THE IMPACT OF CROSS-BORDER FLOWS ON MARKETS FOR LABOR, HIGHER EDUCATION, AND GOODS AND SERVICES

by

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to my parents
for all their sacrifices on my behalf

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CHAPTER I

Introduction

This dissertation consists of three distinct essays on the determinants of cross-border flows, in the form of immigrants and foreign aid, and the impact of those flows on receiving markets. Immigrant and foreign aid destinations are rarely if ever random with respect to outcomes of interest in labor and goods markets, such as wages and GDP growth. As a result, identifying the economic effect of these flows upon their arrival often proves difficult. Through theoretical models and the econometric strategies that those models motivate, I present evidence across these three essays on the factors that initiate global movements of immigrants and foreign aid. The third essay in particular focuses on the nature of such self-selection and the role that cross-border educational quality differences and informational asymmetries play in immigrant location choices. The first two essays, meanwhile, utilize particular flow determinants and the quasi-experimental variation they generate to then examine the impact of the flows on the markets they enter, finding disparate results across studies and evidence of market adjustment to the inflows.

In the first essay, I investigate the impact of immigration on market prices and how natives respond by examining how inflows of immigrant students and immigrant labor affect the postsecondary enrollment of natives. Existing studies have focused on the effect of increased immigrant demand for schooling on native enrollment, omitting the effect that changes in immigrant labor supply also have on prices relevant to native enrollment decisions. I propose, in a unified framework, that immigration-induced price movements in both education and labor markets that change the private return to higher education are mechanisms that can motivate native enrollment responses. Using U.S. Census microdata from 1970 to 2000, I find that a 1 percent increase in relatively unskilled immigrant labor raises the rate of native college enrollment by 0.33 percent, while a 1 percent increase in immigrant college students does not significantly lower enrollment. The positive, crowd-in effect of immigrant labor inflows is driven primarily by natives ages 18-24, consistent with younger natives having college demand that is more sensitive to returns than the demand of older natives. The results imply that the rise in the average college enrollment rate of young natives between 1970 and 2000 would have been 18 percentage points higher if the skill composition of immigrant labor inflows had remained constant over this period. With the identification of a crowd-in effect and, contrary to prior studies, the lack of a significant crowd-out effect, these findings are suggestive of college demand that is fairly wage-sensitive and college slots that are flexibly supplied over a decadal time horizon.

The second essay, which is co-authored with Dean Yang, explores the impact of natural disasters on foreign aid inflows. We utilize this variation to instrument for aid and estimate its effect on economic growth. Because using a country's own disaster exposure as an instrument for aid inflows violates exogeneity assumptions, we instead use the disaster exposure of countries' "aid neighbors," who are defined to be countries' competitors for aid from donors. We find evidence that own disaster exposure increases countries; aid receipts, while aid neighbor disaster exposure can decrease or increase aid receipts, varying by the disaster type. In second stage growth regressions using aid neighbor droughts as an instrument for aid, we show that an inflow of aid equal to 1 percent of GDP increases recipient per capita GDP growth by 1.2-1.7 percentage points in the short- to medium-run within three years. The mechanism for this positive growth effect is increased household consumption, while overall physical capital investment is actually crowded out. We find no effect of aid on proxies for human capital investment and factor productivity, nor do we observe any direct impact of aid on long-term growth. Our results are consistent with the strand of this literature that has found an unconditional, positive growth effect of aid, and yet also provide possible explanation for studies that have not detected any long-run aid-growth effects.

The final essay focuses on the nature of immigrant self-selection itself by investigating the migration decisions of highly-skilled individuals given cross-country differences in educational quality. In a Roy model framework, I examine the nature of individuals' jointly determined decisions of educational and employment locations. The model shows how differences in educational quality between countries, via influences on the return to skill, can affect migratory patterns when individuals are trying to maximize their expected wages. The model then examines how the presence of informational asymmetries between countries may alter such migratory flows. Using U.S. Census microdata as well as proxy data on worldwide college quality and the extent of information flows across borders, I test the predictions of the model. I find no evidence that college quality or informational asymmetries significantly influence the share of high-skilled immigrants acquiring college education in the United States. Despite potential measurement error in the proxy measures, I interpret this finding as evidence against the model's explanation for their influence in migrant self-selection.

CHAPTER II

Does Immigration Crowd Natives Into or Out of Higher Education?

2.1 Introduction

Over the past several decades, the United States has experienced some of its largest immigrant inflows since the Great Depression, with over 26 million legal immigrants admitted into the country from 1970 to 2005 (United States 2006). This higher level of immigration has generated significant academic and public policy discussion on the effects that immigrant inflows have on receiving markets and natives. In particular, much research and debate has centered around the impact of increased immigration on education and labor markets. Education-related studies have focused on inflows of immigrant students and the extent to which their increased demand for schooling displaces natives from educational opportunities. Meanwhile, the labor literature has primarily examined, with mixed findings, the impact of increased immigrant labor on the wages of both similarly-skilled and dissimilarly-skilled natives. The lack of consensus amongst the wage studies has helped generate a burgeoning line of research examining the extent to which natives respond endogenously to immigration. Studies in this area have investigated whether in response to immigration, natives relocate (Card and DiNardo 2000, Card 2001, 2005), specialize in occupations and tasks for which they have a comparative advantage (Peri and Sparber 2007), and increase their labor supply (Cortes and Tessada 2008), to name a few examples.

However, there has not been any research that examines, in a unified framework of the labor and education markets, the extent to which skill level itself is another margin on which natives adjust when facing immigration. This paper fills that gap, adding to the endogenous native response literature by investigating whether increased immigration flows affect native postsecondary enrollment decisions. I model price movements in labor and education markets that change the return to higher education as the mechanisms for this native response. Marginal benefits of higher education can be thought of as the skilled wage relative to the unskilled wage, while marginal costs of higher education can be thought of as tuition and fees and the opportunity cost (the unskilled wage).[1] In this framework, relatively unskilled immigrant labor inflows could raise the net benefits of college enrollment for natives, while inflows of immigrant students could lower net benefits. This heterogeneity in immigrant inflows and their differential impact on net benefits generates distinct predictions for effects on native college enrollment. Relatively unskilled labor immigration should increase or "crowd-in" native enrollment, while student immigration should decrease or "crowd-out" native enrollment. Thus, contrary to many existing studies, this paper highlights and estimates a positive, crowd-in effect of immigration on native education.

To investigate immigration and its effects on native college enrollment, I first construct a dual-market, supply-demand model. The model forms clear predictions on the signs and magnitudes of the reduced-form crowding effects, highlights several aspects of their underlying structural interpretation, and serves as a guide in determining the empirical strategy for estimation.

[1] It should be noted, however, that resources per student may also vary across institutions. Thus, focusing on tuition and fees alone as the marginal costs implicitly holds school quality constant or else discounts its influence on higher education demand.

Using U.S. decennial census microdata from 1970 to 2000, I then estimate the causal impact of immigrant inflows into local markets on the college enrollment rate of natives in those areas. Because immigrants' choice of entry into the college or labor market is likely affected by unobserved labor demand and college supply movements, ordinary least squares (OLS) estimation of the effects of immigrant labor supply and college demand shocks on native college enrollment will tend to be biased. The sign of the bias depends on the correlation of the given immigrant inflow with the unobserved market shock. For instance, if unskilled immigrant labor tends to locate in areas where labor demand for them is increasing, then OLS estimates of crowd-in will be downward biased. Meanwhile, if immigrant students tend to locate in areas where there was a positive college supply shock, OLS estimates of crowd-out will be biased upward.[2] To isolate the exogenous component of foreign-born inflows, I utilize a logit model of immigrant college demand based on characteristics such as age and gender, combined with two-stage least squares estimation that exploits geographic variation in historical immigrant enclaves as instruments. The historical enclave instrument is motivated by the idea that existing immigrant networks are an important determinant of the location choices of prospective immigrants (e.g., Bartel 1989, Card 2001, Munshi 2003, Cortes 2008).

Employing that estimation strategy, I find that a 1 percent increase in relatively unskilled immigrant labor raises the rate of native college enrollment by 0.33 percent, while a 1 percent increase in immigrant college students does not significantly lower enrollment. The positive, crowd-in effect is driven primarily by natives ages 18-24, consistent with younger natives having college demand that is more sensitive to returns than the demand of older natives. Additionally, these results remain robust

[2] A more detailed discussion of the potential biases in estimating crowd-in and crowd-out parameters via OLS occurs in section 5 of the paper.

across various sensitivity analyses and other broad comparisons with the predictions of the model, including explorations of native response heterogeneity as well as measurement error and omitted variable biases that could affect the baseline estimates.

The statistically significant crowd-in effect can be used to generate counterfactual estimates of native college enrollment over the sample period. I find that the average college enrollment rate of young natives between 1970 and 2000 would have been 18 percentage points higher if the skill composition of immigrant labor inflows had remained constant over this period.

Additionally, I examine the role that price elasticities of native college demand and other structural parameters play in the crowding estimates. I show that significant crowd-in coupled with a lack of crowd-out is suggestive of fairly wage-sensitive college demand as well as a high elasticity of college supply, given the long time horizon of analysis with the decadal frequency of the data.

These findings provide indirect evidence of market price effects of immigration on natives, which are of value given the mixed direct evidence in the labor and education literatures. The results additionally provide a caveat for any such direct measurement that stratifies skill levels, given that natives endogenously adjust skill in response to immigration. Meanwhile, the focus in education studies on immigrant student inflows to evaluate whether immigration affects native college enrollment while ignoring the effect of immigrant labor inflows results in the omission of an important native response.

The remainder of the paper is organized as follows: section 2 briefly discusses related literature. Section 3 explains the conceptual framework, while section 4 describes the data. Section 5 discusses the empirical strategy, while section 6 outlines the main results. Section 7 describes sensitivity analyses, and section 8 discusses implications of the results. Finally, section 9 concludes.

2.2 Related Literature

This paper relates to several labor and education market studies that have investigated, with mixed findings, the extent to which immigration affects market prices that determine the net benefits of college enrollment or educational attainment, more generally. By simultaneously analyzing the effects of immigration in both labor and education markets, the unified framework of this study adds to the existing literature in each of those areas.[3]

Regarding labor market studies, there has been much academic debate on the sign and magnitude of the mean impact that immigrant labor inflows have on the wages of natives. Research has ranged from finding no wage effects (e.g., Card 1990), to negative wage effects (e.g., Borjas 2003), and even small positive effects on average due to a lack of substitutability between natives and immigrants (e.g., Ottaviano and Peri 2007).

These mixed labor study findings have helped spawn several papers in a growing literature that examines endogenous native responses to immigrant inflows, which would have implications for the aforementioned wage effects.[4] While notably less work has been done in this endogenous-response area than on the wage effects themselves, the existing work has often focused on native responses unrelated to education. Both Card and DiNardo (2000) and Card (2001, 2005) examine whether immigrant

[3]This paper is not unique with regard to linking labor and education markets in order to study their joint role in some outcome. For instance, Fortin (2006) provides a unified econometric framework between these markets to examine how higher education policies that affect equilibrium college enrollment impact the relative supply of skilled labor and, as a consequence, the college wage premium. However, unlike existing studies, this paper uses such a joint framework to highlight the existence of potential crowd-in effects of immigration on native college enrollment and estimate such crowd-in.

[4]Immigration analyses that, more generally, highlight general equilibrium effects of immigration on natives are also a recently expanding area of research. Lewis (2005) investigates the extent to which the production technologies of firms respond to immigration-driven changes in the local skill mix of labor. Cortes (2008) explores how immigration affects the prices of non-traded goods to determine the net effect on natives' purchasing power, while Ortega and Peri (2009) discuss the role and speed of capital adjustment in mitigating the labor market impacts of immigration.

inflows cause natives to relocate geographically. Peri and Sparber (2007), alternatively, look at how increased immigration may lead natives to specialize in jobs that require knowledge of local networks, rules, customs, and language, where they tend to have a comparative advantage over immigrants. Federman, Harrington, and Krynski (2006) similarly examine an occupation response, finding that Vietnamese manicurists displace natives from the profession by deterring entry. Cortes and Tessada (2009) investigate how low-skilled immigration may increase high-skilled female native labor supply by lowering the price of child care, while Furtado and Hock (2008) extend this analysis to explore how such low-skilled immigration reduces the tradeoff between fertility and employment for high-skilled female natives.

With regard to such labor research, this paper contributes to the literature by indirectly examining the extent of wage effects from immigrant inflows, as well as by exploring native adjustment on the skill margin as an additional endogenous native response to immigration. Eberhard (2009), written independently of and simultaneously as this paper, also explores such an endogenous native skill response, incorporating it into a general equilibrium model. Taking this response into account, he finds the overall effect of immigration on native earnings and welfare to be positive, despite a small negative effect on low-skilled natives.

Regarding studies on the education market, the focus has been on the extent of a negative, displacement effect of immigration on native schooling due to increased demand for education by immigrant students, ignoring any potential effect of immigrant labor on native schooling. This literature has effectively explored the extent of tuition/fee effects (conditional on educational quality) of higher levels of immigrant schooling demand. In the high school and college contexts, respectively, Betts (1998) and Hoxby (1998) examine whether immigrants crowd out disadvantaged natives from educational attainment. Borjas (2004) investigates the extent of native crowd-out by immigrants from certain fields at the graduate school level. Gould, Lavy, and Paserman (2004) explore whether there are long-run, adverse achievement effects of immigration on natives, while Betts and Fairlie (2003) examine whether immigration induces native exodus from public schools into private schools. Neymotin (2009) researches whether immigration into California and Texas affects the SAT scores and college application portfolio of natives in those states.

This study adds to the education literature and research on tuition/fee effects due to immigrant inflows by examining the extent of such crowd-out in a more comprehensive framework.[5] Additionally, because inflows of immigrant labor and immigrant students may be correlated, omitting the former variable from native education displacement studies may bias crowd-out estimates in an unknown direction, an issue that the current study thus addresses.

Finally, by indirectly examining the sensitivity of native college demand to changes in the relative wage of unskilled labor and college tuition/fees, this paper also relates to previous work on the impact of labor and higher education market conditions on educational attainment. Neumark and Wascher (1995) find that increases in the minimum wage decrease school enrollment amongst teenagers, while Goldin and Katz (1997) find lower levels of high school completion in high-manufacturing areas, where they argue that the return to a high school degree is lower. Black, McKinnish and Sanders (2005) examine the impact of the coal boom and bust of the 1970s and 1980s on the relative wages of high school dropouts, finding that a long-term 10 percent increase in the earnings of high school dropouts decreases high school enrollment by

[5] The study thus also adds more generally to the literature that examines the impact on educational attainment of other sources of variation in education demand like cohort-specific demographic shocks (e.g., Bound and Turner 2007).

up to 5-7 percent.[6]

Regarding the direct cost of education, Kane (1999) finds that youth appear to be more sensitive in their college enrollment and completion decisions to college costs than to its wage benefits. Meanwhile, Dynarski (2003) estimates large effects of increases in student aid on college attendance and completion, while Bound and Turner (2002) find a positive net effect of military service and educational cost subsidies from the G.I. Bill on the college attainment of World War II veterans.

2.3 Conceptual Framework

I use a simple dual-market, supply-demand framework to model the impact of heterogenous immigrant inflows on native college enrollment. Native crowd-in and crowd-out from immigration occurs in the static model via the interaction of the labor and higher education markets and movements in prices that affect native skill choice. The model offers testable predictions on the sign and magnitude of the crowding coefficients, as well as aids in the interpretation of these reduced-form estimates in terms of underlying structural parameters. The theoretical framework also provides guidance on an appropriate estimation strategy for examining such crowd-in and crowd-out, as well as identifies potential sources of bias in estimation. I begin with a graphical presentation of the model which captures much of its basic intuition before then moving on to a more formal version.

2.3.1 Setup and Assumptions

I assume that the geographic boundary of the local labor and higher education (college) markets is a state.[7] I focus on the impact of immigration into each of

[6] Some work in this area has also focused on employment rather than wage effects. Duncan (1965), Rumberger (1983), and Rees and Mocan (1997) all find evidence that increased unemployment reduces high school dropout rates, thereby increasing educational attainment. The current study can be interpreted as examining the impact on educational attainment of relative wage changes, conditional on employment levels.

[7] As Bound et al. (2004) discuss, because funding decisions at public institutions occur primarily at the state level, there is support for usage of the state as the appropriate geographic boundary of these markets. Washington, D.C.

these two markets for a given state,[8] nevertheless allowing for the existence of other states in the model and a corresponding role for out-of-state migration by natives or immigrants. All input and product markets are assumed to be competitive.

Individuals are considered skilled if they have at least some college education and are unskilled otherwise. In the college market, both the supply of and demand for college enrollment are potentially sensitive to changes in the relative unskilled wage and tuition/fees.[9][10] Production of enrollment slots by college institutions maximizing their net benefit of student enrollment is accomplished using constant returns to scale technologies and non-labor inputs (e.g., land, capital),[11] where student tuition/fees are received for each slot.[12] Meanwhile, on the enrollment demand side, natives and immigrants of some latent ability decide whether to enroll in order to acquire skill to maximize their utility, concave in consumption, which is itself a partial function of either skilled wages net of college tuition/fees or else unskilled wages.[13] It is assumed that natives acquire skill domestically, while immigrants may either acquire skill in the U.S. or in their home country before migrating.[14] Also, because college enrollment determines skill level, there is a direct link between changes in equilibrium

will be excluded as it is an atypical market with more flexible boundaries for both labor and educational purposes. For instance, the Washington, D.C. Tuition Assistance Grant program (DCTAG) expands higher education choices for District residents by providing grants of up to $10,000 toward the difference between in-state and out-of-state tuition at public four-year colleges and universities throughout the U.S., Guam and Puerto Rico. The grant also provides up to $2,500 per academic year toward tuition at private colleges in the Washington, D.C. Metropolitan area, private Historically Black Colleges and Universities (HBCUs) nationwide, and two-year colleges nationwide (State 2009).

[8]Thus state-specific notation is suppressed in all versions of the model.

[9]I focus on college enrollment rather than attainment because I have defined the former as the margin of skill. However, because this is a static model and equilibrium in both the college and labor markets is determined simultaneously, enrollment can be thought of as a binary, instantaneous decision of whether to acquire skill and is therefore equivalent to attainment in this context. In later empirical analysis where a distinction between the two terms does exist, I estimate effects for native college attainment in addition to enrollment as a sensitivity check.

[10]Although the model focuses on wages and tuition/fees for simplicity, it remains fully possible that labor employment and college quality changes may influence native college enrollment as well.

[11]I assume labor is only utilized for production of the output good. Also, as nonprofit institutions, I allow for the possibility that arguments unrelated to profit such as campus diversity may enter into colleges' objective functions.

[12]Because non-labor input costs will remain in the background both here and in the formal model version, they serve as college supply shifters. Additionally, while college slots are likely infinitely or nearly infinitely supplied at any given relative wage, the model does not presuppose this and allows for a more general wage relationship.

[13]Ability affects the psychic costs of schooling and may also affect the wage benefit, thus impacting the sensitivity of college demand to prices.

[14]Thus foreign-born individuals must decide whether to immigrate for college and/or employment. Jackson (2010) claims that immigrants make this college/employment decision jointly and explores the extent to which differences in educational quality and cross-country informational asymmetries may affect that choice.

enrollment in the college market and shifts in the relative supply of unskilled labor in the labor market. For simplicity in the graphical model, all individuals educated in the college market are assumed to remain in the state and enter its labor force, while this state retention assumption is relaxed in the formal model.[15]

In the labor market, both the supply of and demand for relatively unskilled labor are potentially sensitive to changes in the relative unskilled wage. More specifically, the relative supply of unskilled labor is determined by: equilibrium college enrollment and the retention of a state's college students in the labor market, labor immigration, and the sensitivity of labor supply to the relative unskilled wage.[16] All individuals are in the labor force if they are not students acquiring skill.[17] On the labor demand side, with constant returns to scale technologies, firms produce a composite, non-traded good using both skilled and unskilled labor. The restriction to a single sector with a nontraded commodity simplifies the model considerably to focus on areas of interest for this paper, as consumer output demand (previously discussed) reduces to individuals maximizing their utility over consumption of the composite good by maximizing their discounted stream of net wages. Additionally, cast in the framework of a basic Heckscher-Ohlin model, the sole existence of a nontraded commodity allows the wages of both skilled and unskilled workers to be determined locally, to the extent that labor is immobile across states (Leamer 1995, Cortes 2008).

2.3.2 Graphical Representation

Figures 1 and 2 depict the impact on equilibrium native college enrollment of two types of immigrant inflows. In Figure 1, an exogenous inflow of relatively unskilled

[15] Bound et al. (2004) estimate that approximately 30 percent of students college-educated in a state remain there for employment in the long-run.

[16] Labor supply sensitivity to the relative unskilled wage here reflects within-state outside options and the marginal utility of leisure, as well as the sensitivity of interstate migration to the relative wage.

[17] As mentioned, skill acquisition is instantaneous in this static model. However, I will focus empirically on working-age, pre-retirement individuals in order to support the stated assumption.

immigrant labor increases the total relative supply of unskilled labor from L to $L^{'}$ and lowers the relative unskilled wage from w to $w^{'}$. This decrease in the relative wage return to being unskilled is associated with an increase in college demand, which raises total enrollment from E to $E^{'}$ and tuition/fees from f to f'. Native enrollment increases from E_N to $E_N^{'}$ (i.e., crowd-in), and if only natives respond endogenously, then $\Delta E_N = \Delta E$.

In Figure 2, an exogenous inflow of immigrant students increases the demand for higher education. This increases tuition/fees and induces some natives to no longer enroll in college. Additionally, the enrolled immigrant students join the labor market as skilled labor, decreasing the total relative supply of unskilled labor and raising the relative return to being unskilled. In equilibrium, these effects result in total enrollment increasing from E to $E^{'}$ and tuition/fees rising from f to $f^{'}$. Those changes in the higher education market are associated with a decrease in the relative supply of unskilled labor from L to $L^{'}$ and a rise in the relative unskilled wage from w to $w^{'}$. Native enrollment here, contrary to total enrollment, decreases from E_N to $E_N^{'}$ (i.e., crowd-out).

While the signs of the comparative statics from these two immigrant shocks are clear from the figures, their absolute and relative magnitudes are not. These magnitudes will depend on demand and supply elasticities in both markets, as well as the sensitivity of native college demand to price changes (i.e., wages and tuition/fees). To form more definitive coefficient predictions, gain deeper understanding of their interpretation, and guide the estimation strategy, a more formal version of the model is needed.

2.3.3 Formal Model

Setup

I present here an algebraic version of the conceptual model in the spirit of Bound et al. (2004).[18] Let $N \equiv$ natives (as before), $I \equiv$ immigrants, $U \equiv$ unskilled, $S \equiv$ skilled, and $L_k = N_k + I_k$ for $k = U, S$. Also, for any variable x, let $\dot{x} \equiv d\ln x$, the percent change in x.

The higher education market for college enrollment can be described by the following equations:

$$\dot{d}_E = -\phi \dot{f} - \eta \dot{w} + \dot{\mu} \; [\textit{College Demand}], \tag{2.1}$$

$$\dot{s}_E = \psi \dot{f} + \kappa \dot{w} + \dot{\varphi} \; [\textit{College Supply}], \tag{2.2}$$

where $\dot{f}$ is the percent change in tuition/fees, while $\dot{w} = \dot{W}_U - \dot{W}_S$ is the percent change in the relative unskilled wage. Parameters ϕ and η are respectively tuition/fee and wage elasticities of college demand, while ψ and κ are respectively tuition/fee and wage elasticities of college supply.[19] Total college demand is a function of both native and immigrant college demand, such that $\dot{d}_E = \pi^N \dot{d}_E^N + \pi^I \dot{d}_E^I$. Note that $\pi^N \in [0, 1]$ is the native share of the total population (i.e., the population across the college and labor markets), while $\pi^I = 1 - \pi^N$ is the analogous immigrant share.[20] Combined, this also implies that $\phi = \pi^N \phi^N + \pi^I \phi^I$, $\eta = \pi^N \eta^N + \pi^I \eta^I$, and

[18] This version of the model, however, differs in a few ways from theirs, such as an extended modeling of the college market and an incorporation of immigration into both the labor and college markets.

[19] Although assumed otherwise, it remains a possibility that the college supply function with respect to tuition is actually negatively-sloped, due to a reduction in average costs when colleges expand (Christian 2004). Empirically, such economies of scale would reduce the magnitude of the crowd-out effects I estimate and allow for the possibility that such effects could even be positive.

[20] For college demand, inclusion of the π population shares follows from a Cobb-Douglas production-style framework for the level of college demand D_E, where $D_E = D_E^{N,\alpha} D_E^{I,(1-\alpha)} \Leftrightarrow \ln D_E = \alpha \ln D_E^N + (1-\alpha)\ln D_E^I$, and α is natives' share in college demand, captured by π^N.

$\dot{\mu} = \pi^N \dot{\mu}^N + \pi^I \dot{\mu}^I$. Lastly, college demand and college supply shifters are represented by μ and φ, respectively.

The labor market for relatively unskilled labor can be described by the following equations:

$$\dot{d}_L \equiv \dot{L}_U^d - \dot{L}_S^d = -\theta \dot{w} + \dot{\zeta} \; [\textit{Labor Demand}], \tag{2.3}$$

$$\dot{s}_L \equiv \dot{L}_U^s - \dot{L}_S^s = \gamma \dot{w} + \dot{\xi} \; [\textit{Labor Supply}], \tag{2.4}$$

where θ is the elasticity of substitution between skilled and unskilled labor, and γ is the relative labor supply elasticity, representing wage sensitivity of labor supplied both within-state and between states (i.e., the cross-state migration elasticity).[21] Total labor supply is a function of both native and immigrant labor supply, such that $\dot{s}_L = \dot{(N_U^s + I_U^s)} - \dot{(N_S^s + I_S^s)} = (\pi^N \gamma^N + \pi^I \gamma^I)\dot{w} + \pi^N \dot{\xi}^N + \pi^I \dot{\xi}^I$.[22] This implies that $\gamma = \pi^N \gamma^N + \pi^I \gamma^I$ and $\dot{\xi} = \pi^N \dot{\xi}^N + \pi^I \dot{\xi}^I$. Additionally, labor demand and labor supply shifters are represented by ζ and ξ, respectively.

Lastly, as discussed earlier, since enrollment determines skill, there is a functional link between changes in equilibrium enrollment in the college market and shifts in the relative supply of unskilled labor in the labor market. I specify this link as follows:

$$\dot{\xi} = \dot{\nu} - \lambda \dot{d}_E^*, \tag{2.5}$$

where $\lambda \in [0, 1]$ is the share of the endogenous equilibrium change in college-enrolled students, $\dot{d}_E^*$, that remain in the state's labor market as skilled labor, and I have

[21] This implies that as labor becomes more mobile, $\gamma \to \infty$. Therefore, perfect labor mobility is a sufficient but not necessary condition for perfectly elastic labor supply, consistent with Bound et al. (2004).

[22] For labor supply, inclusion of the π shares follows from the specification of the level of labor supply, $S_L \equiv \frac{N_U^s + I_U^s}{N_S^s + I_S^s} = (\frac{W_U}{W_S})^{(\pi^N \gamma^N + \pi^I \gamma^I)} e^{(\pi^N \ln \xi^N + \pi^I \ln \xi^I)}$.

assumed that $\lambda^N = \lambda^I = \lambda$.[23] Meanwhile, $\dot{\nu}$ represents the exogenous component of $\dot{\xi}$ - namely, labor supply shocks that originate in the labor market, unrelated to the college market (e.g., labor immigration).[24]

Equilibrium

Equations (1)-(4) for demand and supply in the college and labor markets and equation (5) linking equilibrium changes in the college market to labor supply shifts together form an equilibrium in market prices and quantities. All analysis of interest in this paper assumes no confounding labor demand and college supply shifts, so the following equations specify the equilibrium imposing the restriction that $\dot{\zeta} = \dot{\varphi} = 0$:

$$\dot{f}^* = \Delta\dot{\mu} + (\Lambda\Delta\Gamma)\dot{\xi}, \tag{2.6}$$

$$\dot{w}^* = -\Gamma\dot{\xi}, \tag{2.7}$$

$$\dot{s}_L^* = (\theta\Gamma)\dot{\xi}, \tag{2.8}$$

$$\dot{d}_E^* = \frac{(\psi\Delta)\dot{\mu} + (\Omega\Delta\Gamma)\dot{\nu}}{1+\lambda}, \tag{2.9}$$

where $\Delta = (\frac{1}{\psi+\phi})$, $\Gamma = (\frac{1}{\gamma+\theta})$, $\Lambda = \kappa + \eta$, and $\Omega = \eta\psi - \kappa\phi$. Note again for equations (6)-(8) that $\dot{\xi} = \dot{\nu} - \lambda\dot{d}_E^*$, with $\dot{d}_E^*$ specified in equation (9). Positive shifts in relatively unskilled labor supply, $\dot{\nu}$, decrease relative unskilled wages but increase tuition/fees, while positive shifts in college demand, $\dot{\mu}$, increase both tuition/fees and relative unskilled wages.

[23]This assumption, while not necessary, simplifies the exposition quite a bit. Also note that since equilibrium will impose that $\dot{d}_E^* = \dot{s}_E^*$, the latter could equivalently be substituted into equation (5). Although in this static model, enrollment and attainment are equivalent, in reality λ could also partially represent the fact that the enrolled population will form a subset of the total skilled population.

[24]This is consistent with Fortin (2006), who in her econometric dual-market, supply-demand model, specifies equilibrium college enrollment, relative labor supply, and (inverse) relative labor demand functions at the state-year level, with relative labor supply as a function of past enrollment rates (i.e., homegrown relative labor supplies) and relative in-migration to the state.

