly to problems faced by Bratti, De Benedictis and Santoni (2014), who also have trading pair fixed effects. Even if convergence problems were not an issue, Martin and Pham (2015) show that in presence of a large number of zeros, PMLE may introduce serious bias; here the share of zeros is over 80% for imports and about 60% for exports. Not facing the same numerical problems, Aleksynska and Peri (2014) find PMLE results to be generally similar to OLS results. On balance, including fixed effects to account for unobserved heterogeneity is arguably more valuable than potential bias correction through non-linear methods. Overall, observations with zero/missing trade likely do not significantly affect the main findings here.

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2.5 Conclusion

Overall, the analysis in the study suggests that trade with foreign countries is affected not only by immigrants from those countries, but also immigrants from third party countries. Broadly, we find that immigrants from countries proximate to the trading partner country increase trade and distant ones reduce it, but the effect differs by the type of proximity and is not identical for exports and imports. The empirical results suggests that exports are augmented by linguistically proximate immigrants. Specifically, the number of immigrants who speak the same language as those in the export destination country increase exports–more so if the language spoken is native–even if they come from geographically distant countries with a different official language; in contrast, those who are from countries with the same official language yet do not speak the same language, tend to decrease exports. Geographically proximate immigrants (from countries bordering trade partner country) bolster exports only if they are also linguistically proximate. In part as a consequence, the largest export promotion effect is found for immigrants who are both geographically and linguistically proximate to the export destination. Lastly, distant immigrants–those neither geographically nor linguistically proximate–tend to decrease exports.

With regards to import promotion, linguistically proximate immigrants again turn out to be important, but mainly due to speaking the same language as those in the import origin country, as native language does not have an additional independent effect and those from countries with the same official language but who do not share the same spoken language tend to reduce imports. Unlike for export promotion,

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geographically proximate immigrants do not increase imports, even when they do include linguistically proximate immigrants. Just as they decrease exports, distant immigrants tend to decrease imports.

The above results have important implications for understanding of ethnic and inter-ethnic spillovers. First, we provide a new type of evidence for cross-country ethnic spillover effect. Previous research has identified ethnic network effect of certain immigrants groups (Rauch and Trindade (2002), Giovannetti and Lanati (2015), Felbermayr, Jung and Toubal (2010)), but did not differentiate it from the role of common spoken language. We show that at least for exports, groups speaking the same native language do have additional trade promotion effect beyond sharing the same spoken language. Additionally, we show that inter-ethnic effect also takes place, as groups speaking the same non-native language also promote trade. We, furthermore, demonstrate that ethnic and inter-ethnic spillover effects on exports are particularly large for immigrants from geographically proximate regions.

Our main results reflect accounting for the high degree of overlap between the different kinds of immigrant groups considered, which could otherwise lead to misleading interpretations. This is manifested in geographically proximate immigrants increasing exports, but not if they are linguistically distant. This is also reflected in common official and native languages being important for imports, but not after accounting for spoken language. By accounting for important category overlaps, we are able to not just detect that third party country immigrants matter for trade, but also better understand what drives the relationship.

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Tables for Chapter 2

Table 2.1: Studies with the U.S. as the Host Country

Author	Data: # of Countries	Years	Export Elasticity	Method
Bandyopadhyay, Coughlin and Wall (2008)	29 (from U.S. states)	1990-2000	0.14	OLS with FE (state-country)
Bardhan and Guhathakurta (2004)	51 (from 17 U.S. states)	1994-1996	0-0.26	OLS with FE (region)
Co, Euzent and Martin (2004)	28 (from U.S. states)	1993	0.29	OLS
Coughlin and Wall (2011)	29 (48 states; 19 industries)	1990-2000	0.19	OLS with FE (state-county)
Dunlevy (2006)	87 (from U.S. states)	1990-92	(0.24-0.47) 0.39	OLS with FE (state, country)
Dunlevy and Hutchinson (1999)	17	1870-1910	0.08	OLS
Gove (2017)	Mexican states/U.S. states	2006-2010	0.07	OLS with FE (state)
Gould (1994)*	46	1970-1986	0.02*	NLIN LS
Herander and Saavedra (2005)	36	1993-1996	0.18	Tobit (region FE)
Jansen and Piermartini (2009)	> 100	1996-2005	0.01-0.25	Tobit, OLS with FE (time)
Millimet and Osang (2007)	Canadian states/U.S. states	1993,1997	0.0	OLS with FE (state-state)
Mundra (2005)	47	1973-1980	Not estimated	Semi-par with FE and IV
Mundra (2014)	63	1991-2000	0.25	2SLS
Tadesse and White (2010)	75 (from U.S. states)	2000	0.05	Tobit
White (2009*a*)	28 (from 48 U.S. states)	1993	0-0.57	OLS
White (2009*b*)	70	1980-1997	0.0	OLS
This study	152 (50 States; 29 industries)	2002-2016	0.06-0.34;0.07-0.57	2SLS with FE (state-country)

*Wagner et al. (2002) calculation. **Preferred estimate ranges (OLS-IV) for exports and imports, respectively.

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Table 2.2: Descriptive Statistics (in logs)

Variable	N	Mean	SD	Min	Max
Exports	1040053	12.88	2.58	9.21	23.89
Imports	413287	13.42	2.91	9.21	24.83
GDP Product	1102039	48.64	2.61	0.00	57.92
Distance	1111285	8.98	0.52	5.38	9.87
Imm_{scti}	1111285	0.46	1.52	0.00	12.54
Imm_{scti}^{csl}	1111285	3.54	2.77	0.00	12.58
Imm_{scti}^{col}	1111285	2.21	3.08	0.00	12.59
Imm_{scti}^{cnl}	1111285	1.56	2.54	0.00	12.49
Imm_{scti}^{lang}	1111285	3.75	2.89	0.00	12.59
Imm_{scti}^{bord}	1111285	1.04	2.19	0.00	12.57
$Imm_{scti}^{NB,NL}$	1111285	4.05	3.36	0.00	12.57
$Imm_{scti}^{lang,bord}$	1111285	0.64	1.66	0.00	12.57
$Imm_{scti}^{lang,excl}$	1111285	3.63	2.89	0.00	12.59
$Imm_{scti}^{bord,excl}$	1111285	0.24	0.62	0.00	2.49
$Imm_{scti}^{col,-s,excl}$	1111285	1.51	2.37	0.00	11.41
$Imm_{scti}^{cnl,-o,excl}$	1111285	0.13	0.60	0.00	9.16
$Imm_{scti}^{csl,-on,excl}$	1111285	1.87	2.13	0.00	10.36
$Imm_{scti}^{csl,-o,excl}$	1111285	2.53	2.38	0.00	11.07
$Imm_{scti}^{csl,o,excl}$	1111285	1.94	2.81	0.00	12.58

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Table 2.3: Correlation Coefficients for Proximity Measures

	Imm^{csl}_{scti}	Imm^{col}_{scti}	Imm^{cnl}_{scti}	Imm^{bord}_{scti}
Imm^{col}_{scti}	0.673			
Imm^{cnl}_{scti}	0.672	0.780		
Imm^{bord}_{scti}	0.302	0.155	0.289	
Imm^{lang}_{scti}	0.980	0.735	0.638	0.278

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Table 2.4: Descriptive Statistics

Variable	N	Mean	SD	Min	Max
Exports	1040053	14.97	151.75	0.01	23786.54
Imports	413287	40.68	490.01	0.01	60630.21
GDP Product	1102039	1.32e+09	7.55e+09	0.00	1.43e+12
Distance	1111285	8876.97	3566.36	216.37	19434.64
Imm_{scti}	1111285	46.97	1097.05	0.00	278549.00
Imm_{scti}^{csl}	1111285	667.57	4574.50	0.00	290271.48
Imm_{scti}^{col}	1111285	658.60	4937.59	0.00	293591.00
Imm_{scti}^{cnl}	1111285	383.90	3787.41	0.00	266940.99
Imm_{scti}^{lang}	1111285	823.79	5025.24	0.00	294581.95
Imm_{scti}^{bord}	1111285	108.65	1291.19	0.00	287869.00
$Imm_{scti}^{NB,NL}$	1111285	1705.62	8777.32	0.00	286876.76
$Imm_{scti}^{lang,bord}$	1111285	49.15	1126.95	0.00	287869.00
$Imm_{scti}^{lang,excl}$	1111285	774.64	4851.26	0.00	294353.65
$Imm_{scti}^{bord,excl}$	1111285	59.50	620.19	0.00	66455.00
$Imm_{scti}^{col,-s,excl}$	1111285	143.48	1029.74	0.00	90448.47
$Imm_{scti}^{cnl,-o,excl}$	1111285	1.75	38.27	0.00	9537.60
$Imm_{scti}^{csl,-on,excl}$	1111285	78.43	407.38	0.00	31433.06
$Imm_{scti}^{csl,-o,excl}$	1111285	158.25	736.88	0.00	63970.37
$Imm_{scti}^{csl,o,excl}$	1111285	502.38	4499.30	0.00	290132.56

Exports & imports in millions, GDP product in trillions (2009 \$ and \$^{2})

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Table 2.5: Industry Characteristics (2009)

Industry	NAICS	Obs	Ind. GDP	Ind. Pop.	Exp./GDP	Imp./GDP	Imm. Share
Apparel and Leather	315-316	3746	10	340	.58	9.17	.5
Agriculture and Livestock	111-112	3186	110	1790	.44	.23	.32
Textiles and Textile Prod.	313-314	3377	15	290	.57	1.19	.27
Computer and Electronic Prod.	334	5585	229	1380	.64	1.1	.27
Food and Beverages	311-312	4190	243	1920	.19	.2	.24
Misc. Manufacturing Comm.	339	4472	80	1210	.57	1	.21
Furniture and Fixtures	337	3089	23	520	.14	.9	.19
Forestry, Fishing and Other	113-115	2057	28	430	.18	.41	.18
Chemicals	325	4592	310	1370	.42	.46	.17
Plastics and Rubber Prod.	326	4016	62	520	.33	.43	.16
Electrical Equipment	335	4243	50	470	.58	1.09	.16
Nonmetallic Mineral Prod.	327	3224	37	480	.19	.34	.15
Wood Prod.	321	2787	21	460	.18	.47	.15
Petroleum and Coal Prod.	324	2097	115	190	.33	.43	.14
Fabricated Metal Prod.	332	4394	118	1270	.24	.32	.13
Printing and Related	323	2864	39	690	.14	.12	.13
Transportation Equipment	336	5016	48	2380	3.25	3.66	.12
Primary Metal Mfg	331	3149	40	560	.98	1.35	.12
Machinery, Except Electrical	333	5534	116	1400	.92	.74	.11
Paper	322	3039	59	420	.32	.31	.11
Newspapers, Books and Other	511	1236	174	790	0	0	.08
Oil and Gas	211	512	185	50	.04	1.11	.07
Minerals and Ores	212	1875	66	220	.16	.08	.05
Total/Average		78280	2176	19150	.42	.66	.18

Ind. GDP figures in billions ($\$^2$), population – in thousands.

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Table 2.6: Own-Country Immigration Effect on Exports

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. OLS								
GDP Product	0.522***	0.535***	0.530***	0.533***	0.0661***	0.0517***	0.242***	0.0860***
	(0.00619)	(0.00584)	(0.00594)	(0.00597)	(0.00751)	(0.00781)	(0.00689)	(0.00724)
Imm_{scti}	0.0964***	0.0586***	0.0583***	0.0575***	0.0358***	0.0352***	0.00595***	0.00482***
	(0.00230)	(0.00209)	(0.00210)	(0.00211)	(0.00179)	(0.00179)	(0.000888)	(0.000868)
Distance	-1.294***							
	(0.0392)							
R-squared	0.593	0.633	0.637	0.638	0.686	0.687	0.847	0.852
R-sq within	0.0895	0.0817	0.0802	0.0805	0.00101	0.000923	0.00405	0.000466
Panel A. 2SLS								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
GDP Product	0.481***	0.484***	0.479***	0.481***	0.0568***	0.0397***	0.240***	0.0842***
	(0.00635)	(0.00618)	(0.00627)	(0.00632)	(0.00757)	(0.00789)	(0.00689)	(0.00724)
Imm_{scti}	0.331***	0.337***	0.343***	0.344***	0.284***	0.285***	0.0546***	0.0413***
	(0.00817)	(0.0106)	(0.0107)	(0.0108)	(0.0100)	(0.0101)	(0.00763)	(0.00756)
Distance	-1.087***							
	(0.0390)							
R-squared	0.578	0.616	0.619	0.620	0.673	0.673	0.846	0.851
R-sq within	0.0562	0.0383	0.0344	0.0343	-0.0409	-0.0417	0.00150	-0.00101
First Stage								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
GDP Product	0.158***	0.164***	0.167***	0.168***	0.0442***	0.0519***	0.0452***	0.0493***
	(0.00324)	(0.00299)	(0.00303)	(0.00305)	(0.00429)	(0.00448)	(0.00413)	(0.00445)
$IV(Imm_{scti})$	0.489***	0.414***	0.421***	0.419***	0.393***	0.391***	0.264***	0.265***
	(0.00454)	(0.00389)	(0.00396)	(0.00397)	(0.00371)	(0.00372)	(0.00293)	(0.00298)
Distance	-0.370***							
	(0.0284)							
Year	Yes	Yes					Yes	
State and Country	Yes							
Industry	Yes	Yes	Yes	Yes				
State-Country		Yes	Yes	Yes	Yes	Yes		
Country-Year			Yes	Yes	Yes	Yes		Yes
State-Year				Yes		Yes		
State-Industry					Yes	Yes		
State-Country-Ind.							Yes	Yes
Observations	1028702	1028600	1028600	1028600	1028598	1028598	1014043	1014041
R-squared	0.357	0.429	0.432	0.433	0.446	0.448	0.562	0.564
R-sq within	0.146	0.0785	0.0788	0.0784	0.0551	0.0546	0.0192	0.0183

+ $p < 0.10$,* $p < 0.05$, ** $p < 0.010$, *** $p < 0.001$. Standard errors in parenthesis. Standard errors clustered at state-country-industry level.

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Table 2.7: Own-Country Immigration Effect on Imports

Panel A. OLS								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
GDP Product	0.297***	0.288***	0.289***	0.289***	0.0723***	0.0560***	0.115***	0.0954***
	(0.0115)	(0.0108)	(0.0109)	(0.0109)	(0.0150)	(0.0155)	(0.0126)	(0.0131)
Imm_{scti}	0.107***	0.0712***	0.0715***	0.0715***	0.0684***	0.0683***	-0.000145	0.000704
	(0.00406)	(0.00387)	(0.00387)	(0.00388)	(0.00350)	(0.00350)	(0.00127)	(0.00126)
Distance	-0.782***							
	(0.0516)							
R-squared	0.465	0.516	0.518	0.519	0.556	0.556	0.900	0.901
R-sq within	0.0245	0.0167	0.0168	0.0167	0.00218	0.00216	0.000571	0.000360
Panel A. 2SLS								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
GDP Product	0.221***	0.179***	0.177***	0.177***	0.0652***	0.0539**	0.115***	0.0953***
	(0.0118)	(0.0118)	(0.0119)	(0.0120)	(0.0161)	(0.0166)	(0.0126)	(0.0131)
Imm_{scti}	0.455***	0.551***	0.563***	0.563***	0.659***	0.659***	-0.0109	0.00582
	(0.0140)	(0.0195)	(0.0199)	(0.0199)	(0.0202)	(0.0202)	(0.0107)	(0.0109)
Distance	-0.479***							
	(0.0532)							
R-squared	0.434	0.467	0.467	0.467	0.485	0.485	0.900	0.901
R-sq within							0.000399	0.000321
First Stage								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
GDP Product	0.202***	0.215***	0.216***	0.217***	0.0158+	0.00838	0.0284**	0.0226*
	(0.00492)	(0.00459)	(0.00462)	(0.00464)	(0.00945)	(0.00984)	(0.00938)	(0.00977)
$IV(Imm_{scti})$	0.549***	0.475***	0.479***	0.479***	0.446***	0.446***	0.294***	0.292***
	(0.00562)	(0.00523)	(0.00534)	(0.00534)	(0.00512)	(0.00512)	(0.00443)	(0.00452)
Distance	-0.314***							
	(0.0341)							
Year	Yes	Yes					Yes	
State and Country	Yes							
Industry	Yes	Yes	Yes	Yes				
State-Country		Yes	Yes	Yes	Yes	Yes		
Country-Year			Yes	Yes	Yes	Yes		Yes
State-Year				Yes		Yes		
State-Industry					Yes	Yes		
State-Country-Ind.							Yes	Yes
Observations	409311	408845	408834	408834	408829	408829	396948	396937
R-squared	0.399	0.474	0.475	0.476	0.494	0.494	0.628	0.629
R-sq within	0.151	0.0832	0.0828	0.0828	0.0562	0.0563	0.0175	0.0163

+ $p < 0.10$,* $p < 0.05$, ** $p < 0.010$, *** $p < 0.001$. Standard errors in parenthesis. Standard errors clustered at state-country-industry level.