Parameters

I am interested in the effect of exogenous shifts in relatively unskilled immigrant labor supply ($\dot{\nu}^I$) and immigrant college enrollment demand ($\dot{\mu}^I$) on equilibrium native college enrollment demanded ($\dot{d}_E^{N*}$). However, I do not observe the shocks $\dot{\nu}^I$ and $\dot{\mu}^I$ directly, but rather observe the equilibrium immigrant quantities $\dot{s}_L^{I*}$ and $\dot{d}_E^{I*}$, where $\dot{s}_L^I \equiv \dot{I}_U^s - \dot{I}_S^s = \gamma^I \dot{w} + \dot{\xi}^I$ and $\dot{d}_E^I = -\phi^I \dot{f} - \eta^I \dot{w} + \dot{\mu}^I$.

Crowd-in

Regarding the effect of an exogenous increase in immigrant labor supply on native college demand, I intend to estimate this parameter under the assumptions of no correlated, exogenous shifts in labor demand ($\dot{\zeta} = 0$), college supply ($\dot{\varphi} = 0$), native and immigrant college demand ($\dot{\mu}^N = \dot{\mu}^I = 0$), and native labor supply ($\dot{\nu}^N = 0$).

It can be derived, given prior assumptions and definitions, that $\dot{\xi} = [\pi^I - (\frac{\lambda}{1+\lambda})(\Omega\Delta\Gamma)\pi^I]\dot{\nu}^I \equiv \Psi\dot{\nu}^I$.[25] Also recall, related to equation (1), that $\dot{d}_E^N = -\phi^N \dot{f} - \eta^N \dot{w} + \dot{\mu}^N$ and $\dot{d}_E^I = -\phi^I \dot{f} - \eta^I \dot{w} + \dot{\mu}^I$. Substituting $\Psi\dot{\nu}^I$ for $\dot{\xi}$ in equilibrium price equations (6) and (7) and manipulating existing formulations, I derive the following equilibrium equations for native college demand $\dot{d}_E^N$ and immigrant labor supply $\dot{s}_L^I$ under the current assumptions:

$$\dot{d}_E^{N*} = [\eta^N \Psi\Gamma - \phi^N \Psi(\Lambda\Delta\Gamma)]\dot{\nu}^I, \tag{2.10}$$

$$\dot{s}_L^{I*} = \{(\pi^N\gamma^N + \theta)(1 - \lambda[\eta^I\Psi\Gamma - \phi^I\Psi(\Lambda\Delta\Gamma)]) + \lambda\pi^N\gamma^I[\eta^N\Psi\Gamma - \phi^N\Psi(\Lambda\Delta\Gamma)]\}\Gamma\dot{\nu}^I \tag{2.11}$$

[25]This follows in part from the fact that, given $\dot{\nu} - \lambda\dot{d}_E^* = \dot{\xi} = \pi^N\dot{\xi}^N + \pi^I\dot{\xi}^I$ from equations (4) and (5), it can be shown that in general $\dot{\nu} = \pi^N\dot{\nu}^N + \pi^I\dot{\nu}^I$.

The implied crowding parameter of interest is thus defined as:

$$\beta = \frac{\dot{d}_E^{N*}/\dot{\nu}^I}{\dot{s}_L^{I*}/\dot{\nu}^I} = \underbrace{\frac{1}{h(.)}}_{-\epsilon_{wL}} \eta^N + \underbrace{\frac{\Lambda\Delta}{h(.)}}_{-\epsilon_{fL}} \phi^N \in [0, \infty), \tag{2.12}$$

where $h(.)$ is a complicated function of the structural parameters, while ϵ_{wL} and ϵ_{fL} are elasticities of relative unskilled wages and tuition/fees (respectively) to exogenous inflows of relatively unskilled immigrant labor.[26] The lower bound on β occurs for *any* of several scenarios, including: (a) perfectly elastic labor demand ($\theta \to \infty$), (b) perfectly inelastic college supply ($\psi = \kappa = 0$), (c) very small immigrant population shares ($\pi^I \to 0$), or (d) frictionless mobility across states or highly wage-sensitive within-state labor supply (γ^I or $\gamma^N \to \infty$). The upper bound on β requires the opposite extreme on *all* elements (a)-(d): i.e., perfectly inelastic labor demand, perfectly elastic college supply, very large immigrant population shares, and immobile labor with no labor supply sensitivity to wage changes.

The sign of β shows that relatively unskilled immigrant labor inflows weakly increase (i.e., crowd-in) native college enrollment, and the magnitude of this reduced-form effect is a function of the sensitivity of native college demand to changes in wages and tuition/fees, as well as the sensitivity of those market prices to the immigrant inflows.

Crowd-out

Turning now to the effect of an exogenous increase in immigrant college demand on native college demand, I intend to estimate this parameter under the assumptions of no correlated, exogenous shifts in labor demand ($\dot{\zeta} = 0$), college supply ($\dot{\varphi} = 0$), native and immigrant labor supply ($\dot{\nu}^N = \dot{\nu}^I = 0$), and native college demand

[26] In other words, $\epsilon_{wL} = \frac{\dot{w}^*/\dot{\nu}^I}{\dot{s}_L^{I*}/\dot{\nu}^I}$ and $\epsilon_{fL} = \frac{\dot{f}^*/\dot{\nu}^I}{\dot{s}_L^{I*}/\dot{\nu}^I}$.

($\dot{\mu}^N = 0$).

It can be determined, given prior assumptions and definitions, that $\dot{\xi} = [-(\frac{\lambda}{1+\lambda})(\psi\Delta)\pi^I]\dot{\mu}^I \equiv \Phi\dot{\mu}^I$. Again recall, related to equation (1), that $\dot{d}_E^N = -\phi^N\dot{f} - \eta^N\dot{w} + \dot{\mu}^N$ and $\dot{d}_E^I = -\phi^I\dot{f} - \eta^I\dot{w} + \dot{\mu}^I$. Substituting $\Phi\dot{\mu}^I$ for $\dot{\xi}$ in equilibrium price equations (6) and (7) and manipulating existing formulations, I derive the following equilibrium equations for native college demand $\dot{d}_E^N$ and immigrant college demand $\dot{d}_E^I$ under the current assumptions:

$$\dot{d}_E^{N*} = \{\eta^N\Phi\Gamma - \phi^N[\Delta + (\Lambda\Delta\Gamma)\Phi]\}\dot{\mu}^I, \tag{2.13}$$

$$\dot{d}_E^{I*} = \{1 + (\eta^I\Phi\Gamma - \phi^I[\Delta + (\Lambda\Delta\Gamma)\Phi])\}\dot{\mu}^I, \tag{2.14}$$

The implied crowding parameter of interest is thus defined as:

$$\alpha = \frac{\dot{d}_E^{N*}/\dot{\mu}^I}{\dot{d}_E^{I*}/\dot{\mu}^I} = \underbrace{\frac{\Phi\Gamma}{l(.)}}_{-\epsilon_{wE}}\eta^N + \underbrace{\frac{(\Delta + \Lambda\Delta\Gamma\Phi)}{l(.)}}_{-\epsilon_{fE}}\phi^N \in [-1, 0], \tag{2.15}$$

where $l(.)$ is a complicated function of the structural parameters, while ϵ_{wE} and ϵ_{fE} are elasticities for the sensitivity of relative unskilled wages and tuition/fees (respectively) to exogenous inflows of immigrant students.[27] The upper bound on α occurs when there is perfectly elastic college supply with respect to tuition/fees ($\psi \to \infty$) combined with *any* of several scenarios, including: (a) perfectly elastic labor demand ($\theta \to \infty$), (b) frictionless mobility across states or highly wage-sensitive within-state labor supply (γ^I or $\gamma^N \to \infty$), (c) very small immigrant population shares ($\pi^I \to 0$), or (d) no retention of college students in the state's labor market ($\lambda = 0$). Conversely, the lower bound on α simply requires perfectly inelastic college supply with respect to tuition/fees.

[27] In other words, $\epsilon_{wE} = \frac{\dot{w}^*/\dot{\mu}^I}{\dot{d}_E^{I*}/\dot{\mu}^I}$ and $\epsilon_{fE} = \frac{\dot{f}^*/\dot{\mu}^I}{\dot{d}_E^{I*}/\dot{\mu}^I}$.

The sign of α shows that immigrant student inflows weakly decrease (i.e., crowd-out) native college enrollment, and the magnitude of this reduced-form effect is once again a function of the sensitivity of native college demand to changes in wages and tuition/fees, as well as the sensitivity of those market prices to the immigrant inflows. The magnitude range of -1 to 0 in this case also aligns with theory and findings of other immigrant-native displacement studies (e.g., Hoxby 1998, Card 2001, Cortes 2008).

Implications for Estimation

In order to credibly identify the crowd-in and crowd-out effects of immigration on native college enrollment, the model highlights key issues to be aware of and incorporate into both the form of the estimating equation as well as the method of estimation itself.

First, both coefficients focus on how exogenous, unobserved immigrant shifts affect native college enrollment via observed immigrant quantities for relatively unskilled labor supply and college demand. Thus, the regressors of interest in the estimating equation should also be focused on immigrants. It should be noted that when the focus of the model is, alternatively, how exogenous immigrant shifts affect native college enrollment via observed *total* quantities for relatively unskilled labor supply and college demand, the parameter results are qualitatively similar. When considering total quantities, the noteworthy changes are that: (a) the scale of immigrant inflows (via π^I) no longer factors into the formula for β (crowd-in) (Appendix A2 derives a method to examine the importance of this "scale effect" in the empirical analysis and, unlike equation (12), to separately identify it from β); and (b) the lower bound of α (crowd-out) decreases from -1 to -∞.

Second, compared to the graphical representation, the formal model makes it explicitly clear that the relative magnitudes of the crowd-in and crowd-out parameters are ambiguous, as both $|\beta| > |\alpha|$ and $|\beta| < |\alpha|$ are possible depending on structural parameter values.

Third, changes in several parameters (e.g., increases in labor demand elasticity, θ) will cause *both* β and α to tend toward 0. However, there are exceptions to this. More elastic college supply with respect to tuition/fees, ψ, as well as a lower percentage of a state's college students retained for the labor market, λ, both reduce the magnitude of α, whereas they increase or have an ambiguous effect, respectively, on the magnitude of β.[28]

Fourth, the model motivates the need for independent and dependent variables in the estimating equation that are specified in logs, given that the parameters in equations (12) and (15) are for log changes.

Fifth, given that it is always assumed that there are no exogenous increases in native college enrollment ($\dot{\mu}^N = 0$), it is necessary to account for native demographic shocks that would otherwise affect their college demand. This can be accomplished by examining log changes in enrollment rates rather than enrollment levels.

Finally, appropriate estimation is necessary to ensure that other assumptions about the lack of confounding market shifts - e.g., in labor demand ($\dot{\zeta} = 0$) and college supply ($\dot{\varphi} = 0$) - actually hold. To accomplish this, I will utilize a procedure to estimate immigrant college demand, as well as use two-stage least squares (2SLS) estimation, various fixed effects,[29] and an estimating equation specified in first differences.

[28]Additionally, although absent from this static model, in reality the different time horizons for the crowd-in and crowd-out effects due to the lag between college enrollment and labor market entry could also open a role for native expectations and uncertainty to explain differences in the coefficient magnitudes.

[29]This includes those at the state level, since effects in the model are theorized for given labor and higher education markets of a fixed size. The empirical analog of this assumption is thus to estimate effects over time within states, while also accounting for any native demographic shocks, as already discussed.

2.4 Data

The analysis uses population samples from the Integrated Public Use Microsamples (IPUMS) of the decennial U.S. census for the 1970 to 2000 period (Ruggles et al. 2009). I oversample immigrants such that the census data (long form) on immigrants constitutes 1 percent population samples in 1970 and 5 percent population samples in 1980-2000, while data on natives are 1 percent population samples over the entire data range 1970-2000. The sample consists of working-age individuals ages 18 to 64 not living in group quarters unless those quarters are schooling-related (e.g., boarding school). All fifty U.S. states are included (Washington, D.C. is excluded) and represent the local labor and higher education markets. There are 7,400,855 individual-level observations, consisting of 2,319,597 immigrants (to be defined momentarily) and 5,081,258 natives.

To create a pseudo-panel for each state j and year t, aggregations of this data are taken over individuals in each state-year, incorporating census individual sample weights so that the aggregates in each state-year cell are nationally representative, and resulting in 200 state-year observations. Skill is a binary measure, where individuals with four years of high school education or less are classified as unskilled, while individuals with some college education or more are classified as skilled, all based on census information on the highest grade attended (Jaeger 1997).[30]

All individuals are classified as either immigrants or natives. Empirically, an immigrant is defined as an individual born abroad who is currently either a noncitizen or a naturalized citizen.[31]

[30] Jaeger's (1997) recommendations for coding are of particular importance here, since it is this margin of unskilled and skilled labor where the differences exist between the census coding and his. Specifically, in the census consistent recode of educational attainment, respondents who are attending their first year of college or who did not complete that first year are identified with '12th grade' as their highest attended grade of education, whereas I categorize the highest grade attended for these individuals as 'some college'.

[31] Exceptions (i.e., those coded as natives) are: a) individuals born in U.S. territories or possessions (e.g., Puerto Rico, American Samoa); b) individuals born in countries where they are granted automatic U.S. citizenship due to

Additionally, 59,084 individual-level observations of immigrants from census data in 1960 are utilized for both maximum likelihood estimation (MLE) of immigrant college demand and formation of historical immigrant enclave instruments for 2SLS estimation (see sections 5).

The top panel of Figure 2 shows how the relative skilled wage, or skill premium, has changed over the sample period. Initially, the mean wage of skilled workers relative to unskilled workers fell, dropping from 1.5 times as large in 1970 to 1.4 times as large in 1980. Median relative wages exhibited a similar albeit less drastic decrease. However, over the remainder of the sample period from 1980 to 2000, both the mean and median (to a lesser extent) skill premia increased substantially, far surpassing their 1970 initial values. This fall and subsequent rise in the relative wages of skilled workers during the latter part of the twentieth century has been well-documented in the labor literature and the source of policy debates regarding how best to combat the rising wage inequality across skill groups (Fortin 2006).

At the same time, the lower panel of Figure 2 shows that the relative supply of skilled labor measured in the census has been increasing for both natives and immigrants.[32] This implies that the relative demand for skilled labor outpaced relative supply from 1980 to 2000 (Johnson 1997), consequently generating a considerable amount of research to investigate the cause of that demand increase (Krueger 1993, DiNardo and Pischke 1997, Autor, Katz and Krueger 1998). Figure 3 further corroborates the occurrence of an upward trend in individuals' skill levels over this period, as college enrollment increased steadily across various subgroups of the population.

Returning to the Figure 2 panels, there are three points worth noting from the

political unions with the U.S. if not already deemed natives under exception (a) (e.g., Northern Mariana Islands); and c) individuals born abroad of American parents.

[32]This would be an overstatement of the skill increase amongst the foreign-born during the sample period if illegal immigrants, who tend to be undercounted, are disproportionately unskilled.

trends displayed. First, given the negative causal relationships outlined between immigrant skill and native skill in the theory of section 3, the pattern in the lower panel of Figure 2 is somewhat surprising. However, the aggregate positive correlation between immigrant and native skill shown there could simply be masking a negative causal relationship, particularly at the local market level, which may be reflected in part by the widening gap between the native and immigrant ratios of skilled to unskilled labor.

A second noteworthy point is that if the college demand of individuals responds positively to the skilled wage premium, as proposed in the model, then we might expect to see as a result an increase in the growth rate of relatively skilled labor and a subsequent decrease in the growth rate of the skilled wage premium, assuming a steady labor demand trend. This appears to be exactly what occurs from 1980 to 2000 in Figure 2 when focusing on native labor supply and relative wages, but less so when examining foreign-born labor supply. Thus, an alternative explanation for the growing gap between natives and immigrants in relatively skilled labor could be that native college demand is more sensitive to wage changes occurring during this period.

Finally, Figure 2 also highlights, if only suggestively, that differential labor demand trends and shifts across states that would otherwise confound estimates of the crowd-in and crowd-out parameters are a nontrivial possibility. Thus, as noted in the model, it will be important in estimation to take steps to try to address any such confounding labor demand movements.

2.5 Empirical Strategy

2.5.1 Setup and Selection Issues

The model identifies several relevant empirical decisions to implement for the estimation of immigrant crowd-in and crowd-out of native college enrollment (see 3.3.4). This leads to the following general specification to be estimated for state j and year t:

$$\ln\Big(\frac{Native^{CE}}{Native}\Big)_{jt} = \beta\ln\Big(\frac{Immig^{U}}{Immig^{S}}\Big)_{jt} + \alpha\ln(Immig^{CE})_{jt} + \omega_j + \phi_t + \varepsilon_{jt}, \quad (2.16)$$

where CE is college-enrolled, U is unskilled (i.e., high school education or less), S is skilled (i.e., some college education or more), ω_j and ϕ_t are respectively state and year fixed effects, and ε_{jt} is a mean-zero error. The dependent variable $\left(\frac{Native^{CE}}{Native}\right)_{jt}$ is the native college enrollment rate for each state-year. On the right-hand side of the equation, $\left(\frac{Immig^{U}}{Immig^{S}}\right)_{jt}$ represents relatively unskilled immigrant labor in a state-year, while $(Immig^{CE})_{jt}$ represents college enrollment by immigrant students in a state-year.

Because serial correlation in native enrollment rates is likely to occur in this panel dataset and thereby typically bias OLS standard error estimates downward (Bertrand, Duflo and Mullainathan 2004), I cluster standard errors by state in order to allow for an arbitrary variance-covariance structure within states. All specifications will also be unweighted, so that each state-year cell receives equal weight in estimation.[33] The model predicts $\beta \in [0, \infty)$ (crowd-in) and $\alpha \in [-1, 0]$ (crowd-out) when considering consistent estimates of the parameters.

Regarding α, actually, the model's prediction technically holds for the case when

[33] An alternative would be to weight observations by the square root of the underlying sample population for each state-year. Presumptively, this would be to decrease the influence of small-sample, high-variance observations. However, a Breusch-Pagan test for heteroskedasticity on such a specification strongly rejects the null hypothesis of homoskedasticity, suggesting that there is a nontrivial group error component to the state-year data and that weighted estimation actually worsens heteroskedasticity rather than eliminates it (Dickens 1990).

native college demand and immigrant college demand are specified identically, which is not the case in equation (16). There, it was useful to specify the dependent variable as a rate in order to satisfy another model assumption. To correct for this, I run auxiliary regressions in the main results in order to recover an interpretation of α that is consistent with the model's displacement predictions.

Immigrants in the sample are neither randomly assigned to states nor randomly assigned to the labor or college market for a given state. Consequently, time-invariant and time-varying market conditions that differ across states and influence native college enrollment rates[34] may also influence the location and college enrollment decisions of immigrants,[35] thus affecting foreign-born labor supply and college demand. To remove any state-level, time-invariant factors, I re-write equation (16) in first differences. The resulting general specification to be estimated is as follows:

$$\Delta\ln\Big(\frac{Native^{CE}}{Native}\Big)_{jt} = \beta\Delta\ln\Big(\frac{Immig^{U}}{Immig^{S}}\Big)_{jt} + \alpha\Delta\ln(Immig^{CE})_{jt} + \Delta\phi_t + \Delta\varepsilon_{jt}, \quad (2.17)$$

where the state fixed effect, ω_j, has now been differenced-out.

Estimation of equation (17) by OLS still may not lead to unbiased estimates of β and α if immigrants select which markets to participate in based on time-varying unobservable shocks, inducing a correlation between $\Delta\varepsilon_{jt}$ and both $\Delta\ln\Big(\frac{Immig^{U}}{Immig^{S}}\Big)_{jt}$ and $\Delta\ln(Immig^{CE})_{jt}$. For instance, if unskilled immigrant labor tends to locate in areas that experienced a positive labor demand shock, $\hat{\beta}$ will be biased downward and crowd-in will be underestimated. Similarly, if immigrant students tend to locate in areas where there was a positive college supply shock, $\hat{\alpha}$ will be biased upward and crowd-out will be underestimated. Meanwhile, if immigrants to a *given* location that are on the margin of college enrollment and labor force participation tend to enroll

[34]Specifically, for instance, the nature of labor demand and college supply, which affect relative wages and tuition/fees.

[35]Cadena (2008), for instance, finds evidence that immigrants endogenously select their destination based in part on its labor market conditions.

when the area has experienced a negative labor demand shock or positive college supply shock, $\hat{\alpha}$ will again be biased upward. If conversely they tend to join the labor force when the area has experienced a negative college supply shock or positive labor demand shock, $\hat{\beta}$ will again be biased downward.

Although the previous scenarios bias against finding crowd-in or crowd-out, alternate, more problematic biases remain possible. Immigrant students may opt for markets where a positive labor demand shock occurred in order to improve post-college employment prospects, biasing $\hat{\alpha}$ downward and overestimating crowd-out. Unskilled immigrant labor, especially with college-age or younger children, meanwhile, might prefer markets where college supply is expanding, leading to upward-biased $\hat{\beta}$ estimates and overstating crowd-in.[36] If this type of selection is occurring, it may reflect more long-term market selection on the part of immigrants, since both scenarios exhibit forward-looking behavior and longer time horizons.

I attempt two methods to address such non-random market selection by immigrants, beginning with non-random selection of labor vs. college markets for a given location ("non-spatial selection"). I would like to determine which immigrant inflows contribute to labor supply vs. college demand without using actual labor force participation and enrollment status, which are affected by labor demand and college supply movements. To achieve this, I predict in-sample immigrant college demand using consistent estimates from a logit model of immigrant enrollment on pre-sample data (discussed in detail in section 5.2). These predictions are then utilized to determine how to allocate observed immigrant inflows to the measure of either immigrant labor supply or immigrant college demand. Because this estimation-based procedure may result in mismeasurement of the true contribution of immigrant inflows to im-

[36] Higher *social* returns to college education in areas with larger stocks of skilled labor (e.g., Moretti 2004) might also induce a positive correlation between college supply and unskilled immigrant labor, with or without young children.

migrant labor supply and college demand shifts, it will be important to assess the extent of such measurement error later in the paper.

Secondly, I turn to non-random "spatial selection" of local markets by immigrants. To address this, I utilize 2SLS estimation that exploits geographic variation in historical immigrant enclaves as instruments. Under certain assumptions (discussed in detail in section 5.3), these instruments further isolate the exogenous component of immigrant inflows from endogenous flows that vary with unobserved movements in labor demand and college supply.[37]

Lastly, in addition to potential classical measurement error from the procedure estimating immigrant college demand (see section 5.2), measurement error in the immigrant inflows may occur for other reasons as well. Mismeasurement of immigration in the census data due to small immigrant inflows or unobserved inflows of undocumented immigrants will both lead to biased crowding estimates. Regarding the former, because immigrants account for less than 10 percent of the population in most of the sample period, small flows are going to be prevalent, particularly in certain states. This will likely mean a higher likelihood of measurement error which, if classical, should lead to attenuation bias in both $\hat{\beta}$ and $\hat{\alpha}$ (Aydemir and Borjas forthcoming). Regarding undocumented immigration, if legal and illegal immigrant flows of a given type (i.e., labor, students) are positively correlated, and illegal immigrant inflows cause similar or even identical price effects, then this would result in an upward bias in $\hat{\beta}$ and a downward bias in $\hat{\alpha}$.[38]

[37]It should be noted that 2SLS alone, if valid, would be sufficient to address both spatial and non-spatial selection, and should therefore purge estimation of any residual, non-spatial endogeneity not already addressed by the logit procedure of immigrant college demand. However, if both types of selection are relatively severe, then OLS estimates addressing neither may be uninformative due to large biases, compared to the OLS estimates from the current approach to address biases sequentially. Table 2 in section 6 assesses the former approach to the OLS estimates and indeed finds the bias to be substantial.

[38]Hanson (2006) discusses evidence that illegal immigrants are already represented to a degree in official household surveys like the U.S. Census, which would tend to diminish this bias. Moreover, because the omitted variables in this case are still immigrant-related, an alternative to classifying this as bias, if non-trivial, would be a reinterpretation of the estimated crowding parameters as a reflection of both legal and illegal immigration.

2.5.2 Predicting Immigrant Student and Labor Inflows

To exogenously determine which immigrants contribute to college demand, I use 1960 census cross-section data on immigrants to run a logit model of college enrollment on individual characteristics as follows, for individual i in state j:

$$Immig_{ij}^{CE} = \vartheta_0 + \vartheta_1 Age_{ij} + \vartheta_2 Age_{ij}^2 + \vartheta_3 Female_{ij} + Race_{ij}'\vartheta_k + Country_{ij}'\vartheta_h + \varepsilon_{ij}, \quad (2.18)$$

where $Female$ is a dummy variable for women, while $Race'$ and $Country'$ are a set of race/ethnicity and country dummies, respectively.

As shown in Appendix A1, if market shocks are not correlated with any of these chosen characteristics, I can consistently estimate how each of the covariates impacts college-enrollment via a change in underlying college demand. Using the coefficient estimates, I predict enrollment out of sample for 1970 to 2000 and designate immigrants during the period into quintiles based on these predicted values. The highest quintile[39] individuals are designated as immigrant students, while the lowest four quintiles are designated as immigrant labor. In the latter case, skill levels are then determined using actual educational attainment information, which is no longer endogenous given that these individuals are predicted to no longer be acquiring human capital.

One caveat with this procedure is that the observed geographic variation of the immigrant covariates from 1970 to 2000 is still subject to confounding market shocks from labor demand and college supply. This implies that this approach would likely, at best, only be able to address non-spatial selection. 2SLS estimation will remain necessary to address spatial selection of immigrant inflows with market conditions,

[39] This is a purposely conservative allocation, given that observed immigrant enrollment during the sample period has a mean of 5 percent, thus notably lower than 20 percent. However, this may be due to less than perfectly elastic college supply. In the presence of perfectly elastic supply, immigrant college enrollment may have indeed more closely approached 20 percent, thus motivating the quintile choice.

as well as any residual non-spatial selection not purged in the OLS estimates. By not addressing both types of selection with 2SLS alone, the OLS estimates can thus be more informative.

Additionally, even if the estimates of the $\boldsymbol{\vartheta}$ parameters are consistent, to the extent that the predictive power of the model is low, this procedure may introduce a classical measurement error problem. Specifically, a large proportion of the variance in the main equation (17) crowding regressors will be due to prediction error from the logit estimation rather than the true, unobserved immigrant college demand and labor supply. In the absence of immigrant spatial selection, this would tend to attenuate both $\hat{\beta}$ and $\hat{\alpha}$ (Angrist and Krueger 1999). However, if spatial selection is indeed a problem, then depending on the nature of such selection, this could even result in wrongly signed coefficients, with $\hat{\beta} < 0$ and $\hat{\alpha} > 0$. As a result, it will be important to verify that the predictive power of the logit model is at least moderate. Moreover, 2SLS estimation will again be useful in the presence of this issue, in order to eliminate the bias resulting from this measurement error.

2.5.3 Instruments

The previous procedure, while helpful for addressing endogeneity in immigrants' choice of entrance into labor markets vs. college markets, fails to address any endogeneity in immigrants' location choices. To address the latter spatial selection, as well as purge estimation of any residual endogeneity from non-spatial selection not eliminated by the logit procedure, I employ 2SLS estimation. The instruments utilize the historical, 1960 distribution of immigrants in the U.S. to form predictions about the flow of immigrants over the sample period, 1970 to 2000. These instruments are motivated by the idea that existing immigrant networks and enclaves are an important determinant of the location choices of prospective immigrants (e.g., Bartel 1989, Card 2001, Munshi 2003, Cortes 2008). The enclaves, by increasing cultural benefits and reducing informational and legal costs, increase the net marginal benefits of migration into U.S. local markets by the foreign-born.

For state j and year t, the instruments take the following form, where h is source country:

$$\sum_h \left(\frac{Immigrants_{hj,1960}}{Immigrants_{h,1960}}\right) \times \Delta Immigrant\_Type_{\ ht}, \tag{2.19}$$

The three $Immigrant\_Type$ stocks utilized are 1) immigrant students, 2) unskilled immigrant labor, and 3) skilled immigrant labor, where all three cases are the predicted stocks (as described in section 5.2) rather than actual stocks. The validity of these instruments and the identification strategy hinges on two assumptions related to the two components of the instrument.

First, it is assumed that unobserved, relative market shocks between any two states in 1960 (i.e., labor demand shifts or college supply shifts) that caused immigrants to locate in one state rather than the other, are not correlated with unobserved *changes* in relative market shocks between the same two states from 1970 to 2000. Secondly, validity requires that the national immigrant flows of each type, $Immigrant\_Type$, are exogenous to such unobserved, relative market shocks between states from 1970 to 2000.[40]

Combined, this is a restatement of the exclusion restriction, which in this case claims that the only channel by which the 1960 distribution of immigrants, interacted with various types of source country inflows, impacts native enrollment rates is through its effect on the actual distribution of the immigrant inflows of interest. The

[40]This assumption is questionable in theory since immigrants may have strong preferences for certain U.S. states. If so, relative market shocks of those states my cause them not to immigrate at all. However, as Cortes and Tessada (2009) note, Boustan (2007) compares results from instruments that use actual migrant flows vs. those that use migrant flows predicted from source area push factors and finds little difference between the two sets of results, suggesting that the assumption may hold in practice.

inclusion of division-year fixed effects in estimation (there are a total of nine U.S. Census divisions) helps to ensure that the exclusion restriction holds, which would be violated with the omission of such fixed effects if some divisions' economies (due to labor demand and/or college supply movements) have been growing or shrinking consistently relative to other divisions since 1960.[41]

Because there are two endogenous variables and estimation will be made robust to a clustered error structure, all 2SLS results will be reported with the Kleibergen-Paap rk statistic (Kleibergen and Paap 2006) to assess instrument relevance and guard against weak instrument estimation, which could severely bias crowding coefficients and lead to spuriously significant estimates (Bound, Jaeger, and Baker 1995).[42] The statistic will be assessed in light of the Stock and Yogo (2005) weak instrument identification critical values.[43]

Lastly, assuming that the instruments here are valid, the econometric interpretation of the crowding parameters $\hat{\beta}$ and $\hat{\alpha}$ still remains. Although the cross-sectional unit is a state, it is important to remember that it is an aggregation of individual native and immigrant units at which agent behavior is actually operating. Because, as discussed in the model, there exists a latent native ability distribution in each state, this can be thought of as determining a state-specific college enrollment impact of the two treatments (i.e., the immigrant inflows). Since different native ability distributions across states j seems probable, it is likely that there are heterogenous impacts of these treatments across states, $\beta_j = \bar{\beta} + \beta_j^*$ and $\alpha_j = \bar{\alpha} + \alpha_j^*$.