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Table 2.8: Proximate Immigrant Effect on Exports and Imports

	Exports				Imports			
	OLS		2SLS		OLS		2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
GDP Product	0.497***	0.494***	0.478***	0.481***	0.257***	0.252***	0.201***	0.186***
	(0.00601)	(0.00602)	(0.0104)	(0.0103)	(0.0114)	(0.0115)	(0.0224)	(0.0218)
Imm_{scti}	0.0526***	0.0520***	0.320***	0.299***	0.0677***	0.0668***	0.545***	0.536***
	(0.00205)	(0.00205)	(0.0116)	(0.0116)	(0.00380)	(0.00382)	(0.0217)	(0.0215)
Imm_{scti}^{lang}	0.0276***		0.0656***		0.0749***		0.462***	
	(0.00205)		(0.0116)		(0.00494)		(0.0238)	
Imm_{scti}^{bord}	0.0283***		0.0265**		0.0131***		-0.0992***	
	(0.00155)		(0.00924)		(0.00317)		(0.0183)	
$Imm_{scti}^{NB,NL}$	0.0155***	0.0125***	-0.0678***	-0.0838***	-0.0256***	-0.0310***	-0.407***	-0.435***
	(0.00172)	(0.00174)	(0.0102)	(0.0105)	(0.00384)	(0.00389)	(0.0223)	(0.0226)
$Imm_{scti}^{lang,excl}$		0.0316***		0.0555***		0.0819***		0.498***
		(0.00207)		(0.0117)		(0.00499)		(0.0242)
$Imm_{scti}^{bord,excl}$		0.00781***		-0.0773***		0.00309		-0.0947***
		(0.00217)		(0.0106)		(0.00465)		(0.0209)
$Imm_{scti}^{lang,bord}$		0.0464***		0.209***		0.0340***		0.0310
		(0.00227)		(0.0140)		(0.00498)		(0.0274)
Observations	1028600	1028600	1028600	1028600	408834	408834	408834	408834
R-squared	0.639	0.639	0.621	0.617	0.520	0.520	0.435	0.432
R-sq within	0.0827	0.0834	0.0365	0.0268	0.0187	0.0192		
F-Stat, Inst.			10032.5	8711.4			4349.5	3609.7

All regressions include trading pair, country-year, state-year, and industry effects. + $p < 0.10$,* $p < 0.05$, ** $p < 0.010$, *** $p < 0.001$. Standard errors in parenthesis. Standard errors clustered at state-country-industry level. Imm_{scti} – immigrants from trading partner country, $Imm_{scti}^{NB,NL}$–no common border and no common language, Imm_{scti}^{bord} – geographically proximate, Imm_{scti}^{lang} – linguistically proximate, $Imm_{scti}^{bord,excl}$ – geographically but not linguistically proximate, $Imm_{scti}^{lang,excl}$ – linguistically but not geographically proximate, $Imm_{scti}^{lang,bord}$ – linguistically and geographically proximate.

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Table 2.9: Proximate Immigrant Effect on Exports (with Different Measures for Language)

	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
GDP Product	0.497***	0.496***	0.497***	0.463***	0.477***	0.473***
	(0.00600)	(0.00601)	(0.00599)	(0.00980)	(0.0103)	(0.00944)
Imm_{scti}	0.0519***	0.0526***	0.0514***	0.301***	0.321***	0.304***
	(0.00204)	(0.00205)	(0.00204)	(0.0118)	(0.0116)	(0.0117)
$Imm_{scti}^{NB,NL}$	0.0248***	0.0128***	0.0243***	-0.0481***	-0.0754***	-0.0509***
	(0.00162)	(0.00176)	(0.00162)	(0.00978)	(0.0105)	(0.00978)
Imm_{scti}^{bord}	0.0290***	0.0274***	0.0251***	0.0360***	0.0219*	0.0151
	(0.00155)	(0.00155)	(0.00156)	(0.00908)	(0.00935)	(0.00929)
Imm_{scti}^{col}	0.0248***			0.0959***		
	(0.00166)			(0.00758)		
Imm_{scti}^{csl}		0.0341***			0.0817***	
		(0.00228)			(0.0127)	
Imm_{scti}^{cnl}			0.0366***			0.117***
			(0.00212)			(0.00904)
Observations	1028600	1028600	1028600	1028600	1028600	1028600
R-squared	0.639	0.639	0.639	0.620	0.620	0.621
R-sq within	0.0829	0.0828	0.0831	0.0361	0.0361	0.0363
F-Stat, Inst.				9409.8	9992.0	10010.3

All regressions include trading pair, country-year, state-year, and industry effects.
+ $p < 0.10$,* $p < 0.05$, ** $p < 0.010$, *** $p < 0.001$. Standard errors in parenthesis.
Standard errors clustered at state-country-industry level. Imm_{scti} – immigrants from trading partner country, $Imm_{scti}^{NB,NL}$–no common border and no common language, Imm_{scti}^{bord} – geographically proximate, $Imm_{scti}^{col,excl}$ – those from countries with the same official language, $Imm_{scti}^{csl,excl}$ – those with the same spoken language, $Imm_{scti}^{cnl,excl}$ – those with the same native language.

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Table 2.10:
Proximate Immigrant Effect on Imports (with Different Measures for Language)

	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
GDP Product	0.269***	0.254***	0.270***	0.322***	0.183***	0.351***
	(0.0114)	(0.0114)	(0.0113)	(0.0209)	(0.0221)	(0.0202)
Imm_{scti}	0.0671***	0.0676***	0.0667***	0.506***	0.556***	0.520***
	(0.00380)	(0.00380)	(0.00379)	(0.0223)	(0.0218)	(0.0220)
$Imm_{scti}^{NB,NL}$	0.00285	-0.0330***	0.00254	-0.288***	-0.454***	-0.295***
	(0.00349)	(0.00395)	(0.00350)	(0.0216)	(0.0227)	(0.0216)
Imm_{scti}^{bord}	0.0179***	0.0110***	0.0139***	-0.0336+	-0.129***	-0.0681***
	(0.00316)	(0.00317)	(0.00319)	(0.0184)	(0.0185)	(0.0184)
Imm_{scti}^{col}	0.0340***			0.178***		
	(0.00379)			(0.0145)		
Imm_{scti}^{csl}		0.0921***			0.578***	
		(0.00545)			(0.0258)	
Imm_{scti}^{cnl}			0.0441***			0.173***
			(0.00481)			(0.0176)
Observations	408834	408834	408834	408834	408834	408834
R-squared	0.519	0.520	0.519	0.449	0.426	0.448
R-sq within	0.0177	0.0191	0.0178			
F-Stat, Inst.				3978.0	4318.9	4305.8

All regressions include trading pair, country-year, state-year, and industry effects.
+ $p < 0.10$,* $p < 0.05$, ** $p < 0.010$, *** $p < 0.001$. Standard errors in parenthesis.
Standard errors clustered at state-country-industry level. Imm_{scti} – immigrants from trading partner country, $Imm_{scti}^{NB,NL}$–no common border and no common language, Imm_{scti}^{bord} – geographically proximate, $Imm_{scti}^{col,excl}$ – those from countries with the same official language, $Imm_{scti}^{csl,excl}$ – those with the same spoken language, $Imm_{scti}^{cnl,excl}$ – those with the same native language.

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Table 2.11:
Proximate Immigrant Effect on Exports (with Different Exclusive Measures for Language)

		OLS			2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
GDP Product	0.496***	0.493***	0.496***	0.491***	0.488***	0.489***
	(0.00601)	(0.00602)	(0.00600)	(0.00984)	(0.0103)	(0.00965)
Imm_{scti}	0.0510***	0.0520***	0.0504***	0.291***	0.313***	0.298***
	(0.00204)	(0.00204)	(0.00203)	(0.0119)	(0.0115)	(0.0117)
$Imm_{scti}^{NB,NL}$	0.0245***	0.00948***	0.0239***	-0.0577***	-0.0946***	-0.0585***
	(0.00161)	(0.00178)	(0.00162)	(0.00982)	(0.0108)	(0.00981)
$Imm_{scti}^{col,excl}$	0.0241***			0.0283***		
	(0.00170)			(0.00804)		
$Imm_{scti}^{bord,col,excl}$	0.0276***			-0.000672		
	(0.00181)			(0.00987)		
$Imm_{scti}^{col,bord}$	0.0432***			0.208***		
	(0.00249)			(0.0147)		
$Imm_{scti}^{csl,excl}$		0.0384***			0.0629***	
		(0.00230)			(0.0129)	
$Imm_{scti}^{bord,csl,excl}$		0.00303			-0.138***	
		(0.00248)			(0.0144)	
$Imm_{scti}^{csl,bord}$		0.0490***			0.287***	
		(0.00289)			(0.0191)	
$Imm_{scti}^{cnl,excl}$			0.0380***			0.0771***
			(0.00219)			(0.00976)
$Imm_{scti}^{bord,cnl,excl}$			0.0241***			-0.0262*
			(0.00187)			(0.0121)
$Imm_{scti}^{cnl,bord}$			0.0249***			0.163***
			(0.00302)			(0.0193)
Observations	1028600	1028600	1028600	1028600	1028600	1028600
R-squared	0.639	0.639	0.639	0.618	0.614	0.620
R-sq within	0.0832	0.0835	0.0833	0.0307	0.0193	0.0346
F-Stat, Inst.				8104.4	8734.0	7729.2

All regressions include trading pair, country-year, state-year, and industry effects.
+ $p < 0.10$, * $p < 0.05$, ** $p < 0.010$, *** $p < 0.001$. Standard errors in parenthesis. Standard errors clustered at state-country-industry level. Imm_{scti}–immigrants from trading partner country, $Imm_{scti}^{NB,NL}$–no common border and no common language, $Imm_{scti}^{col,excl}$–with common official language but no common border, $Imm_{scti}^{bord,excl}$–with common border but different official language, $Imm_{scti}^{col,bord}$–with the same official language and common border, $Imm_{scti}^{csl,excl}$–with the same spoken language but no common border, $Imm_{scti}^{bord,excl}$–with common border but different spoken language, $Imm_{scti}^{csl,bord}$–with the same spoken language and common border, $Imm_{scti}^{cnl,excl}$–with the same native language but no common border, $Imm_{scti}^{bord,excl}$–with common border but different native language, $Imm_{scti}^{cnl,bord}$–with the same native language and common border.

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Table 2.12:
Proximate Immigrant Effect on Imports (with Different Exclusive Measures for Language)

	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
GDP Product	0.271***	0.251***	0.269***	0.365***	0.182***	0.340***
	(0.0114)	(0.0115)	(0.0114)	(0.0207)	(0.0222)	(0.0209)
Imm_{scti}	0.0673***	0.0671***	0.0667***	0.528***	0.562***	0.527***
	(0.00380)	(0.00381)	(0.00378)	(0.0225)	(0.0218)	(0.0220)
$Imm_{scti}^{NB,NL}$	0.00313	-0.0393***	0.00285	-0.297***	-0.493***	-0.286***
	(0.00349)	(0.00401)	(0.00349)	(0.0218)	(0.0231)	(0.0219)
$Imm_{scti}^{col,excl}$	0.0272***			0.0773***		
	(0.00392)			(0.0155)		
$Imm_{scti}^{bord,col,excl}$	0.0227***			-0.0510*		
	(0.00356)			(0.0199)		
$Imm_{scti}^{col,bord}$	0.0142*			0.0140		
	(0.00565)			(0.0300)		
$Imm_{scti}^{csl,excl}$		0.0997***			0.601***	
		(0.00551)			(0.0268)	
$Imm_{scti}^{bord,csl,excl}$		-0.00557			-0.180***	
		(0.00547)			(0.0291)	
$Imm_{scti}^{csl,bord}$		0.0406***			0.115**	
		(0.00638)			(0.0374)	
$Imm_{scti}^{cnl,excl}$			0.0467***			0.172***
			(0.00514)			(0.0196)
$Imm_{scti}^{bord,cnl,excl}$			0.0360***			0.0495*
			(0.00363)			(0.0243)
$Imm_{scti}^{cnl,bord}$			-0.0376***			-0.222***
			(0.00670)			(0.0393)
Observations	408834	408834	408834	408834	408834	408834
R-squared	0.519	0.520	0.519	0.448	0.420	0.447
R-sq within	0.0174	0.0198	0.0180			
F-Stat, Inst.				3344.5	3582.6	3311.9

All regressions include trading pair, country-year, state-year, and industry effects.
+ $p < 0.10$,* $p < 0.05$, ** $p < 0.010$, *** $p < 0.001$. Standard errors in parenthesis.
Standard errors clustered at state-country-industry level. Imm_{scti}–immigrants from trading partner country, $Imm_{scti}^{NB,NL}$–no common border and no common language, $Imm_{scti}^{col,excl}$–with common official language but no common border, $Imm_{scti}^{bord,excl}$–with common border but different official language, $Imm_{scti}^{col,bord}$–with the same official language and common border, $Imm_{scti}^{csl,excl}$–with the same spoken language but no common border, $Imm_{scti}^{bord,excl}$–with common border but different spoken language, $Imm_{scti}^{csl,bord}$–with the same spoken language and common border, $Imm_{scti}^{cnl,excl}$–with the same native language but no common border, $Imm_{scti}^{bord,excl}$–with common border but different native language, $Imm_{scti}^{cnl,bord}$–with the same native language and common border.

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Table 2.13: Detailed Linguistic Proximity Effect on Exports

	OLS					2SLS				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
GDP Product	0.496***	0.488***	0.488***	0.488***	0.486***	0.485***	0.452***	0.460***	0.468***	0.448***
	(0.00601)	(0.00606)	(0.00606)	(0.00607)	(0.00606)	(0.00988)	(0.00955)	(0.00958)	(0.00972)	(0.00944)
Imm_{scti}	0.0516***	0.0508***	0.0507***	0.0510***	0.0506***	0.318***	0.292***	0.301***	0.313***	0.299***
	(0.00204)	(0.00204)	(0.00204)	(0.00204)	(0.00204)	(0.0118)	(0.0118)	(0.0118)	(0.0118)	(0.0118)
$Imm_{scti}^{NB,NL}$	0.0229***	-0.0230***	-0.0222***	0.00123	-0.0228***	-0.0699***	-0.269***	-0.277***	-0.126***	-0.296***
	(0.00165)	(0.00287)	(0.00287)	(0.00205)	(0.00286)	(0.00996)	(0.0216)	(0.0215)	(0.0123)	(0.0218)
Imm_{scti}^{bord}	0.0316***	0.0307***	0.0304***	0.0305***	0.0275***	0.0272**	0.0347***	0.0216*	0.0192*	-0.00258
	(0.00153)	(0.00153)	(0.00153)	(0.00153)	(0.00156)	(0.00936)	(0.00931)	(0.00941)	(0.00944)	(0.00991)
$Imm_{scti}^{col,excl}$	0.0250***	0.0245***	0.0246***	0.0255***		0.0587***	0.0632***	0.0606***	0.0681***	
	(0.00172)	(0.00172)	(0.00172)	(0.00172)		(0.00749)	(0.00748)	(0.00747)	(0.00746)	
$Imm_{scti}^{cnl,-o,excl}$	0.0447***		0.0276***	0.0459***		0.237***		0.189***	0.234***	
	(0.00635)		(0.00645)	(0.00633)		(0.0195)		(0.0197)	(0.0195)	
$Imm_{scti}^{csl,-o,excl}$		0.0825***	0.0792***		0.0819***		0.355***	0.339***		0.390***
		(0.00459)	(0.00467)		(0.00457)		(0.0269)	(0.0271)		(0.0273)
$Imm_{scti}^{csl,-on,excl}$				0.0597***					0.142***	
				(0.00436)					(0.0142)	
$Imm_{scti}^{csl,o,excl}$					0.0248***					0.222***
					(0.00278)					(0.0201)
$Imm_{scti}^{col,-s,excl}$					0.00797*					-0.147***
					(0.00327)					(0.0213)
Observations	1028600	1028600	1028600	1028600	1028600	1028600	1028600	1028600	1028600	1028600
R-squared	0.639	0.639	0.639	0.639	0.639	0.619	0.618	0.616	0.618	0.612
R-sq within	0.0830	0.0837	0.0837	0.0837	0.0839	0.0318	0.0307	0.0247	0.0310	0.0152

All regressions include trading pair, country-year, state-year, and industry effects. + $p < 0.10$,* $p < 0.05$, ** $p < 0.010$, *** $p < 0.001$. Standard errors in parenthesis. Standard errors clustered at state-country-industry level. Imm_{scti} – immigrants from trading partner country, $Imm_{scti}^{NB,NL}$–no common border and no common language, Imm_{scti}^{bord} – geographically proximate, $Imm_{scti}^{col,excl}$ – those from countries with the same official language, $Imm_{scti}^{cnl,-o,excl}$ – proximate by common native language from countries with a different official language, $Imm_{scti}^{csl,-o,excl}$ – proximate by common spoken language from countries with a different official language, $Imm_{scti}^{csl,-on,excl}$ – proximate by common spoken non-native language from countries with a different official language $Imm_{scti}^{csl,o,excl}$ – proximate by common spoken language from countries with the same official language, $Imm_{scti}^{col,-s,excl}$ – those from countries with the same official language but not sharing the same spoken language.