With a heterogenous treatment model, parameters estimated by 2SLS are often

[41] Cortes (2008) notes the Sun Belt region as one such example.

[42] The Cragg-Donald statistic (Cragg and Donald 1993) assumes i.i.d. errors and is hence less appropriate given the error structure here.

[43] It should be noted that Stock and Yogo's critical values are constructed assuming i.i.d. errors. For the weak instrument test, at a 5 percent significance level, based on 2SLS maximal bias relative to OLS, the corresponding critical value here for 5 percent maximal bias is 11.04. For the weak instrument test, at a 5 percent significance level, based on maximal 2SLS actual size compared to hypothesized size, the corresponding critical value here for 10 percent maximal actual test size is 16.87, and for 15 percent is 9.93.

interpreted as local average treatment effects (LATEs) - namely, marginal effects for those units induced to treatment by the instrument (Imbens and Angrist 1994). However, if the stronger exogeneity assumption for a valid instrument in the heterogenous treatment model actually holds,[44] then an average treatment effect (ATE) interpretation of the crowding parameters is still valid.[45] Because the enclave-based "cost" of immigration is known and considered by immigrants, but the native ability-based enrollment "benefit" of the immigrant inflow treatments is known and considered by natives, immigrants may not likely know both that state-specific cost and benefit of immigration.[46] Thus, under this informational asymmetry assumption, the estimated crowding parameters $\hat{\beta}$ and $\hat{\alpha}$ can be interpreted to represent marginal effects averaged across all state-year observations.

2.6 Main Results

2.6.1 Immigrant Student and Labor Predictions

Table 1 displays the results from estimation of immigrant college demand in 1960.[47] Average marginal effects are shown, so that each estimate measures the mean change in the probability of being college-enrolled from a one unit increase in the given covariate.[48] For instance, the full logit model in column (1) estimated by maximum likelihood shows that being female decreases immigrant enrollment

[44] For example, with heterogeneous treatment effects, the exogeneity assumption relevant for the immigrant student inflow treatment (letting $(Immig^{CE})_{jt} \equiv T_{jt}$, and $\mathbf{Z'}_{jt} \equiv$ the vector of enclave instruments) would be: $E[(\alpha_j^* \Delta T_{jt} + \Delta\varepsilon_{jt})|\Delta\phi_t, \Delta T_{jt}, \Delta\mathbf{Z'}_{jt}] = 0$. In other words, substantively, the assumption is that conditional on the immigrant inflow treatment, the values of the enclave-based instruments are not correlated with the state-specific impact of the treatment on native enrollment rates. This contrasts with the weaker exogeneity assumption of a common treatment model: $E[\Delta\varepsilon_{jt}|\Delta\phi_t, \Delta T_{jt}, \Delta\mathbf{Z'}_{jt}] = 0$.

[45] This is of particular interest in this case since there may be non-linear, diminishing effects of the enclave instruments on actual immigrant inflows. This could require the inclusion of quadratic terms as additional instruments, thus making the monotonicity assumption necessary for valid LATE interpretation more questionable (Imbens and Angrist 1994), although not necessarily violated.

[46] In other words, for an immigrant inflow "participation" equation, $\Delta T_{jt} \neq \iota_1\alpha_j^* + \iota_2\Delta\mathbf{Z'}_{jt} + \Delta\phi_t + \varsigma_{jt}$, but rather $\Delta T_{jt} = \varpi_1\Delta\mathbf{Z'}_{jt} + \Delta\phi_t + \o_{jt}$. If immigrants did know α_j^*, however, one could then perhaps appeal to their knowledge of the extent of immigrant-native substitutability in production to motivate why the native ability distributions would matter to them in their immigration decision.

[47] Appendix Table 13 examines averages and changes of the covariates used in specification (18) for the college-enrolled and not-college-enrolled foreign-born population in the U.S.

[48] For the logistic models, the average marginal effects are calculated at the sample average rate of college enrollment in 1960, $\hat{\beta^*}\bar{y} * (1 - \bar{y})$

probability by 1.3 percentage points relative to being male. Additionally, the probability of enrollment decreases significantly with age, as well as for all of the identified race/ethnicities relative to white non-Hispanic immigrants, although not significantly. The full logit model predicts the correct outcome for enrollees at a higher rate than non-enrollees, and also performs better for predictions in-sample rather than out-of-sample, as expected.[49]

Column (2) shows that the linear probability model (LPM) estimated by OLS has qualitatively and often quantitatively similar results to the logit specification, although the age effects are now significantly non-linear and the negative enrollment effect of being Hispanic is now significant. However, the indicated measures of model fit are worse for the LPM estimation, other than the model performing somewhat better at predicting enrollees. This is also the case for the logit model with age only in column (3), although not by a large margin. Appendix Table 14 displays average (weighted) characteristics of each quintile in the college demand index, which are qualitatively similar to the estimation results of Table 1, as expected.

In addition to specifications (2) and (3) of Table 1, there are other potential alternatives to the full logit model to designate immigrant students and labor. One possibility is not to even distinguish the immigrant inflows at all, presuming that students and labor have a homogenous effect on native enrollment, contrary to the model's predictions. Alternatively, returning to the theory-driven assumption that the effects are distinct, I can determine an age cutoff using the distribution of enrolled immigrants in 1960, where immigrants of age equal to or below the cutoff age will be designated as immigrant students, and immigrants older than the age cutoff will be designated as immigrant labor. Finally, I can also use the endogenous labor force

[49]Because, as Appendix Table 13 shows, the unconditional probability of immigrant enrollment in 1960 is very low at 1 percent, this 0.01 value is used as the threshold for evaluation of the logit predictions rather than the standard threshold of 0.5 (Heckman et al. 1998).

participation and college enrollment information to designate the immigrant inflows appropriately.

Table 2 displays OLS results from estimation of baseline equation (17) using the above methods to determine the immigrant inflow regressors. Column (1) shows that when the assumption of homogenous immigrant inflows is made, there is no significant effect of immigration on native college enrollment rates. Column (2), the preferred method, now differentiates the types of immigrant inflows as prescribed by the model and finds support for the theoretical predictions, as there is both significant crowd-in and crowd-out. With elasticities of 0.26 for $\hat{\beta}$ and -0.14 for $\hat{\alpha}$, the empirical results are completely consistent with the model's derived coefficient values in equations (12) and (15).

Columns (3) to (5) show that alternative, justifiable methods yield quantitatively similar results to column (2), supporting the robustness of the procedure. However, once endogenous labor and college market information is used in column (6), the coefficient magnitudes are severely mitigated. As discussed in section 5, this suggests that non-spatial selection into the college market is negatively correlated with labor demand shifts or positively correlated with college supply shifts, while non-spatial selection into the labor market is conversely positively correlated with labor demand shifts or negatively correlated with college supply shifts. Any degree of bias in column (2)'s estimates is notably more moderate when compared to column (6).

2.6.2 Descriptive Statistics

Before proceeding to the main OLS and 2SLS results, it is useful to examine the nature of the variation in the predicted immigrant inflows and native college enrollment rates.

There is substantial geographic variation in the predicted immigrant labor and student regressors over the sample period shown by Figures 5 and 6. Nearly all states saw large percentage decreases - some over 100 percent in magnitude - in the relative labor supply of unskilled immigrant labor, with the exception of Idaho and Kansas, which saw small, positive increases. There were similarly widespread positive percent increases in immigrant students over the sample period (particularly in the Sun Belt area), with the sole exception of Vermont which had a small negative change. Both maps are thus consistent with the upward skill trends shown in Figures 2 and 3.

The degree of precision of the separately estimated coefficients for crowd-in and crowd-out relies on how collinear the predicted immigrant labor and student flows are. Figure 7 shows that precise identification (at least, for OLS; additional factors matter for 2SLS) does not come from large immigrant flow states such as California, New York, and Florida, but rather from much smaller flow states like Nebraska. This will be further explored in the main estimates.

Although illustrative, these maps do not purge the labor and student immigrant flows of national trends, nor do they remove variation in each state that does not change over time. To the degree that such variation reflects unobservables that are correlated with immigrant flows and native college enrollment rates, utilizing it for parameter identification would lead to biased estimation.

Table 3 shows, in addition to the statistically significant change that each dependent and independent variable experienced over the sample period, that such year and state-specific variation is substantial (columns 8 and 9). As equation (17) notes, all estimates will account for state and year fixed effects so that the identifying variation is only from within states over time, which accounts for approximately one-fifth to one-quarter of total variation (column 10).

2.6.3 Baseline OLS and 2SLS Estimates

Table 4 shows the estimates from the first stage regressions for relatively unskilled immigrant labor and immigrant students. When only levels of the enclave-based instruments are specified as regressors in columns (1) and (3), historical immigrant shares predict relatively small inflows of actual immigrants, with coefficients that are largely not statistically significant. This is reflected in the low F statistics. One possibility however, since these instruments reflect changes in the net marginal benefit to immigrants, is that there are significant nonlinearities in the impact of the historical enclaves on actual immigrant inflows. Another possible source of such nonlinearities is that there is a minimum threshold for an enclave's size to reach before it has value to a new migrant entrant (i.e., a network externality).

Columns (2) and (4) confirm that there are significant nonlinear effects of historical immigrant enclaves on actual immigrant flows. In all cases, when both the level and quadratic term are significant, they are of opposite sign, providing support for diminishing net marginal benefits to the network.

Table 5 presents the main OLS and 2SLS estimates. Column (1) is identical to column (2) from Table 2, with one exception. For all specifications now, as discussed in section 5, auxiliary regressions are also run where the dependent variables are $\Delta\left(\frac{Native^{CE}}{Native}\right)_{jt}$ and $\Delta\left(\frac{Immig^{CE}}{Native}\right)_{jt}$. The last row of Table 5 reflects the ratio of crowd-out coefficients from those auxiliary specifications, which can be interpreted as the number of natives that disenroll for every immigrant enrollee. This displacement interpretation, not directly discernible from the crowd-out coefficient $\hat{\alpha}$ alone, is more closely aligned with the magnitude prediction for $\hat{\alpha}$ from the model, as displacement should be bounded between -1 and 0.[50]

[50]In the presence of any confounding biases, however, displacement estimates could lie outside of these bounds.

In the OLS results, there is no statistically significant evidence of crowding once division-year fixed effects or state-specific linear trends are included in columns (2) and (3). In both cases, compared to column (1), the magnitude of both crowding coefficients is also reduced. This supports the notion that the division-year fixed effects are accounting for nontrivial positive selection by immigrants of markets across divisions in response to market shocks. Immigrant labor is dynamically locating in divisions with better college prospects (in terms of college supply shifts), while immigrant students are choosing divisions with better employment opportunities (in terms of labor demand shifts). This is reflected in column (1) by upward omitted variable bias on the crowd-in coefficient and downward omitted variable bias on the crowd-out coefficient. As discussed in section 5, this may mean that immigrant moves across divisions are made with longer-term prospects in mind. A similar interpretation holds for the comparison to column (3), except now the immigrant selection is across states rather than divisions, and based on state market trends rather than division shocks. From the coefficient magnitudes, it appears that immigrant selection is occurring more at the division level than the state level.[51] Additionally, column (4) confirms the expectation from Figure 4 that precise OLS identification is not coming from states with large immigrant flows like California, but rather more so from states like Idaho with low correlations between immigrant inflows.

Turning to 2SLS estimation, comparing columns (1) and (6), both crowd-in and crowd-out estimates are larger in magnitude. This supports the notion that, in terms of response to shorter-term state-year shocks, immigrant labor is endogenously locating in states where the labor market for them is improving, downward biasing the OLS estimates compared to 2SLS, while immigrant students are locating in

[51] Alternatively, it may be that the linear trends at the state level are not sufficient to account for the immigrant selection occurring, if it is a response to state-year shocks.

states with expanding college markets, upwarding biasing the OLS estimates relative to 2SLS. Once division-year fixed effects are included in preferred column (7), the crowd-out coefficient falls to nearly zero, while the crowd-in coefficient is also smaller but still significant, with an elasticity of 0.33. In terms of displacement effects for crowd-out, although not statistically significant, I nevertheless estimate a crowd-out ratio of -0.24. This implies that for every four immigrants enrolled in college, one native does not enroll, which falls in a range consistent with the model as well as with other studies.[52][53] Although the results of column (8) are not significant, an F test fails to reject that the coefficients on the state-specific linear trends are jointly zero. Furthermore, unlike the OLS regressions, the 2SLS results do appear to be nontrivially identified from large immigrant flow states like California, which is not surprising given that these would likely be the states where the historical enclaves better predict actual immigrant inflows. However, as in OLS, exclusion of states like Idaho tends to inflate the standard errors due to increased collinearity of the remaining observations without significantly affecting the point estimates.

Regarding interpretation of the crowding parameters, the model shows that the level of immigrant inflows plays a role in estimated crowd-in (and crowd-out) due to the focus on relatively unskilled immigrant labor as a regressor rather than total labor. Appendix A2 examines how to separately identify the scale effect from the two existing immigrant inflow covariates, determining that the inclusion of a regressor for skilled immigrant labor inflows would capture this effect. In columns (1), (2), (5), and (6) of Table 6, as in Appendix A2, the coefficient on skilled immigrant labor inflows

[52] In her instrumental variables specification, the significant estimates that Hoxby (1998) finds imply a crowd-out ratio ranging from -0.24 to -0.64. Additionally, although not focusing on immigrant-native displacement, Bound and Turner (2007) find in their study of cohort-crowding that a 10 percent state-specific increase in the size of the college-age population decreases the fraction attaining a BA degree by 4 percent, an elasticity of -0.4.

[53] Another advantage of this empirical strategy to examine displacement effects in this paper is that it avoids division bias issues (Borjas 1980) often inherent in the OLS specifications of other displacement studies (e.g., Hoxby 1998, Card 2005).

reflecting the scale effect is positive, and statistically significant in columns (1), (2) and (5). However, in preferred column (6) (i.e., 2SLS with division-year fixed effects), while the magnitude of the crowd-in and crowd-out coefficients are very similar to column (7) of Table 5, the scale effect is now an order of magnitude smaller and no longer significant. The inclusion of three endogenous regressors in column (6) has reduced estimate precision, resulting in crowd-in and crowd-out estimates that are not significant either.

The remaining columns of Table (6) explore an alternative for capturing the scale effect alluded to in the model and Appendix A2: including a regressor for relatively unskilled total labor rather than immigrant labor. However, there are empirical issues with this strategy that make it undesirable, despite its theoretical sensibility. First, if native labor internal migration and location choice is more sensitive to labor market conditions than it is for immigrants, relatively unskilled total labor flows will be more severely correlated with labor demand movements, downward biasing the OLS coefficient on total flows. Columns (3) and (4) suggest that this is the case, as the coefficient on the total labor flow variable is significantly negative. OLS estimates of the impact of total labor flows are therefore relatively uninformative.[54] Meanwhile, the enclave-based instruments utilized for this paper do not have strong theoretical grounding to have predictive power for natives. The fact that they are not weak in columns (7) and (8), given the low percentage of immigrants in total labor supply, causes some suspicion that perhaps immigrant enclaves are predictive for native location decisions for reasons correlated with labor demand, which would serve to bias the 2SLS coefficients downward toward OLS. The negative (albeit not significant) coefficients in columns (7) and (8) seem to suggest this interpretation.

[54]One possibility to address this, however, would be to run a logit model of total college demand, like the current immigrant model, to form predicted flows to utilize in the OLS regressions.

Thus, the previous strategy to separately identify the scale effect is preferable, and given its lack of statistical significance in column (6), I omit the scale effect from any further estimation.

2.7 Sensitivity Analyses

2.7.1 Native Response Heterogeneity

Table 7 explores heterogeneity in the native enrollment response. First, there appear to be differential responses by age. The crowd-in effect is identical in magnitude for young natives ages 18-24, with an elasticity of 0.33. However, it is more precisely measured, suggesting that this is the group accounting for the statistical significance of the effect for natives ages 18-44 in Table 5, column (7). While the results are qualitatively similar for 25-34 year-old natives, the crowd-in effect is smaller in magnitude and the crowd-out effect is larger in magnitude, although neither coefficient is significant.

For female natives, both the crowd-in and crowd-out coefficients are slightly larger in magnitude compared to the baseline estimates, although not significantly so. Nevertheless, this is consistent with more elastic enrollment demand for women than men. Additionally, while crowd-in is larger in magnitude compared to the baseline results for natives on the margin of public enrollment, crowd-out is smaller in magnitude. This possibly reflects more elastic college supply at the public level, which we might expect to be true for at least a subset of public colleges (Bound and Turner 2007). This is also consistent with the model, which predicts that ceteris paribus, as college supply becomes more elastic, the crowd-out effect should decrease in magnitude while the crowd-in effect increases. Given, additionally, the decadal nature of the census data and the longer time horizon of the effects being examined here, more elastic college supply at both public and private institutions is likely.[55]

Table 8 explores the extent of an attainment response that is similar to that of enrollment. Qualitatively, the results are indeed similar. However, now the only statistically significant crowd-in response occurs for 25-34 year-olds. This may be due to the fact that attainment, unlike enrollment, is persistent. So the significant 25-34 year-old native response may reflect both 25-34 year-olds currently experiencing immigrant inflows into their markets, as well as 18-24 year-olds who experienced *earlier* immigrant inflows. Additionally, there is limited evidence in the table, again coming from 25-34 year-olds, that the marginal natives who are responding to immigration are possibly being drawn from the group that has had less than four years of high school education, perhaps reflecting their wages being most affected by the immigrant inflows.

2.7.2 False Experiment

Now focusing on 18-24-year-old natives for all analysis, I conduct a false experiment to examine the extent to which the logit model of immigrant college demand has led to incorrect assignment of immigrants to market shock regressors. To do this, I switch the immigrant student and labor designations from those determined by the procedure outlined in section 5.2. If the logit model has little or no predictive power and is not particularly informative in terms of actual immigrant college demand,[56] then the allocations of immigrants to markets would solely reflect confounding labor demand and college supply shocks implicit in the error term of estimating equation

[55] Additionally, given the estimate of Bound et al. (2004), the retention of students from a state's college market to its labor market is only 30 percent, which would also mitigate estimated crowd-out while leaving crowd-in potentially unaffected, as shown in the model.

[56] Although conducted for 18-44-year-old natives, the results of Table 2, along with a priori reasoning, lend support to the claim that the parameters estimated from the logit procedure are at least consistent, which allows this placebo experiment to be viewed through the lens of classical measurement error (Angrist and Krueger 1999).

(17).[57] Depending on the nature of immigrant market selection based on those confounding shocks, as discussed in section 5, the observed OLS crowding estimates could be biased over- or underestimates of the true parameters.

Under that scenario, switching designations would have little to no effect on the OLS estimates, as these would only reflect the confounding shocks. Similarly for the 2SLS estimates, as long as the instruments were still relevant, the switch in immigrant designation would also have little to no effect on the estimates. However, if the logit model does have predictive power for actual immigrant college demand, then switching immigrant designation should cause crowd-in estimates to be lower in value and crowd-out estimates to be higher in value in both OLS and 2SLS estimates.

Table 9 displays the results of this experiment. Compared to column (1) of Table 7, the OLS estimates in Table 9 show that both crowd-in and crowd-out are significant and of the wrong sign. Similarly, comparing column (2) of Table 7 to the 2SLS estimates of Table 9, which are not significant, the crowd-in estimate is lower in Table 9 and the crowd-out estimate is higher. These results, together, lend support for the logit model-based immigrant designations actually being informative.[58]

2.7.3 Assessing Measurement Error in Immigrant Inflows

I turn now toward the assessment of two other potential sources of measurement error in the immigrant regressors. As noted earlier, the prevalence of small immigration inflows will increase the probability of classical measurement error, attenuating crowd-in and crowd-out estimates. To explore the influence of any such error in the results, I exclude Kansas and Vermont in columns (3) and (4), which have the

[57] This is because switching immigrant designations, as opposed to randomly re-assigning them, preserves any systematic correlation between the immigrant regresssors and the error term.

[58] Alternatively, I also redesignate immigrants randomly (not shown). This removes the predictive power of the logit model, but also eliminates possible correlation between the error term of equation (17) and the immigrant regressors that would bias estimation. OLS results in this case show no significant crowd-in or crowd-out. However, because the instruments are actually still reasonably predictive for the immigrant regressors in this case, the 2SLS estimates are somewhat similar to Table 7 column (2).

smallest flows of exogenous immigrant labor and immigrant students. Focusing again on the 2SLS estimates, and compared to the baseline estimates reposted in column (2), the crowd-in elasticity increases by 0.05, while the crowd-out elasticity, still not significant, has decreased by 0.01. This suggests that, while measurement error from small flows is indeed attenuating the baseline estimates, it does not appear to be doing so substantially.

Additionally, illegal immigrants may only be partially reflected in the census data and yet could be relatively large inflows in some states, thus likely affecting native enrollment. As previously discussed, if legal and illegal immigrant flows of a given type (i.e., labor, students) are positively correlated, then the crowd-in estimate will be upward biased while the crowd-out estimate will be downward biased, assuming that illegal immigrant inflows cause similar price effects on wages and tuition/fees. To evaluate the extent of such omitted variable bias arising from incorrect measurement of immigrant flows, in columns (5) and (6) I exclude Arizona and New Mexico, the two border states that do not have the largest immigrant inflows.[59] Upon doing so, I observe the expected changes in the crowding coefficients, as the magnitudes of both $\hat{\beta}$ and $\hat{\alpha}$ are now reduced. However, the crowd-in result is still significant and remains quite close in magnitude to the column (2) baseline, showing that this source of error in immigrant measurement is not particularly problematic either. The small degree of bias may be due in part to illegal status mitigating the extent of market price effects that undocumented immigrants can exert, perhaps as a result of labor market employment or college market enrollment restrictions. Moreover, as noted earlier, because the omitted variables here are still immigrant-related, even any existing, small bias could be eliminated given a reinterpretation of the existing elasticities as

[59] Unlike California and Texas, where historical immigrant enclaves are particularly strong predictors of actual immigrant inflows due to the consistently high inflows that are the largest for the country as a whole as well.

reflections of both legal and illegal immigration.

2.8 Implications

2.8.1 Counterfactual Simulation

Given the preferred crowd-in estimate of Table 7, specification (2), a simple counterfactual simulation in Table 11 can be used to assess the role of crowd-in to the aggregate change in young native mean college enrollment rates observed from 1970 to 2000. This counterfactual exercise supposes that the immigrant skill mix had stayed constant at its 1970 value.[60] This is consistent with a counterfactual increase in relatively unskilled immigrant labor over the sample period of 120.3 percent, which would have led to an increase in mean enrollment rates of young natives ages 18-24 of 39.7 percent, 18.3 percentage points larger than the observed enrollment rate increase of 21.4 percent.

This is a sizable aggregate effect from crowd-in suggested by this simulation. However, it should be noted that undercounting of undocumented, unskilled immigrants in the census data (which Table 10 suggests might be somewhat of an issue) could be contributing to the large magnitude, as it would overstate both the change in immigrant skill mix during the sample period and the resultant aggregate impact of crowd-in.

2.8.2 Native College Demand Elasticities

The formal model discussed earlier illustrates the theoretical link between the crowding parameters estimated in this paper and underlying structural parameters for the relative unskilled wage and college tuition/fee elasticities of native college enrollment demand. It is thus useful to determine what values of these price elastic-

[60]The actual 1970 immigrant (unskilled/skilled) labor force ratio is 2.4, while the actual 2000 immigrant (unskilled/skilled) labor force ratio is 1.1.

ities, under certain restrictions on the remaining variables in the model, are implied by the crowd-in and crowd-out estimates.

Equations (12) and (15) of the model describe the nonlinear system to be solved for the college demand elasticities. I use the crowding estimates $\hat{\beta}$ and $\hat{\alpha}$ from Table 5, column 7 for β and α, respectively. For $\hat{\alpha}$, I utilize the adjusted, crowd-out ratio estimate in place of the coefficient estimate since the former is more closely aligned with the model, as previously discussed. To obtain a determinate system of two equations (12 and 15) and two unknowns (η^N and ϕ^N), I first make a simplifying assumption that there are no differences in the price sensitivities of native and immigrant college demand. While this is admittedly restrictive, it may be the case that the distributions of natives vs. immigrants across states do not differ systematically with regard to these parameters. More functionally, however, this assumption allows the illustrative exercise to continue without having to further specify immigrant behavior in the model or include additional, nearly identical equations (using other crowding estimates) which might cause the system to be indeterminate.

This leaves seven remaining free parameters from the model whose values need to be fixed: $\pi^N, \gamma^N, \gamma^I, \kappa, \lambda, \psi$, and θ. For π^N, the native share of the total population, I use the native share of the 18-64 population from 1970 to 2000 in the census data. The relative labor supply elasticities for natives and immigrants, γ^N and γ^I respectively, cannot however be similarly observed directly from the census data. To proxy for these variables, I assume that the wage sensitivity of labor supply within-state is the same for natives and immigrants, focusing instead on the wage sensitivity of labor supply between states (i.e., the cross-state migration elasticity) and how it differs for the skilled and unskilled. Separately for natives and immigrants, ages 18 to 64 from 1970 to 2000, I calculate the proportion of unskilled individuals who migrated across states in the five years prior to being surveyed, *relative* to the proportion of skilled individuals who migrated across states in the five years prior to being surveyed. I use these ratios as the proxies for γ^N and γ^I.[61] To fix κ, I assume that college supply is completely wage inelastic. I obtain from Bound et al. (2004) a value for λ, the share of the endogenous equilibrium change in college-enrolled students that remain in the state's labor market as skilled labor.

Lastly, multiple values are assigned for ψ, the tuition/fee elasticity of college supply, and θ, the elasticity of substitution between skilled and unskilled labor, in order to observe how the price elasticities of native college demand change in response. The initial value of ψ is calculated to be inversely related to an estimate from Bound and Turner (2007) for the elasticity of college enrollment with respect to cohort size.[62] The alternate value of ψ is simply assumed to be larger and more elastic. Recall the model predicts, ceteris paribus, that as college supply becomes more elastic, the crowd-in effect increases in magnitude while the crowd-out effect decreases in magnitude. Thus, as ψ increases, in order to observe given values of β and α, natives must be increasingly less wage-sensitive and increasingly more tuition/fee-sensitive. Therefore η^N should decrease in magnitude and ϕ^N should increase in magnitude.

Meanwhile, the two chosen values of θ come from Katz and Murphy (1992) and Card and Lemieux (2001), although it should be noted that their definition of skilled and unskilled differs somewhat from this paper's definition.[63] Recall here the model

[61] In all cases, individual observations are weighted using census person weights. For immigrants, the ratio is calculated using only immigrants who have lived in the U.S. for more than five years. Solely previous country of residence information, rather than cross-state migration activity, is reported in the census for the more recent immigrants.

[62] Specifically, I assume $\psi = \frac{x}{1-x}$, where $x \in [0,1]$ is the Bound and Turner (2007) estimate, equal to 0.79. This ensures $\psi \in [0,\infty)$ and positively correlated to x.

[63] For this exercise, I opt for the Card and Lemieux (2001) estimate from their males-only sample rather than their estimate from the sample pooling men and women. While the latter is more comparable to this paper's sample, the former differs from Katz and Murphy (1992) more starkly and so is more illustrative.

predicts, ceteris paribus, that as relative labor demand becomes more elastic, both the crowd-in and crowd-out effects decrease in magnitude. Thus, as θ increases, in order to observe given values of β and α, natives must be increasingly more wage- and tuition/fee-sensitive. Therefore both η^N and ϕ^N should increase in magnitude.

Table 12 summarizes the results for η^N and ϕ^N from this exercise. Because no closed-form solution to the nonlinear system exists, a numerical solution is determined. Also note from the model that both parameters enter into college demand negatively. Thus, although the derived elasticities are positive, increases in the relative unskilled wage and college tuition/fees both decrease native college demand, as expected. For all values of ψ and θ, natives have fairly wage-sensitive demand for college enrollment, as η^N lies between approximately 5.8 and 8.6. In other words, a 1 percent increase in the relative unskilled wage would decrease the rate of native college enrollment by 5.8 to 8.6 percent. However, for the low and high ψ values respectively, native college demand ranges from being tuition/fee inelastic with a ϕ^N of approximately 0.7, to being quite elastic with a value of approximately 18.1. Thus, a 1 percent college tuition/fee increase would decrease the native college enrollment rate by 0.7 to 18.1 percent. However, only the former estimate is derived using a value of ψ that is not purely hypothetical and so is likely more plausible.

Regardless, the results confirm the predictions of the model and that for reasonable parameter values, the paper's crowding estimates suggest that native college demand is quite wage-sensitive. As θ increases for a given ψ value, η^N increases notably and ϕ^N either increases slightly or remains the same. Meanwhile, as ψ increases for a given θ value, η^N decreases slightly while ϕ^N increases substantially. This further supports the paper's hypothesis that college enrollment slots being flexibly supplied over a decadal time horizon likely does help account for the observed crowding results.

It should also be briefly noted that the implied elasticities of immigrant inflows on wages and tuition/fees (not shown) are non-zero but fairly small. For instance, for the first set of values shown in Table 12 for ψ, θ, η^N, and ϕ^N, a 10 percent increase in relatively unskilled immigrant labor reduces relative unskilled wages by about 0.24 percent. Thus, the model and results imply that while immigrants do indeed affect market prices, these effects need not be particularly large to result in the crowding effects observed because of natives' sensitivity to market price changes.

2.9 Conclusion

This paper estimates how inflows of immigrant students and immigrant labor, by changing the private return to higher education, affect the postsecondary enrollment of natives. I first construct a basic dual-market, supply-demand model to form predictions for how immigration will affect native skill acquisition. Using U.S. Census microdata from 1970 to 2000, I test the predictions of the model by estimating the causal impact of heterogeneous immigrant inflows into local markets on native college enrollment rates in those areas. To isolate the exogenous component of immigrant inflows from endogenous flows that vary with unobserved movements in labor demand and college supply, I use a logit model of immigrant college demand combined with two-stage least squares estimation that utilizes geographic variation in historical immigrant enclaves.