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Table 2.14: Detailed Linguistic Proximity Effect on Imports

	OLS					2SLS				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
GDP Product	0.271***	0.248***	0.248***	0.237***	0.244***	0.359***	0.240***	0.234***	0.222***	0.233***
	(0.0114)	(0.0115)	(0.0115)	(0.0115)	(0.0115)	(0.0208)	(0.0205)	(0.0205)	(0.0204)	(0.0205)
Imm_{scti}	0.0673***	0.0662***	0.0663***	0.0669***	0.0654***	0.526***	0.532***	0.529***	0.555***	0.539***
	(0.00380)	(0.00380)	(0.00380)	(0.00378)	(0.00380)	(0.0220)	(0.0224)	(0.0223)	(0.0220)	(0.0225)
$Imm_{scti}^{NB,NL}$	0.00267	-0.106***	-0.107***	-0.0794***	-0.106***	-0.298***	-1.167***	-1.165***	-0.658***	-1.187***
	(0.00353)	(0.00566)	(0.00564)	(0.00456)	(0.00566)	(0.0219)	(0.0436)	(0.0435)	(0.0272)	(0.0436)
Imm_{scti}^{bord}	0.0215***	0.0182***	0.0185***	0.0178***	0.0163***	-0.0322+	-0.0785***	-0.0673***	-0.0792***	-0.128***
	(0.00312)	(0.00311)	(0.00310)	(0.00309)	(0.00318)	(0.0187)	(0.0191)	(0.0192)	(0.0188)	(0.0198)
$Imm_{scti}^{col,excl}$	0.0267***	0.0255***	0.0254***	0.0282***		0.0898***	0.0839***	0.0842***	0.132***	
	(0.00398)	(0.00396)	(0.00397)	(0.00395)		(0.0142)	(0.0143)	(0.0143)	(0.0142)	
$Imm_{scti}^{cnl,-o,excl}$	0.0147		-0.0226	0.0108		0.0480		-0.155***	-0.0187	
	(0.0143)		(0.0144)	(0.0142)		(0.0410)		(0.0416)	(0.0411)	
$Imm_{scti}^{csl,-o,excl}$		0.188***	0.190***		0.186***		1.378***	1.395***		1.396***
		(0.00869)	(0.00873)		(0.00866)		(0.0517)	(0.0521)		(0.0520)
$Imm_{scti}^{csl,-on,excl}$				0.212***					0.894***	
				(0.00873)					(0.0289)	
$Imm_{scti}^{csl,o,excl}$					0.0186**					0.354***
					(0.00595)					(0.0408)
$Imm_{scti}^{col,-s,excl}$					0.0288***					-0.233***
					(0.00703)					(0.0417)
Observations	408834	408834	408834	408834	408834	408834	408834	408834	408834	408834
R-squared	0.519	0.521	0.521	0.522	0.521	0.448	0.379	0.381	0.414	0.366
R-sq within	0.0174	0.0202	0.0203	0.0227	0.0207					
F-Stat, Inst.						3226.5	3139.7	2622.3	2675.5	2526.6

All regressions include trading pair, country-year, state-year, and industry effects. + $p < 0.10$,* $p < 0.05$, ** $p < 0.010$, *** $p < 0.001$. Standard errors in parenthesis. Standard errors clustered at state-country-industry level. Imm_{scti} – immigrants from trading partner country, $Imm_{scti}^{NB,NL}$–no common border and no common language, Imm_{scti}^{bord} – geographically proximate, $Imm_{scti}^{col,excl}$ – those from countries with the same official language, $Imm_{scti}^{cnl,-o,excl}$ – proximate by common native language from countries with a different official language, $Imm_{scti}^{csl,-o,excl}$ – proximate by common spoken language from countries with a different official language, $Imm_{scti}^{csl,-on,excl}$ – proximate by common spoken non-native language from countries with a different official language $Imm_{scti}^{csl,o,excl}$ – proximate by common spoken language from countries with the same official language, $Imm_{scti}^{col,-s,excl}$ – those from countries with the same official language but not sharing the same spoken language.

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Table 2.15: Detailed Linguistic Proximity Effect on Exports (after 2008)

	OLS					2SLS				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
GDP Product	0.503***	0.496***	0.496***	0.497***	0.495***	0.512***	0.485***	0.493***	0.501***	0.479***
	(0.00662)	(0.00667)	(0.00667)	(0.00669)	(0.00668)	(0.0110)	(0.0107)	(0.0108)	(0.0109)	(0.0106)
Imm_{scti}	0.0503***	0.0495***	0.0495***	0.0498***	0.0493***	0.313***	0.288***	0.300***	0.310***	0.295***
	(0.00231)	(0.00231)	(0.00231)	(0.00231)	(0.00231)	(0.0132)	(0.0131)	(0.0132)	(0.0132)	(0.0132)
$Imm_{scti}^{NB,NL}$	0.0235***	-0.0165***	-0.0156***	0.00672**	-0.0163***	-0.0814***	-0.233***	-0.238***	-0.120***	-0.260***
	(0.00199)	(0.00331)	(0.00332)	(0.00238)	(0.00331)	(0.0112)	(0.0229)	(0.0228)	(0.0136)	(0.0231)
Imm_{scti}^{bord}	0.0318***	0.0310***	0.0307***	0.0309***	0.0271***	0.00419	0.0166	0.00147	-0.00132	-0.0205+
	(0.00175)	(0.00175)	(0.00174)	(0.00175)	(0.00179)	(0.0103)	(0.0102)	(0.0103)	(0.0104)	(0.0108)
$Imm_{scti}^{col,excl}$	0.0276***	0.0271***	0.0272***	0.0280***		0.0470***	0.0529***	0.0490***	0.0536***	
	(0.00201)	(0.00201)	(0.00201)	(0.00201)		(0.00805)	(0.00804)	(0.00804)	(0.00803)	
$Imm_{scti}^{cnl,-o,excl}$	0.0443***		0.0282***	0.0448***		0.246***		0.208***	0.244***	
	(0.00687)		(0.00702)	(0.00687)		(0.0206)		(0.0209)	(0.0206)	
$Imm_{scti}^{csl,-o,excl}$		0.0738***	0.0700***		0.0734***		0.280***	0.257***		0.315***
		(0.00532)	(0.00543)		(0.00530)		(0.0282)	(0.0285)		(0.0286)
$Imm_{scti}^{csl,-on,excl}$				0.0476***					0.0968***	
				(0.00501)					(0.0151)	
$Imm_{scti}^{csl,o,excl}$					0.0320***					0.227***
					(0.00328)					(0.0216)
$Imm_{scti}^{col,-s,excl}$					0.00333					-0.161***
					(0.00376)					(0.0223)
Observations	646038	646038	646038	646038	646038	646038	646038	646038	646038	646038
R-squared	0.647	0.647	0.647	0.647	0.647	0.628	0.629	0.626	0.627	0.623
R-sq within	0.0848	0.0853	0.0854	0.0852	0.0856	0.0348	0.0380	0.0311	0.0342	0.0232
F-Stat, Inst.						4867.6	5003.9	4076.8	4053.4	3854.7

All regressions include trading pair, country-year, state-year, and industry effects. + $p < 0.10$,* $p < 0.05$, ** $p < 0.010$, *** $p < 0.001$. Standard errors in parenthesis. Standard errors clustered at state-country-industry level. Imm_{scti} – immigrants from trading partner country, $Imm_{scti}^{NB,NL}$–no common border and no common language, Imm_{scti}^{bord} – geographically proximate, $Imm_{scti}^{col,excl}$ – those from countries with the same official language, $Imm_{scti}^{cnl,-o,excl}$ – proximate by common native language from countries with a different official language, $Imm_{scti}^{csl,-o,excl}$ – proximate by common spoken language from countries with a different official language, $Imm_{scti}^{csl,-on,excl}$ – proximate by common spoken non-native language from countries with a different official language $Imm_{scti}^{csl,o,excl}$ – proximate by common spoken language from countries with the same official language, $Imm_{scti}^{col,-s,excl}$ – those from countries with the same official language but not sharing the same spoken language.

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Table 2.16: Detailed Linguistic Proximity Effect on Exports and Imports (Including Observations with Zero Trade)

	Exports					Imports				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
GDP Product	0.0596***	0.0518***	0.0526***	0.0568***	0.0595***	0.0225***	0.0187***	0.0176***	0.0168***	0.0173***
	(0.00328)	(0.00321)	(0.00321)	(0.00323)	(0.00313)	(0.00206)	(0.00204)	(0.00204)	(0.00203)	(0.00200)
Imm_{scti}	0.761***	0.700***	0.704***	0.756***	0.689***	0.399***	0.367***	0.361***	0.389***	0.362***
	(0.0125)	(0.0123)	(0.0123)	(0.0124)	(0.0122)	(0.00876)	(0.00859)	(0.00864)	(0.00861)	(0.00861)
$Imm_{scti}^{NB,NL}$	0.0648***	-0.219***	-0.219***	0.00476	-0.227***	0.00531+	-0.185***	-0.185***	-0.111***	-0.185***
	(0.00435)	(0.0111)	(0.0111)	(0.00725)	(0.0110)	(0.00274)	(0.00689)	(0.00687)	(0.00459)	(0.00683)
Imm_{scti}^{bord}	0.0368***	0.0263***	0.0226**	0.0306***	-0.0363***	-0.00704	-0.0225***	-0.0168***	-0.0192***	-0.0291***
	(0.00700)	(0.00684)	(0.00698)	(0.00702)	(0.00709)	(0.00465)	(0.00459)	(0.00467)	(0.00464)	(0.00480)
$Imm_{scti}^{col,excl}$	0.0597***	0.0644***	0.0642***	0.0631***		-0.00823**	-0.00574+	-0.00543+	-0.00169	
	(0.00463)	(0.00459)	(0.00458)	(0.00461)		(0.00305)	(0.00303)	(0.00303)	(0.00302)	
$Imm_{scti}^{cnl,-o,excl}$	0.169***		0.0509**	0.163***		0.00170		-0.0776***	-0.00892	
	(0.0168)		(0.0172)	(0.0168)		(0.0114)		(0.0115)	(0.0112)	
$Imm_{scti}^{csl,-o,excl}$		0.483***	0.479***		0.495***		0.314***	0.321***		0.310***
		(0.0154)	(0.0157)		(0.0151)		(0.00968)	(0.00978)		(0.00964)
$Imm_{scti}^{csl,-on,excl}$				0.132***					0.256***	
				(0.0105)					(0.00662)	
$Imm_{scti}^{csl,o,excl}$					0.336***					0.0442***
					(0.0130)					(0.00858)
$Imm_{scti}^{col,-s,excl}$					-0.266***					-0.0399***
					(0.0136)					(0.00911)
Observations	2513802	2513802	2513802	2513802	2513802	2513802	2513802	2513802	2513802	2513802
R-squared	0.958	0.959	0.959	0.958	0.958	0.960	0.959	0.960	0.959	0.959
R-sq within	0.881	0.883	0.882	0.881	0.881	0.896	0.895	0.895	0.895	0.895
F-Stat, Inst.	17198.6	18134.4	14425.5	14318.9	14733.7	17352.3	18281.8	14548.0	14446.6	14779.8

All regressions include trading pair, country-year, state-year, industry, and trade participation effects. + $p < 0.10$,* $p < 0.05$, ** $p < 0.010$, *** $p < 0.001$.Standard errors in parenthesis. Standard errors clustered at state-country-industry level. Imm_{scti} – immigrants from trading partner country, $Imm_{scti}^{NB,NL}$–no common border and no common language, Imm_{scti}^{bord} – geographically proximate, $Imm_{scti}^{col,excl}$ – those from countries with the same official language, $Imm_{scti}^{cnl,-o,excl}$ – proximate by common native language from countries with a different official language, $Imm_{scti}^{csl,-o,excl}$ – proximate by common spoken language from countries with a different official language, $Imm_{scti}^{csl,-on,excl}$ – proximate by common spoken non-native language from countries with a different official language $Imm_{scti}^{csl,o,excl}$ – proximate by common spoken language from countries with the same official language, $Imm_{scti}^{col,-s,excl}$ – those from countries with the same official language but not sharing the same spoken language.

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CHAPTER 3

FARM SIZE, POLICY DISTORTIONS, AND CROSS-COUNTRY VARIATION IN AGRICULTURAL PRODUCTIVITY

3.1 Introduction

Why the measured labor productivity gap between rich and poor countries is much larger in agriculture than in non-agriculture is a long-standing puzzle in development economics. In fact, the gap is about two times larger in agriculture (Caselli (2005), Gollin, Lagakos and Waugh (2014*a*), Adamopoulos and Restuccia (2014)). Since more productive labor is key to higher standards of living and most of the world's poor working adults make a living through agriculture (Castaneda et al. (2016)), increasing agricultural labor productivity in low-income countries could have tremendous impact on poverty reduction. This has motivated a large body of work dedicated to trying to understand the reasons why the cross-country differences in agricultural labor productivity are as large as they are.

This paper aims to understand which potential explanations for the international agricultural productivity differences are consistent with analysis herein and which are not, as both failing to find the cause and misdiagnosing the true cause of the differences is undesirable. Because smallholders constitute the majority of farmers and are poorer, and since the role of smallholders in structural transformation is a broader question in development economics, we are particularly interested in investi-

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gating what role farm size plays in explaining international differences in agricultural productivity. One type of evidence for the role of farm size is what is known as inverse relationship (IR) between farm size and land productivity (yield) with accompanying positive relationship between farm size and labor productivity within farms. This relationship seems to suggest that reallocating labor to larger farms should both decrease land productivity and increase labor productivity.

While the above line of reasoning does not necessitate total factor productivity advantage of large farms, Adamopoulos and Restuccia (2014) argue that larger farms are more productive even for the same level of input use, and further that one of the chief reasons for the vast agricultural labor productivity differences between rich and poor countries is size-dependent policies, especially tax policies, that disproportionately favor small farms and lead to production factor misallocation. They attribute as much as 3 quarters of the differences in agricultural labor productivity between top and bottom quintiles of countries in terms of per capita income to policy interventions and market distortions that lead to fewer large farms and more small farms; in particular, one-quarter of the variation is explained by crop-specific price distortions in poor countries that favor small farms. According the model that their paper is built on, a farmer draws productivity from a lognormal distribution, and this productivity parameter directly enters the production function. The implications are somewhat similar to those in the industrial organization and trade literature, in that exogenous (usually Pareto or lognormal) productivity distribution results in a small number of large, more productive firms and a large number of less productive firms. Importantly, the large firms with a higher productivity draw will have higher pro-

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ductivity even when using the same amount of inputs as the smaller firms. If this is also the case in agriculture, it is a strong argument to identify and consider removing policies that incentivize creating and maintaining small farms. If these policies are more prevalent in poorer countries, this could be a major source of the agricultural labor productivity disparity between rich and poor countries. Here, we test both whether average farm size and size distribution explain cross-country productivity differences and whether crop-specific price distortions affect average farm size.