I find that a 1 percent increase in relatively unskilled immigrant labor raises the rate of native college enrollment by 0.33 percent. Meanwhile, a 1 percent increase in immigrant college students lowers the native enrollment rate by 0.04 percent, but this effect is not statistically significant. The lack of a significant crowd-out effect, coupled with the presence of significant crowd-in, is suggestive of fairly wage-sensitive native college demand as well as elastic college supply (particularly at the decadal frequency of the data). The positive, crowd-in effect of immigrant labor inflows is driven primarily by natives ages 18-24, consistent with younger natives having college demand that is more sensitive to returns than the demand of older natives. The results imply that the rise in the average college enrollment rate of young natives between 1970 and 2000 would have been 18 percentage points higher if the composition of immigrant labor inflows had not become increasingly skilled during this period.

These findings provide indirect evidence of how immigrants impact market prices and how natives respond to immigration, as well as implications for education and labor studies that have attempted to directly measure such effects with mixed results. Regarding the former, the focus in the literature on displacement effects of immigrant students on natives may be misguided, at least with regard to higher education and enrollment. Not only do such effects appear to be nonexistent - at least, over longer time horizons - but also ignoring inflows of immigrant labor misses a component of immigration that actually does appear to have notable effects on native skill choice. Further research needs to be done, however, to more extensively determine the degree to which the contrasting results of this study with existing crowd-out studies are being driven by differences in market structure (e.g., the elasticity of college supply) in the long-run vs. the short-run.

Regarding the implications of this work for labor market research of immigration's effect on natives, by emphasizing a more unified framework between labor and education studies, the paper highlights the nature of an endogenous skill acquisition response by natives to inflows of differentially skilled immigrants. This contributes to the burgeoning literature on how such general equilibrium responses may play a role in the seemingly rapid absorption of immigrants into local markets, mitigating direct native wage effects of immigration.

Additionally, through the model and empirics, this paper highlights a framework for identifying price elasticities of native college demand from native enrollment responses to immigration. This allows for a more explicit structural interpretation of the crowding estimates, to determine the extent to which they stem from high or low native college demand sensitivity to tuition/fees and or relative wages. Distinguishing the degree to which natives, and individuals more broadly, respond to each of those distinct components of the private return to higher education has direct implications for whether government policy to increase college enrollment rates should be targeted at lowering the costs or increasing the benefits of college. This is a framework that I will explore in even more depth in future work.

2.10 Figures and Tables

Table 2.1: Estimating Immigrant College Demand in 1960, Marginal Effects

Dependent Variable:	College-Enrolled (0/1)		
	Logit [MLE] (1)	LPM [OLS] (2)	Logit [MLE] (3)
Age	0.001	-0.013	-0.003
	(0.001)	(0.001)***	(0.000)***
Age^2	-0.000	0.000	
	(0.000)***	(0.000)***	
Female	-0.013	-0.011	
	(0.001)***	(0.001)***	
Black non-Hispanic	-0.001	-0.003	
	(0.004)	(0.009)	
Asian non-Hispanic	-0.002	-0.013	
	(0.003)	(0.012)	
Hispanic	-0.008	-0.014	
	(0.006)	(0.006)**	
Other (excl. white non-Hispanic)	-0.001	0.031	
	(0.003)	(0.021)	
Constant	0.002	0.320	0.037
	(0.010)	(0.013)***	(0.002)***
Source country fixed effects	Yes	Yes	No
Observations	58,578	59,084	59,084
Log likelihood	-1917.33		-2265.20
(Pseudo) R^2	0.46	0.10	0.37
% Correct, enrolled=1 (1960)	0.94	0.99	0.95
% Correct, enrolled=0 (1960)	0.88	0.67	0.85
% Correct, enrolled=1 (1970-2000)	0.69	0.84	0.73
% Correct, enrolled=0 (1970-2000)	0.73	0.47	0.68

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: All specifications are estimated in 1960 using U.S. Census data and weight individual observations using census person weights. Specifications (1) and (2) also include source country dummies (not reported). "White Non-Hispanic" is the omitted racial category. Average marginal effects are reported for all specifications, and for the logistic models are calculated at the sample average rate of college enrollment in 1960, $\hat{\beta} * \bar{y} * (1 - \bar{y})$. "% Correct" is the proportion of accurate in-sample (1960) or out-of-sample (1970-2000) predictions for enrolled or not-enrolled individuals. An individual is predicted to be enrolled if $\hat{y} > \bar{y}$, and not enrolled if $\hat{y} \leq \bar{y}$. Heteroskedasticity-robust standard errors in parentheses.

Table 2.2: Comparing Immigrant Differentiation Methods: Baseline (OLS)

Dependent Variable:	Δln[Native College Enrollment as fraction of Native Population]					
	Homog.	Full Spec, Logit	Full Spec, LPM	Age Only, Logit	CDF(Age)	LF & Enroll
	(1)	(2)	(3)	(4)	(5)	(6)
Δln[Immigrants, Total]	0.054					
	(0.044)					
Δln[Immigrants (unskilled / skilled), labor]		0.263	0.248	0.290	0.304	0.097
		(0.090)***	(0.096)**	(0.096)***	(0.093)***	(0.033)***
Δln[Immigrants, students]		-0.143	-0.114	-0.138	-0.139	0.050
		(0.065)**	(0.064)*	(0.056)**	(0.055)**	(0.045)
R^2	0.47	0.54	0.53	0.55	0.55	0.52
Observations	150	150	150	150	150	149

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Native enrollment and population is for ages 18-44. All specifications include Δ(year) fixed effects. Specification (1) uses homogenous, rather than heterogeneous, immigrant inflows as a regressor. Specifications (2)-(4) determine labor and students immigrants from first-step procedures, as noted, to predict immigrant college demand (see Table 1). Specification (5) determines labor and students immigrants using an age cutoff (students if age $\leq$ 28, labor otherwise) from the cumulative distribution function of age for college-enrolled immigrants in 1960 (cutoff is age where $\sim$90% of immigrants are enrolled). Specification (6) uses endogenous information on actual labor force participation and college enrollment to define labor and students immigrants, respectively. Standard errors clustered at the state level are in parentheses.

Table 2.3: Descriptive Statistics for Immigrant Inflows and Native Outcomes

	All Years (1970-2000)							Analysis of Variance		
	Mean (1)	Median (2)	Min (3)	Max (4)	Mean_{1970} (5)	Mean_{2000} (6)	$\Delta_{2000-1970}$ (7)	Year (8)	State (9)	Within (10)
Immigrants (unskilled / skilled), labor	1.830 (1.131)	1.465	0.500	8.128	3.129 (1.320)	1.254 (0.550)	-1.875 (0.192)***	0.48	0.30	0.22
Immigrants, students (millions)	0.054 (0.144)	0.014	0.000	1.242	0.021 (0.042)	0.095 (0.200)	0.074 (0.023)***	0.04	0.76	0.20
Native college enroll. / native population	0.092 (0.018)	0.093	0.046	0.165	0.089 (0.021)	0.099 (0.014)	0.010 (0.002)***	0.14	0.60	0.26
Native population (millions)	1.671 (1.677)	1.163	0.110	9.458	1.304 (1.369)	1.851 (1.774)	0.547 (0.088)***	0.02	0.96	0.02
Native college enroll. / native pop., 18-24	0.237 (0.048)	0.234	0.107	0.390	0.229 (0.050)	0.278 (0.039)	0.049 (0.005)***	0.27	0.51	0.22
Native college enroll. / native pop., 25-34	0.048 (0.015)	0.047	0.010	0.094	0.021 (0.012)	0.055 (0.013)	0.023 (0.001)***	0.37	0.46	0.17
Native female col. enroll. / native pop.	0.093 (0.021)	0.094	0.030	0.158	0.073 (0.018)	0.107 (0.016)	0.034 (0.002)***	0.38	0.38	0.24
Native public col. enroll. / native pop.	0.072 (0.017)	0.072	0.034	0.123	0.062 (0.020)	0.078 (0.014)	0.015 (0.002)***	0.20	0.55	0.25
Some college natives / native population	0.297 (0.068)	0.301	0.153	0.455	0.221 (0.048)	0.357 (0.037)	0.136 (0.004)***	0.59	0.36	0.04
4 years col.+ natives / native population	0.174 (0.055)	0.171	0.064	0.338	0.111 (0.022)	0.215 (0.047)	0.104 (0.005)***	0.56	0.37	0.07
4 years HS natives / native population	0.355 (0.042)	0.353	0.262	0.455	0.364 (0.041)	0.332 (0.039)	-0.032 (0.007)***	0.13	0.60	0.28
< 4 years HS natives / native population	0.174 (0.097)	0.143	0.054	0.470	0.304 (0.076)	0.096 (0.027)	-0.208 (0.008)***	0.72	0.23	0.05
Observations	200	200	200	200	50	50		200	200	200

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Native population is ages 18-44 unless otherwise noted. Descriptive statistics shown for predicted, not actual, immigrant students and labor. Columns (1), (5) and (6) contain variable means and standard deviations (in parentheses), while columns (2)-(4) contain other descriptive statistics, all from the U.S. Census years noted. Column (7) contains differences in means for 2000 and 1970 variables and their significance levels, with standard errors clustered at the state level in parentheses. Columns (8)-(10) analyze the relative sources of variation for each variable.

Table 2.4: First Stage Results (OLS)

Dependent Variable:	Δln[Immigrants (unskilled / skilled), labor]		Δln[Immigrants, students]	
	(1)	(2)	(3)	(4)
[(1960 immigrant share)× Δ(immigrants, labor, unskilled)]	0.022	0.164	0.011	0.263
	(0.018)	(0.048)***	(0.028)	(0.063)***
[(1960 immigrant share)× Δ(immigrants, labor, skilled)]	-0.097	-0.250	-0.091	0.042
	(0.046)**	(0.101)**	(0.065)	(0.162)
[(1960 immigrant share)× Δ(immigrants, students)]	0.062	-0.128	0.072	-0.514
	(0.065)	(0.105)	(0.147)	(0.200)**
[(1960 immigrant share)× Δ(immigrants, labor, unskilled)]^2		-0.012		-0.020
		(0.003)***		(0.004)***
[(1960 immigrant share)× Δ(immigrants, labor, skilled)]^2		0.022		-0.026
		(0.013)*		(0.021)
[(1960 immigrant share)× Δ(immigrants, students)]^2		0.068		0.165
		(0.022)***		(0.044)***
R^2	0.50	0.53	0.45	0.51
Observations	150	150	150	150
F test: instruments=0	1.62	37.57	2.14	49.76
Prob > F	0.20	0.00	0.11	0.00

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Regressors are constructed from individual-level 1960 U.S. Census data and aggregated to the state-year level. All immigrant inflows are predicted, not actual, as described in text (section 5.2). For source country h, state j, and year t, the general form of the regressor is $\sum_h \left(\frac{Immigrants_{hj,1960}}{Immigrants_{h,1960}}\right) \times \Delta\left(Immigrant\_Type\right)_{ht}$. The $Immigrant\_Type$ stocks utilized are: (1) unskilled immigrant labor, (2) skilled immigrant labor, and (3) immigrant students. All specifications include Δ(year) fixed effects. All coefficients and standard errors are multiplied by 100,000. Reported coefficients are thus the marginal effects of increases in the predicted (via 1960 historical shares) flows of 100,000 immigrants on the flows of immigrant labor and students, as specified by the dependent variable. Standard errors clustered at the state level are in parentheses.

Table 2.5: Impact of Immigration on Native College Enrollment

Dependent Variable:	Δln[Native College Enrollment as fraction of Native Population]									
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	2SLS (6)	2SLS (7)	2SLS (8)	2SLS (9)	2SLS (10)
Δln[Immigrants (unskilled / skilled), labor]	0.263	0.137	0.170	0.262	0.263	0.416	0.330	0.620	0.229	0.433
	(0.090)***	(0.105)	(0.168)	(0.090)***	(0.101)**	(0.147)***	(0.179)*	(0.568)	(0.095)**	(0.167)**
Δln[Immigrants, students]	-0.143	-0.087	-0.113	-0.149	-0.141	-0.237	-0.039	-0.096	-0.170	-0.238
	(0.065)**	(0.069)	(0.102)	(0.067)**	(0.069)**	(0.129)*	(0.090)	(0.258)	(0.127)	(0.132)*
Δ(Division × year) fixed effects	No	Yes	No	No	No	No	Yes	No	No	No
Δ(State-specific linear trends)	No	No	Yes	No	No	No	No	Yes	No	No
Excluding CA, FL, NY	No	No	No	Yes	No	No	No	No	Yes	No
Excluding ID, NE, NC	No	No	No	No	Yes	No	No	No	No	Yes
R^2	0.54	0.77	0.80	0.53	0.54	0.51	0.73	0.71	0.52	0.51
Observations	150	150	150	141	141	150	150	150	141	141
Kleibergen-Paap rk (H_0: rank(r)=0)						36.97	23.49	84.34	9.91	37.14
Crowd-out ratio ($\frac{Ntv}{Img}$)	-13.128	-4.035	-13.974	-14.437	-11.687	-6.724	-0.243	-1.535	-67.616	-6.578

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Native enrollment and population is for ages 18-44. All immigrant inflows are predicted, not actual, as described in text (section 5.2). Instruments in specifications (6)-(10) are level and quadratic of $\sum_h \left(\frac{Immigrants_{hj,1960}}{Immigrants_{h,1960}}\right) \times \Delta\left(Immigrant\_Type\right)_{ht}$, for source country h, state j, and year t. The $Immigrant\_Type$ stocks utilized are: (1) unskilled immigrant labor, (2) skilled immigrant labor, and (3) immigrant students. All specifications include Δ(year) fixed effects. Divisions are nine U.S. Census divisions. F-statistic version of the Kleibergen-Paap rk statistic for weak identification, which is robust to a clustered error structure, is reported. Standard errors clustered at the state level are in parentheses. Crowd-out ratio is the estimated number of natives displaced from college enrollment by every immigrant enrolled. To determine this, two separate regressions are run that vary the specification noted by replacing the dependent variable Δln[Native College Enrollment as fraction of Native Population] with: (a) Δ[Native College Enrollment as fraction of Native Population]; and (b) Δ[Immigrant College Enrollment as fraction of Native Population]. The crowd-out ratio is calculated as the ratio of Δln[Immigrants, students] coefficients from those specifications [i.e., (a)/(b)].

Table 2.6: Scale Effect in Impact of Immigration

Dependent Variable:	Δln[Native College Enrollment as fraction of Native Population]							
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	2SLS (5)	2SLS (6)	2SLS (7)	2SLS (8)
Δln[Immigrants (unskilled / skilled), labor]	0.289 (0.084)***	0.171 (0.099)*			0.382 (0.156)**	0.341 (0.330)		
Δln[Immigrants, students]	-0.233 (0.068)***	-0.167 (0.070)**	-0.067 (0.053)	-0.048 (0.051)	-0.339 (0.161)**	-0.049 (0.252)	-0.051 (0.070)	0.087 (0.062)
Δln[Immigrants, skilled labor]	0.191 (0.082)**	0.183 (0.068)**			0.272 (0.136)*	0.013 (0.297)		
Δln[Total (unskilled / skilled), labor]			-0.372 (0.143)**	-0.327 (0.137)**			-0.382 (0.231)	-0.187 (0.190)
Δ(Division $\times$ year) fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.56	0.78	0.50	0.77	0.55	0.73	0.50	0.74
Observations	150	150	150	150	150	150	150	150
Kleibergen-Paap rk (H_0: rank(r)=0)					47.18	456.47	46.04	42.72

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Native enrollment and population is for ages 18-44. All immigrant inflows are predicted, not actual, as described in text (section 5.2). Δln[Immigrants, skilled labor] added as an additional endogenous variable in specifications (1)-(2) and (5)-(6), while Δln[Total (unskilled/skilled), labor] replaces Δln[Immigrants (unskilled/skilled), labor] as an endogenous variable in specifications (3)-(4) and (7)-(8). Δln[Total (unskilled/skilled), labor] is composed of predicted immigrant labor inflows and native population ages 45-64. Instruments in specifications (5)-(8) are level and quadratic of $\sum_h \left(\frac{Immigrants_{hj,1960}}{Immigrants_{h,1960}}\right) \times \Delta\left(Immigrant\_Type\right)_{ht}$, for source country h, state j, and year t. The $Immigrant\_Type$ stocks utilized are: (1) unskilled immigrant labor, (2) skilled immigrant labor, and (3) immigrant students. All specifications include Δ(year) fixed effects. Divisions are nine U.S. Census divisions. F-statistic version of the Kleibergen-Paap rk statistic for weak identification, which is robust to a clustered error structure, is reported. Standard errors clustered at the state level are in parentheses.

Table 2.7: Heterogeneity in Native Education Response to Immigration

Dependent Variable:	Δln[Native College Enrollment as fraction of Native Population]			
	OLS		2SLS	
	Immig. Labor	*Immig. Students*	*Immig. Labor*	*Immig. Students*
Natives 18-24 Years Old	(1)	(1)	(2)	(2)
	0.166	-0.070	0.330	0.001
	(0.120)	(0.075)	(0.149)**	(0.078)
R^2	0.74		0.70	
Observations	150		150	
Natives 25-34 Years Old	(3)	(3)	(4)	(4)
	0.160	-0.254	0.271	-0.141
	(0.108)	(0.069)***	(0.223)	(0.118)
R^2	0.71		0.69	
Observations	150		150	
Female Natives	(5)	(5)	(6)	(6)
	0.172	-0.134	0.336	-0.058
	(0.133)	(0.091)	(0.170)*	(0.134)
R^2	0.73		0.70	
Observations	150		150	
Natives, Public Colleges	(7)	(7)	(8)	(8)
	0.144	-0.122	0.356	-0.037
	(0.107)	(0.078)	(0.188)*	(0.105)
R^2	0.81		0.77	
Observations	150		150	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Native enrollment and population differs across specifications as noted in table and is for ages 18-44 unless otherwise stated. All immigrant inflows are predicted, not actual, as described in text (section 5.2). Instruments in specifications (2), (4), (6), (8), (10), and (12) are level and quadratic of $\sum_h \left(\frac{Immigrants_{hj,1960}}{Immigrants_{h,1960}}\right) \times \Delta\Big(Immigrant\_Type\Big)_{ht}$, for source country h, state j, and year t. The $Immigrant\_Type$ stocks utilized are: (1) unskilled immigrant labor, (2) skilled immigrant labor, and (3) immigrant students. All specifications include Δ(year) and Δ(division × year) fixed effects, where divisions are nine U.S. Census divisions. Standard errors clustered at the state level are in parentheses.

Table 2.8: Impact of Immigration on Native Educational Attainment

Dependent Variable:	Δln[Native Educational Attainment as fraction of Native Population]							
	Some College		4 Years College +		4 Years HS		Less than 4 Years HS	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Natives 18-44 Years Old	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δln[Immigrants (unskilled	0.036	0.201	0.043	0.066	-0.024	0.161	0.019	-0.009
/ skilled), labor]	(0.027)	(0.129)	(0.055)	(0.119)	(0.041)	(0.087)*	(0.046)	(0.175)
Δln[Immigrants, students]	-0.011	-0.077	0.014	-0.005	-0.008	-0.085	-0.038	-0.004
	(0.024)	(0.065)	(0.031)	(0.072)	(0.024)	(0.046)*	(0.024)	(0.095)
R^2	0.77	0.67	0.88	0.88	0.65	0.43	0.86	0.86
Observations	150	150	150	150	150	150	150	150
Natives 18-24 Years Old	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Δln[Immigrants (unskilled	0.066	0.236	0.100	0.287	-0.113	-0.064	0.064	-0.306
/ skilled), labor]	(0.044)	(0.146)	(0.081)	(0.315)	(0.064)*	(0.120)	(0.045)	(0.249)
Δln[Immigrants, students]	-0.021	-0.042	0.017	0.125	0.071	0.005	-0.099	0.081
	(0.030)	(0.070)	(0.049)	(0.121)	(0.032)**	(0.057)	(0.024)***	(0.135)
R^2	0.67	0.60	0.53	0.45	0.71	0.70	0.52	0.22
Observations	150	150	150	150	150	150	150	150
Natives 25-34 Years Old	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
Δln[Immigrants (unskilled	0.037	0.310	0.001	0.105	0.019	0.216	-0.006	-0.116
/ skilled), labor]	(0.043)	(0.173)*	(0.038)	(0.113)	(0.047)	(0.096)**	(0.070)	(0.208)
Δln[Immigrants, students]	-0.030	-0.122	0.069	0.047	-0.024	-0.134	-0.067	-0.067
	(0.024)	(0.099)	(0.037)*	(0.046)	(0.028)	(0.068)*	(0.045)	(0.145)
R^2	0.86	0.77	0.92	0.91	0.68	0.56	0.77	0.76
Observations	150	150	150	150	150	150	150	150

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Native attainment and population differs across specifications for ages noted in table. All immigrant inflows are predicted, not actual, as described in text (section 5.2). Instruments in 2SLS specifications are level and quadratic of $\sum_h \left(\frac{Immigrants_{hj,1960}}{Immigrants_{h,1960}}\right) \times \Delta\Big(Immigrant\_Type\Big)_{ht}$, for source country h, state j, and year t. The $Immigrant\_Type$ stocks utilized are: (1) unskilled immigrant labor, (2) skilled immigrant labor, and (3) immigrant students. All specifications include Δ(year) and Δ(division × year) fixed effects, where divisions are nine U.S. Census divisions. Standard errors clustered at the state level are in parentheses.

Table 2.9: False Experiment - Switching Immigrant Designations

Dependent Variable:	Δln[Native College Enrollment as fraction of Native Population]	
	OLS	2SLS
	(1)	(2)
Δln[Immigrants (unskilled / skilled), labor]	-0.106	0.171
	(0.044)**	(0.111)
Δln[Immigrants, students]	0.140	0.057
	(0.054)**	(0.102)
R^2	0.76	0.60
Observations	150	150
Kleibergen-Paap rk (H_0: rank(r)=0)		72.84

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Native enrollment and population is for ages 18-24. All immigrant inflows are predicted, not actual, as described in text (section 5.2). In above specifications, immigrant students are redesignated as immigrant labor (with skill level subsequently determined), while immigrant labor is redesignated as immigrant students. Instruments in specification (2) are level and quadratic of $\sum_j \left(\frac{Immigrants_{ji,1960}}{Immigrants_{j,1960}}\right) \times \Delta\Big(Immigrant\_Type\Big)_{jt}$, for source country j, state i, and year t. The $Immigrant\_Type$ stocks utilized are: (1) unskilled immigrant labor, (2) skilled immigrant labor, and (3) immigrant students. F-statistic version of the Kleibergen-Paap rk statistic for weak identification, which is robust to a clustered error structure, is reported. All specifications include division $\times$ year fixed effects, where divisions are nine U.S. Census divisions. Standard errors clustered at the state level are in parentheses.

Table 2.10: Influence of Measurement Error in Impact of Immigration

Dependent Variable:	Δln[Native College Enrollment as fraction of Native Population]					
	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Δln[Immigrants (unskilled / skilled), labor]	0.166	0.330	0.181	0.377	0.159	0.312
	(0.120)	(0.149)**	(0.124)	(0.159)**	(0.126)	(0.153)**
Δln[Immigrants, students]	-0.070	0.001	-0.082	-0.012	-0.057	0.028
	(0.075)	(0.078)	(0.077)	(0.080)	(0.074)	(0.075)
Excluding KS, VT (small flows)	No	No	Yes	Yes	No	No
Excluding AZ, NM (border)	No	No	No	No	Yes	Yes
R^2	0.74	0.70	0.74	0.68	0.75	0.71
Observations	150	150	144	144	144	144
Kleibergen-Paap rk (H_0: rank(r)=0)		23.49		27.19		28.14

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Native enrollment and population is for ages 18-24. All immigrant inflows are predicted, not actual, as described in text (section 5.2). Instruments in specifications (2) and (4) are level and quadratic of $\sum_h \left(\frac{Immigrants_{hj,1960}}{Immigrants_{h,1960}}\right) \times \Delta\Big(Immigrant\_Type\Big)_{ht}$, for source country h, state j, and year t. The $Immigrant\_Type$ stocks utilized are: (1) unskilled immigrant labor, (2) skilled immigrant labor, and (3) immigrant students. F-statistic version of the Kleibergen-Paap rk statistic for weak identification, which is robust to a clustered error structure, is reported. All specifications include Δ(year) and Δ(division × year) fixed effects, where divisions are nine U.S. Census divisions. Standard errors clustered at the state level are in parentheses.

Table 2.11: Simulation - Effect on Young Native College Enrollment of the 1970-2000 Change in Immigrant Labor Skill Composition

Native Population	Estimated Crowd-in Elasticity (1)	Counterfactual % Change in Mean Immigrants (unskilled / skilled), labor 1970-2000 (2)	Simulated % Change in Mean Native Enrollment Rate 1970-2000 (3)	Observed % Change in Mean Native Enrollment Rate 1970-2000 (4)	$\frac{\lvert\text{Simulated }\%\Delta\rvert}{\lvert\text{Observed }\%\Delta\rvert}$ (5)
Natives ages 18-24	0.330	120.3%	39.7%	21.4%	1.855

Notes: Descriptive statistics and a coefficient from Tables 2 and 7, respectively, were used in this computation. Counterfactual supposes that the immigrant skill mix had stayed constant at its 1970 value. Actual 1970 immigrant (unskilled/skilled) labor force ratio is 2.4, while actual 2000 immigrant (unskilled/skilled) labor force ratio is 1.1.

Table 2.12: Implied Wage and Tuition/Fee Elasticities of Native College Enrollment Demand

Parameter	Value				Source
π^N	0.90				U.S. Census and author's calculation
γ^N	0.85				Author's calculation
γ^I	0.90				Author's calculation
κ	0				Assumed
λ	0.30				Bound et al. (2004)
β	0.330				Table 5, column 7
α	-0.243				Table 5, column 7
ψ	3.7		100		Bound & Turner (2007) and author's calc.; Assumed
θ	1.4	2.5	1.4	2.5	Katz & Murphy (1992); Card & Lemieux (2001)
η^N	5.770	8.584	5.769	8.582	Equations 10-15, numerical solution
ϕ^N	0.669	0.669	18.081	18.093	Equations 10-15, numerical solution

Notes: To obtain the numerical solutions for η^N and ϕ^N, it is assumed that $\eta^N = \eta^I$ and that $\phi^N = \phi^I$. While all values of η^N and ϕ^N above are positive, note that both parameters enter negatively into college demand (see section 3.3.1). Thus, increases in the relative unskilled wage and college tuition/fees both decrease native college demand. π^N is the native share of the population. γ^N and γ^I are the relative labor supply elasticities for natives and immigrants, respectively. κ is the wage elasticity of college supply, while λ is the share of the endogenous equilibrium change in college-enrolled students that remain in the state's labor market as skilled labor. β and α are estimates of crowd-in and crowd-out, respectively, where α here is the adjusted crowd-out ratio estimate. Finally, ψ is the tuition/fee elasticity of college supply, while θ is the elasticity of substitution between skilled and unskilled labor. See text for further details on parameters, model, and author's calculations.

Figure 2.1: Inflow of Relatively Unskilled Immigrant Labor

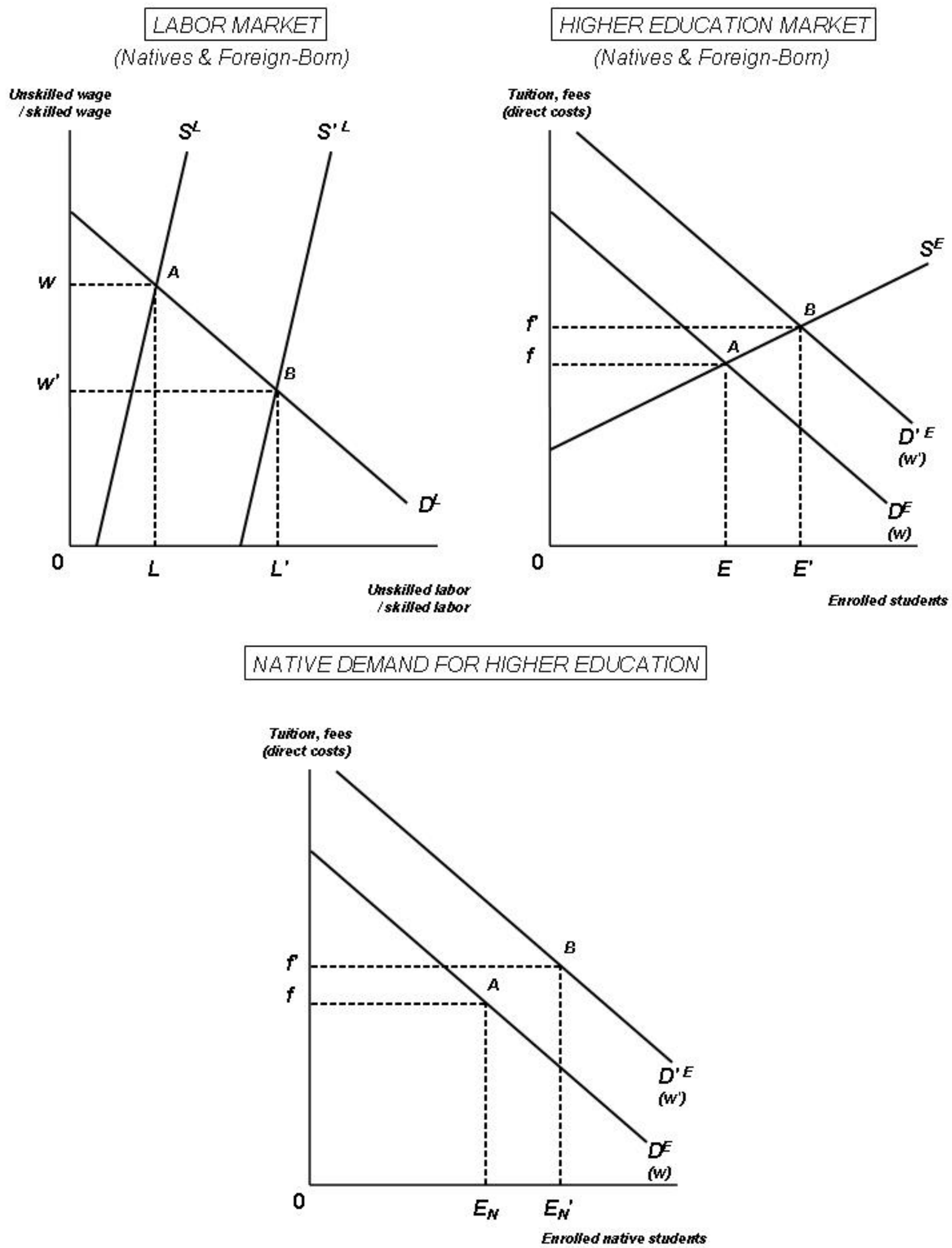


Figure 2.2: Inflow of Immigrant Students

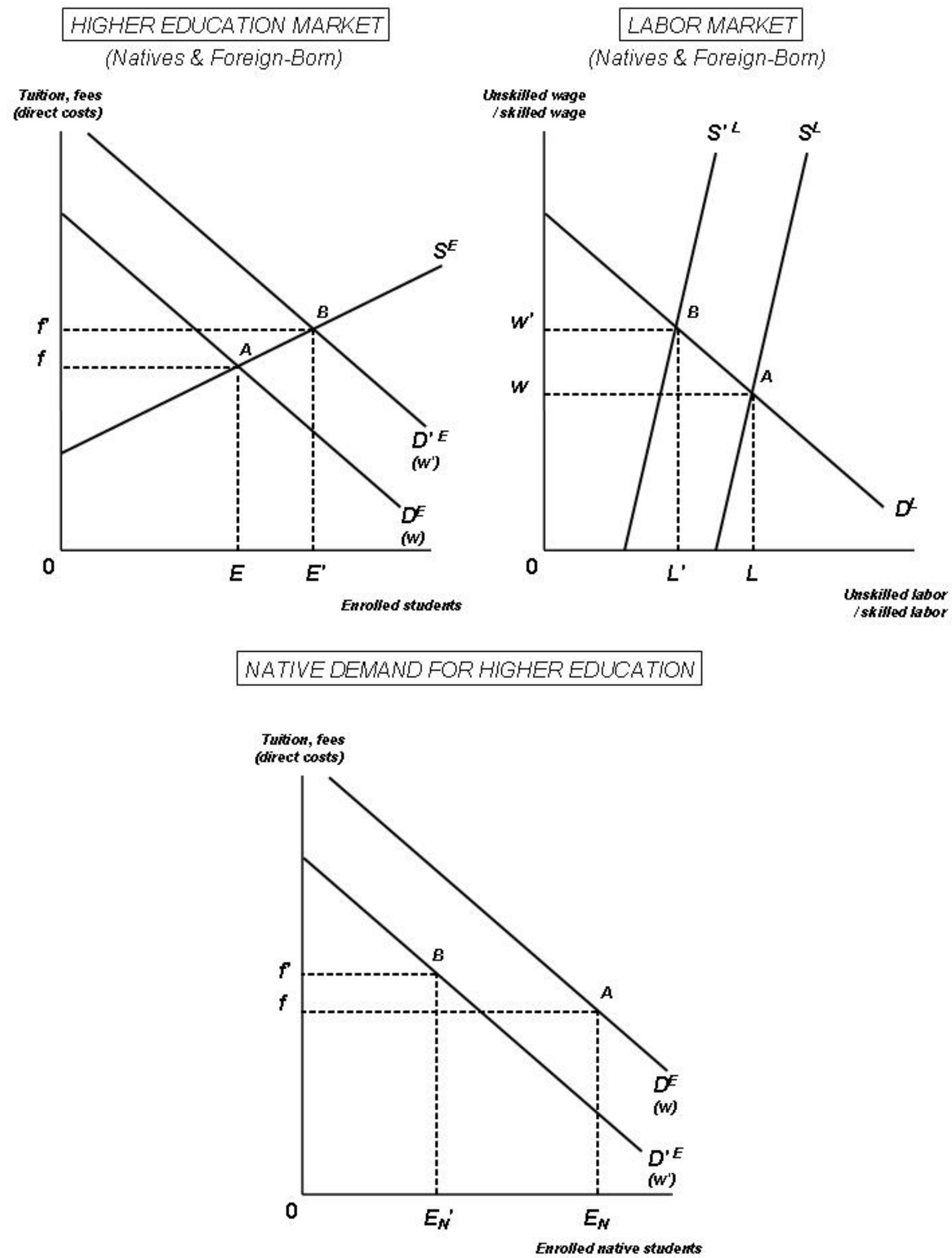
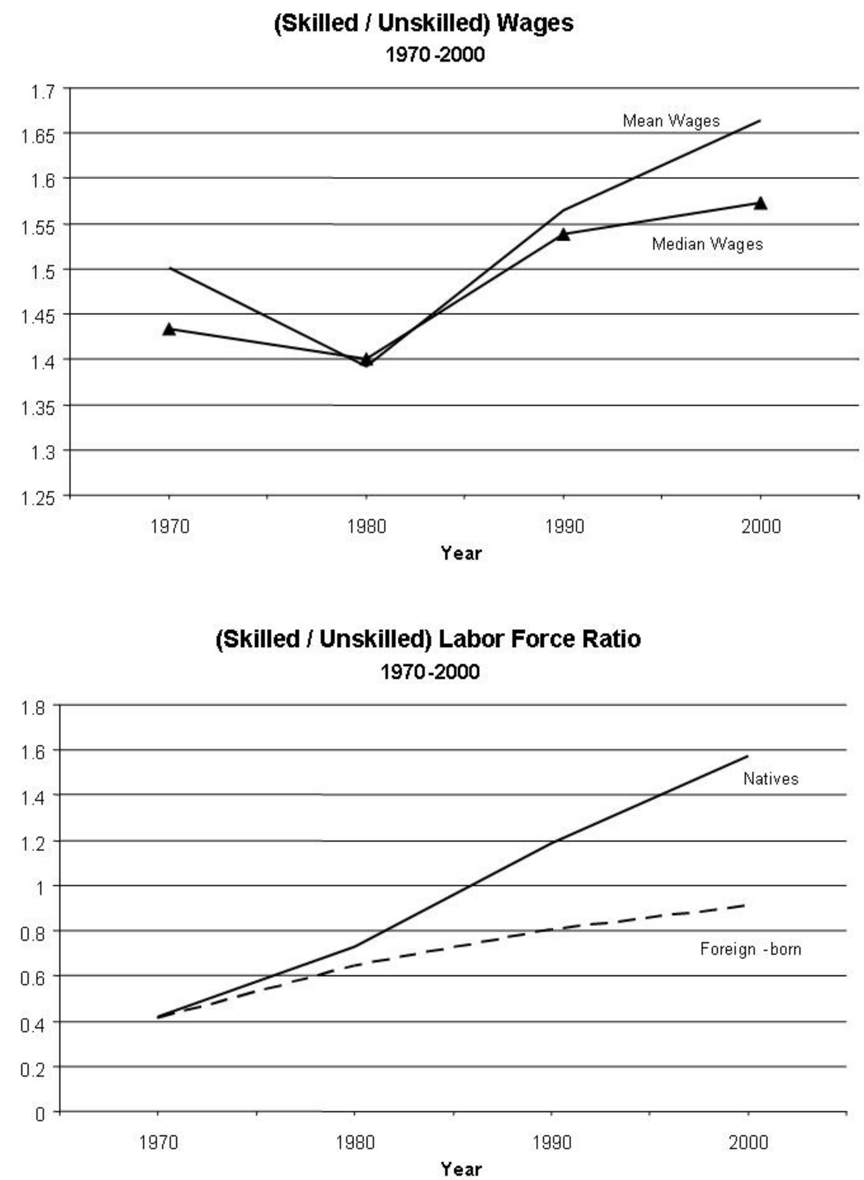
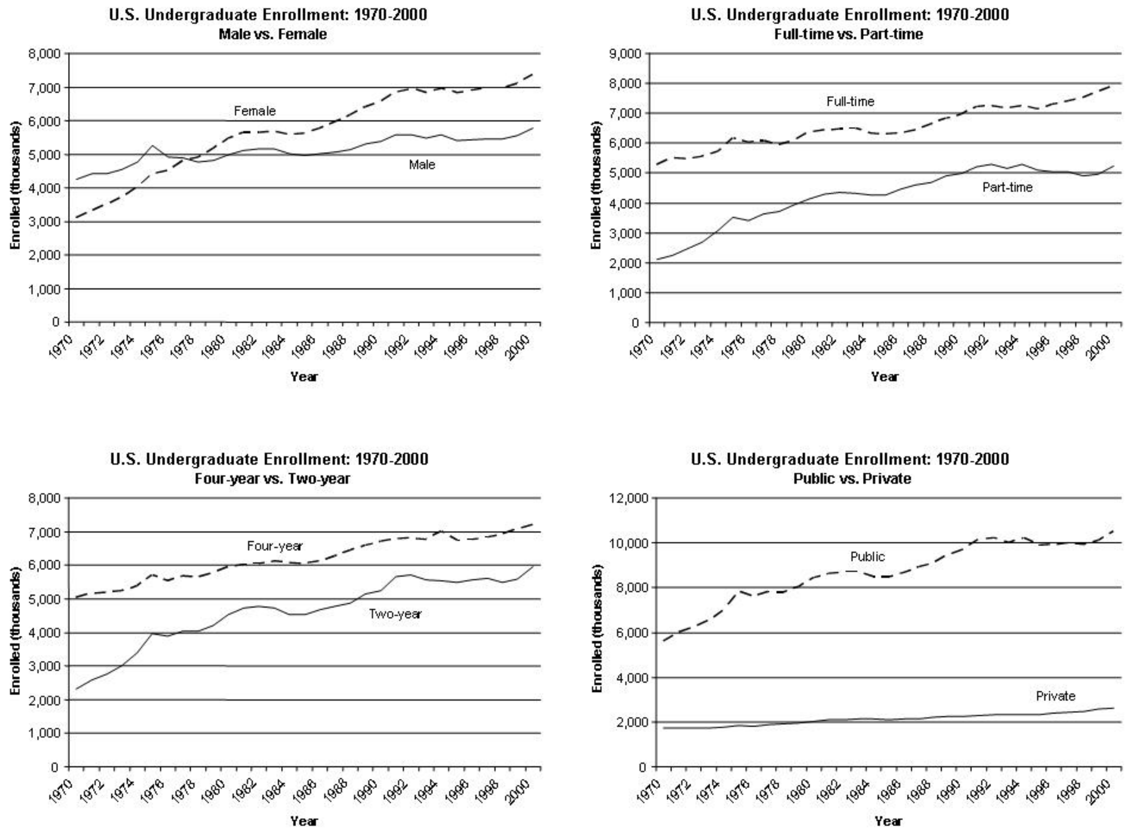


Figure 2.3: Relative Skilled Wage and Relative Supply of Skill



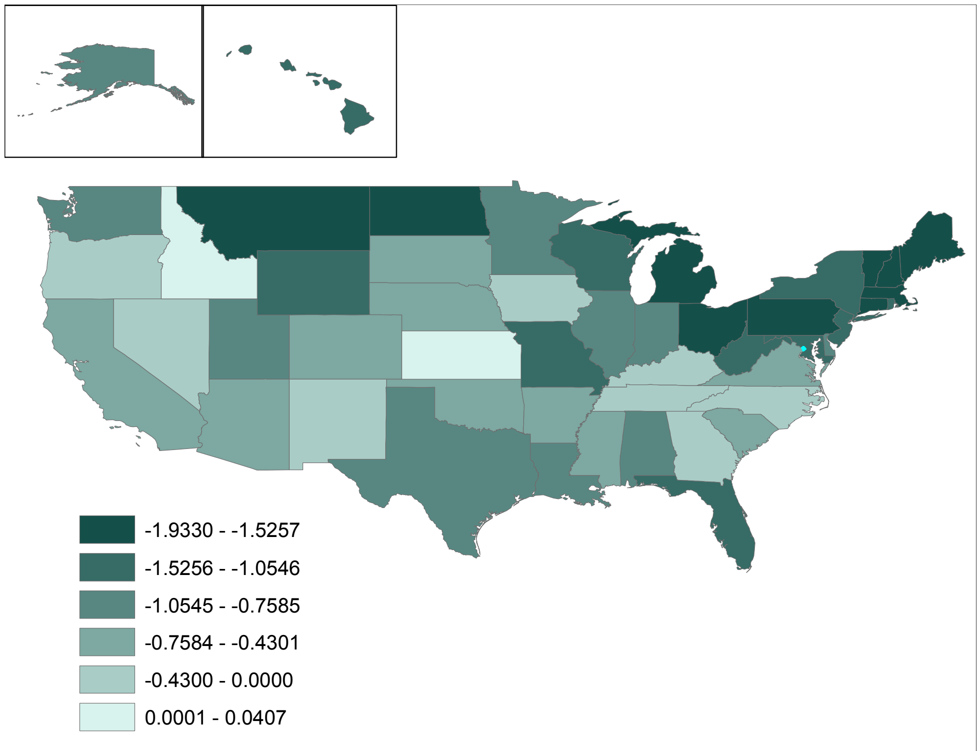
Source: U.S. Census 1970-2000 and author's calculations. To be nationally representative, trends in top and bottom panel are constructed weighting individual observations with census person weights. *Top panel:* wages are estimated by dividing wage/salary income (in constant 1995 USD using Bureau of Labor Statistics Consumer Price Index for All Urban Consumers (CPI-U)) by hours worked last week (1970) or usual hours worked per week (1980-2000). Sample is restricted to employed 18-64 year-old individuals with non-missing, non-zero earnings and hours, and who are neither in school nor living in group quarters.

Figure 2.4: Trends in U.S. College Enrollment by Group



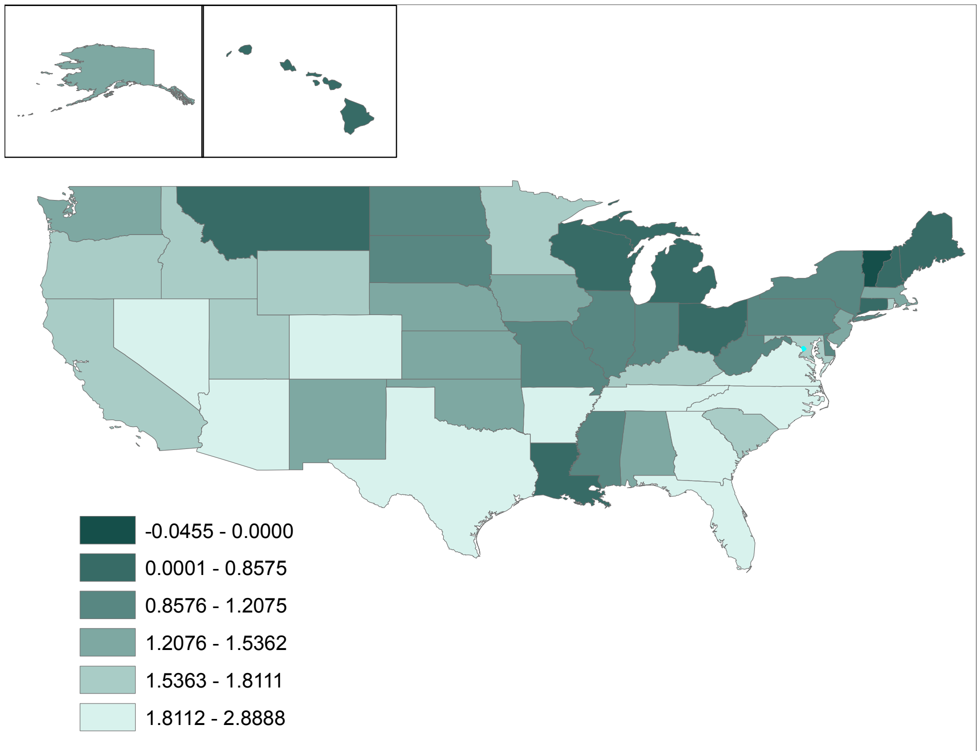
Source: U.S. Department of Education, National Center for Education Statistics (NCES), *Digest of Education Statistics, 2007.*

Figure 2.5: Geographic Variation of Predicted Immigrant Inflows 1970-2000, Δln[Immigrants (unskilled / skilled), labor]



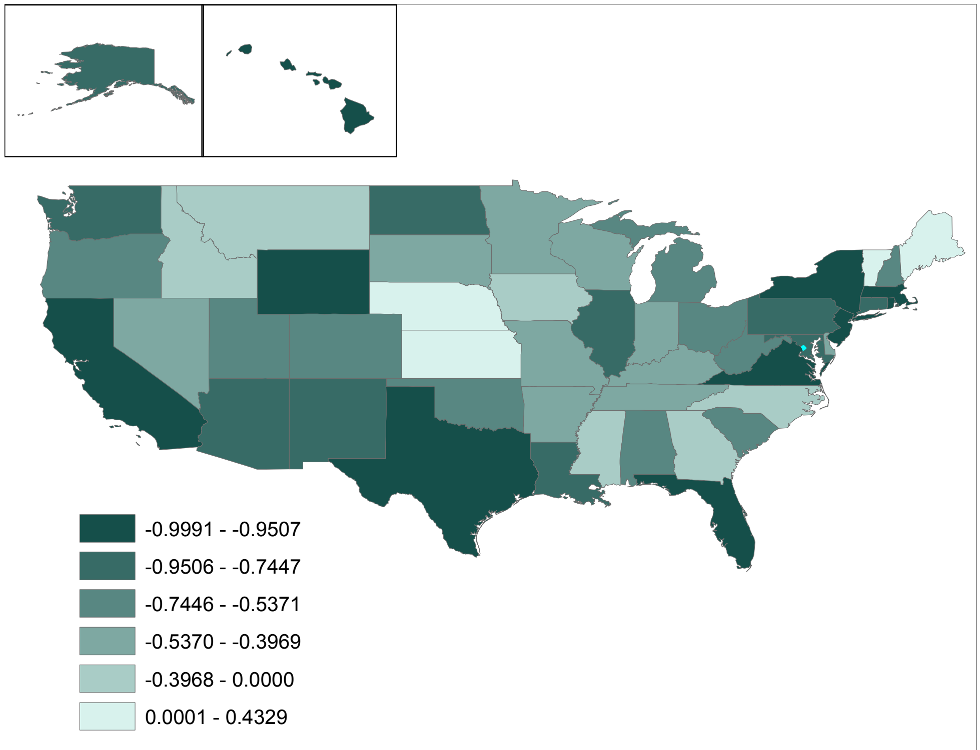
Source: U.S. Census 1970-2000 and author's calculations. Immigrant inflows are predicted, not actual, as described in text (section 5.1). Differences shown for each state are between 2000 and 1970 values of the variable (i.e. $\Delta \equiv \Delta_{2000-1970}$).

Figure 2.6: Geographic Variation of Predicted Immigrant Inflows 1970-2000, Δln[Immigrants, students]



Source: U.S. Census 1970-2000 and author's calculations. Immigrant inflows are predicted, not actual, as described in text (section 5.1). Differences shown for each state are between 2000 and 1970 values of the variable (i.e. $\Delta \equiv \Delta_{2000-1970}$).

Figure 2.7: Geographic Variation of Predicted Immigrant Inflows 1970-2000, $\rho_{\mathbf{labor,students}}$



Source: U.S. Census 1970-2000 and author's calculations. Immigrant inflows are predicted, not actual, as described in text (section 5.1). Correlations shown for each state, $\rho_{labor,students}$, are between ln[Immigrants (unskilled/skilled), labor] and ln[Immigrants, students], over all years 1970-2000.

2.11 Appendix

Appendix A1: Latent Variable Model of Immigrant College Demand

For any immigrant i, let

$y_i^* \equiv$ college demand (latent),

$y_i \equiv$ college-enrolled (observed), where

$$y_i^* = \mathbf{x'}_i \boldsymbol{\vartheta} + \varepsilon_i,$$

and

$$y_i = \begin{cases} 1 & \text{if } \ y_i^* > c \\ 0 & \text{if } \ y_i^* \leq c \end{cases}.$$

$\mathbf{x'}_i$ is a vector of individual characteristics (e.g., age - see section 5.2 for details), ε_i is the error term, and c is some unknown threshold value. Further allowing for differential observations and innovations by state j yields

$$y_{ij}^* = \mathbf{x'}_{ij} \boldsymbol{\vartheta} + \varepsilon_{ij},$$

where

$$\varepsilon_{ij} = \delta_{ij} + \underbrace{\omega_j + \varphi_j}_{market\ shocks}.$$

$\omega_j = -\zeta_j$ is a *negative* labor demand shock, φ_j is a *positive* college supply shock, and δ_{ij} is the idiosyncratic component of the composite error. [64]

Given that the population of interest is immigrants and *not* the foreign-born, non-random sample selection of immigrants into U.S. states is not problematic.[65]

[64] Assuming native labor supply and native college demand are determined endogenously by y_{ij}^*, then y_{ij}^* completely characterizes labor supply and college demand.

[65] Demand for immigration into particular states can similarly be thought of as a latent variable, m_{ij}^*, where only binary m_{ij} (immigration into state j) is observed, and $m_{ij}^* = \mathbf{x'}_{ij} \boldsymbol{\varrho} + \tau_{ij}$. If τ_{ij} is a composite error that is similarly a function of shocks ω_j and φ_j, then correlation $\rho_{\varepsilon\tau} \neq 0$, and sample selection bias prevents consistent estimation of the $\boldsymbol{\varrho}$'s for the foreign-born population via OLS without further corrections. Note that labor demand or college supply shocks having an influence on immigration demand is a sufficient but not necessary condition for $\rho_{\varepsilon\tau} \neq 0$ and the existence of sample selection bias.

Consistent estimation of the $\boldsymbol{\vartheta}$'s $\Leftrightarrow E(x_{ij}\varepsilon_{ij}) = 0$.[66]

[66] A violation of this exogeneity condition would occur, for instance, if New York universities added 10,000 additional enrollment seats specifically for non-traditional college students ages 25 and older.

Appendix A2: Scale Effect in Impact of Immigration

As shown in section 3, because one of the regressors in the main empirical specifications is Δln[Immigrants (unskilled / skilled), labor] rather than Δln[Total (unskilled / skilled), labor], the level/scale of immigrant (labor) inflows also affects native college enrollment. Because this effect operates primarily through relative wages (which may then, in turn, impact tuition prices through induced changes in native college demand), I can focus solely on this mechanism.

I return to the relative unskilled wage function from section 3, now in levels rather than log changes and with slightly more generic notation g for the wage function, for simplicity:

$$w = g(S_L, D_L) \text{ , where } \tfrac{\partial g}{\partial S_L} \geq 0 \text{ , } \tfrac{\partial g}{\partial D_L} \leq 0 \text{ .}$$

Note that S_L and D_L are the relative supply and demand for unskilled (u) labor, respectively. The magnitudes of the above comparative statics, respectively, are inversely related to the relative labor supply and relative labor demand elasticities, γ and θ. For natives N and immigrants I, recall

$$L_k = N_k + I_k, \; k = \{u, s\} \text{ ,}$$

$$S_L^I = I_u / I_s \text{ ,}$$

where s is skilled.[67] Want to solve for the sign of $\frac{\partial w}{\partial S_L^I}$:

$$\frac{\partial w}{\partial S_L^I} = \frac{\partial g}{\partial S_L}\frac{\partial S_L}{\partial S_L^I}.$$

Note that

$$S_L \equiv \frac{L_u}{L_s} = \frac{N_u + I_u}{N_s + I_s} = \frac{(N_u/I_s) + (I_u/I_s)}{(N_s/I_s) + 1} = \frac{(N_u/I_s)}{(N_s/I_s) + 1} + \frac{S_L^I}{(N_s/I_s) + 1}.$$

$$\Longrightarrow \frac{\partial S_L}{\partial S_L^I} = 0 + \frac{1}{(N_s/I_s) + 1} = \frac{I_s}{N_s + I_s} = \frac{I_s}{L_s} \in [0, 1] \Longleftrightarrow \frac{\partial w}{\partial S_L^I} \geq 0 \ ,$$

which is consistent with section 3.

Call $\chi_s^I = \frac{I_s}{L_s} \equiv$ skilled immigrant share of total skilled labor. Thus, if χ_s^I (or a proxy, specified instead in logs) were included in a regression to account for the scale effect, the expected coefficient sign would be weakly positive. However, in the denominator of χ_s^I, N_s is endogenous, as it is related to the outcome of interest. Still, χ_s^I can at least be approximated in regressions with I_s to account for the scale effect. In this case, the expected coefficient sign is still weakly positive, since

$$\frac{\partial \chi_s^I}{\partial I_s} = \frac{1}{N_s + I_s}\left[1 - \frac{I_s}{N_s + I_s}\right] \geq 0.$$

[67] Unlike section 3, because all quantities in this appendix are quantities supplied, superscripts indicating such are suppressed.

Appendix B: Additional Analyses

Table 2.13: Immigrant Covariate Averages in 1960, by College Enrollment Status

	All	College-Enrolled [CE]	Not College-Enrolled [NCE]	Δ_{CE-NCE}
	(1)	(2)	(3)	(4)
Age	46.84	22.92	47.10	-24.181
	(12.74)	(4.05)	(12.55)	(0.167)***
Female	0.53	0.33	0.53	-0.201
	(0.50)	(0.47)	(0.50)	(0.019)***
White non-Hispanic	0.84	0.71	0.84	-0.138
	(0.36)	(0.46)	(0.36)	(0.018)***
Black non-Hispanic	0.01	0.04	0.01	0.023
	(0.11)	(0.19)	(0.11)	(0.007)***
Asian non-Hispanic	0.03	0.09	0.03	0.053
	(0.18)	(0.28)	(0.18)	(0.011)***
Hispanic	0.11	0.13	0.11	0.028
	(0.31)	(0.34)	(0.31)	(0.013)**
Other	0.003	0.037	0.003	0.034
	(0.05)	(0.19)	(0.05)	(0.007)***
Sample probabilities	1.00	0.01	0.99	
Observations	59,084	648	58,436	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Columns (1)-(3) contain covariate means and standard deviations (in parentheses) from the U.S. Census in 1960. Column (4) contains differences in means for enrolled and not-enrolled immigrants and their significance levels, with heteroskedasticity-robust standard errors in parentheses.

Table 2.14: College Demand Index 1970-2000, Quintiles

	Lowest (1)	2nd (2)	3rd (3)	4th (4)	Highest (5)
Age (mean)	57.1	45.5	36.9	29.6	23.6
Female (%)	0.59	0.55	0.54	0.52	0.35
Black non-Hispanic (%)	0.04	0.06	0.06	0.06	0.08
Asian non-Hispanic (%)	0.15	0.20	0.21	0.20	0.27
Hispanic (%)	0.30	0.38	0.46	0.50	0.39
Other (excl. white non-Hispanic) (%)	0.01	0.02	0.02	0.02	0.03
Source country A1 (country % in quintile)	Mexico 0.16	Mexico 0.22	Mexico 0.30	Mexico 0.34	Mexico 0.18
Source country A2 (country % in quintile)	Canada 0.07	Canada 0.05	Philippines 0.05	Philippines 0.04	Philippines 0.05
Source country A3 (country % in quintile)	Italy 0.07	Philippines 0.04	Cuba 0.03	Vietnam 0.03	India 0.05
Source country B1 (quintile % in country)	Estonia 0.49	North Korea 0.34	Kiribati 0.31	Turks & Caicos 0.50	UAE 0.88
Source country B2 (quintile % in country)	Lithuania 0.47	Botswana 0.33	Anguilla 0.29	Gambia 0.39	Oman 0.84
Source country B3 (quintile % in country)	Madeira 0.46	British Vir. 0.32	Guadeloupe 0.29	Zambia 0.36	Qatar 0.79
Observations (actual)	442,034	458,244	471,055	454,028	464,563

Notes: U.S. Census 1970-2000 and author's calculations. See text for details on the construction of the college demand index. Individual observations utilized for descriptive statistics above are weighted using census person weights. Source country B rankings are based on weighted proportions for countries with at least 10 immigrants (actual, not weighted) over 1970-2000.

CHAPTER III

Natural Disasters, Foreign Aid, and Economic Growth

3.1 Introduction

The occurrence of natural disasters often results in sizeable financial and human losses. A 1969 flood in Singapore lead to nearly $20 million in damages (1995 USD)[1] and left 3,100 homeless, while a 1985 earthquake in Mexico is estimated to have killed 9,500-35,000 people and caused $4-6 billion in damages (Chia 1971, USGS 2008).[2] Over more than two decades at the end of the 20th century, natural disasters accounted for nearly $900 billion in damages, over 1,500 deaths and more than 2,100 injuries worldwide (see Table 1).

However, despite these considerable and immediate disaster-related losses, the eventual economic toll of these phenomena is often mitigated by the response of international financial flows such as foreign aid. Bilateral foreign aid flows to Singapore increased by over $100 million following its 1969 flood, while Mexico experienced a $200 million increase in bilateral aid in the wake of its 1985 earthquake (see Figure 1). Meanwhile, large capital inflows such as these have long been theorized in economic growth models to have positive implications for per capita economic growth.

[1] Unless otherwise noted, all monetary amounts in this paper will similarly be reported in constant 1995 U.S. dollars.

[2] The 1969 flood in Singapore produced 15 inches (0.38 meters) of rainfall, which was a national record for 24-hour rainfall, while the 1985 Mexico earthquake was registered as magnitude 8.0 on the Richter scale, tied for the largest worldwide that year.

However, the empirical evidence for any such positive impact of aid on growth has been notably mixed, with studies ranging from finding an unconditional positive effect, to finding only a conditional positive effect, to finding no significant effect at all (e.g., Burnside and Dollar 2000, Clemens, Radelet and Bhavnani 2004, Hansen and Tarp 2000, 2001, Rajan and Subramanian 2005a,b).