There are other explanations for the lower labor productivity of smaller farms, such as bimodal production structure (especially in developing countries), whereby large farms have a cost advantage in purchasing non-labor primary and intermediate inputs and small farms have an advantage in purchasing labor inputs/using family labor; if these are true, focusing on alleviating disadvantages faced by small farmers may potentially be a better strategy efficiency- and welfare-wise. If these distortions are more prevalent in developing countries, this may be a large part of the explanation for the large agricultural labor productivity differences between high-income and low-income countries.

A different type of explanation for the larger gap in agriculture is misallocation between broad sectors–agriculture and non-agriculture. Market failures in a number of markets, but in particular, labor market, may lead to difficulty in relocating from agriculture to non-agriculture. Additionally, policies that favor agriculture over non-agriculture may be subsidizing the inefficient sector and keeping more labor therein. Here, we test whether absolute and relative rates of assistance to agriculture in poor

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countries make agriculture labor relatively less productive there. Other explanations include different rates of use of intermediate inputs, selection into agriculture and measurement error. We are able to test the first reason, but not the latter two, although we keep them in mind.

To explore what explains international differences in agricultural productivity, we compile data on agricultural output per worker, primary and intermediate inputs, institutional setting and development level proxies, and farm size using a variety of sources, including many individual country agricultural censuses. We also calculate GINI index of farm size inequality and a measure of farm size distortion similar to that used by Adamopoulos and Restuccia (2014)–correlation between crop specific nominal rate of assistance (NRA) and average farm size by crop–and include measures for overall nominal and relative rates of assistance to agriculture. To our, knowledge this is the most extensive and recent dataset compiled for this purpose.

In our analysis, we opt for a reduced form estimation rather than calibration. In this way, we obviate some of the strong assumptions and limitations of structural models, such as that in Adamopoulos and Restuccia (2014) study, admittedly at the cost of others. One assumption that we want avoid making is taking optimal farm size in every country as being a function of the same managerial ability distribution, based on the data from the United States. For one, managerial ability may be a function of relevant education and experience (i.e. not randomly drawn from the same lognormal distribution common to all countries). It may also be a result of U.S. agricultural policies, which Cai (2015) find favor large farms. Additionally, it

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may be complimentary to available capital inputs. Importantly, it is difficult to test for accuracy of this assumption.

Another reason to pursue a reduced-form approach is to decrease reliance on the U.S. as the benchmark economy. A number of calibration-based studies have relied on this approach, likely due to availability of reliable and detailed agricultural statistics for the United States and due to assumption of fewer distortions in the U.S. On the flip side, relying on one country seems more likely to bias results due to idiosyncratic factors than relying equally on information from all countries with available data; for the former, we only need the U.S. to systematically differ from all other countries, whereas for the latter, we need systematic differences across countries not accounted for in the model.

We follow aggregate production function estimation approach similar to Vollrath (2007), but look at labor instead of land productivity and also examine the effect of agricultural tax policy, as a measure of potential distortions, both with regards to average farm size and productivity. Other deviations from Vollrath (2007) methods include simultaneous estimation of average farm size and agricultural labor productivity determinants, to account for their potentially simultaneous determination, and variance decomposition. The latter is an especially apt approach here, since 1), there may be various channels of influence between the nominally independent variables, complicating regression coefficient interpretation, and 2), we are interested in looking at how much of the agricultural output per worker variation is explained by the main farm size distortion variable as well as the other variables of interest.

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Overall, we find that controlling for primary and intermediate inputs, the evidence for average farm size affecting agricultural labor productivity is weak. This does not mean that factors are not misallocated between different farms–different levels of primary and intermediate input use may already be a result of that–but it is inconsistent with larger farms having higher total factor productivity, suggesting the effects of misallocation may be less severe. As further evidence inconsistent with misallocation away from large farms, we find that neither the share of large farms nor farm size inequality increase labor productivity. Additionally, even if average farm size were to independently affect productivity, we find that it is implausible that crop-specific price distortions decrease average farm size in countries of the poorest quintile, as beginning with the 2000 World Census of Agriculture the correlation between nominal rate of assistance and farm size per crop has been positive in this group of countries.

In terms of other explanations for the productivity gap, there is little evidence of overall price support for agriculture as the culprit, as 1) neither nominal (NRA) nor relative (RRA) rates of assistance seem to affect output per worker and 2) it is the rich rather than the poor countries that appear to be subsidizing agriculture both in absolute and in relative terms; this does not mean there is no sectoral misallocation, however, as agricultural land per worker may already reflect some misallocation and it increases labor productivity, but it is unlikely to be due to taxes/subsidies. In addition to land per worker, primary inputs, such as machinery and livestock, also increase labor productivity; the results indicated that agricultural inputs explain most of the labor productivity variation, but we cannot tell if they are misallocated

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to begin with.

Variance decomposition results echo marginal effects results, in that less than 10% of the variation in output per worker is explained by average farm size and most of it is explained by primary and intermediate inputs. Most of the variation in average farm size is also explained by primary and intermediate inputs, with over 30% attributable to agricultural land per worker.

The paper proceeds as follows: the next section provides brief discussion of related literature, Section 3.3 describes data used for empirical analysis and presents descriptive statistics, Section 3.4 explains empirical strategy, Section 3.5 presents results and Section 3.6 concludes.

3.2 Related Literature

The literature related to farm size distribution and agricultural productivity can be divided into studies on international farm size distribution differences, studies on international agricultural labor productivity gap (that is, the tendency for agricultural productivity differences to be much higher than for non-agriculture), and studies on within-country farm size and productivity relationship. The first kind of literature is usually descriptive and is rather scarce. The second kind of literature is less scarce and generally uses macroeconomic variables and calibration approach to explain the differences between countries at the extremes of income distribution.

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The third kind of literature is quite prolific, usually based on micro data, but generally does not directly attempt to explain international differences in farm-size or agricultural labor productivity.

3.2.1 International Farm Size Differences

It is useful to consult literature on international farm size differences because it reveals some of the chief factors associated with larger farm sizes, which are generally related to factors other than government policies leading to size distortion. One of the first efforts to compare and examine international differences in average farm size was made by Grigg (1966), who primarily used data from the FAO's 1950 World Census of Agriculture. Farm land area was chosen as a measure of size, given scarce availability of total value of output or other alternative estimates. Some of the differences identified in the study continue to be observed today. The largest average farm size was observed in more recently settled and less densely populated regions, such as Australia, North America, parts of South America and the Soviet Union (which had socialized agriculture, complicating the comparison). The smallest average farm size was found in the densely populated areas of East Asia, South Asia, Africa, parts of Central and Southern Europe. The regions in-between were in Central America, parts of South America, and Western and Northern Europe. The first observational inference was of a *negative correlation between (agricultural) land scarcity/(rural) population density and farm size, especially important at the extremes*. Another was the importance of inheritance laws, specifically primogeni-

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ture versus partible inheritance. The third notable issue was measurement reliability, stemming from sources such as uncertain land tenure, especially in parts of Africa. Farm activity type was found to be another important determinant/correlate of farm size, with livestock farms having the largest area and horticulture activities and "self-sufficing" the least. Lastly, a variety of economic factors, such as relative scarcity of capital and market access where posited to be potentially important. No statistical estimate of any of these factors was provided, however.

In a more recent study, Eastwood, Lipton and Newell (2010) show that between 1930 and 1990, average farm size in Europe and North America has increased, and in South America, Asia and Africa (since 1970)–declined; consequently the gap in average farm size between large parts of Africa and Asia and the other regions (except South America) increased compared to Grigg's study. They also find a positive relationship between average farm size and GINI measure of (farm size) inequality. As determinants of farm size distribution, Eastwood, Lipton and Newell (2010) discuss importance of concerted human effort, such as "colonial land grab" or land reforms, market interventions, such as taxes and subsidies, and the effects of liberalization and economic development, where the latter is positively related to the farm size.

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3.2.2 Agricultural Productivity Differences

International differences in agricultural labor productivity are significantly larger than non-agricultural productivity differences. Caselli (2005) observes that the difference between the 10th and 90th labor productivity percentiles to be a factor of 22 in non-agriculture and 45 in agriculture. Similarly, Adamopoulos and Restuccia (2014) find a 47-fold agricultural productivity difference between the average of the rich and poor country *quintiles* (in 1990 World Census), compared to a factor of 20 for non-agriculture. Gollin, Lagakos and Waugh (2014*a*) look at physical-output-based productivity differences (based on rice, wheat, and maize), and find the top to bottom *decile* differences to be on average around 50.

Because the difference in labor productivity between rich and poor countries is about twice as large in agriculture as in non-agriculture, this suggests that some feature (or features) of agriculture in poor countries makes it especially unproductive (over and above factors that affect labor productivity in other sectors). Several plausible explanations have been put forth in the literature.

One potential reason is lower rates of use of *intermediate inputs* in poorer countries; of course, this is more of an intermediate reason, as the reason(s) for differential use is(are) the ultimate cause. Potential causes for lower intermediate input use include risk aversion in the face of agriculture-specific shocks, higher prices, and other direct or indirect barriers (Donovan (2012); Restuccia, Yang and Zhu (2008)). Potentially because these aspects are more characteristic of developing countries, the

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share of intermediate inputs in rich-country agriculture is estimated to be around 0.4, compared to estimates of 0.04-0.12 in poor countries.

Another potential explanation for agricultural productivity differences is *selection into agriculture*, as discussed by Lagakos and Waugh (2013), whereby in poor countries, those relatively less productive in agriculture select in, whereas in the rich countries those relatively more productive in agriculture select in; subsistence constraint is one of the possible reasons for the selection into agriculture in poor countries.

A broad potential explanation is *misallocation* of labor between agriculture and non-agriculture. McMillan, Rodrik and Verduzco-Gallo (2014) and Vollrath (2009), among others, have argued that reallocation of labor from agriculture to non-agriculture would substantially increase labor productivity. There is strong evidence of benefits of structural transformation, whereby low-income societies with large share of labor in agriculture become high-income societies with smaller but more productive agriculture, but some disagreement persists about the way to achieve it, including the role of small vs. large farms (Barrett et al. (2017)).

Misallocation within agriculture has also been examined as one of the potential sources of productivity gaps, particularly in regard to allocation between farms with heterogeneous productivity, leading to *farm size distribution distortion*. This idea is related to the study of size-distorting effect of taxes that are positively correlated with productivity, discussed in the case of manufacturing in Restuccia and Rogerson (2008). In case of agriculture, the basic principle for the explanation is that larger

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farms are more productive and policies that distort farm size distribution towards lower average size and more smaller farms diminish productivity (Adamopoulos and Restuccia (2014)). Using micro-level data from Malawi, Restuccia and Santaeulalia-Llopis (2017) find that removing farm-size distortions has a potential of tripling agricultural labor productivity. Using cross-country data calibrated to observed U.S. indicators, Adamopoulos and Restuccia (2014) find that aggregate factors, such as capital, land, and economy-wide productivity, account for one quarter of observed differences between rich and poor countries in size and labor productivity, with policies and institutions that misallocate resources across farms potentially accounting for the rest. More specifically, their results indicate that crop-specific price distortions correlated with farm size account for 25% of cross-country differences in size and productivity.

A different kind of explanation is *measurement error*. Gollin, Lagakos and Waugh (2014*b*) find that taking sector differences in hours and human capital per worker into consideration jointly reduces the size of the average agricultural productivity gap from around four to around two. They also look at the alternative measures of value-added, sectoral differences in labor's share of value-added, urban-rural differences in the cost of living and other factors, but these factors do not seem to explain more of the agricultural productivity gap. Relatedly, using detailed data on 4 African economies, McCullough (2017) finds that accounting for hours worked, generally much smaller in agriculture, reduces the productivity gap by half. In line with these two studies, Vollrath (2013) finds that taking into account better estimates of labor effort and human capital reduces the estimate of productivity gap by half.

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Herrendorf and Schoellman (2015) show that even in the U.S.–where severe misallocation is unlikely–there is a significant labor productivity gap between agriculture and non-agriculture, that measured through wages rather than labor productivity, gaps are much smaller, and that a big part of the explanation is that productivity in agriculture is severely mismeasured in the U.S. and several foreign countries of varying income levels. Using detailed data from three African countries, Gollin and Udry (2017) find that once one takes into account idiosyncratic shocks, measurement error, and heterogeneity in land quality, the importance of misallocation in accounting for productivity dispersion across farms drops substantially, whereby optimal reallocation would increase aggregate output by 15% in Ghana and 50% in Uganda (compared to 200% improvement Restuccia and Santaeulalia-Llopis (2017) find for Malawi).

Overall, it is likely that multiple factors affect the agricultural labor productivity gap, and that it is probably overstated without accounting for measurement error.

3.2.3 Inverse Size-Productivity Relationship

A related strand of literature concerns not inter- but intranational relationship between farm size and productivity. These studies generally focus on yield and farm size (not *average* farm size) relationship. In particular, explanation of the apparent inverse relationship (IR) between farm size and land productivity has been the topic of a number of articles. Some of the explanations for the apparent relation-

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ship include price risk (Barrett (1996)), unobserved land quality (Benjamin (1995)), labor market imperfections (Barrett, Bellemare and Hou (2010); Foster and Rosenzweig (2011); Binswanger and Rosenzweig (1986); Feder (1985)), different "mode of production" (Carter (1984)) and others. It has been suggested that bimodal production, with different utilization of resources by large and small farms is one possible result of factor market imperfections (Cornia (1985); Vollrath (2007)). While most of these studies do not attempt to directly explain international farm size distribution differences, if the within-country factors that affect both farm size and productivity systematically differ between countries, they may be part of the explanation of international differences in farm size distribution.

One problem trying to connect the IR literature to that on cross-country differences is that the most often former focuses on land productivity and the latter on labor productivity. Some of the more recent papers alleviate this problem by focusing on total factor productivity and using detailed farm-level panels. Rada and Fuglie (2018) summarize the findings in 5 special issue studies that examine a number of countries of varying income levels by concluding that, "The evidence suggests that the relationship between farm size and productivity evolves with the stage of economic development. In conjunction with economic growth and market development, initial productivity advantages of small farms appear to gradually attenuate over time, moving toward constant and eventually increasing returns to size. Nonetheless, the small farm sector can be quite dynamic, and need not be a drag on agricultural growth until perhaps well into the development process." (Rada and Fuglie (2018), p. 2). One implication of these results is that if true, they make in-

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ferences about misallocation in low-income countries based on the U.S. (a developed country) as a benchmark economy potentially misguided.

The present study aims to address the question of international farm size distribution and agricultural productivity differences in a way more consistent with the literature on international agricultural labor productivity differences, that is, using macro level variables. However, we opt for a reduced-form approach, obviating some of the assumptions associated with structural models used for this purpose, especially those that depend on the U.S. as a benchmark economy, admittedly, at the cost of others.

3.3 Data

To examine what explains cross-country differences in agricultural labor productivity, we combine data from several sources. The data on yield, area harvested, number of agricultural workers, labor productivity, agricultural inputs, arable and total land per capita comes from FAOSTAT. Estimates of nominal rate of assistance for specific crops and for agriculture on average are from the World Bank's Distortions of Agricultural Incentives Database. Data on average farm size, farm size by crop, and farm size distribution is from the FAO's World Census of Agriculture and country agricultural census websites. Non-agricultural productivity, employment, population and other country characteristics are from Penn World Tables 8.0. Land quality estimates come from Wiebe (2003). Institutional quality is the average of six

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measures of institutional quality compiled by Kaufmann, Kraay and Zoido (2002). Legal origin is from La Porta et al. (1999).

3.3.1 Descriptive Statistics

We present descriptive statistics for the cross-section and panel specifications in Table 3.1 and Table 3.2. The first set of variables is comprised of the latest observations for 89 countries with data available to estimate specifications (1) through (4) in Table 3.1. The next set includes variables used in the estimation with GINI and tax distortion variables, which is the set of the latest observations from 44 countries. For panel data, the number of observations for variables in the regressions without farm size distribution and policy variables is 285, but it is reduced to 147 for the set where all the variables are present. Table 3.1 shows that the mean of average farm size is 42 hectars, but it ranges from 0.47 to over 779. NRA/average farm size correlation is on average negative, which means that crops with smaller average farm size tend to benefit from greater assistance. NRA average is positive, meaning that the net effect on prices is on average a subsidy, while small but positive RRA means assistance to agriculture is generally slightly greater than to non-agriculture.

To form an idea of sources of differences in agricultural productivity between rich and poor countries, it is informative to look at the means of the key variables by quintile of GDP per capita by each WCA decade, shown in Table 3.4. Starting with agricultural output per worker, we can see that in 2010 WCA, the top quintile showed

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output per worker valued at about 31,000 in 2005 dollars, compared to about 3,444 for the bottom quintile, a difference of a factor of 10, significantly larger than the factor of 6 difference for non-agricultural productivity. It should be pointed out that this is an underestimate of what the differences would be if all countries were included, since the table only includes countries with key data available (340 observations over 5 decades, an average of 68 per decade). Compared to 1970, we see about the same difference in non-agricultural productivity and a somewhat smaller differences in agricultural productivity. The average difference in the ratio of nonagricultural to agricultural productivity (which is not equal to the ratio of the average nonagricultural and agricultural productivity), has remained roughly the same, at 2. The share of labor in agriculture in every decade is strictly decreasing with respect to the income quintile. Additionally, it almost always decreases from one decade to the next for each quintile. It decreased more for the top quintile than for the bottom, but it decreases the most for the second-highest quintile .

The difference in average farm size depends significantly on the countries in the sample, especially the ones with the largest farm size (Australia has been excluded from all analysis as an outlier). The difference between the top and bottom quintiles ranges from a factor of 10 in 1980 to almost 100 in 2010, and is about 50 on average, significantly larger than difference in productivity. The growing difference in average farm size is consistent with findings elsewhere, however, that average farm size has grown in North America and parts of Europe, but decreased in most of Africa and Asia, exacerbating international differences. The GINI measure of inequality of farm size distribution does not seem to have changed significantly over the five decades,

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nor is it dramatically different between the top and bottom quintiles; in fact it seems to be the largest in the middle quintile.

Turning to the tax variables, we see an increase in nominal rate of assistance (increase in subsidy rates) between 1970 and 1990, but a sharp decrease from 1990 to 2010, consistent with trade liberalization in most of the world in 1990's and 2000's. Relative rate of assistance saw the same general pattern of change. Notably, however, the rich countries still subsidize agriculture, both in absolute terms and compared to nonagriculture, whereas the poor countries have on average negligible subsidies for agriculture, less than other sectors, for a negative RRA.

Looking at the main variable meant to capture farm size distortion, correlation between crop-specific farm size and crop-specific NRA, we find that in the 1990 WCA, the decade in Adamopoulos and Restuccia (2014) study, there is indeed a much stronger negative correlation for the countries in the lowest income quintile, 12 times larger than in the top quintile, at -0.47 compared to -0.038, which means that they subsidized less or taxed more the crops with larger farm size. In 2000 and 2010, however, the correlation switched to positive for the poor countries and became even more negative for the rich countries, which suggests that if the mechanism played a role in explaining productivity differences in 1990 (as well as in 1980 and 1970), this may no longer be the case for 2000 and 2010.

Perhaps the most dramatic difference between rich and poor countries is in the use of intermediate inputs. The differences in aggregate capital stock per worker, livestock per worker, agricultural machinery per worker and fertilizer use per worker

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are at least a factor of 10 in almost every decade for every variable, but go as high as 70 for agricultural machinery. This is likely a key source of difference in agricultural labor productivity.

That countries with greater output per worker and larger average farm size tend to to be more intensive in non-labor input use is further demonstrated in Table 3.3. The correlation between output per worker, average farm size and the main inputs is extremely high, at least 0.5 and generally higher, with correlation between output per worker and agricultural machinery use at 0.865. We further illustrate the three-way relationship between output per worker, average farm size and agricultural land per worker in Figure 3.1. At all levels of the variables, there is an obvious, and not unexpected, positive relationship (notably, output per hectar tends to be lower with more land per worker). Whereas land per worker increases with average farm size throughout the distribution, Figure 3.2 shows that capital use per worker seems to rise only after log of average farm size exceeds 2; it also appears to begin to rise after land per worker reaches a threshold. Overall, Figures 3.1 and 3.2 suggests that at least part of the reason for the strong correlation between output per worker and average farm size is likely to be the tendency of countries with larger farm size to also use more primary and intermediate inputs. Another thing to note in Figure 3.2 is that three countries, Canada, New Zealand and, notably, USA, have significantly more capital per worker compared to other countries, deviating substantially from the fitted values line, which should prompt caution in using any of the three as a benchmark economy for a comparative study.

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Further exploring the relationship between farm size distribution and agricultural labor productivity, we look at the top two graphs in Figure 3.2. As previously found by Vollrath (2007), GINI seems to be positively and rather strongly correlated with average farm size. Whereas it was found to be negatively correlated with output per hectar in Vollrath (2007), it is positively, but weakly correlated with output per worker. As an alternative measure of distribution, we look at the share of small (<5 Ha) and large (>20 Ha) farms in Figure 3.3 and Figure 3.4. There is a clear tendency for countries with a larger share of small farms to have lower output per worker and the countries with higher share of large farms to have higher output per worker, but only above initial threshold (about 0.1), consistent with Rada and Fuglie (2018) analysis that posits different farm sizes may be optimal at different levels of development. Analogously to previous findings, capital and land per worker tend to be positively related to the share of large farms and negatively to the share of small farms, with USA and New Zealand deviating from the overall trend (share of small and large farms not available for Canada).

In Figure 3.5 we look at the general direction of correlation between the crop-specific price distortion measures (correlation between nominal rate of assistance per crop and average size per crop)–CORR and $CORR^2$–and average farm size and output per worker. There appears to be essentially no unconditional correlation between output per worker and CORR, and a slightly negative one between output per worker and $CORR^2$. Average farm size is positively correlated with CORR, which suggests higher subsidies for crops with larger farm size may indeed be leading to larger average farm size and more negative CORR–to smaller average farm size.

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3.4 Empirical Strategy

Moving from examining unconditional relationships between variables considered, we motivate empirical methodology by following the approach of Vollrath (2007) in examining how farm size distribution can affect average productivity, applying it to labor rather than land productivity. Initially, if we take all farms as sharing the same production function per unit of labor, total agricultural output for a country can be written as

$$Y = A[Mf_{av}(\mathbf{X})] = A[Q_f N f_{av}(\mathbf{X})], \text{or} \tag{1}$$

$$Y = A[Lg_{av}(\mathbf{X})] = A[Q_f (L/M) N g_{av}(\mathbf{X})], \tag{2}$$

where A is total factor productivity (TFP), M is total agricultural land, Q_f is an average farm size, N is the number of farms, L is total agricultural labor, and $f_{av}(.)$ and $g_{av}(.)$ are production functions per hectar and per unit of labor, respectively, for a farm of an average size, and **X** is a vector of input used. To get output per hectar or per worker, we can divide both sides by total land area or the agricultural labor force, respectively. It the production function does not vary by farm size, then output per worker is

$$Y_L = A[g_{av}(\mathbf{X})] \tag{3}$$

and is independent of average farm size. But if production function varies by farm size, such that $\partial g(.)/\partial Q_f \neq 0$, total output per worker (and, analogously, per hectar) may differ in countries with different average farm sizes. Consider the case where production functions differ by farm size and farm size distribution is characterized by

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a certain degree of inequality. To proxy for inequality of farm size, Vollrath (2007) shows that average output per hectar can be expressed as follows:

$$y_M = A[(1-\lambda)\theta_s f_s(X_s) + \lambda\theta_l f_l(X_l)], \tag{4}$$

where λ is the share of large farms, Q_s and Q_l are average farm sizes for small and large farms, and f_s and f_l are the corresponding production functions. For our purposes, we can multiply both sides by agricultural area per worker by M/L and express $f(.)$ in terms of $g(.)$ to obtain

$$y_L = (M/L)A[(1-\lambda)\theta_s(L/M)_s g_s(X_s) + \lambda\theta_l(L/M)_l g_l(X_l)], \tag{5}$$

which can be further simplified if we assume worker to land ratio to be the same across farms, but this is rarely the case empirically. Because $L_l/N_l > L_s/N_s$ is likely to hold (more overall labor per large farm), a negative regression coefficient on the share of larger farms or GINI suggests $g_s(.) > g_l(.)$, while a positive coefficient does not necessarily indicate the opposite (it needs to be sufficiently large).

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3.4.1 Cross-Section

To transform (3) to estimable equation, we can take logs of both sides.[1] This approach follows the literature in expressing partial productivity as a function of input quantity and quality (human capital proxies, land quality). Additionally, to test whether labor productivity is affected by average farm size, GINI, and tax/subsidy distortions (for example, through decreasing TFP), we add terms that capture these factors, obtaining the following estimating equation (CRS form most similar to Vollrath (2007)):

$$\begin{aligned} lny_i^L = \beta_o + \beta_1 g_i + \beta_2 ln\overline{l_i} + \beta_x lnX_{wk,i} + \beta_z Z_{wk,i} + \beta_{NRA} NRA_i^{5yr} + \\ \beta_{CORR} CORR_i^{5yr} + \epsilon_i, \end{aligned} \tag{6}$$

where y_i^L is output per worker in country i, $\overline{l_i}$ is land per holding (average farm size), g_i is the GINI coefficient of farm sizes, X_i is a vector of inputs in per worker terms, Z_i is a vector of other country-specific controls expected to affect labor productivity; NRA_i^{5yr} is the average nominal rate of assistance for the previous five years and $CORR_i^{5yr}$ is the correlation between farm size per crop and the average nominal rate of assistance per crop over the previous five years; X includes livestock per worker, value of agricultural machinery per worker, fertilizer inputs per worker, and land per worker. Z controls include input quality and type, including land quality index, percent of land irrigated, and share of land in pasture. It also contains quality of

[1] A production function of a generally acceptable type, such as Cobb-Douglas, is amenable to this operation

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labor indicators, with life expectancy and fertility rate as proxies. These variables are highly correlated with other indicators of human capital, such as levels of education, but are characterized by higher availability. Z also contains a measure of institutional quality and indicator for legal origin. Research and development is another relevant variable but its availability in FAO data is limited, and cannot be used in most specifications. Finally, ϵ_i is a potentially heteroskedastic error term.

We expect all of the inputs in production as well as land quality to have positive effect on output. Similarly, better institutions should positively affect productivity, through better property rights, better contract enforcement, less corruption and fewer distortionary government interventions (although we are not directly interested in the coefficients on institutional controls). Similarly we expect labor quality/human capital variables to positively affect labor productivity.

The fist of the main variables of interest is average farm size. Because the evidence for unconditional positive correlation between farm size and labor productivity is strong, we should expect larger average farm size to have a positive correlation when not controlling for input use. Controlling for input use and country TFP-related controls, however, we should see a positive coefficient on average farm size if larger farms have productivity advantage from a source other than the use of main primary and intermediate inputs, such as through exogenously given managerial ability that is higher for larger farms. Absence of such an effect would suggest that that smaller average farm size, given the same input use, would lead to the same agricultural output per worker.

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When discussing the expected GINI coefficient effect, it is useful to consider the potential sources for higher or lower GINI. Generally, a small number of big farms in conjunction with a large number of very small farms should lead to higher GINI. Higher GINI may reflect lower barriers to efficient reallocation of land from smaller, potentially less-productive farms to larger, potentially more productive farms; this is the case consistent with the hypothesis of Adamopoulos and Restuccia (2014). In fact, if the American farm size distribution with GINI of 0.8 is optimal, the average for the countries considered, at 0.62, is more than 1 standard deviation lower than optimal. We should then expect a positive coefficient on GINI (which, as (5) suggests, does not necessarily mean that larger farms have higher TFP). On the other hand, higher GINI coefficient may reflect market failures preventing efficient allocation of land from larger farms to more efficient smaller farms. A negative coefficient on GINI is indicative of smaller farms being more productive in terms of TFP.

The first policy variable, correlation between average farm size per crop and the crop-specific nominal rate of assistance, interests us in two ways. First, we want to see if small-size-favoring policy leads to lower agricultural labor productivity directly, such as through lower efficiency manifested in lower TFP. We observe this if the coefficient on CORR variable is positive–that is, the lower the relative preference for small farms, the higher the productivity. If there is no effect of CORR other than through farm size distribution, it should not be significant in the regression. We also want to see if CORR affects productivity through its impact on average size, so we test whether larger relative preference for larger farms increases average farm size. Since it is possible that any distortions, rather than distortions in one

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particular direction matter, we construct a CORR-squared variable, which reflects the deviation from no distortion.

The other two policy variables of interest are NRA and RRA. With regards to the NRA, the predicted coefficient is ambiguous, without controlling for non-agriculture tax, since farms of all sizes are affected the same way and NRA does not tell us about reallocation to/from agriculture. For this reason, we also include Relative Rate of Assistance, RRA, which reflects ratio of agriculture to non-agriculture NRA. Subsidizing (less taxation) of the agricultural sector should lead to reallocation of mobile factors (labor and capital) to agriculture (or slower reallocation out of it); if this means misallocation of labor to the sector with lower productivity, larger RRA should have a negative effect on labor productivity. The results of cross-section regression are presented in Table 3.5.

3.4.2 Panel

Panel specification has the advantage of allowing us to control for country fixed effects, which is especially useful in cross-country analysis, as there is a large degree of heterogeneity across countries on a number of accounts, some of which may introduce omitted variable bias by affecting both labor productivity and one or more of the explanatory variables. We cannot solve this problem completely, but we can go at least part of the way by accounting for the heterogeneity that is time-unvarying. Additionally, panel specification has the advantage of allowing us to use significantly

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more data points, which may decrease noise in the estimation and increase the robustness of results. The downside is that a lot of the relevant variation may be between countries rather than within. So, we rely on combined insights from cross-section and panel results. We drop the "5yr" superscript for notational clarity and obtain the following panel regression:

$$lny^L_{it} = \beta_o + \beta_1 g_i t + \beta_2 ln\overline{l_{it}} + \beta_x lnX_{wk,it} + \beta_z Z^{'}_{it} + \beta_{NRA} NRA_{it} + \\ \beta_{CORR} CORR_{it} v_i + \omega_t + \epsilon_{it}, \tag{7}$$

where t is World Agricultural Census decade and Z' are the variables in Z that are time-varying. Panel results are presented in Table 3.6.

3.4.3 2SLS, SUR and 3SLS

There are reasons to believe that average farm size may be endogeneous, for example, due to inheritance practices or land reforms that also affect productivity. To account for this, we instrument for it using the total country area per capita, which is assumed to be exogenous. In the first stage, we regress average farm size on the explanatory variables used in the the OLS regression and total country area per worker:

$$\overline{l_{it}} = \alpha_o + \alpha_1 g_i + \alpha_2 lnCAW_{it} + \alpha_x lnX_{it} + \alpha_z Z^{'}_{it} + \alpha_{NRA} NRA_i + \alpha_{CORR} CORR_i + \delta_i, \tag{8}$$

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where CAW_{it} is country area per capita in WCA decade t. In the second stage, we obtain the following specification:

$$lny_{it}^{L} = \beta_o + \beta_1 g_{it} + \tilde{\beta}_2 l\hat{n}l_{it} + \beta_x lnX_{it} + \beta_z Z_{it}^{'} + \beta_{NRA} NRA_{it} + \beta_{CORR} CORR_{it} + \epsilon_{it}, \tag{9}$$

where $l\hat{n}l_i$ are predicted values from the first stage. Since there may be correlation between errors of the first stage and the second stage, to increase efficiency, we also use seemingly unrelated regression (SUR) to increase efficiency of the estimation. The results of these estimations are presented in Table 3.7.