This paper has two related aims. First, we seek to examine how the occurrence of various types of natural disasters affect aid inflows. We explore this aid response to disasters not only for those recipient countries actually exposed to the disaster shocks, but also for recipient countries not directly exposed to the disasters but who share donors with the affected countries. To the extent that donors reallocate aid flows amongst their recipients following a natural disaster to one or more of them, the spillover effects to the disaster-unaffected countries could potentially be negative or positive. Secondly, utilizing any such disaster-driven variation in aid to these unaffected countries, we then also seek to reexamine the question of aid's impact on economic growth by employing instrumental variables (IV) estimation.

Because donors may consider recipient country growth rates in their aid allocation decisions, ordinary least squares (OLS) estimates of aid's effect on growth are likely to be biased. A priori, the direction of such potential bias is unclear. If donors use aid flows to assist slow-growth countries, then OLS estimates of aid's effect on growth will be downward biased. However, if aid is instead used by donors to reward and encourage rapid-growth countries, OLS estimation of the growth impact of aid will be biased upward. Moreover, even when employing IV estimation instead of OLS, because many factors may influence growth, finding valid instruments can be difficult. The use of invalid instruments, meanwhile, leads to inconsistent estimates whose finite sample bias may be even larger than that of OLS (e.g., Buse 1992, Bound, Jaeger, and Baker 1995, Deaton 1997). We feel that the IV strategy employed in this paper, however, is an improved attempt to address these concerns and provide consistent estimates of the structural parameter reflecting the growth effect of aid.

In first analyzing the aid impact of natural disasters, we are interested in how bilateral aid is reallocated from donor countries to recipient countries following disaster shocks. We define a country's aid competitors as its "aid neighbors," and examine the response of aid inflows to both own disaster exposure as well as aid neighbor disaster exposure. Own disaster exposure is shown to significantly increase countries' aid receipts in the case of droughts and, in one specification, floods. Meanwhile, aid neighbor disaster exposure decreases aid receipts in the case of earthquakes, but increases receipts in the case of droughts and, in some instances, floods.

In proceeding to the estimation of foreign aid's impact on growth, we utilize this post-disaster variation in aid flows to unaffected countries. We use the objective measure of drought exposure to recipients' aid neighbors as an instrument for own aid inflows in an IV analysis of aid's effect on growth. In second stage growth regressions, we find that an inflow of aid equal to 1 percent of gross domestic product (GDP) increases recipient growth by 1.2-1.7 percentage points in the short- to medium-term, although the point estimates are not statistically distinguishable from 1 using either conventional or weak instrument robust confidence bounds.

We are unable to detect any significant effect of aid on growth in longer time horizons. The short-term positive growth result that we do find is in contrast to both inconsistent OLS estimates with a zero or notably smaller effect, as well as inconsistent two-stage least squares (2SLS) estimates with an alternative set of instruments whose validity is more questionable. Our results help to further clarify some of the econometric issues that have led to the existing mixed findings in this literature. The omission of recipient fixed effects[3] and/or usage of invalid instruments can produce biased estimates of aid's effect on growth in all time horizons. Usage of such fixed effects, as well as a measure of GDP that reduces division bias, leads to qualitatively similar OLS results in our estimation compared to the IV outcomes, contrary to much other work in this area. Lastly, especially in light of the sensitivity of cross-country growth regressions to the choice of estimator and specification, we also run various robustness checks of our analysis. We find that these main results are not sensitive to several of these checks. However, in the most stringent specifications, low predictive power of the neighbor disaster instrument(s) on aid inflows prevents much conclusive analysis.

The positive, shorter-term growth effect that we observe in the main results is driven by increased household consumption rather than factors that growth models like those of Solow (1956) or its augmented analog (Mankiw, Romer and Weil 1992) theorize would foster medium- to long-term growth, such as physical and human capital deepening or improvements in factor productivity. Our results also show that increased aid inflows actually appear to decrease and crowd out physical capital investment. We are unable to find any evidence of a positive effect of aid on secondary school enrollment or expenditures on research & development (R&D) and health, which we use as proxies for human capital investment and productivity improvements to physical capital and labor, respectively. These results together are consistent with our inability to detect any longer-term effects of aid on growth. Using our instrumented aid, we also estimate the medium-run growth dynamics of a one-time aid shock and conclude that the observed dynamics further support the finding of only a short- to medium-run growth effect of aid. Finally, in comparing the sum

[3]This issue is also discussed in Hansen and Tarp (2001), Rajan and Subramanian (2005b), and Werker et al. (2007) as part of the motivation for their usage of dynamic panel estimation.

of the estimated aid effects on national accounts components with the aid effect on overall GDP per capita growth, we do not observe any statistically significant difference between these two values. Such a differential, had it existed, could have been attributable to a longer-run growth impact of aid, but its absence supports the lack of such a long-run effect.

In characterizing the nature of heterogeneity in the aid-growth effects that we find, it appears that countries with a lower estimated marginal benefit of aid account for a disproportionate share of the IV results. Thus, to the extent that these particular countries respond differently in their usage of aid inflows - perhaps due to liquidity constraints and/or low returns to investment - the consumption increase, investment decrease, and absent long-run growth results observed for our estimation sample may not be characteristic of the response that all countries would undergo with similar aid increases.

The positive albeit limited impact of aid on growth that we do observe, and the fact that it is unconditional on any traits of the recipient country such as its policy environment, contrasts with several existing findings and lends support to the "unconditional" branch of this literature. Nevertheless, the lack of any conclusive long-run impact of aid on growth in our results lends credence to studies in this area that similarly are unable to detect any long-run growth effect of aid. It also provides a potential explanation for the "micro-macro paradox" of aid-growth research, where macro-level studies have often failed to detect any significant impact of aid on growth despite micro-level findings of the beneficial impacts of investments in, for instance, education and infrastructure (e.g., Angrist and Lavy 1999, Duflo and Pande 2007, Dinkelman 2008). If aid is not actually being spent on such investment projects or factor productivity improvements but rather on consumption goods, then even with any multiplier effects of increased consumption on GDP, we still might not expect to see a significant long-run impact of aid on growth.

The remainder of the paper is organized as follows: section 2 briefly discusses related literature on foreign aid and natural disasters as well as foreign aid and growth. Section 3 describes the various data for our analyses, while section 4 explains the methodology and estimation strategy. Section 5 presents the main results and section 6 further explores issues in the interpretation and channels of those results. Section 7 outlines various sensitivity analyses, and finally section 8 concludes.

3.2 Related Literature

Relatively little work exists on the impact of natural disasters on foreign aid, and what has been done is both fairly recent and focused on the countries that actually experience the disaster shocks. Strömberg (2007) explores how natural disasters affect foreign aid inflows to affected countries and whether these effects differ by recipient characteristics, while Yang (2008) looks at how hurricanes impact various international financial flows, including foreign aid. This paper adds to the ongoing research on natural disasters and foreign aid by also examining the extent to which disaster shocks alter aid flows to countries not directly affected by the disasters.

On the contrary, there is an extensive literature examining the impact of foreign aid on economic growth, both theoretically and empirically.[4] However, as mentioned earlier, the empirical results from this research are mixed, due both to different econometric methodologies across studies, some of which are flawed, as well as data differences (Deaton 2009). Some researchers, like Burnside and Dollar (2000), determine that aid only has a positive effect on growth conditional on good policies being undertaken by the recipient country. Clemens, Radelet and Bhavnani (2004)

[4]For excellent surveys of this literature, see Hansen and Tarp (2000) and Clemens et al. (2004).

find that it is necessary to disaggregate aid flows into short- and long-term impact in order to appropriately estimate the effect of aid in a given time horizon.

In contrast, other studies like those of Hansen and Tarp (2001) find that aid actually has an unconditional, positive effect on growth, regardless of recipient characteristics or type of aid received. Finally, there is a third strand of research in this literature which finds no significant impact of aid on growth whatsoever. Easterly (2001, 2003) finds a lack of a significant relationship between aid and investment as well as investment and growth. He then discusses how potentially poor investment incentives and significant heterogeneity in economic climates across countries are among the reasons why a large beneficial impact of aid on growth at the aggregate level might be unrealistic to find. Rajan and Subramanian (2005b) likewise find virtually no evidence across a variety of specifications of a significant relationship between aid and growth. They suggest that this lack of a growth effect results from an adverse impact that aid has on recipient country competitiveness via a real exchange rate overvaluation (2005a).[5] Meanwhile, Werker, Ahmed and Cohen (2007), in order to examine aid's impact on growth and more generally how foreign aid is spent by recipients, use variation in oil prices and the fact that OPEC donor countries often pass along the financial windfalls or damage from these price shocks to Muslim recipient countries. They similarly do not detect a significant growth effect of aid, and argue that this stems from their finding that the aid is primarily consumed by households in the short-run, largely in the form of increased non-capital goods imports.

Put together, these and other conflicting research findings have resulted in a long-standing academic and policy debate regarding aid's impact on growth that has yet

[5] This decrease in "competitiveness" is reflected in the reduction of the share of labor intensive and tradeable industries in manufacturing.

to be resolved. This paper hopes to contribute to the aid-growth discussion through its usage of a heretofore unexplored estimation strategy involving the effect that natural disasters have on the aid allocations of donors to their recipients.

3.3 Data

Regarding the natural disaster shocks that we explore and their effects on aid flows, we focus on wind storms, earthquakes, floods and droughts.[6] According to several measures of disaster impact, these four phenomena together are responsible for the majority of harm. As Table 1 shows, during our estimation period of 1979-2002, these four disaster types account for US$893 billion in economic damages worldwide and nearly 94 percent of total worldwide economic damages across twelve disaster types over that same time period. They are similarly devastating in terms of human losses from 1979 to 2002, as they account for approximately 95 percent of disaster-related injuries and 72 percent of disaster-related deaths.

These data on disaster damages, people killed and injuries are from EM-DAT: the CRED/OFDA International Disaster Database, maintained by the Center for Research on the Epidemiology of Disasters (CRED), Université Catholique de Louvain.[7] These estimates of financial damage and human losses are at least in part, however, self-reported from the affected countries' governments (EM-DAT 2008). One resulting concern is that the usage of these measures may lead to biased estimates of the impact of disasters on aid flows and, if used as instruments, biased estimates of aid's effect on growth in IV analysis. For instance, reverse causation may occur, where countries anticipating or currently receiving small amounts of post-disaster aid may exaggerate their disaster losses in order to try to encourage larger aid inflows, leading

[6] The "wind storm" category includes phenomena variously referred to as cyclones, hurricanes, storms, tornadoes, tropical storms, typhoons, or winter storms.

[7] The EM-DAT data is publicly available at http://www.em-dat.net.

to downward biased estimates of the effect of disaster losses on aid. There may also be a problem of omitted variables if, for example, deteriorating economic conditions or government functionality leads to both a decline in aid inflows as well as increased vulnerability to disaster damage (Yang 2008). We therefore compile meteorological and geological data on storms, rainfall, and earthquakes in order to construct objective measures of disaster exposure to use in our main analyses (see section 4 and Data Appendix).

Formulated by Yang (2008), our storm data comes from meteorological data on hurricanes available worldwide from two U.S. government agencies: the National Oceanic and Atmospheric Administration (NOAA) Tropical Prediction Center (for Atlantic and eastern North Pacific hurricanes) and the Naval Pacific Meteorology and Oceanography Center/Joint Typhoon Warning Center (NMFC/JTWC) (for hurricanes in the Indian Ocean, western North Pacific, and Oceania).[8] Our flood and drought data is constructed using additional meteorological data from the Global Precipitation Climatology Project (GPCP) on monthly rainfall estimates.[9] Lastly, our geological data on earthquakes comes from the U.S. Geological Survey (USGS).[10]

Along with the disaster data, there are several other measures necessary for our analysis. The measure of foreign aid flows is net Official Development Assistance (ODA), obtained from the Development Assistance Committee (DAC) of the Organisation for Economic Co-operation and Development (OECD) and available from 1960 onward.[11] ODA is disbursements of loans and grants made on concessional terms[12] to promote economic development in developing countries. We focus on bi-

[8]The NOAA data is publicly available at http://www.noaa.gov, while the NMFC/JTWC data is available at http://metocph.nmci.navy.mil/index.shtml.

[9]The GPCP data is publicly available at http://neic.usgs.gov/neis/epic/epic_global.html.

[10]The USGS data is publicly available at http://precip.gsfc.nasa.gov.

[11]The OECD DAC data is publicly available at http://www.oecd.org/dac.

[12]To qualify as ODA, if the aid flow is a loan, it must have a grant element of at least 25 percent.

lateral aid flows between countries, thus generally excluding both multilateral donor and multilateral recipient institutions from our analysis except when otherwise noted. The aid flows are initially in current US dollars and are converted to constant 1995 US dollars using consumer price index (CPI) estimates and exchange rates from the World Bank's World Development Indicators (WDI) 2004.[13]

Our measure of per capita GDP growth comes from GDP estimates of the World Bank's World Development Indicators (WDI) 2004. Some other control or outcome variables we use, such as total trade value of goods and services or private household consumption, similarly come from the WDI 2004. We also utilize the International Monetary Fund (IMF) Balance of Payment Statistics 2004 for data on net flows of migrants' remittances. Additional control variables on trade openness as well as civil and interstate war occurrence come from Sachs and Warner (1995) and the Correlates of War (COW) project 2002 of Penn State University, respectively.[14]

3.4 Methodology & Estimation Strategy

3.4.1 Determining Aid Neighbor Disaster Exposure

Before discussing how we utilize our objective disaster data to form disaster exposure measures for each type of natural phenomenon, we first turn to our methodology for constructing "aid neighbor" disaster exposure. We define some aid recipient country j's "aid neighbors" to be the set of country j's competitors for aid receipts from its donors. Country j's "closest" aid neighbors are the countries who are its strongest competitors for aid. Thus, closeness in the aid neighbor sense need not be related to closeness in the geographic sense, although in practice the two concepts may indeed

[13]It still remains for a later revision to utilize the GDP deflator rather than CPI as the appropriate conversion variable, as is used for all of the other financial flows in the paper. However, it should be noted that the aid flows from the two deflation methods are, expectedly, extremely highly correlated (correlation coefficient $\rho = 0.998$). As expected, preliminary estimates comparing a subset of regressions from Table 5 show that the results are nearly identical when using GDP deflator-adjusted aid flows rather than the current CPI-adjusted flows. Thus, it is unlikely that the results will change substantively after such a revision.

[14]The COW data is publicly available at http://www.correlatesofwar.org.

be related since some donors concentrate their aid outflows in particular geographic regions.[15] The goal in constructing the empirical measure of aid neighbor disaster exposure is to capture how a country's disaster shock should alter aid flows to a disaster-unaffected aid competitor.

Let i index donor countries and d index disaster types, where $d \in \{$*storm, earthquake, flood, drought*$\} \equiv D$, the set of disaster types. Also, R_{it} is the set of donor i's recipients in year t. Similarly, define S_{jt} as the set of donors from whom recipient j receives aid in year t. A donor allocation model (see Theory Appendix) suggests the following relationship for the change in aid g to recipient j in year t as a function of a disaster shock of type d to some recipient $k \neq j$, as well as other variables:

$$dg_{jt} = f_j(\boldsymbol{\theta}_t, \mathbf{g}_t, \mathbf{I}_t) \frac{\partial I_{kt}}{\partial OWN_{dkt}} dOWN_{dkt}, \tag{3.1}$$

where OWN_{dkt} is actual own disaster exposure for recipient k in year t and disaster type d, I is income, and $\theta \in (0, 1]$ is a utility weight for recipients in a donor welfare function. It is assumed that $\partial I_{kt}/\partial OWN_{dkt} < 0 \ \forall d, k, t$ (i.e., own disaster shocks decrease own income, ceteris paribus).

We can use equation (1) to motivate the empirical definition of aid neighbor disaster exposure, NBR_{djt}, for recipient j in year t and disaster type d as

$$NBR_{djt} = \sum_{i \in S_{jt}} \sum_{k \neq j \in R_{it}} \theta_{ikt} OWN_{dkt}, \tag{3.2}$$

where θ_{ikt} is a weight for the share of donor i's aid outflows going to recipient k in year t, to proxy for recipient k's importance to the donor i in year t.[16] Because using aid

[15] As a nomenclature issue, we will use "aid neighbor" and "neighbor" interchangeably throughout the paper. In the latter case, we will thus never be referring to countries that are of close, geographic proximity unless we explicitly state otherwise. However, as Table 2 displays, it is nevertheless possible (but not necessary) that aid neighbors are also geographic neighbors.

[16] In going from theory (see Theory Appendix) to this empirical construction, a few adjustments are made. While the model presents differentials of aid and disaster exposure, as both of these variables are in reality flows, they are included in "level" form, rather than changes. We also approximate $f_j(\boldsymbol{\theta}_t, \mathbf{g}_t, \mathbf{I}_t)$ with $\theta_{ikt} \ \forall i, k \neq j, t$, as $\mathbf{g}_t$ and $\mathbf{I}_t$ actually enter into f_j via the unknown utility functions u_j and U. Finally, in contrast to the model, we account for the fact that there may be many simultaneous neighbor disasters over which to aggregate, as well as multiple donors over which to aggregate.

outflows in year t to form θ_{ikt} in practice would induce endogeneity in later estimation, an alternative strategy is necessary. One possibility is to construct the weights over a single, initial pre-estimation period. However, if donors' recipient sets change substantially over time, then donor outflows during some initial weighting period may not be an accurate representation of donor outflows and recipient importance during the estimation period.

Table 2 examines the primary aid recipients for major donors France and the United States in the 1960s and 1990s. As it shows, donor's recipient sets do indeed change over time. In particular, when examining the aid outflow patterns displayed, three points become evident. First, we can see examples of how donors' recipients, both in terms of new countries, as well as in terms of the rank order of existing countries, have changed over time. Secondly, simply by examining which countries receive the majority of donors' aid outflows, it is possible to deduce which factors donors deem as most important in deciding their aid allocations. For instance, while France appears to place a premium on colonial history by giving much of their aid to former colonies like Algeria and Côte d'Ivoire, the United States seems to hold its political and military interests as most important, changing over time from Vietnam and warring southeast Asia in the 1960s, to the Middle East and former USSR in 1990s. Thus, rather than try to determine these underlying factors a priori (although work has been done on this topic, like that of Alesina and Dollar (2000)), we can use the recipient share of realized donor outflows, θ_{ikt}, as a proxy measure of recipient importance to the donor.[17] Lastly, Table 2 also shows how the concentration of donor outflows of top recipients has lessened over time, as donors have increased the number of countries to whom they send aid. However, this last point may be in part

[17]This also helps, to some extent, to support the assumption in the aid allocation model to focus on one donor rather than several. It does appear empirically that donors make their allocation decisions relatively irrespective of each other, as individual donor motivations (at least for top recipients) are discernible in the table.

an artifact of the DAC data coverage and detail improving over time.

Given such empirical support that the θ_{ikt} weights should be continually updated over time, but to reduce year to year volatility in the measure due to annual aid variation, we construct them using a 10-year moving average from years $t-1$ to $t-10$. The weights are formed such that $\sum_k \theta_{ikt} = 1 \; \forall i,k,t$, which thus helps take into account the affected recipient j's importance to donor i. This allows the share value θ_{ikt} to be not only an ordinal measure of aid neighbor importance rank, but also a cardinal measure of recipient importance. Also, if aid flows are zero or missing for all relevant years, then the weight itself will similarly be zero or missing (e.g., if a recipient doesn't receive any aid from a particular donor).

NBR_{djt} reflects that the way in which a disaster shock to some recipient k alters recipient j's aid inflows depends not just on the size of the shock, but also on how important recipient k is to every shared donor i. Thus, we can weight the actual disaster shock data by these aid flow shares in order to better capture expected, post-disaster aid reallocations.

When including negative aid inflows (i.e., recipient repayments to donors) from the donor-year-recipient source dataset (which account for ≈ 4 percent of 65,040 observations with non-missing ODA flows in that data), we scale up all aid flows for a given donor-year by some constant c_{it}[18] before calculating θ_{ikt} so that $\theta_{ikt} \in (0,1] \; \forall i,k,t$. When excluding negative aid inflows from the donor-year-recipient source dataset, as in one of our robustness checks, we calculate θ_{ikt} with the remaining data, which automatically ensures that $\theta_{ikt} \in (0,1]$ without any such scaling adjustment.

We also tried relaxing this weighting assumption by constructing a neighbor disaster exposure measure where $\theta_{ikt} = 1 \; \forall i,k,t$. This removes the weights so that

[18]Constant $c_{it} = \min(aid\_flow_{it}) + 1$.

every recipient is assumed to be equally important to each donor, and is what we refer to as "unweighted" neighbor exposure in Tables 6, 8 and 17.

Lastly, concern over equation (2) may arise because recipient j's donors are weighted equally (i.e., unweighted) when calculating aid neighbor disaster exposure. Specifically, an intuitive alternative might be formulated as

$$NBR_{djt} = \sum_{i \in S_{jt}} \gamma_{ijt} \sum_{k \neq j \in R_{it}} \theta_{ikt} OWN_{dkt}, \tag{3.3}$$

where γ_{ijt} is a weight for the share of recipient j's aid inflows coming from donor i in year t, to proxy for donor i's importance to recipient j in year t. In other words, the relevance (in terms of potential aid reallocation) to recipient j of a disaster shock to country k with whom it competes for aid could also be weighted by how important their shared donor i is for recipient j's aid inflows.

In practice, however, differential γ weights (rather than $\gamma_{ijt} = 1 \; \forall i, j, t$, implicit in equation (2)) might not help explanation of post-disaster aid variation. The donor model, although admittedly a single donor framework, does not explicitly suggest inclusion of γ weights. If anything, it only suggests that differential donor income, via donor utility, would alter the magnitude of post-disaster aid reallocations. However, because the form of this utility function for donors is unknown, and because donor income itself only partially relates to γ, its inclusion may introduce more noise than signal. We compare the aid neighbor disaster exposure formulations of equations (2) and (3) in Table 6.

3.4.2 Defining Disaster Exposure Across Disaster Types

As equation (2) shows, in order to define aid neighbor disaster exposure, it is first necessary to define measures of own disaster exposure for each disaster type. We attempt to construct own disaster measures that are, at least approximately, per capita measures of exposure, consistent with representative agent recipient income, disaster exposure, and utility. The disaster measures are also constructed to have low thresholds of inclusion in order to reduce the number of zero or missing observations (see Data Appendix for further details).

The disaster measures we construct are broadly analogous to those of Strömberg (2007), who uses data from Dilley et al. (2005). However, our disaster exposure variables do differ, and are also somewhat more inclusive than his regarding what qualifies as a disaster event.[19]

Table 3 displays descriptive statistics over our 1979-2002 estimation sample for the own disaster exposure measures, the aid neighbor disaster exposure measures per equation (2), as well as the other outcome and control variables to be discussed in more detail later. Due to both weighting and the aggregation across multiple countries, the aid neighbor exposure variables have larger means than their own exposure analogs. For instance, the mean own storm index is 0.003, while the mean neighbor storm index is an order of magnitude larger at 0.035.

3.4.3 Estimation

Disasters and Aid

In order to assess the impact of disaster exposure on aid inflows, we use OLS to estimate variants of the following model for recipient j and year t over 1979-2002:

$$A_{jt} = \psi_0 + \sum_{d \in D} \alpha_d NBR_{djt} + \sum_{d \in D} \beta_d OWN_{djt} + \omega_j + \phi_t + \epsilon_{jt}. \tag{3.4}$$

A_{jt} is net ODA for recipient j in year t as a fraction of GDP. We express aid (as well as all other currency-denominated variables) as a fraction of GDP in order

[19]For instance, Strömberg uses earthquakes of magnitude 4.5 or higher, and drought events when the magnitude of monthly precipitation was less than or equal to 50 percent of its long-term median value for three or more consecutive months.

to both better analyze results across economies of different sizes, as well as to be consistent (in the case of aid) with the variable that we would like to instrument for in the aid-growth analysis. However, as discussed by Yang (2008), because disasters may also affect the denominator of these statistics (i.e., the level of GDP), we always use mean GDP in the three years prior to disaster exposure (the earliest included lag, when applicable) as the denominator. We make analogous adjustments in later regressions (Table 9) to financial damages and human losses due to disasters, except that the denominator in the latter case is mean population in the three years prior to the given observation. Further, ω_j and ϕ_t are recipient and year fixed effects, respectively, and ϵ_{jt} is a mean-zero error term.

Because serial correlation in aid flows as a fraction of GDP is likely to occur in this panel dataset and thereby typically bias OLS standard error estimates downward (Bertrand, Duflo and Mullainathan (2004)), we cluster standard errors by recipient country in order to allow for an arbitrary variance-covariance structure within countries.

The coefficients of interest in specification (4) are the various α's and β's, which are respectively the impacts of aid neighbor and own exposure from each type of disaster on net ODA inflows as a fraction of GDP.

Aid and Growth

In order to assess the impact of aid on economic growth, we use IV to estimate variants of the following model for recipient j and year or period t over 1979-2002:

$$Y_{jt} = \tau_0 + \pi A_{jt} + \xi OWN_{drought,jt} + \mathbf{X'}_{jt}\psi + \zeta_j + \kappa_t + \nu_{jt}, \tag{3.5}$$

$$A_{jt} = \psi_0 + \alpha NBR_{drought,jt} + \beta OWN_{drought,jt} + \mathbf{X'}_{jt}\eta + \omega_j + \phi_t + \epsilon_{jt}, \tag{3.6}$$

where we now also add definitions for $\mathbf{X}'_{jt}$, which are exogenous controls (besides own disaster exposure and the fixed effects, that is) relevant for the first or second stage, and Y_{jt}, which is per capita GDP growth. Other variables not explicitly defined are analogously specified as before (e.g., ν_{jt} is a mean-zero error term like ϵ_{jt}, ζ_j is a recipient fixed effect like ω_j, etc.). Also, in the period-averaged data, aid and other dollar-denominated variables are divided by GDP in the prior period, while individual-denominated variables are divided by population in the prior period, unless stated otherwise. As depicted above, in most of the specifications, aid neighbor drought exposure, $NBR_{drought,jt}$, is our instrument in equation (5) for aid, A_{jt}.

3.5 Main Results

3.5.1 Disasters and Aid

We first examine the impact of natural disasters on foreign aid inflows. Before turning to specification (4) and our measures of objective own and aid neighbor disaster exposure, it is of interest to explore how the EM-DAT measures of disaster financial damage and human losses impact aid inflows. We would like to see whether there is any indication of the endogeneity of these measures given that they are at least partially self-reported from the affected countries' governments, as discussed in section 3.

Table 4 displays how damages, deaths and injuries due to each of the four disaster types impact aid inflows as a fraction of GDP. Damage and deaths due to floods significantly increase aid inflows. One thousand flood-related deaths increase net ODA by 0.06 percent of GDP, while \$1 billion of flood-related damage increases net ODA by 0.10 percent of GDP. However, the remainder of the coefficients are not significant, and in many cases are actually negative. This is suggestive evidence of an endogeneity issue with the EM-DAT measures, and helps to motivate our usage of alternative disaster exposure measures.

We turn now to Table 5 and estimation of specification (4) using our objective measures of own and aid neighbor disaster exposure. Equation (4) focuses on the contemporaneous effect of disasters on aid inflows, in part because our static donor allocation model does the same and assumes that the donor re-optimizes each period. In the first two columns of Table 5, own disaster exposure generally has a positive but not significant effect on aid inflows. Meanwhile, aid neighbor earthquakes significantly decrease aid inflows, while neighbor floods and droughts increase aid. In the case of neighbor droughts, for example, an aggregate decrease in rainfall below aid neighbor countries' median levels equal to ten percent of those medians leads to an increase in aid equal to 0.3 percent of GDP. This pattern of coefficients is consistent with Proposition 1, Case 2 of the donor allocation model. While we have no definitive explanation with our current data as to the reason for these disparate aid responses to neighbor disasters, one possibility might be the relatively disparate impact of the disasters themselves. As Table 1 shows for example, droughts tend to have quite distinct effects from earthquakes or the other disaster types, due to the very high and inherently irreversible number of deaths they cause but the very low amount of financial damage or injuries they lead to, which are inherently reversible but costly. Further exploration of this issue is necessary in other research. However, we do assess later in Table 8 whether the observed aid neighbor results are at least consistent with what we would theoretically predict, even if we cannot fully determine their underlying mechanisms.

Two seemingly puzzling results of column (2) are the lack of significant effects of own exposure on aid inflows despite the significant aid neighbor effects, as well as the fact that, potentially contrary to model Proposition 2, the magnitude of the own exposure coefficient for a given disaster type is always smaller than that of its aid neighbor equivalent. Regarding the latter puzzle, this could stem from the fact that the own disaster variables reflect actual exposure, while the neighbor variables reflect exposure weighted by recipient importance to donors. Column (3) thus explores the alternative, unweighted version of aid neighbor disaster exposure discussed in section 4.1, so that the neighbor variables and own variables are now of the same weighting metric and hence comparable measures of exposure. In this case, in line with Proposition 2, now the magnitude of the own coefficient for each disaster type is larger than the analogous aid neighbor coefficient. Additionally, only aid neighbor droughts now significantly impact aid inflows, displaying that the positive neighbor drought effect is robust to different weighting schemes.

Columns (4) through (8) explore potential reasons for the other puzzle of why the own disaster effects in columns (1) to (3) are not particularly strong. Columns (4) through (6) show that, at least for own drought exposure, there is a significant, positive effect of exposure on aid inflows for poorer countries, identified as those recipients whose 1976-1979 mean GDP is below the median value. Column (7) weights own exposure similarly to neighbor exposure. Specifically, in our donor-recipient-year level data, recipient own disaster exposure is weighted by how "important" the given recipient is to its donor (i.e., θ_{ijt}), and then summed across donors for each recipient. While the neighbor exposure results are similar to column (2), we now observe significantly positive own exposure results for both droughts and floods. Furthermore, we might anticipate that the own disaster effects would be more strongly positive when including aid flows from multilateral donors since these donors may be less financially constrained than individual donors, while the aid neighbor, spillover effects would be unaffected if not weaker. Column (8) shows this to be the case, as the aid neighbor effects are generally similar or smaller, while own drought exposure now significantly increases aid flows. Finally, given that Ethiopia's drought of 1984-85 was a notable outlier in our sample (Figure 1 illustrates the significant trend break in aid inflows that occurred for Ethiopia in 1984), column (9) excludes Ethiopia to examine whether the results are driven by that country alone, which does not appear to be the case.