3.4.4 Variance Decomposition

Finally, since our interpretation of marginal effects may be complicated by multiple pathways through which variables on the right hand side potentially influence each other, and since we are interested in comparing the share of variation in agricultural labor productivity explained by tax policy to the Adamopoulos and Restuccia (2014) figures, we also use a variance decomposition approach. We look both at the variance explained directly by the tax policy other than though average farm size and also through average farm size. We use the following Shapley-based $R-squared$ decomposition approach:

$$M^k = R^2[y = a + \sum_{j \in S} b_j x_j + b_k x_k + e] - R^2[y = a^* + \sum_{j \in S} b_j^* x_j + e*] \tag{10}$$

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where M^k is the lower bound for the contribution to R-squared of factor k and the two terms, in order, are $R-squared$ with and without variable k, but with the other variables present. We estimate this for each of the variables of interest from the pooled OLS specification; decomposition is conducted both for output per worker R^2 and average farm size R^2.

3.5 Results

In Table 3.5, we start by regressing output per worker on average farm size. The elasticity of output per worker with respect to farm size, not conditional on other variables, is 0.68 and is highly statistically significant. Additionally, more than half the variation in output per worker is explained by average farm size. As we progressively add agricultural inputs, land quality controls and labor quality/human capital controls, the elasticity shrinks to about 0.2. In specifications (5)-(9), we limit the sample to countries with farm size GINI coefficient, nominal rate of assistance and correlation between NRA and crop-specific farm size data available. In all specifications with the reduced sample average farm size is not statistically significant, whereas the point estimate stays close to 0.2. Neither the farm size distribution inequality nor the tax policy variables appear significant. Livestock per worker and agricultural machinery per worker are positive and significant throughout. Since column (5) suggests that lack of statistical significance may be due to smaller sample size rather than additional controls, we shift attention to panel results with significantly

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more observations.

In Table 3.6, we start with a pooled OLS model. The main changes from the cross-section results are that the correlation between crop-specific farm size and crop tax is negative and significant and land per worker is now highly significant. The sign on NRA/Size Corr. is contrary to expectation, since it suggests higher subsidies for crop with larger average farm size lead to lower output per worker. In column (2), we present fixed effects regression results including only average farm size, and, again, find it to be highly significant, although with an elasticity of 0.35, about half that of cross-section result. Column (3) includes input use and land quality controls, but not GINI coefficient or the tax variables. The output elasticity estimate is similar to cross-section results and statistically significant. When we add the GINI coefficient and tax policy variables, neither these variables nor average farm size appear significant. We experiment with a more efficient random effects model (column (8)), but Hausman test suggests random effects estimator is not consistent here. The main variable that seems to be driving the change in output per worker is the use of agricultural land per worker, with an average elasticity across specifications of around 0.5. We also experimented with including RRA instead of NRA, but it did not significantly alter the results.

Since average farm size may be endogenous, in Table 3.7 we instrument for it using country area per capita. Although the Adamopoulos and Restuccia (2014) theory predicts tax policy operates though altering farm size, it is evident from farm size determinants regression in first stage that NRA/Size correlation does not affect

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average farm size; first stage also reveals that the explanatory power of country area per capita is not sufficient. This results in large standard errors on average farm size in the second stage. Consequently, we take steps to increase efficiency of estimation. First, we do not instrument for average farm size, but estimate the regression with farm size as the dependent variable simultaneously with the main regression. In regression with farm size as the dependent variable, standard errors on country area per capita decrease to the point where it has a statistically significant effect on average farm size. Additionally, agricultural land per work and livestock per worker appear to be positively correlated with farm size and machinery–negatively correlated. Neither average NRA nor correlation with average farm size significantly affect farm size. In the main regression, we again see an elasticity of output with respect to land per worker of above 0.5. Average farm size effect is negative and not statistically significant. Lastly, we combine both 2SLS and SUR approaches in the 3SLS model. The results are very similar to SUR. None of these attempts indicate tax policy operating through farm size, as neither the impact of tax policy nor the impact of average farm size on agricultural labor productivity are statistically significant. F-statistic on the instrument is around 6, below the threshold where we would be comfortable with instrument strength, but given that the effect of average farm size on output per worker is negative and close to being statistically different from zero, it is unlikely that lack of positive effect is due to weak instrument.

For alternative specifications, we separately use share of small farms and share of large farms instead of average farm size and GINI and find essentially equivalent results, in that unconditionally, the share of small farms is negatively and share

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of large farms positively correlated with agricultural output per worker (Figure 4), but controlling for inputs, land and institutional quality, the relationship completely disappears. Table 3.8 presents only the main pooled OLS and fixed-effects results of this approach. Neither share of large nor share of small farms affect output per worker. We again see a negative effects of NRA and RRA in pooled OLS but not the fixed-effects model. Agricultural land per worker and share of land irrigated are the main variables affecting output per worker in the fixed-effects specification.

Because of multiple pathways between independent variables and since we are interested in the share of variation in agricultural labor productivity explained by farm size, tax policy and other variables, we conduct R-squared decomposition for output per worker as well as for average farm size. Looking at the results in Table 3.9, we confirm findings from marginal effects estimation. We find that almost one third of total explained variation (which, in turn, accounts for 80 percent of total variation) in average farm size can be attributed to the amount of arable land per capita and almost two thirds–to agricultural input use and labor quality covariates, with tax policy and GINI of land distribution accounting for around 6%. For agricultural labor productivity variation, we find that about 10 percent of R-squared (of overall 0.9) is explained by farm size and farm size inequality and potential policy distortions, with the rest being accounted for by input quantity and quality and TFP proxies.

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3.6 Conclusion

Multiple reasons for low agricultural labor productivity in poor countries compared to rich have been proposed by economics literature. By examining determinants of international differences in agricultural labor productivity, we present evidence consistent with some explanations but not others. Motivated by the observation that larger farms generally have lower land and higher labor productivity, one explanation suggests that intrasector factor misallocation in form of lower average farm size in poor countries is directly responsible for low agricultural labor productivity in low-income countries. A particular position argued by Adamopoulos and Restuccia (2014) is that policies that distort farm size distribution towards lower mean account for as much as three quarters of the agricultural labor productivity gap between rich and poor countries, and that crop-specific price distortion favoring small farms accounts for one quarter. The reason is that larger farms are more productive, everything else equal, due to being managed by more productive managers, and misallocation of labor away from the most productive farms reduces productivity. Most specifically, we find that it is implausible that crop-specific price distortions favoring small farms affect productivity through lowering average farm size in poor countries because 1) beginning with 2000 World Census of Agriculture, crop-specific price distortions in poor countries favor larger farms and 2) we find no evidence of such distortions affecting average farm size. More broadly, despite unconditional positive relationship, we find that average farm size has no independent effect on labor productivity when controlling for input quantity and quality, suggesting larger

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farms do not systematically have higher TFP.

Another potential explanation is factor misallocation across sectors, with one reason being preferential treatment of agriculture. While we cannot rule out misallocation across sectors, we do not fine evidence that it is due to over-subsidizing of agriculture in poor countries, as analysis suggests 1) high-income countries support agriculture more, both in absolute terms and compared to other sectors and 2) neither nominal nor relative rates of assistance appear to markedly impact productivity. Additionally, some of the recent literature suggests much of the measured labor productivity difference between agriculture and non-agriculture may be due to measurement error.

This does not rule out other common explanations, including misallocation. Most of the variation in output per worker is explained by primary and intermediate input use, which is consistent with previous findings in the literature that explain agricultural labor and land productivity differences between large and small farms through differences in prices and availability of factors of production and intermediate inputs, credit access, price risk and land quality, among others. On the other hand, some recent studies (Rada and Fuglie (2018)) suggest that optimal operational size and input ratio may differ across stages of development, suggesting differential input intensity need not mean misallocation.

In sum, we find that there is little evidence of cross-country differences in agricultural labor productivity being explained by distortions of agricultural incentives (of the type considered), including nominal and relative rates of assistance, overall

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and crop-specific, and especially through affecting farm size. Instead, most of the variation is explained by differential input quantity and quality, which may or may not mean misallocation. If it does mean misallocation, especially between large and small farms, the causes of it matter. If the reason for smaller average farm size is that large farms do not grow larger because of disincentives they face or that small farms are incentivized to stay small, the implications are different than if the reason is that small farms cannot grow bigger because of credit constraint, uninsurable risk, higher input prices or other reasons preventing them from acquiring more and better quality inputs and making productivity-enhancing investments. This paper suggests need for caution in interpreting ostensible correlation between average farm size and agricultural labor productivity as evidence of innate advantages of larger farms that poor countries are unable to make use of due factor misallocation caused by policy and institutional barriers.

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Figures and Tables for Chapter 3

Table 3.1: Descriptive Statistics for Cross-Section

Variable	N	Mean	SD	Min	Max
Output/worker (1000$ 2005)	89	13.69	20.55	0.14	94.27
Avg. Farm Size (HA)	89	41.02	119.97	0.47	778.69
Farm Size Gini	49	0.62	0.17	0.25	0.94
NRA/Size Corr.	57	-0.14	0.39	-1	0.67
$NRA/SizeCorr.^2$	57	0.17	0.25	0	1
Average NRA	58	0.11	0.27	-0.26	1.4
Average RRA	55	0.03	0.32	-0.47	1.61
Livestock/wk (Cow equivalents)	89	0.09	0.16	0.00	0.98
Fertilizers/wk (Tons)	89	1.9	4.07	0.00	24.79
Ag. Machinery/wk (1000s $ 2005)	89	0.02	0.05	0.00	0.2
Ag. Land/wk (ha)	89	0.02	0.04	0.00	0.25
Land Quality	89	6.74	1.25	3.04	9.86
Percent of Agr. Land Irrigated	89	8.73	13.15	0	67.12
Agr. Land Share in Pasture	89	53.33	28.43	1.44	98.65
Fertility Rate	89	3.75	2.08	1.25	7.71
Average Life Expectancy	89	66.21	12.61	37.02	82.34
Quality of Institutions	89	0.08	0.83	-1.88	1.72
Legal Orig. UK	89	0.29	0.46	0	1
Legal Orig. French	89	0.54	0.5	0	1
Legal Orig. Socialist	89	0.08	0.27	0	1
Legal Orig. German	89	0.04	0.21	0	1
Legal Orig. Scand	89	0.04	0.21	0	1

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Table 3.2: Descriptive Statistics for Panel

Variable	N	Mean	SD	Min	Max
Output/worker (1000$ 2005)	285	14.71	19.09	0.14	116
Avg. Farm Size (HA)	285	42.24	95.76	0.47	778.69
Farm Size Gini	188	0.63	0.17	0.1	0.94
NRA/Size Corr.	186	-0.11	0.4	-1	0.98
$NRA/SizeCorr.^2$	186	0.17	0.23	0	1
Average NRA	191	0.29	0.5	-0.36	3.55
Average RRA	186	0.21	0.55	-0.65	3.44
Livestock/wk (Cow equivalents)	285	0.1	0.14	0.00	0.98
Fertilizers/wk (Tons)	285	2.82	4.87	0.00	45.43
Ag. Machinery/wk (1000s $ 2005)	285	0.03	0.05	0.00	0.55
Ag. Land/wk (ha)	285	0.02	0.04	0.00	0.25
Percent of Agr. Land Irrigated	285	9.84	13.62	0	67.12
Agr. Land Share in Pasture	285	45.9	28.27	0.6	98.65
Fertility Rate	285	3.28	1.92	1.15	8.36
Average Life Expectancy	285	68.56	9.58	34.61	82.34

Table 3.3: Correlation Between Ag. Output/wk, Avg. Farm Size and Inputs/wk (2000)

	Ag. Land/wk	Livestock/wk	Fertilizer/wk	Machinery/wk	Average Farm Size
Ag. Land/wk	1				
Livestock/wk	0.609	1			
Fertilizer/wk	0.505	0.806	1		
Machinery/wk	0.592	0.801	0.870	1	
Average Farm Size	0.775	0.638	0.614	0.656	1
Ag. Output/Wk	0.608	0.845	0.864	0.865	0.696

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Table 3.4: Variable Means by Decade and GDP/cap. Quintile

			Avg. Farm Size (HA)			
WCA Decade	1	2	3	4	5	Total
1970	2.439	7.021	17.202	108.442	51.954	39.501
1980	3.508	14.427	32.667	43.044	36.094	28.262
1990	2.615	12.816	80.241	17.363	66.864	39.083
2000	2.927	28.235	17.444	57.03	51.414	33.909
2010	6.936	87.952	41.358	137.431	688.909	217.248
Total	3.606	29.532	35.402	73.652	158.645	68.803
			GINI			
WCA Decade	1	2	3	4	5	Total
1970	0.494	0.522	0.792	0.658	0.551	0.6
1980	0.578	0.763	0.66	0.646	0.572	0.631
1990	0.593	0.688	0.653	0.574	0.604	0.621
2000	0.47	0.621	0.611	0.672	0.672	0.626
2010	0.671	0.783	0.746	0.583	0.606	0.66
Total	0.542	0.67	0.679	0.628	0.608	0.628
			NRA/Farm Size Corr.			
WCA Decade	1	2	3	4	5	Total
1970	-0.197	-0.098	0.249	0.107	-0.159	-0.069
1980	-0.498	-0.388	-0.257	-0.094	-0.157	-0.241
1990	-0.417	-0.716	0.104	-0.047	-0.038	-0.131
2000	0.02	-0.04	-0.209	0.003	-0.112	-0.067
2010	0.108	-0.068	-0.167	-0.126	-0.142	-0.111
Total	-0.166	-0.182	-0.118	-0.043	-0.117	-0.119
			NRA 5yr Average			
WCA Decade	1	2	3	4	5	Total
1970	0.001	-0.089	0.066	0.339	0.541	0.242
1980	-0.049	-0.138	0.119	0.499	0.735	0.299
1990	0.075	0.062	0.266	0.444	0.922	0.493
2000	-0.034	0.144	0.288	0.419	0.364	0.249
2010	0.038	0.023	0.091	0.232	0.219	0.149
Total	0.008	-0.005	0.171	0.383	0.542	0.28

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RRA 5yr Average

WCA Decade	1	2	3	4	5	Total
1970	-0.309	-0.282	-0.107	0.315	0.398	0.089
1980	-0.308	-0.4	-0.017	0.438	0.681	0.199
1990	-0.052	-0.051	0.156	0.342	0.887	0.422
2000	-0.176	0.058	0.201	0.349	0.342	0.18
2010	-0.087	-0.042	0.005	0.227	0.193	0.102
Total	-0.165	-0.139	0.063	0.329	0.488	0.196

Agriculture Employment Share

WCA Decade	1	2	3	4	5	Total
1970	0.794	0.558	0.302	0.251	0.15	0.352
1980	0.733	0.534	0.439	0.196	0.188	0.382
1990	0.649	0.417	0.32	0.184	0.155	0.318
2000	0.568	0.374	0.206	0.139	0.115	0.263
2010	0.556	0.247	0.235	0.077	0.071	0.219
Total	0.64	0.421	0.304	0.167	0.137	0.304

Output/wk (1000$ 2005)

WCA Decade	1	2	3	4	5	Total
1970	1.151	2.478	6.905	9.959	19.342	8.815
1980	1.259	2.063	4.868	13.403	10.934	7.039
1990	2.502	4.599	7.135	15.598	19.026	10.885
2000	3.931	11.492	15.55	24.806	28.718	18.114
2010	3.444	18.532	20.963	32.377	30.952	22.272
Total	2.54	7.943	11.197	19.342	21.859	13.548

WCA Decade	1	2	3	4	5	Total
	Nonag. Output/wk (1000$ 2005)					
1970	9.205	12.629	18.814	31.373	53.615	29.825
1980	15.111	11.058	21.353	29.756	37.14	24.475
1990	9.859	14.122	23.675	36.255	44.609	27.954
2000	7.642	18.25	29.619	33.374	51.849	30.648
2010	10.163	23.04	33.14	52.973	65.919	39.725
Total	10.179	16.066	25.656	36.54	50.414	30.519

Ratio of Nonag./Ag. Output/wk

WCA Decade	1	2	3	4	5	Total
1970	11.479	7.839	4.021	4.758	5.438	6.158
1980	16.259	6.546	7.779	3.039	7.934	7.912
1990	8.01	5.836	6.356	4.114	5.277	5.781
2000	3.717	4.849	3.413	2.837	4.136	3.801
2010	6.72	3.191	3.356	2.778	3.177	3.662
Total	8.507	5.57	5.03	3.453	5.177	5.372