Before further investigating the weak own exposure effects observed, we examine the alternative weighting scheme of aid neighbor exposure prescribed by equation (3) where donors are differentially weighted, as compared to equation (2) with no donor weighting. Table 6 displays these results, where the first two specifications reproduce columns (2) and (3) of Table 5, with column (2) reflecting equation (2). Column (3) of Table 6, reflecting equation (3), shows qualitatively similar results to column (2), although the aid neighbor coefficient magnitudes are now larger. However, the model fit is somewhat poorer in comparison, and the neighbor exposure variables all have less significant effects on aid inflows. Column (4) explores the final possible combination of θ and γ weights for the construction of aid neighbor exposure, finding once again qualitatively similar results to column (2) but with poorer fit and a weaker relationship between neighbor exposure and aid inflows. Thus, there is both theoretical and empirical support for remaining with equation (2) as the formulation for aid neighbor disaster exposure in further analysis. However, it is comforting that the qualitative results observed thus far do not appear to be sensitive to the weighting scheme.

Returning now to the lack of significant own exposure effects in Table 5, another potential explanation is that an assumption of an exclusively contemporaneous relationship between disaster exposure and aid inflows is too limiting. If there are lagged effects of own or aid neighbor disaster exposure on aid inflows, then our current specifications would not detect this. Table 7 examines the impact of lagged disaster exposure on aid. Specifically, we estimate the effect of mean disaster exposure over different time horizons on current aid inflows. The choice of the number of lags in specifications (2) and (3) is motivated by our later usage of four-year average data in the growth analysis (i.e., columns (2) and (3) capture mean disaster exposure over one and two four-year periods, respectively). As expected, coefficient magnitudes on own variables generally increase with more lags, while the aid neighbor coefficient magnitudes often decrease when a broader period of disaster exposure is considered. Additionally, we now see a significant impact of own disaster exposure, via droughts, on aid inflows in all three specifications.

Figures 2 and 3 illustrating global disaster coverage for each of the four disaster types further support our observed results for own disaster exposure. Flood and drought occurrences are much more widespread than storm and earthquake occurrences. Having fewer zero or missing observations explains in part why we're able to more precisely estimate drought and flood exposure effects on aid inflows.

In terms of the donor allocation model, as noted earlier, the observed own exposure and aid neighbor exposure effects on aid inflows in Table 5 are consistent with Case 2 of Proposition 1. The most robust, significant results throughout all the specifications are the positive effects for own droughts, neighbor droughts, and to a lesser degree neighbor floods, as well as the negative effect for neighbor earthquakes. If we take the model seriously, we would therefore expect the relationship between the recipient income shocks and donor income available for aid, dI/dI_j, to be negative for recipient shocks related to droughts and floods, and non-positive for shocks related to earthquakes. We are especially interested in the aid neighbor drought effect, both as the most robust result as well as because it is a positive neighbor spillover.

Table 8 tests Proposition 1 of the model. Because recipient GDP would be endogenous if included directly as a regressor, and since we have no valid instruments for that variable, we examine in a more reduced form framework how recipient disaster shocks impact donor aid outflows and donor GDP per capita, controlling for donor shocks since the same shocks could affect both groups. Additionally, this allows us to differentiate between shocks to recipient GDP due to each disaster type, which is important given the differentiated aid responses by disaster type that we observe. As earlier, we also distinguish unweighted and weighted recipient disaster exposure.

In three of four specifications in Table 8, the necessary condition of Proposition 1 for the observed positive neighbor drought effect indeed holds. Namely, increased recipient drought exposure, which ceteris paribus decreases recipient income, is associated with increases in donor GDP and donor aid outflows. While the effects are often sizable, this is usually the case when weights of recipient importance to donors are incorporated (analogous unweighted coefficients are much smaller), and are also measured somewhat imprecisely. The observed results are also consistent with the positive own drought effect and, in two of four specifications, the necessary condition for the observed positive neighbor flood effect similarly holds. Meanwhile, for all four specifications, the positive or statistically indistinguishable from zero earthquake coefficients in the table are compatible with the earlier observed, negative neighbor earthquake effects. Additionally, while the sizeable, negative coefficient magnitudes on donor own earthquake exposure in specifications (1) and (2) seem implausibly large, it should be recalled that aid outflows can be negative with recipient repayments, and that the imprecision of the point estimates lead reasonable values to be within 95 percent confidence bounds.

Lastly, Table 9 presents a final specification check on the aid neighbor disaster exposure variables. If these variables are constructed accurately, then they should have no impact on measures of own recipient disaster damage and human losses when controlling for own disaster exposure. As shown in the table, in none of the three specifications does aid neighbor disaster exposure significantly impact own damage or human losses. Additionally, even despite the potential endogeneity problems of the EM-DAT data discussed in Table 4, we would still expect to see at least some evidence of positive effects of own disaster exposure on damage and human losses. Own storms appear to have a significant, positive effect on financial disaster damage as a fraction of GDP, while both own storms and floods have significantly positive effects on individuals killed due to disasters as a fraction of the population.

3.5.2 Aid and Growth

Short-run to Medium-run Effects

We turn now to our IV analysis of foreign aid on per capita GDP growth, focusing first on short-run to medium-run effects. Because the most robust results on aid inflows for both own disasters and aid neighbor disasters come from drought exposure, we now focus solely on aid neighbor drought exposure as an instrument for aid inflows. For neighbor drought exposure to be a valid instrument for aid, however, the exclusion restriction must hold. Namely, it must be the case that, conditional on controls, the only impact that aid neighbor drought exposure has on own GDP growth is through its effect on aid inflows.

One possible violation of the exclusion restriction is a direct economic link between aid neighbors. In such a case, damage to an aid neighbor's economy from a disaster shock, ceteris paribus, would also harm and help the recipient's own economy and affect GDP growth. A violation could also occur if individuals in disaster-affected, aid neighbor countries migrate to unaffected recipient countries following disaster shocks and impact GDP growth. To address these potential threats, we include total trade value of the recipient as well as approximate population growth as regressors in all IV analysis.[20]

Additionally, exclusion restriction violations may be caused by other international financial flows responding to the occurrence of an aid neighbor disaster shock in a similar manner as net ODA flows. Following the outcome variables of Yang (2008), we would like to account for any such post-disaster response by migrants' remittances, multilateral institutional lending, bank and trade-related lending, foreign direct investment, or portfolio investment. However, such financial flows could be endogenous in second stage growth regressions for the same reasons that foreign aid is, and in first stage regressions because their movements may be influenced by an omitted, unobserved variable that also impacts aid flows. Thus, the financial flow measures cannot simply be included as control variables in IV analysis. Moreover, none of our remaining neighbor disaster exposure measures are sufficiently strongly related to those variables to serve as instruments for them (analysis omitted for brevity), and even if they were, could only account for three of the five financial flows with which we are concerned. Table 10, alternatively, examines whether aid neighbor drought exposure significantly influences any of the five financial inflows we examine. Because it does not, we can be less concerned about an exclusion restriction violation of this manner.

Before exploring the second stage growth analysis, we turn to the first stage regressions of aid inflows on aid neighbor drought exposure in Table 11. As in Table 10 and previously discussed, total trade value and approximate population growth

[20]The population growth measure is "approximate" as the variable is the change in population as a fraction of mean population in the three years (or period) prior. We are unable to use more direct measures of changes in the immigrant and refugee population in the WDI 2004 due to numerous missing observations for these variables.

are included as controls to help maintain instrument validity, along with own drought exposure. To smooth some of the annual fluctuations that are simply noise in order to examine medium-run effects, we also run first and second stage analysis for four-year averages of the annual data. This also allows for easier comparison of our results to the rest of the aid-growth literature, as this approach is common for cross-country growth regressions.

In both column (2) for the annual data and column (4) for the four-year period data, the aid neighbor drought coefficients are strongly positive and significant. A proportional decrease in neighbor rainfall equal to ten percentage points increases own aid by 0.3 to 0.4 percent of GDP. However, only in column (4) is the own drought coefficient significantly positive. The first stage F statistic is 10.3 in column (2) and 7.1 in column (4). These are somewhat weak in both cases. The Stock and Yogo (2005) critical values for the maximal actual size of a 5 percent Wald test of $\beta = \beta_0$ are 16.38, 8.96 and 6.66 for maximal test sizes of 10, 15 and 20 pcercent, respectively. Due to this borderline weak nature of the neighbor drought instrument, Anderson-Rubin 95% confidence intervals which are robust to weak instruments (Anderson and Rubin 1949) are included in most cases in the second stage analysis, following the procedure of Chernozhukov and Hansen (2005).

The second stage growth regressions, as modeled in equations (5) and (6), are displayed in Table 12. Columns (1) and (6) display the OLS specifications of aid on growth. An increase in aid equal to ten percent of GDP is estimated to increase per capita GDP growth by 0.4 percentage points in the annual data and 0.9 percentage points in the four-year period data, although only the latter coefficient is significant. Hausman tests of endogeneity that are robust to a clustered error structure strongly reject at 1 percent significance the null hypothesis that the aid flows are exogenous.

The nature of national accounts components and how they factor into GDP and GDP growth means that, somewhat mechanically, it is reasonable to expect aid coefficients anywhere in the [0,1] interval, unless there are long-run growth effects. Thus, a Wald test of $\beta_{ODA} = 1$ is also reported for all relevant estimation.

Reduced form specifications of aid neighbor drought exposure on GDP growth in columns (2) and (7) show a significantly positive relationship between neighbor droughts and own recipient growth. The main IV specifications (3) and (8) show that an inflow of aid equal to 1 percent of GDP increases recipient per capita GDP growth by 1.2 to 1.7 percentage points in the short-run (annual data) to medium-run (four-year period data), respectively. Wald tests show that these coefficients are not statistically distinguishable from 1, however, and so we cannot reject that they lie in the [0,1] interval. The IV coefficients are more than an order of magnitude larger than their OLS analogs but are also less precisely measured. Anderson-Rubin tests strongly reject both zero and the OLS aid coefficients in specifications (3) and (8). The 95 percent AR confidence interval is wider than the standard 95 percent interval, ranging from 0.4-4.0 percentage points in column (3) and 0.8-7.6 percentage points in column (8). Thus, in contrast to some of the prior aid-growth literature but consistent with the work of some like Hansen and Tarp (2000, 2001), we do observe that aid has a significant, positive effect on growth that is unconditional on recipient country characteristics. However, due both to the current analysis reflecting short- to medium-term effects, as well as our inability to reject that the aid coefficients lie within the [0,1] interval, we cannot yet determine whether there is any evidence of a longer-run effect of aid on growth or what component(s) of GDP the current shorter-run effect is being driven by. We explore both of these questions later in the paper.

Additionally, it is worth noting that also contrary to some previous work in this literature, we do still observe qualitatively similar results to the IV estimation in our OLS specifications, particularly in the four-year data. This difference in our OLS results with prior research stems from two factors. The first is our inclusion of recipient fixed effects in the OLS specifications, which has not always been done in this literature.[21] The second factor stems from the fact that, ordinarily, because GDP in year t appears both in the denominator of the aid regressor variable and in the numerator of the dependent variable, per capita GDP growth, we might be concerned about potential downward division bias in the OLS aid coefficient toward -1 if there is any measurement error in GDP (Borjas 1980). However, discussed earlier, in order to avoid endogeneity with disaster exposure, we use mean GDP in the three years prior to disaster exposure as the denominator of aid as a fraction of GDP, rather than contemporaneous GDP. Table 3 verifies that mean GDP is very similar to contemporaneous GDP, as expected due to the high persistence in level GDP. However, one important distinction is that the usage of mean GDP actually reduces possible downward division bias in the aid coefficient. For instance, usage of contemporaneous GDP rather than mean GDP in specification (1) actually leads to a negative aid coefficient of -0.08 (omitted for brevity).[22]

Specifications (4) and (9) compare our main IV analysis with alternative 2SLS analysis where the instruments for aid inflows are now log(GDP per capita) and log(population), variants of which are often used in the literature. When recipi-

[21]In some cases, this has been due in part to the usage in corresponding IV analysis of instruments for aid that were not time-varying (e.g., dummies for regions or countries like Egypt), which is not an issue in our case.

[22]It should be noted that there actually still remains some risk of upward bias toward 1, even with the usage of mean GDP in the aid denominator, since GDP in year $t-1$ appears both in the aid regressor variable as well as in the numerator and denominator of the left-hand side growth variable. However, the magnitude of any such bias, even if present, would be expectedly much smaller compared to the potential bias from the usage of contemporaneous GDP. An alternative specification (1) that we ran with mean GDP over years $t-2$ to $t-4$ in the aid denominator rather than $t-1$ to $t-3$ resulted in an aid coefficient of 0.038, which is indeed slightly smaller in magnitude than our current coefficient of 0.039, but nevertheless nearly identical.

ent fixed effects are included, these instruments are somewhat weak. Additionally, cluster-robust Sargan-Hansen tests of overidentifying restrictions strongly reject either instrument exogeneity or the model specification. Thus, the extreme aid coefficient estimates are neither necessarily surprising nor likely consistent. Specifications (5) and (10) show results somewhat analogous to what has been estimated in the past in much of the aid-growth literature until more recently[23], with an exclusion of recipient fixed effects and instruments whose exogeneity is fairly questionable. In these specifications, there is no significant effect of aid on growth, with aid coefficients that are even slightly negative.

Medium-run Dynamics

Continuing from our exploration of short-run and medium-run effects, it is also of interest to examine the medium-run growth dynamics of a one-time aid shock and compare such estimated dynamics with the simulated dynamics from a basic Solow (1956) growth model (see Theory Appendix). Our neighbor drought instrument for aid allows us to actually examine the estimated impact of a such a one-time, exogenous aid increase.

The estimated dynamics result from consideration of the distributed lag model

$$Y_{jt} = \tau_0 + \pi(L)\widehat{A_{jt}} + \xi(L)OWN_{drought,jt} + \mathbf{X'}_{jt}\psi(L) + \zeta_j + \kappa_t + \nu_{jt}, \tag{3.7}$$

where L is a polynomial in the lag operator (here, we include 10 lags), $\widehat{A_{jt}}$ are the fitted values of net ODA as a fraction of GDP from first stage equation (6), and all other variables are as defined previously. Because of the inclusion of a generated regressor, $\widehat{A_{jt}}$, in the model's estimation, the displayed 95 percent confidence intervals are appropriately adjusted (Pagan 1984).

[23]Arellano-Bond style estimation has gained popularity more recently (e.g., Hansen and Tarp 2001, Rajan and Subramanian 2005b, Werker et al. 2007).

Figure 4 displays the estimated and simulated medium-run dynamics, which are actually broadly similar. The estimated dynamics are based on 1,400 observations of the annual data. The simulated dynamics are from an exogenous increase in capital equal to 1 percent of GDP in year 0, which assumes that all aid received were invested. In both cases, we see that the aid increase leads to a large initial, positive spike in growth within the first year after the aid shock. The magnitude of this spike is 0.7 percentage points in the simulated dynamics and 1.0 percentage point in the estimated dynamics, significant at the 10 percent level. However, in the case of the estimated dynamics, we also observe a latter, "echo" increase in growth of 0.6 percentage points four years after the aid shock, although this increase is not statistically significant. Additionally, in both the estimated and simulated dynamics, we see the absence of any longer-run effects of the aid shock on growth ten years afterwards. In the case of the simulated Solow model, we know that this is because capital increases only have effects on intermediate growth and not long-run, steady-state growth, which is determined exogenously. Regarding the estimated dynamics, however, we cannot yet speculate as to the reasons for the lack of a long-run effect until our later exploration of the underlying mechanisms for the growth increase itself. If the estimated growth increase seen here and in the previous section stem from capital increases and the Solow model is the appropriate guiding theory, then the lack of a longer-term growth effect occurs for the same reason as in the simulated dynamics. However, if the GDP growth increases we have seen thus far are, alternatively, consumption-driven, then the observed similarities between the estimated and simulated dynamics shown here are merely coincidental, and the lack of longer-term growth effects in the estimated dynamics are expected (assuming the existence of only small and/or short-run consumption multiplier effects).

Medium-run to Long-run Effects

Finally, Table 13 examines medium-run to long-run effects of aid on growth in an IV framework analogous to our earlier estimation, but now for data averaged over 8 years, 12 years, and 24 years (a cross-section). Although in no case do we observe a significant effect of aid on growth, essentially all of the results over these time horizons are inconclusive, largely due to aid neighbor drought exposure having no explanatory power for aid inflows in the first stage (the F statistics are always below 1). Similarly, because the neighbor drought instrument is so weak in these specifications, we are unable to bound the AR 95 percent confidence intervals, as such bounds may not actually exist (Chernozhukov and Hansen 2005).

3.6 Interpretation and Channels of Aid's Impact on Growth

3.6.1 Assessing Monotonicity

We turn now to additional issues of the interpretation of the main results. We would typically be inclined to interpret our IV aid coefficients in Table 12, columns (3) and (8) as local average treatment effects (LATE), as defined by Imbens and Angrist (1994). This is due to our belief that there are likely heterogenous effects of aid on growth (to be discussed later), and the possibility that the neighbor drought instrument could be correlated with the recipient country-specific component of the treatment, conditional on the latter (especially since donors, as shown in the allocation model, likely reallocate aid following disaster shocks based on recipient-specific characteristics). However, we might particularly worry in our case about whether, for such LATE interpretation of the aid parameter, the necessary assumption of monotonicity of the effect of aid neighbor drought exposure on aid inflows actually holds (Imbens and Angrist 1994).

While the monotonicity assumption is inherently untestable, Table 14 attempts a simple procedure to try to assess the degree to which monotonicity is likely to hold. Essentially, the procedure first determines whether there are significant non-linear effects of the instrument on the conditional mean of the endogenous treatment variable. If not, then one can proceed with a LATE interpretation of the identified parameter. If there are significant non-linear effects, however, then two approaches can be taken, one of which is shown in Table 14, and the other in Appendix Table 19. The underlying intuition of this procedure is essentially just to characterize the conditional mean function of the endogenous variable, examine the extent to which it differs for subsamples of the estimation data, and finally determine whether any of these differences, if significant, would lead us to necessarily conclude that monotonicity does not hold (see Data Appendix for further details).

Table 14 shows that we cannot reject in any case that the quartile turning points Z_q^* in columns (3)-(6) are identical to the overall subsample turning point in column (2), Z_{all}^*. This gives us at least suggestive evidence that the monotonicity assumption does hold for the given subsample, and that if we re-run our second stage estimation for this sample as in column (7), we can more credibly interpret the IV aid parameter as a LATE. As shown, the IV aid coefficient for the subsample is nearly identical to the aid coefficient from the full sample in Table 12, column (3). Column (8) shows that for the four-year period data, there are no significant non-linear effects in the full sample first stage, and so we can indeed interpret the aid parameter in Table 12, column (6) as a LATE.

3.6.2 Mechanisms of the Aid-Growth Effect

In order to understand more about the nature of the positive, short- to medium-run growth effect and its dynamics estimated in our main results, it is necessary to decompose the national accounts components that are actually being affected by the aid increase and accounting for the growth increase. While we do not go into as detailed a description as Werker et al. (2007) of how aid inflows are being spent, our discussion follows in the same spirit.

Table 15 focuses on the effect of aid on national accounts for the medium-term, four-year period data. The table shows that the positive effect of aid on growth is occurring through an increase in household consumption, where an inflow of aid equal to 1 percent of GDP increases consumption by 4.7 percent of GDP.[24] Physical capital investment (the sum of both private and government investment) is actually somewhat crowded out by the aid inflow, surprisingly, as a similar increase in aid equal to 1 percent of GDP decreases investment by 1.8 percent of GDP. However, unlike the positive consumption effect, this negative effect is significant only in IV estimation and not OLS. If households are somewhat liquidity constrained and there are sufficiently poor returns to investment (so that it is a relatively less attractive alternative), then such a result could occur, for instance, if following the aid increase, consumers transition from consumption of inferior goods to more costly, previously unaffordable normal goods. Or alternatively, if more "lumpy," costly durable good consumption occurred following the aid increase.[25] Easterly (2001) discusses how low returns to investment might play a role in the absence of strong empirical evidence for a positive effect of aid on growth. Werker et al. (2007) also find a decrease in investment following increased aid inflows in their work. However, it is also possible that measurement error and the misclassification of some investment as consumption spending plays a role in this result. Moreover, both the standard and AR 95 percent

[24]The mechanism for this consumption increase could be lowered taxes by government following aid receipt, or alternatively increased transfers from government to households.

[25]We unfortunately did not have ready access to more fully disaggregated consumption data for all of the countries in our estimation sample to explore these hypotheses.

confidence bounds on these consumption and investment estimates are rather large, although coefficients of zero are rejected in all cases.

No other national accounts components are affected by aid inflows, as neither government consumption nor net exports are significantly different from zero. In line with the augmented Solow model (Mankiw, Romer, and Weil 1992), we also explore whether aid has any effect on human capital investment, as measured by secondary school enrollment as a fraction of the population. However, there is no discernible effect of aid here either. Finally, given the exclusive role that the Solow model suggests for technological progress and long-run growth, as well as the similar importance suggested by other growth models and empirical growth studies for such total factor productivity (TFP) growth (e.g., Bosworth and Collins 2003), we also examine the impact of aid on physical capital productivity and labor productivity via measures of R&D expenditures and health expenditures, respectively. Once again, we see no statistically significant impact of aid on these TFP proxies. This is consistent with the lack of long-term growth effects we observe throughout our analysis in section 5.

Finally, there is an additional test of the existence of long-run growth effects that can be run which seems relatively unexplored in the aid-growth literature. Assuming small and/or short-run multiplier effects from household and government spending, if there were only a mechanical increase in GDP growth from aid, then there should not exist any significant difference between the sum of the aid coefficients in the national accounts specifications, β_Y, and the aid coefficient in the overall growth specification, β_{ODA}. If aid inflows were having some longer-term effect on growth that was not being picked up by our other explorations (i.e., Figure 4, Table 13, and columns (2) and (5)-(7) of the current table), then we might expect there to be a significant, residual difference between β_Y and β_{ODA}. However, a Wald test strongly fails to reject the equality of those coefficients at even 10 percent significance, providing yet further support of the lack of a long-run growth effect.

3.6.3 Heterogenous Effects

It is of great interest to explore which recipient countries are accounting most for the results we have seen thus far, in order to better understand both the internal validity of our analysis as well as the extent to which the external validity of our estimates would hold. Such understanding is essential before any kind of policy considerations can or should be taken from any such research in this area.

Table 16 explores theoretically-motivated, potential avenues of heterogeneity in the aid-growth response for the four-year period data. In the broad context of a Solow model guide, region-focused regressions (1)-(5) might be motivated by the existence of country or region-specific aggregate production functions (Islam 1995). An alternative source of motivation for such analysis, to be further explored in Figure 5, is that we might simply worry that countries in some regions are being disproportionately induced to the aid treatment by the neighbor drought instrument and that these are the countries identifying the LATE IV aid coefficients (Imbens and Angrist 1994). To the extent that these countries have systematic, underlying differences in variables that we believe may matter for the nature of the aid treatment effect, it is important to identify whether this is actually occurring. Columns (1)-(4) show that European and African countries may be somewhat driving the estimated IV aid coefficients in our main results. The aid coefficient of 1.7 percentage points in column (5) when countries from Latin America and the Caribbean are excluded is identical to the overall sample coefficient in Table 12, column (8).[26]

[26] It should be noted that small sample sizes for each region (e.g., 50 observations for Europe) motivated the choice

Specifications (6) and (7), which examine quadratic specifications for aid, follow directly from the diminishing returns to capital assumption of the Solow model, in the event that aid is being invested and converted into capital. Unfortunately, while the aid coefficient estimate of 1.7 percentage points is quite similar to before, it is very imprecisely measured due to the weak "first stage" analog here, as evidenced by the very low values of the Cragg-Donald statistic (Cragg and Donald 1993) and the cluster-robust Kleibergen-Papp rk statistic (Kleibergen and Papp 2006), for use with multiple endogenous regressors. Moreover, the coefficient on the quadratic aid term of -0.2, while negative and thus consistent with diminishing marginal returns, is not significant, in addition to being relatively small and also imprecisely measured. However, estimation issues aside, these results are not necessarily surprising given our earlier finding that the aid inflows appear to be predominantly consumed rather than invested, although we still might have expected to observe some diminishing returns to consumption if this reflects representative agent utility.

As noted earlier, Imbens and Angrist (1994) discuss how LATE parameters from IV are local to the observations that are actually induced to treatment by the instrument(s). In other words, the IV estimates only represent the average treatment effect for those aforementioned observations. Thus, in our case, only country-years that, due to an aid neighbor drought, actually receive a different amount of aid compared to some counterfactual amount, actually contribute to the IV estimates. To further explore this, Figure 5 characterizes the nature of heterogeneity regarding which countries are generally induced by the instrument to be treated, in order to help in the interpretation of the results and the extent to which their external va-

to exclude one region from each estimation sample rather than run estimation on individual region samples. Also, although omitted for brevity, when countries from Oceania and Asia regions are similarly excluded, the resulting IV aid coefficients (1.9 and 1.8, respectively) are both significant at the 5 percent level and once again quite similar in magnitude to the overall sample coefficient of 1.7 percentage points.

lidity might hold. Following procedure outlined in Card (1995) and Kling (2001), we form a "marginal benefit index" from the predicted values (for all observations) of a regression of aid on lagged log(GDP per capita) and lagged log(population), estimated using recipients below the median value of neighbor drought exposure in a country-average sample (i.e., a cross-section). The regressors to form the index correspond to the donor allocation model, where the income of the representative agent from each recipient country affects the marginal utility of the next aid dollar received and, hence, donor allocation decisions. Countries are then grouped into bins based on quartiles of the benefit index, and median aid values (to better capture central tendency) of those bins are plotted.

Figure 5 illustrates that countries with a higher estimated marginal benefit of aid receive larger aid inflows, as both lines slope upward. Also, as expected from all of the prior regression analysis, countries whose aid neighbors had more severe or frequent droughts tend to receive more aid. Additionally, consistent with the results of Table 14, the two lines in the figure do not cross at any point, which is necessary (although not sufficient) to claim that monotonicity of the neighbor drought instrument's effect on the endogenous aid variable actually holds. Finally, the gap between the two lines indicates for which marginal benefit quartile neighbor droughts influence treatment (aid inflows) the most.[27]

Because, for ease of interpretation, the figure does not incorporate any weighting of recipient importance to their donors, it is difficult to form definitive priors on which quartile should respond the most. In this case, it appears that countries in the low, second marginal benefit quartile account for the majority of the IV estimate, relatively speaking (given that the 4th and 3rd quartiles together also appear to

[27] All but the third of these figure discussions are directly analogous to interpretation by Dinkelman (2008).

account for a non-trivial proportion of the estimate). Some countries in this group of 18 (i.e., the 18 countries with above-median neighbor drought exposure) are, for instance, Botswana, Bulgaria, El Salvador, Jamaica, and Vietnam, which span a wide array of regions, contrary to the potential concern raised earlier.[28] Moreover, the analysis of Table 16 combined with this analysis suggests that the seven African and European nations that are a subset of this group - namely, Botswana, Bulgaria, Cameroon, Egypt, Estonia, Latvia, and Morocco - may be most important in determining the magnitude and significance of the IV estimates. Thus, further exploration beyond the scope of this paper of the characteristics of these nations (e.g., investment climate, consumption expenditures, etc.) is necessary, as well as for the full 18 countries in this group. This would allow for further determination of whether the aid-growth results we estimate are specific to these recipients and time period, or are more generally applicable - an important distinction for both deeper understanding and policy analysis.

3.7 Sensitivity Analyses

We now explore how robust the main aid-growth IV estimates of Table 12, columns (3) and (8), are to changes in the estimator and control variables. This is of particular interest in the case of cross-country growth regressions, as they can be sensitive to such choices.

Table 17 displays these results, and there are several cases where the estimates remain fairly similar even with the change in specification. For instance, the stability of the first check using unweighted neighbor drought exposure is reassuring in the event of any concern that the aid share weighting structure induces endogeneity

[28]The full list of 18 countries with the most neighbor drought exposure in the second quartile of the aid marginal benefit index is: Bangladesh, Bolivia, Botswana, Bulgaria, Cameroon, Ecuador, Egypt, El Salvador, Estonia, Jamaica, Kazakhstan, Latvia, Lebanon, Morocco, Pakistan, Sri Lanka, Vietnam, and Yemen.

in the instrument. The robustness of the second check, meanwhile, addresses any fear that the handful of observations where recipients actually repay donors are completely driving the results. The third alternative specification where aid flows from multilateral donors are included leads to smaller and more precisely estimated aid coefficients. However, the results here are qualitatively similar to the main results in section 5. The lack of sensitivity of the results to specification check 7, when inflation as a control is included, is also comforting, as one might worry that aid neighbor drought occurrence also changes the price structure of goods and services for the aid neighbors. Additionally, the four-year period results are relatively stable as well for specifications 4 and 5 when aid neighbor earthquake exposure is also included as an instrument and when a further, slight change is made to estimation in first differences, respectively. However, the instruments are jointly quite weak in these specifications, and so not much faith can be placed in the estimates. Moreover, the annual results for these specifications are inconclusive. The same holds for robustness check 6, where the results are fairly similar to specification 5.[29]

However, admittedly, the explanatory power of the neighbor drought instrument is not strong enough to hold up to the most demanding, remaining specifications, such as the additional controls of robustness checks 8 and 9 (which Solow growth model analysis can motivate, essentially, as institutional proxies for recipient domestic savings behavior). As a result, subsequent inference and conclusions need to be more cautious in light of any potential model misspecification in these analyses. The final two robustness checks, which utilize GMM estimation from an Arellano-Bond inspired procedure (Arellano and Bond 1991) but are similarly inconclusive, were included for comparison due to the usage of such a procedure by several recent pa-

[29]The odd results in specifications 5 and 6, column (1) stem from the low raw correlation of changes in neighbor drought exposure with changes in aid at the annual frequency (correlation coefficient $\rho = 0.007$) compared to the four-year period frequency (correlation coefficient $\rho = 0.061$), where the correlation is an order of magnitude larger.

pers in this literature as an alternative approach to try to consistently estimate the impact of aid on growth in the presence of recipient fixed effects (e.g., Hansen and Tarp 2001, Rajan and Subramanian 2005b, Werker et al. 2007).