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Capital Stock/wk (1000$ 2005)						
WCA Decade	1	2	3	4	5	Total
1970	0.005	0.015	0.027	0.058	0.139	0.056
1980	0.008	0.009	0.043	0.13	0.077	0.057
1990	0.008	0.015	0.05	0.068	0.177	0.076
2000	0.009	0.047	0.074	0.084	0.152	0.081
2010	0.011	0.073	0.135	0.214	0.179	0.129
Total	0.009	0.032	0.067	0.108	0.143	0.079
Livestock/wk (Cow equivalents)						
WCA Decade	1	2	3	4	5	Total
1970	0.005	0.009	0.066	0.048	0.149	0.062
1980	0.008	0.014	0.048	0.093	0.123	0.065
1990	0.008	0.051	0.062	0.144	0.127	0.085
2000	0.03	0.049	0.117	0.129	0.215	0.117
2010	0.026	0.07	0.104	0.232	0.194	0.131
Total	0.016	0.038	0.08	0.126	0.164	0.093
Ag. Machinery/wk (1000s $ 2005)						
WCA Decade	1	2	3	4	5	Total
1970	0	0	0.005	0.007	0.04	0.013
1980	0	0.001	0.006	0.031	0.031	0.016
1990	0.001	0.002	0.005	0.025	0.069	0.026
2000	0.001	0.016	0.03	0.03	0.047	0.027
2010	0.001	0.018	0.054	0.102	0.074	0.053
Total	0.001	0.008	0.021	0.038	0.051	0.026
Fertilizer/wk (Tons)						
WCA Decade	1	2	3	4	5	Total
1970	0.036	0.1	0.731	1.075	4.086	1.408
1980	0.125	0.288	0.967	4.015	2.931	1.806
1990	0.233	0.272	0.818	3.493	6.377	2.697
2000	0.185	1.411	2.883	2.177	5.606	2.708
2010	0.22	2.443	5.439	4.901	6.623	4.284
Total	0.155	0.911	2.234	3.005	5.061	2.543

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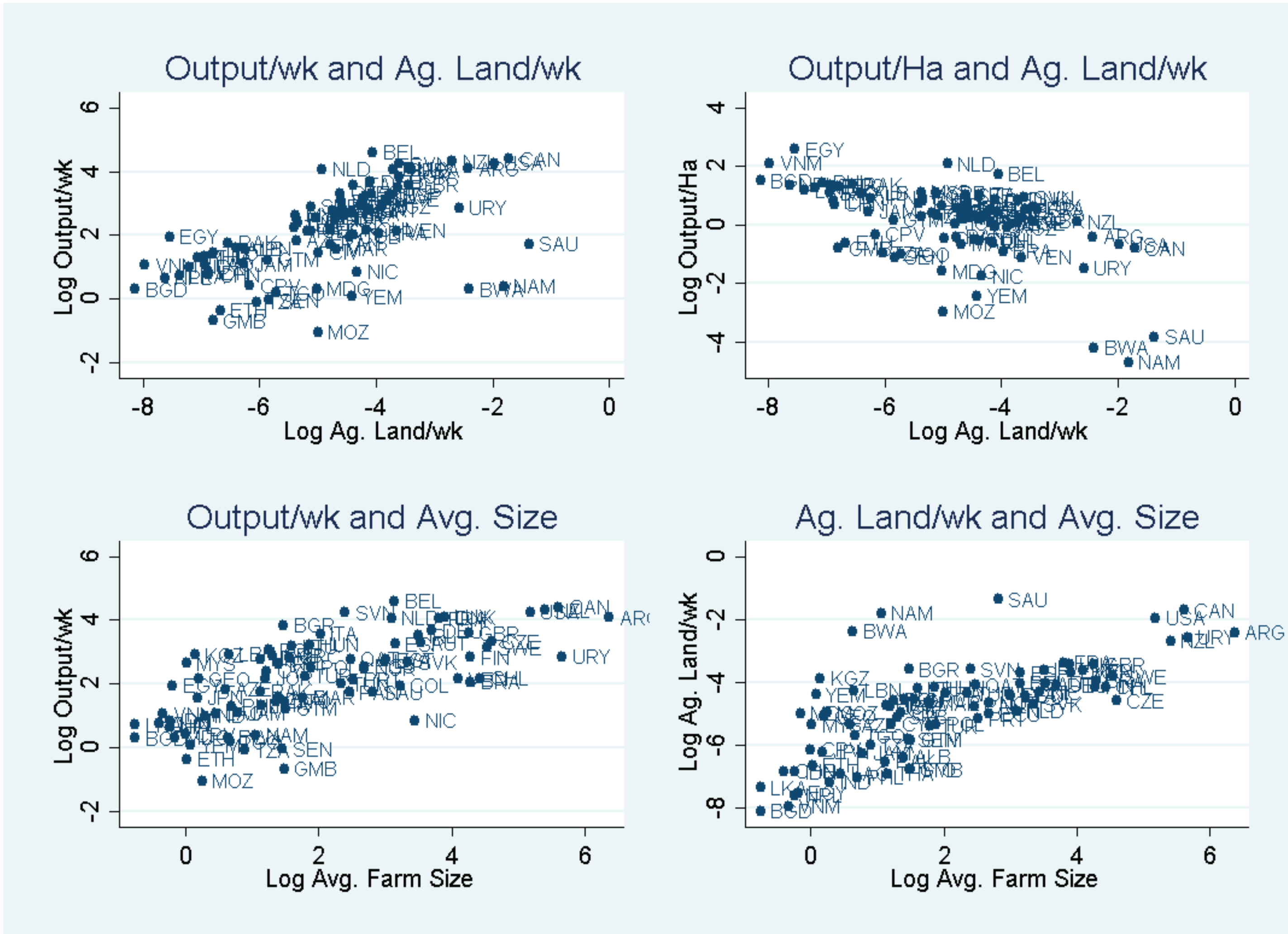

Figure 3.1: Output/wk, Output/Ha, Land/wk and Avg. Size (2000)

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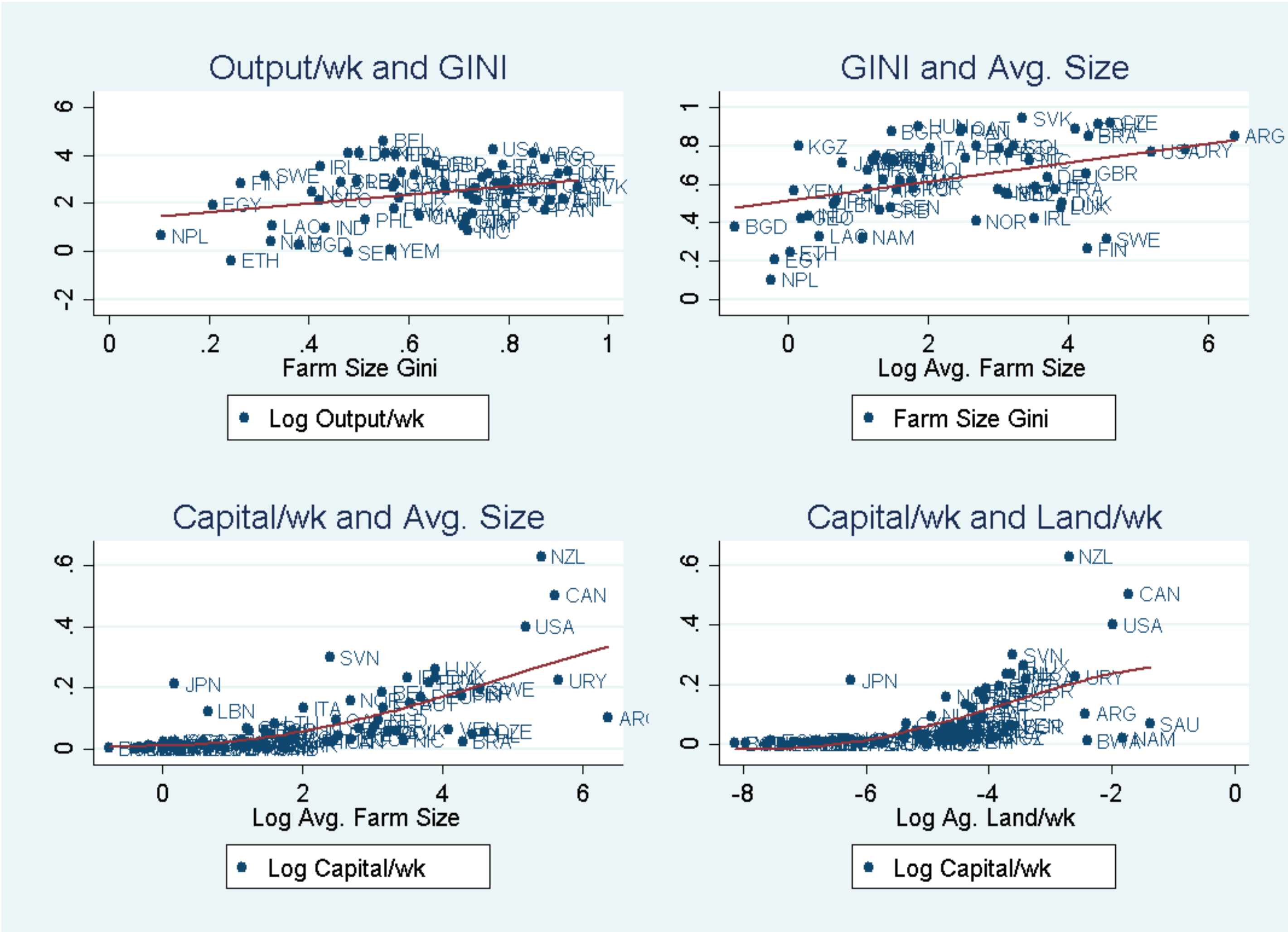

Figure 3.2: Output/wk, GINI, Avg. Size, Capital/wk and Land/wk (2000)

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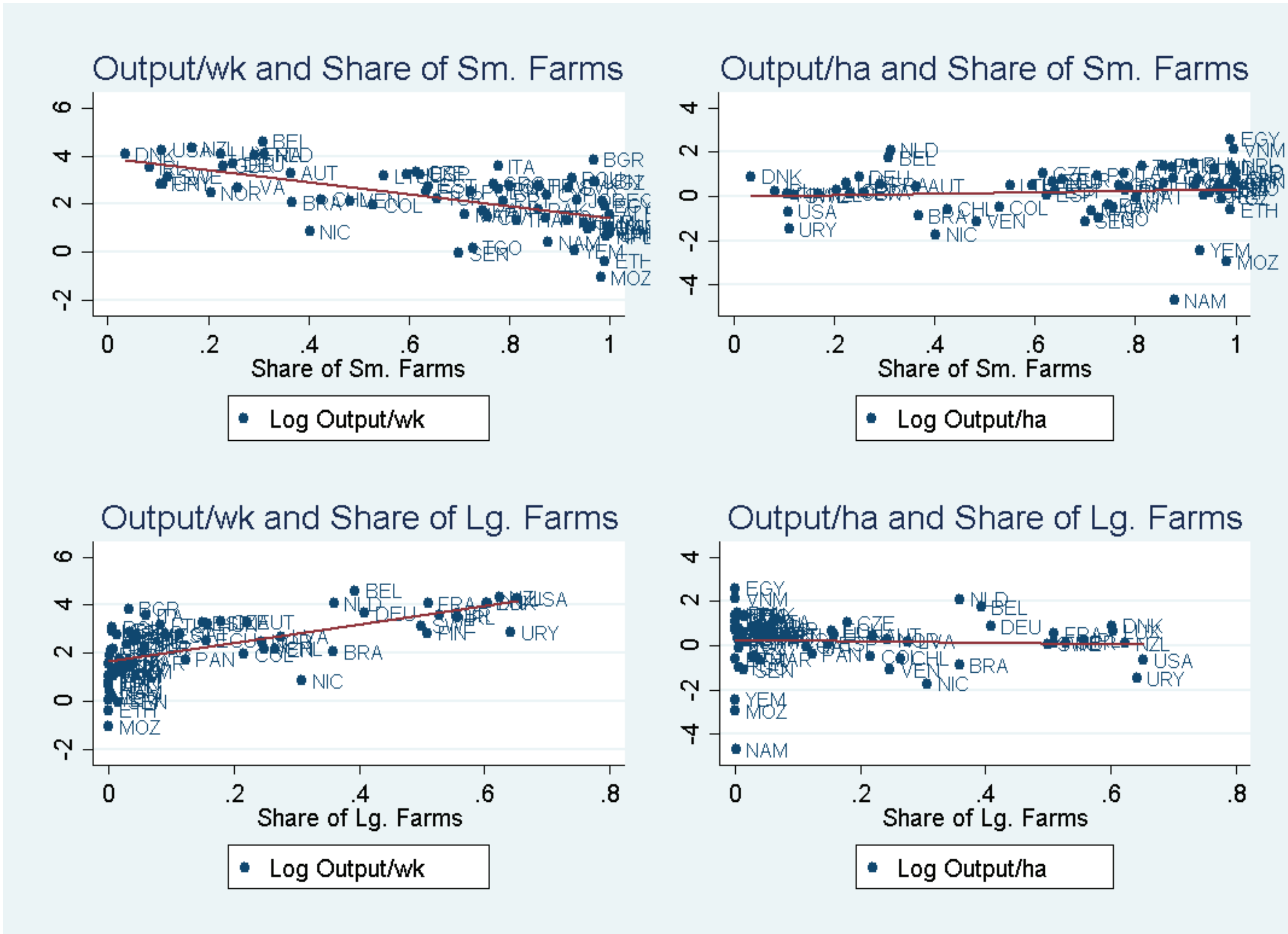

Figure 3.3: Output/wk, Output/Ha, Share of Small (<5 Ha) and Large (>20 Ha) Farms (2000)

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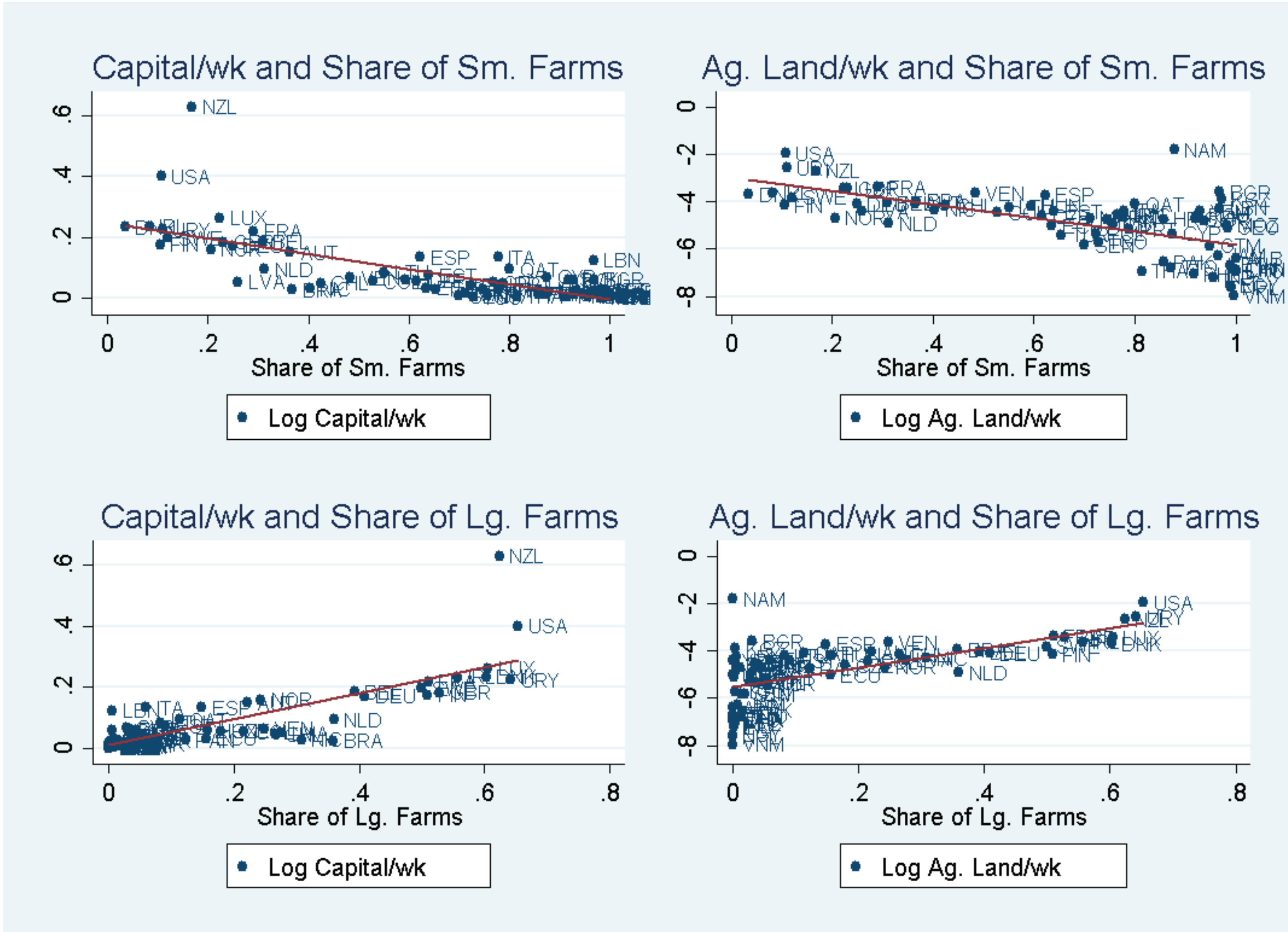

Figure 3.4: Capital/wk, Land/wk, Share of Small (<5 Ha) and Large (>20 Ha) Farms (2000)

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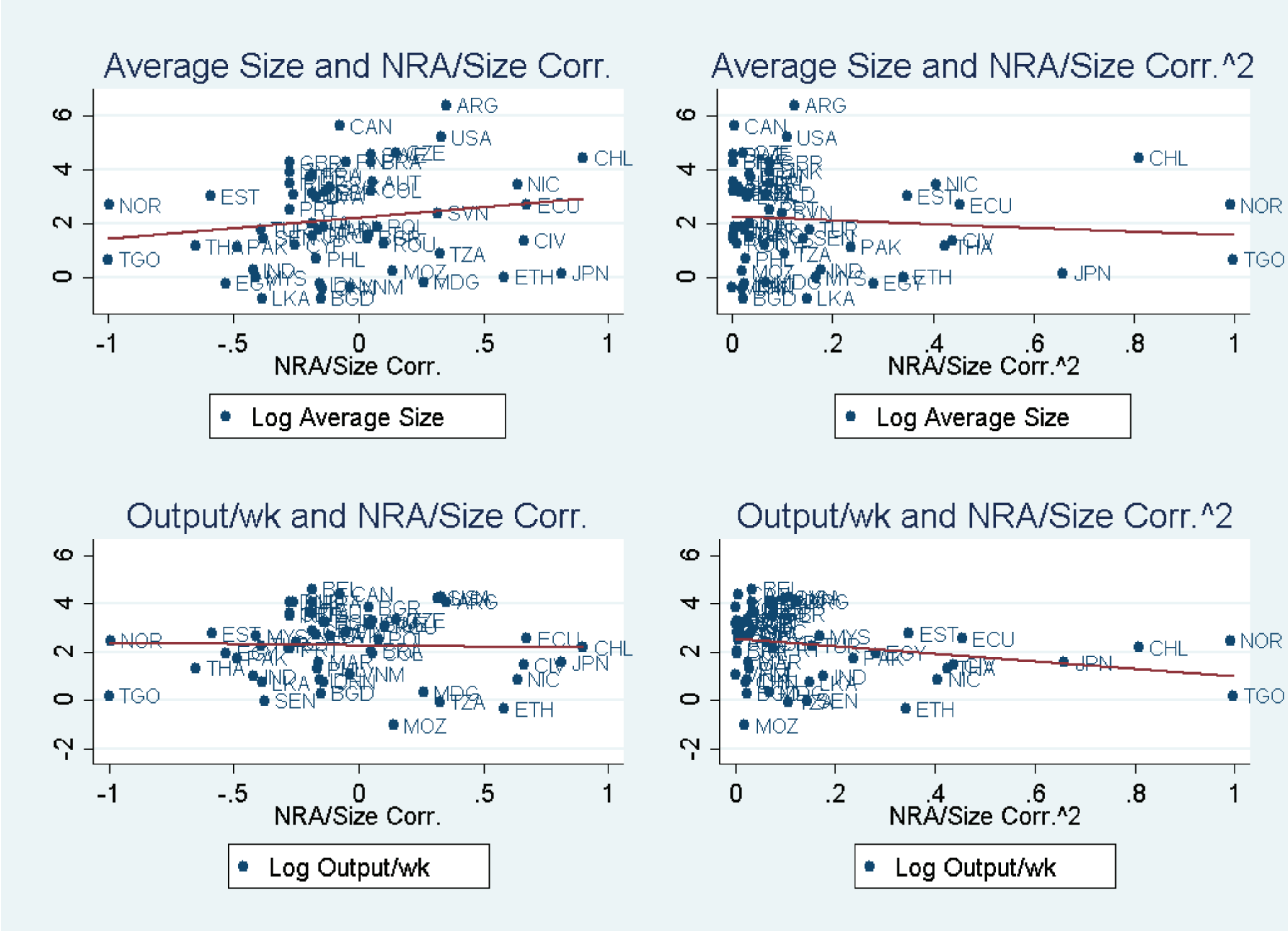

Figure 3.5: Avg. Size and Size/NRA Correlation (2000)

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Table 3.5: Cross-Section Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Avg. Farm Size	0.681***	0.155	0.194*	0.219*	0.205	0.209	0.192	0.192	0.201
	0.055	0.082	0.090	0.098	0.148	0.180	0.197	0.192	0.185
Livestock/wk		0.417***	0.379***	0.324**	0.454**	0.453**	0.459**	0.466**	0.478**
		0.087	0.097	0.105	0.139	0.142	0.151	0.150	0.152
Fertilizers/wk		0.071	0.017	0.011	0.037	0.035	0.042	0.051	0.063
		0.054	0.064	0.065	0.099	0.112	0.121	0.122	0.119
Ag. Machinery/wk		0.222***	0.161***	0.189**	0.261*	0.261*	0.252*	0.248	0.250*
		0.041	0.041	0.062	0.112	0.115	0.121	0.121	0.112
Ag. Land/wk		-0.165***	0.049	0.047	-0.006	-0.004	0.005	-0.004	-0.033
		0.045	0.095	0.102	0.178	0.193	0.201	0.201	0.206
Land Quality			0.128*	0.084	0.155	0.158	0.139	0.131	0.122
			0.051	0.054	0.076	0.087	0.102	0.096	0.097
Percent Irrigated			0.015	0.012	0.018*	0.019*	0.018	0.018	0.018
			0.009	0.010	0.008	0.009	0.010	0.010	0.010
Share in Pasture			-0.005	-0.006	-0.002	-0.002	-0.002	-0.002	-0.001
			0.003	0.003	0.004	0.004	0.004	0.005	0.005
Farm Size Gini						-0.055	-0.041	-0.041	-0.106
						0.763	0.795	0.784	0.746
NRA/Size Corr.							-0.024	-0.045	
							0.279	0.279	
Average NRA							-0.130		
							0.334		
Average RRA								-0.232	-0.187
								0.264	0.261
$NRA/SizeCorr.^2$									-0.483
									0.526
R-squared	0.558	0.869	0.885	0.890	0.941	0.941	0.941	0.942	0.944
N	89	89	89	89	44	44	44	44	44

Institutions, Legal Origin, Fertility, Life Expectancy Included (4)-(9)

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

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Table 3.6: Panel Results

	(1) OLS	(2) FE	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE	(8) RE
Avg. Farm Size	-0.076	0.350*	0.242*	0.113	0.045	0.037	0.033	-0.063
	0.044	0.156	0.120	0.058	0.101	0.089	0.088	0.056
Farm Size Gini	0.155				-0.340	-0.329	-0.304	-0.004
	0.269				0.340	0.517	0.517	0.320
NRA/Size Corr.	-0.353***			-0.019		0.032		
	0.102			0.075		0.125		
Average NRA	-0.174*			-0.109		-0.080	-0.086	-0.138
	0.082			0.087		0.095	0.097	0.088
Livestock/wk	0.403***		-0.188	0.067	0.108	0.061	0.069	0.273*
	0.059		0.119	0.114	0.130	0.147	0.148	0.116
Fertilizers/wk	0.140*		0.095*	0.075	0.060	0.034	0.033	0.111
	0.058		0.044	0.060	0.067	0.075	0.075	0.065
Ag. Machinery/wk	-0.030		0.147	-0.037	-0.034	-0.097	-0.106	-0.027
	0.042		0.109	0.071	0.092	0.096	0.100	0.072
Ag. Land/wk	0.353***		0.557**	0.526***	0.522**	0.627***	0.628***	0.455***
	0.080		0.174	0.119	0.163	0.170	0.163	0.119
Percent Irrigated	-0.000		0.015	0.012	0.017	0.019**	0.020**	0.004
	0.003		0.009	0.009	0.010	0.007	0.007	0.005
Share in Pasture	-0.002		-0.003	-0.010	-0.010	-0.011	-0.011	-0.004*
	0.002		0.007	0.006	0.008	0.006	0.006	0.002
$NRA/SizeCorr.^2$							-0.157	-0.252
							0.198	0.154
R-squared	0.902	0.073	0.482	0.709	0.639	0.713	0.715	
Observations	147	285	285	186	188	147	147	147

Standard errors clustered at country level; Hausman P(RE vs FE)<0.01; Human capital controls included, except (2)
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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Table 3.7: 2SLS, SUR, 3SLS

	2SLS		SUR		3SLS	
	1st Stage	2nd Stage	Farm Size	Main	Farm Size	Main
Farm Size Gini	-0.5	-2.125	-0.472	-0.383	-0.5	-2.125
	0.723	1.745	0.344	0.399	0.344	1.35
NRA/Size Corr.	-0.035	-0.072	-0.035	0.029	-0.035	-0.072
	0.066	0.305	0.079	0.093	0.079	0.236
Country Area/Capita	0.523		0.602*		0.523*	
	0.281		0.244		0.245	
Average NRA	0.025	0.061	0.021	-0.075	0.025	0.061
	0.082	0.272	0.069	0.08	0.069	0.211
Livestock/wk	0.364*	0.909	0.370***	0.086	0.364***	0.909
	0.156	0.671	0.096	0.115	0.096	0.519
Fertilizers/wk	0	-0.011	0.002	0.033	0	-0.011
	0.071	0.161	0.043	0.049	0.043	0.125
Ag. Machinery/wk	-0.325*	-0.923	-0.326***	-0.122	-0.325***	-0.923
	0.131	0.62	0.074	0.092	0.074	0.48
Ag. Land/wk	0.311	1.692*	0.297*	0.658***	0.311*	1.692*
	0.205	0.858	0.133	0.153	0.133	0.664
Percent Irrigated	0.001	0.02	0.001	0.019*	0.001	0.02
	0.01	0.028	0.007	0.009	0.007	0.022
Share in Pasture	-0.006	-0.021	-0.007	-0.011*	-0.006	-0.021
	0.009	0.018	0.005	0.005	0.005	0.014
Avg. Farm Size		-2.601		-0.042		-2.601
		1.769		0.096		1.369
R-squared	0.494			0.966		0.793
Observations	147	147	147	147	147	147

Standard errors clustered at country level

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

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Table 3.8: Panel Results: Share of Small/Large Farms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	OLS	OLS	OLS	FE	FE	FE	FE
Share of Sm. Farms	0.150		0.090		0.646		0.651	
	0.243		0.260		0.527		0.548	
NRA/Size Corr.	-0.259	-0.239			-0.061	-0.030		
	0.139	0.144			0.102	0.124		
Average NRA	-0.300*	-0.300*			-0.079	-0.051		
	0.117	0.120			0.081	0.091		
Livestock/wk	0.404***	0.426***	0.408***	0.423***	0.090	0.095	0.152	0.153
	0.095	0.095	0.104	0.102	0.132	0.153	0.138	0.159
Fertilizers/wk	0.207*	0.201*	0.211*	0.217*	-0.019	0.011	-0.044	-0.012
	0.077	0.080	0.082	0.083	0.066	0.064	0.070	0.064
Ag. Machinery/wk	0.051	0.042	0.033	0.026	-0.120	-0.097	-0.153	-0.126
	0.068	0.069	0.078	0.076	0.081	0.082	0.082	0.083
Ag. Land/wk	0.151	0.179	0.144	0.179	0.569**	0.521*	0.503**	0.442*
	0.098	0.098	0.105	0.103	0.170	0.197	0.169	0.201
Percent Irrigated	0.004	0.004	0.004	0.004	0.020**	0.022**	0.020**	0.022***
	0.005	0.005	0.005	0.005	0.006	0.006	0.006	0.006
Share in Pasture	-0.002	-0.002	-0.003	-0.004	-0.009	-0.010	-0.009	-0.010
	0.002	0.002	0.002	0.002	0.007	0.007	0.007	0.007
Share of Lg. Farms		-0.378		-0.413		0.001		0.099
		0.249		0.240		0.477		0.519
$NRA/SizeCorr.^2$			-0.264	-0.261			0.122	0.072
			0.224	0.214			0.170	0.169
Average RRA			-0.251*	-0.254*			-0.073	-0.032
			0.114	0.114			0.080	0.085
R-squared	0.904	0.903	0.898	0.900	0.743	0.737	0.734	0.727
Observations	146	145	143	143	146	145	143	143

Standard errors clustered at country level

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

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Table 3.9: Variance Decomposition. Pooled OLS

	Output/Worker			Average Farm Size		
	Coeff.	Std.Err.	Shapley %R2			
Avg. Farm Size	-0.075*	0.044	7.602			
Farm Size Gini	0.1546	0.269	1.526	0.5863	0.533	2.842
NRA/Size Corr.	-0.352***	0.102	1.024	0.3379*	0.203	2.239
Average NRA	-0.173**	0.082	1.313	0.0094	0.163	0.891
Livestock/wk	0.4034***	0.059	21.985	-0.191*	0.115	10.269
Fertilizers/wk	0.1403**	0.058	15.514	0.2880**	0.111	10.320
Ag. Machinery/wk	-0.029	0.042	12.748	-0.121	0.081	10.497
Ag. Land/wk	0.3528***	0.080	15.590	0.7791***	0.152	30.167
Percent Irrigated	-0.000	0.003	2.697	-0.015**	0.006	10.968
Share in Pasture	-0.002	0.002	1.295	-0.002	0.004	5.208
Fertility	-0.065	0.062	8.206	0.3080**	0.119	3.968
Life Expectancy	0.0059	0.012	10.502	0.0699***	0.022	5.671
Country Area/Cap.				0.0329**	0.015	6.961
Intercept	11.923	1.090		-0.793	2.149	
Observations	147			147		
Overall R2	0.9015			0.7788		
Root MSE	0.3809			0.7434		
F-stat. Model	102.20***			39.317***		
Log Likelihood	-59.92			-158.1		

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A Note on Imputed Values

We attempted to keep imputations to the minimum. Some imputations did take place, however, due to data availability limitations.

Share of Land: For Ireland only, share of land irrigated in all decades was missing in all examined sources and was taken as the average of the oldest 12 EU members.

Agricultural Labor: For countries for which agricultural labor was not present in FAO data until 1980, we used the ratio of agricultural labor to population in 1980 and multiplied by the population in the given year ($L_{i,t} = (L_{i,1980}/Pop_{i,1980})/Pop_{i,t}$). This only affects a small number of countries in the WCA 1970 decade, and should not significantly affect the results.

Average Farm Size per Crop and NRA Correlation: As mentioned before, the strategy was to normalize average farm size per crop in the United States and in the world, and combine them for maximum coverage. If there are reasons no-tax farm size by crop ordering differs between the United States and world average, this may not be an ideal strategy to use. Perhaps predicting farm size by crop in a given country based on crop-specific farm size regression would be a better strategy. We have not attempted this.

Average 5-year NRA: In a few cases we moved the 5-year period of calculation by 1-5 years due to unavailability of the most recent NRA data for some countries.

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