3.8 Conclusion

This paper addresses two closely-related research questions. We first use objective, meteorological and geological data on natural disaster exposure and OECD data on foreign aid flows to examine from 1979-2002 how disasters impact aid flows to affected countries as well as the unaffected countries with whom the affected nations compete for aid. We define the latter, unaffected countries as the "aid neighbors" of the affected countries. Own drought exposure is shown to significantly increase aid inflows. Meanwhile, we estimate that aid neighbor drought exposure and, at times, flood exposure significantly increase aid inflows, while aid neighbor earthquake exposure significantly decreases aid inflows. These observed patterns are in turn consistent with a basic model of donor aid allocation that we explore.

In the second part of the paper, we then utilize the disaster-induced variation in aid flows to unaffected countries to reexamine from 1979-2002 the question of how foreign aid impacts per capita GDP growth. We use aid neighbor drought exposure as an instrument for aid inflows, and estimate in this IV setting the effect of aid on growth and channels of growth. We find a significantly positive effect of aid on growth in the short- to medium-run, with an inflow of aid equal to 1 percent of GDP increasing recipient per capita GDP growth by 1.2-1.7 percentage points. This positive growth effect occurs via increased household consumption, while overall physical capital investment appears to decrease. We find no effect of aid on proxies for human capital investment and factor productivity, nor do we observe any direct impact of aid on long-term growth through a variety of settings. Further analysis of the heterogeneity in this growth response to aid reveals that countries from Africa and Europe may particularly account for the observed results. Additionally, countries with lower estimated marginal benefits of aid are also shown to predominantly drive the IV results, as they are the recipients that are most induced to aid treatment by neighbor drought exposure. While our main results are not sensitive to several robustness checks, under the most stringent specifications, weakness of the aid neighbor disaster instrument(s) prevents much conclusive analysis. Our results are consistent with the strand of the aid-growth literature that has found an unconditional, positive growth effect of aid, and yet also support studies that have not found any long-run aid-growth effects, providing plausible explanation for those results.

While this analysis is suggestive, several caveats apply and further analysis remains before drawing definitive conclusions. The paper focuses on bilateral and, in a few cases, multilateral aid to recipient countries. However, any private aid flows that are not redirected through multilateral institutions (which many are) would not be accounted for in our analysis. To the extent that such flows are both substantial and impacted by neighbor disaster exposure, this would bias our results. Additionally, it would be of interest to gain a deeper understanding of the source of the disparate responses of aid flows to disasters of different types, to determine the underlying mechanisms of the positive drought aid spillover but negative earthquake aid spillover that we observe. Moreover, the negative effect that increased aid inflows appear to have on investment in our results is somewhat surprising and atypical. As discussed, while this could be an issue of measurement error, there are scenarios where this result could be explained. Additional analysis with more disaggregated consumption data would help to address this issue by revealing how the types of goods consumed by foreign aid recipients changed, if at all, before and after the inflow. Similarly, further exploration of the investment climate and other characteristics of the subsample of 18 countries that appear to drive the IV results could also help in explaining this investment result.

Caveats aside, it is clear that how aid is used by the recipient country is crucial to understanding its estimated growth impact and what effect we should expect to observe. If aid is indeed predominantly consumed, then the lack of a long-term growth effect is not surprising. Nevertheless, a temporary growth increase and a rise in the level of GDP per capita for aid recipients are non-trivial benefits, even if longer-term growth was the primary donor objective. However, if further work were to substantiate the investment crowd-out of aid that we appear to observe, then this might be of some concern. Furthermore, it would also be of interest to examine whether the increased household consumption from aid is evenly or unevenly spread across the population. If the latter - whether due to unequal government transfers or tax decreases, or alternatively perhaps due to corruption and misappropriation of aid inflows - this may of concern to recipients where income equity or alternative distributions are desired.

Despite the econometric difficulties and shortcomings of such macro-level, aid-growth analysis, the aid usage issue is precisely the type of inquiry that necessitates such investigation. Micro-level analyses of the effects of increases in investment or factor productivity on growth are only suggestive of the impact of aid, conditional on its usage in those manners. The new approach in this paper to credibly estimate the effect of aid on growth at the macro-level is an additional contribution to this ever-growing literature. While such macro-level, cross-country studies are difficult to implement convincingly and as a result have often, rightfully, been criticized for their methodology, such analyses still merit pursuit if improved policy recommendations and a greater understanding of the causes and growth consequences of how aid is utilized are to be achieved.

3.9 Figures and Tables

Table 3.1: Damages and human losses from natural disasters worldwide, 1979-2002

Type of disaster	Damage (1995 US$, 000s)	% of total damage	Killed (000s)	% of total killed	Injured (000s)	% of total injured
Earthquake	280,000,000	31.35%	153	9.68%	628	28.71%
Flood	260,000,000	29.12%	161	10.21%	975	44.58%
Wind storm	245,000,000	27.46%	258	16.35%	477	21.80%
Drought	53,400,000	5.98%	559	35.48%	0	0.00%
Wild fire	27,900,000	3.12%	1	0.07%	2	0.10%
Extreme temperature	17,000,000	1.90%	20	1.28%	10	0.43%
Volcano	5,340,000	0.60%	26	1.66%	8	0.36%
Earth slide	3,820,000	0.43%	18	1.14%	7	0.34%
Insect infestation	248,000	0.03%	0	0.00%	0	0.00%
Famine	71,800	0.01%	232	14.75%	0	0.00%
Wave / Surge	4,700	0.00%	3	0.21%	1	0.03%
Epidemic	1,500	0.00%	145	9.18%	80	3.65%
Total	892,786,000	100.00%	1,575	100.00%	2,187	100.00%

Notes: All figures are in thousands. Data are worldwide totals between 1979-2002 from EM-DAT: the OFDA/CRED International Disaster Database, Université Catholique de Louvain, Brussels, Belgium. (Available at www.em-dat.net). Damage figures in EM-DAT converted to constant 1995 US dollars using GDP deflators and exchange rates from World Bank's World Development Indicators 2004. Disaster types in table sorted by financial damage. Wind storm category includes phenomena variously referred to as cyclones, hurricanes, storms, tornadoes, tropical storms, typhoons, or winter storms. The time period examined corresponds to the estimation period for the main second stage growth analyses in the paper.

Table 3.2: Stability of donors' aid recipients over time

	1960-1969			1990-1999		
Donor	*Top Ten Recipients*	*Aid Outflows*	*% Aid Outflows*	*Top Ten Recipients*	*Aid Outflows*	*% Aid Outflows*
France	Algeria	14,477	53.36%	Cote d'Ivoire	4,231	7.73%
	Morocco	1,686	6.22%	New Caledonia	4,015	7.33%
	Senegal	1,144	4.22%	French Polynesia	3,954	7.22%
	Madagascar	1,002	3.69%	Egypt	3,163	5.78%
	Tunisia	996	3.67%	Cameroon	2,731	4.99%
	Cote d'Ivoire	896	3.30%	Senegal	2,392	4.37%
	Cameroon	563	2.07%	Morocco	2,367	4.32%
	Niger	509	1.88%	Poland	1,885	3.44%
	Chad	419	1.54%	Algeria	1,880	3.43%
	Congo, Rep.	418	1.54%	Madagascar	1,453	2.65%
	All Ten	22,110	81.50%	All Ten	28,071	51.27%
United States	India	31,258	22.31%	Israel	13,244	20.91%
	Vietnam	14,316	10.22%	Egypt	12,763	20.15%
	Pakistan	13,474	9.61%	Russia	3,643	5.75%
	Korea	9,787	6.98%	Poland	3,196	5.05%
	Brazil	8,515	6.08%	Haiti	1,482	2.34%
	Turkey	6,320	4.51%	El Salvador	1,460	2.30%
	Egypt	4,499	3.21%	Philippines	1,447	2.28%
	Chile	3,772	2.69%	Somalia	1,385	2.19%
	Indonesia	2,956	2.11%	Bolivia	1,056	1.67%
	Taiwan	2,806	2.00%	Nicaragua	1,023	1.62%
	All Ten	97,704	69.72%	All Ten	40,699	64.26%

Notes: All aid flows are in constant 1995 million USD, and represent disbursements only. Top recipients in table sorted by % of aid outflows.

Table 3.3: Descriptive statistics

Variable	N (1)	Mean (2)	Annual Data Std Dev (3)	Min (4)	Max (5)
Net ODA / mean GDP	2,853	0.065	0.100	-0.007	1.405
GDP (billions)	2,853	39.7	103.0	0.032	1,210
Mean GDP (billions)	2,853	36.9	94.7	0.033	1,040
Population (millions)	2,853	33.5	130.0	0.041	1,280
Mean population (millions)	2,853	32.3	127.0	0.041	1,260
Per capita GDP growth rate	2,853	0.011	0.060	-0.454	0.667
Own exposure : storm index	2,816	0.003	0.022	0.000	0.469
Own exposure : no. of earthquakes >=4 magnitude, 50 buffer, per sq. mile	2,836	0.001	0.005	0.000	0.094
Own exposure : floods, proportional deviations from median rainfall	2,853	0.079	0.172	0.000	3.073
Own exposure : droughts, proportional deviations from median rainfall	2,853	0.068	0.105	0.000	0.699
Aid neighbor exposure : storm index	2,853	0.035	0.024	0.000	0.114
Aid neighbor exposure : no. of earthquakes >=4 magnitude, 50 buffer, per sq. mile	2,853	0.008	0.005	0.000	0.022
Aid neighbor exposure : floods, proportional deviations from median rainfall	2,853	1.187	0.552	0.022	2.861
Aid neighbor exposure : droughts, proportional deviations from median rainfall	2,853	1.150	0.519	0.053	2.586
Damage / mean GDP	2,807	0.008	0.102	0.000	4.332
Killed / population	2,853	0.000	0.000	0.000	0.008
Injured / population	2,853	0.000	0.001	0.000	0.029
Trade value (goods & services) / mean GDP	2,853	0.829	0.469	0.017	4.943
Population growth (approx.)	2,853	0.021	0.016	-0.316	0.249
Migrants' remittances / mean GDP	2,020	0.030	0.086	-0.142	0.962
Multilateral institution lending / mean GDP	2,450	0.025	0.039	-0.125	0.509
Bank and trade-related lending / mean GDP	2,450	0.005	0.023	-0.117	0.235
Foreign direct investment / mean GDP	2,387	0.023	0.062	-0.283	2.078
Portfolio investment / mean GDP	2,384	0.000	0.033	-0.655	0.418
Household consumption / mean GDP	2,248	0.762	0.216	0.129	2.135
Gross capital formation / mean GDP	2,310	0.258	0.137	-0.061	1.647
Government consumption / mean GDP	2,265	0.165	0.077	0.025	0.645
Net exports / mean GDP	2,853	-0.099	0.184	-1.376	0.629
Secondary school enrollment / mean population	1,151	0.070	0.037	0.004	0.224
R&D expenditure / mean GDP	190	0.008	0.010	0.000	0.067
Health expenditure / mean GDP	671	0.057	0.021	0.010	0.137
Inflation rate (GDP deflator, annual %)	2,853	60.5	456.7	-25.7	13,612
Money and quasi-money (M2) / mean GDP	2,680	0.402	0.281	0.009	2.140
Civil War Initiation or Continuation (COW)	2,756	0.075	0.264	0.000	1.000
Interstate War Initiation or Continuation (COW)	2,756	0.007	0.085	0.000	1.000
Trade openness (Sachs/Warner 1995)	1,034	0.266	0.442	0.000	1.000
Budget balance / mean GDP	1,878	-335.9	598.3	-5,391	5,187
Inflation rate, food prices (annual %)	2,197	284.8	10,672	-100.0	499,900

Notes : All descriptive statistics reflect estimation sample for annual data (e.g., Table 11, columns 1-5). Net ODA and all other monetary variables are in constant (1995) USD unless noted otherwise. Mean GDP is average of GDP 1-3 years before, and mean population is average of population 1-3 years before. Own disaster exposure measures are actual exposure, while neighbor disaster exposure measures are constructed as described in the paper. Population growth (approx.) is the change in population divided by mean population. Civil and interstate war data is from Correlates of War (COW) project, 2002 (Penn State University, http://www.correlatesofwar.org/). Trade openness is from Sachs and Warner (1995).

Table 3.4: Impact of disaster-related financial and human losses on recipient aid inflows (OLS)

	Dependent Variable: Net ODA as fraction of GDP		
Explanatory Variable	Damage	Killed	Injured
	(1)	(2)	(3)
Own Exposure, year t			
Damage, Killed, or Injured due to:			
Storms	-0.001	-18.939	-75.502
	(0.002)	(163.035)	(120.482)
Earthquakes	0.000	-198.330	-11.152
	(0.001)	(185.719)	(50.622)
Floods	0.001	588.717	1.363
	(0.000)*	(263.983)**	(35.947)
Droughts	0.004	41.246	
	(0.004)	(27.302)	
R^2	0.69	0.69	0.69
Number of observations	4,430	4,502	4,502

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Net ODA is in constant (1995) USD and is divided by mean GDP 1-3 years before. Own financial and human loss data due to each disaster type is from EM-DAT, with damage figures in constant (1995) billion USD and human loss data in billion individuals. Regressions have recipient and year fixed effects and recipient clustered standard errors.

Table 3.5: Impact of disasters on recipient aid inflows (OLS)

Dependent Variable: NetODA as fraction of GDP

Explanatory Variable	Own (1)	Own + Neighbor (2)	Own + Unweighted Neighbor (3)	Mean GDP 1975-79 Low (4)	Mean GDP 1975-79 High (5)	Mean GDP Interactions (6)	Own Weighted + Neighbor (7)	Multilateral Donors Included (8)	Excluding Ethiopia (9)
Neighbor Exposure, year t									
Storm Index		0.174	0.000	0.270	0.360	0.389	0.144	0.116	0.170
		(0.216)	(0.001)	(0.356)	(0.297)	(0.228)*	(0.224)	(0.141)	(0.217)
No. of earthquakes >=4 magnitude, 50 buffer, per sq. mile		-2.850	-0.001	-7.770	-0.942	-3.212	-2.885	-1.284	-2.858
		(1.219)**	(0.008)	(3.275)**	(0.753)	(1.438)**	(1.214)**	(1.073)	(1.224)**
Floods: prop. deviations from median rainfall		0.022	0.000	-0.002	-0.008	-0.002	0.022	0.030	0.022
		(0.010)**	(0.000)	(0.018)	(0.008)	(0.008)	(0.011)**	(0.010)***	(0.010)**
Droughts: prop. deviations from median rainfall		0.032	0.000	0.054	0.014	0.015	0.031	0.028	0.032
		(0.007)***	(0.000)***	(0.020)***	(0.010)	(0.009)	(0.007)***	(0.007)***	(0.007)***
Own Exposure, year t									
Storm Index	0.006	0.029	0.014	0.067	0.035	0.048	-0.061	0.001	0.029
	(0.027)	(0.033)	(0.031)	(0.064)	(0.028)	(0.047)	(0.238)	(0.030)	(0.033)
No. of earthquakes >=4 magnitude, 50 buffer, per sq. mile	0.094	-0.011	0.059	-0.319	0.240	2.456	0.536	-0.080	-0.004
	(0.173)	(0.205)	(0.221)	(1.384)	(0.238)	(1.545)	(3.176)	(0.205)	(0.206)
Floods: prop. deviations from median rainfall	0.002	0.002	0.003	-0.014	0.000	-0.011	0.096	0.005	0.002
	(0.004)	(0.005)	(0.005)	(0.017)	(0.003)	(0.016)	(0.032)***	(0.008)	(0.005)
Droughts: prop. deviations from median rainfall	0.013	0.016	0.016	0.067	-0.009	0.044	0.186	0.036	0.016
	(0.013)	(0.013)	(0.013)	(0.030)**	(0.012)	(0.027)*	(0.100)*	(0.021)*	(0.013)
Own Storms x (Mean GDP 1975-79 > median)						-0.010			
						(0.051)			
Own Quakes x (Mean GDP 1975-79 > median)						-2.486			
						(1.550)			
Own Floods x (Mean GDP 1975-79 > median)						0.010			
						(0.016)			
Own Droughts x (Mean GDP 1975-79 > median)						-0.052			
						(0.028)*			
R^2	0.79	0.79	0.79	0.70	0.86	0.79	0.80	0.82	0.80
Number of observations	2,983	2,982	2,982	1,124	1,137	2,261	2,982	2,982	2,961

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: NetODA is in constant (1995) USD and is divided by mean GDP 1-3 years before. Own disaster exposure measures are actual exposure except for column (7) when they are weighted. Neighbor disaster exposure measures are weighted exposure except for column (3) when they are unweighted, actual exposure. Columns (4) and (5) are run on a split sample of countries whose mean GDP in 1975-1979 is below (or equal to) and above the median, respectively. Specification (6) pools those countries together and alternatively interacts with each own disaster variable a dummy variable of mean GDP in 1975-1979 above the median. Column (7) weights own disaster exposure similarly to neighbor exposure, while specification (8) is run on a sample that incorporates flows from multilateral donors to the recipients. Column (9) excludes all Ethiopia observations from analysis. Regressions have recipient and year fixed effects and recipient clustered standard errors.

Table 3.6: Impact of disasters on recipient aid inflows by weighting scheme (OLS)

	Dependent Variable: Net ODA as fraction of GDP			
Explanatory Variable	Unweighted Neighbor (no θ, no γ) (1)	Weighted Neighbor #1 (yes θ, no γ) (2)	Weighted Neighbor #2 (yes θ, yes γ) (3)	Weighted Neighbor #3 (no θ, yes γ) (4)
Neighbor Exposure, year t				
Storm index	0.000	0.174	-0.948	0.003
	(0.001)	(0.216)	(0.858)	(0.014)
No. of earthquakes >=4 magnitude, 50 buffer, per sq. mile	-0.001	-2.850	-12.093	-0.150
	(0.008)	(1.219)**	(15.064)	(0.099)
Floods: prop. deviations from median rainfall	0.000	0.022	0.120	0.003
	(0.000)	(0.010)**	(0.127)	(0.002)
Droughts: prop. deviations from median rainfall	0.000	0.032	0.267	0.005
	(0.000)***	(0.007)***	(0.138)*	(0.002)***
Own Exposure, year t				
Storm index	0.014	0.029	0.005	0.010
	(0.031)	(0.033)	(0.029)	(0.028)
No. of earthquakes >=4 magnitude, 50 buffer, per sq. mile	0.059	-0.011	0.129	0.069
	(0.221)	(0.205)	(0.187)	(0.164)
Floods: prop. deviations from median rainfall	0.003	0.002	0.001	0.004
	(0.005)	(0.005)	(0.005)	(0.005)
Droughts: prop. deviations from median rainfall	0.016	0.016	0.013	0.017
	(0.013)	(0.013)	(0.013)	(0.013)
R^2	0.79	0.80	0.79	0.79
Number of observations	2,982	2,982	2,982	2,982

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Net ODA is in constant (1995) USD and is divided by mean GDP 1-3 years before. Own disaster exposure measures are actual exposure in all specifications. Neighbor disaster exposure measures are weighted exposure as noted, except for column (1) where they are unweighted, actual exposure. More specifically, θ weights measure recipient importance to the donor, and are proxied using the recipient share of donor outflows, while γ weights measure donor importance to the recipient, and are proxied using the donor share of recipient inflows. Regressions have recipient and year fixed effects and recipient clustered standard errors.

Table 3.7: Mean lagged impact of disasters on recipient aid inflows (OLS)

	Dependent Variable: Net ODA as fraction of GDP		
Explanatory Variable	Years *t* to *t-1*	Years *t* to *t-3*	Years *t* to *t-7*
	(1)	(2)	(3)
Mean Neighbor Exposure			
Storm index	0.343	0.286	-0.619
	(0.285)	(0.381)	(0.879)
No. of earthquakes >=4 magnitude, 50 buffer, per sq. mile	-3.246	-2.690	-0.042
	(1.284)**	(1.361)**	(3.486)
Floods: prop. deviations from median rainfall	0.021	0.027	0.020
	(0.013)	(0.016)*	(0.025)
Droughts: prop. deviations from median rainfall	0.036	0.044	0.052
	(0.009)***	(0.014)***	(0.034)
Mean Own Exposure			
Storm index	0.056	0.023	0.027
	(0.062)	(0.084)	(0.229)
No. of earthquakes >=4 magnitude, 50 buffer, per sq. mile	-0.002	0.305	0.504
	(0.282)	(0.529)	(1.103)
Floods: prop. deviations from median rainfall	0.003	0.010	0.028
	(0.009)	(0.013)	(0.036)
Droughts: prop. deviations from median rainfall	0.036	0.084	0.180
	(0.021)*	(0.033)**	(0.076)**
R^2	0.81	0.82	0.81
Number of observations	2,952	2,847	2,606

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Net ODA is in constant (1995) USD and is divided by mean GDP 1-3 years preceding earliest included lag of disaster exposure. Regressions have recipient and year fixed effects and recipient clustered standard errors.

Table 3.8: Impact of disasters on donor aid outflows and GDP per capita (OLS)

	Dependent Variable:			
	Log(Net ODA outflows)		Log(GDP per capita)	
Explanatory Variable	Unweighted	Weighted	Unweighted	Weighted
	(1)	(2)	(3)	(4)
Recipient Exposure, year t				
Storm index	-0.260	0.115	0.007	-0.331
	(0.581)	(9.967)	(0.026)	(0.611)
No. of earthquakes >=4 magnitude, 50 buffer, per sq. mile	5.129	56.540	-0.019	10.543
	(1.075)***	(66.298)	(0.235)	(9.639)
Floods: prop. deviations from median rainfall	0.013	3.444	0.008	0.306
	(0.015)	(1.708)*	(0.003)***	(0.261)
Droughts: prop. deviations from median rainfall	0.091	4.895	0.013	0.189
	(0.032)***	(2.490)*	(0.004)***	(0.230)
Donor Own Exposure, year t				
Storm index	6.512	12.018	2.363	2.307
	(4.160)	(4.358)**	(0.761)***	(0.752)***
No. of earthquakes >=4 magnitude, 50 buffer, per sq. mile	-203.796	-211.536	12.395	13.227
	(96.474)**	(101.460)**	(16.917)	(16.552)
Floods: prop. deviations from median rainfall	-0.702	-0.637	-0.038	-0.012
	(0.665)	(0.732)	(0.031)	(0.040)
Droughts: prop. deviations from median rainfall	-0.571	-0.350	0.005	0.034
	(0.508)	(0.585)	(0.060)	(0.071)
R^2	0.94	0.93	0.99	0.99
Number of observations	530	530	526	526

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes Net ODA and GDP per capita are in constant (1995) USD. Donor own disaster exposure measures are actual exposure. Recipient disaster exposure measures reflect an aggregate of all recipients for each donor, where that sum is weighted or unweighted as noted. Regressions have donor and year fixed effects and donor clustered standard errors.

Table 3.9: Impact of disasters on recipient financial and human losses (OLS)

	Dependent Variable		
Explanatory Variable	Damage as fraction of GDP (1)	Killed as fraction of population (2)	Injured as fraction of population (3)
Neighbor Exposure, year t			
Storm index	-0.371	-0.152	4.564
	(0.694)	(0.670)	(3.599)
No. of earthquakes >=4 magnitude, 50 buffer, per sq. mile	0.023	-2.153	-12.493
	(1.203)	(1.924)	(9.704)
Floods: prop. deviations from median rainfall	-0.039	-0.013	-0.309
	(0.030)	(0.009)	(0.277)
Droughts: prop. deviations from median rainfall	0.039	0.003	0.039
	(0.033)	(0.007)	(0.044)
Own Exposure, year t			
Storm index	0.544	0.351	9.238
	(0.245)**	(0.135)***	(7.467)
No. of earthquakes >=4 magnitude, 50 buffer, per sq. mile	-0.706	-0.266	-1.367
	(0.766)	(0.316)	(3.105)
Floods: prop. deviations from median rainfall	-0.001	0.035	0.031
	(0.008)	(0.019)*	(0.045)
Droughts: prop. deviations from median rainfall	0.001	0.085	0.075
	(0.009)	(0.081)	(0.065)
R^2	0.09	0.06	0.20
Number of observations	2,935	3,401	3,401

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes Damage is in constant (1995) USD and is divided by mean GDP 1-3 years before. Killed and injured in disasters are divided by mean population 1-3 years before. Own disaster exposure measure is actual exposure. Regressions have recipient and year fixed effects and recipient clustered standard errors.

Table 3.10: Impact of disasters on other recipient financial inflows (OLS)

	Dependent Variable				
	Migrants' remittances as fraction of GDP	Multilateral institution lending as fraction of GDP	Bank & trade-related lending as fraction of GDP	Foreign direct investment as fraction of GDP	Portfolio investment as fraction of GDP
Explanatory Variable	(1)	(2)	(3)	(4)	(5)
Neighbor Exposure, year / period t					
Droughts: prop. deviations from median rainfall	0.017	0.011	0.002	-0.011	0.029
	(0.011)	(0.007)	(0.006)	(0.009)	(0.018)
Own Exposure, year / period t					
Droughts: prop. deviations from median rainfall	0.007	0.015	0.004	-0.012	0.018
	(0.017)	(0.009)*	(0.006)	(0.009)	(0.012)

Trade value (goods & services) as fraction of GDP	0.032	0.004	0.000	0.101	-0.002
	(0.013)**	(0.009)	(0.003)	(0.060)*	(0.008)
Population growth (approx.)	0.073	0.164	0.063	-0.010	0.122
	(0.266)	(0.094)*	(0.074)	(0.066)	(0.118)
R^2	0.84	0.43	0.23	0.42	0.23
Number of observations	2,020	2,450	2,450	2,388	2,385
F test: neighbor drought = 0	2.12	2.64	0.10	1.38	2.65
Prob > F	0.15	0.11	0.75	0.24	0.11

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: All dependent variables are divided by mean GDP 1-3 years before. Population growth (approx.) is the change in population divided by mean population 1-3 years before. Own disaster exposure measure is actual exposure. All regressions have recipient and year fixed effects and recipient clustered standard errors.

Table 3.11: Impact of disasters on recipient aid inflows, first stage (OLS)

	Dependent Variable: Net ODA as fraction of GDP			
Explanatory Variable	Annual		4-year average	
	(1)	(2)	(3)	(4)
Neighbor Exposure, year or period t				
Droughts: prop. deviations from median rainfall	0.032	0.027	0.044	0.037
	(0.008)***	(0.008)***	(0.013)***	(0.014)***
Own Exposure, year or period t				
Droughts: prop. deviations from median rainfall	0.018	0.015	0.074	0.070
	(0.013)	(0.013)	(0.033)**	(0.032)**

Trade value (goods & services) as fraction of GDP		0.018		0.021
		(0.018)		(0.023)
Population growth (approx.)		-0.215		-0.197
		(0.090)**		(0.155)
R^2	0.78	0.81	0.87	0.89
Number of observations	3,026	2,853	748	716
F test: neighbor drought = 0	16.40	10.33	10.90	7.14
Prob > F	0.00	0.00	0.00	0.01

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Net ODA and trade value are in constant (1995) USD and are divided by mean GDP 1-3 years before in annual data and 1 period before in 4-year average data. Population growth (approx.) is the change in population divided by mean population 1-3 years before in annual data and 1 period before in 4-year average data. Own disaster exposure measures are actual exposure. Regressions have recipient and year or period fixed effects and recipient clustered standard errors.

Table 3.12: Impact of foreign aid on economic growth: short- to medium-run

	Dependent Variable: Per Capita GDP Growth Rate									
	Annual					4-year average				
Explanatory Variable	OLS	OLS	IV	2SLS	2SLS	OLS	OLS	IV	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Net ODA as fraction of GDP	0.039		1.193	2.327	-0.042	0.087		1.693	1.861	-0.045
	(0.033)		(0.653)*	(0.712)***	(0.031)	(0.040)**		(0.864)*	(0.583)***	(0.028)
Neighbor exposure: Droughts, prop. deviations from median rainfall		0.032					0.063			
		(0.013)**					(0.017)***			
Own exposure: Droughts, prop. deviations from median rainfall	-0.016	-0.013	-0.031	-0.046	-0.027	0.010	0.020	-0.098	-0.109	-0.009
	(0.014)	(0.014)	(0.022)	(0.031)	(0.013)**	(0.023)	(0.023)	(0.069)	(0.062)*	(0.022)
Trade value (goods & services) as fraction of GDP	0.105	0.102	0.081	0.057	0.032	0.079	0.075	0.038	0.034	0.030
	(0.009)***	(0.009)***	(0.027)***	(0.048)	(0.006)***	(0.008)***	(0.007)***	(0.047)	(0.048)	(0.005)***
Population growth (approx.)	0.040	0.052	0.309	0.573	-0.101	-0.277	-0.270	0.064	0.100	-0.205
	(0.283)	(0.274)	(0.321)	(0.361)	(0.183)	(0.582)	(0.605)	(0.636)	(0.654)	(0.190)
Recipient fixed effects	yes	yes	yes	yes	no	yes	yes	yes	yes	no
Alternative instruments	.	.	no	yes	yes	.	.	no	yes	yes
R^2	0.28	0.29	.	.	0.09	0.54	0.58	.	.	0.18
Root MSE	0.05	0.05	0.07	0.12	0.06	0.03	0.03	0.07	0.07	0.04
Number of observations	2,853	2,853	2,853	2,853	2,853	716	716	716	716	716
Standard 95% C.I. (ODA)	[-0.03-0.10]	.	[-0.09-2.47]	[0.93-3.72]	[-0.10-0.02]	[0.01-0.17]	.	[0.00-3.39]	[0.72-3.00]	[-0.10-0.01]
AR 95% C.I. (ODA)	.	.	[0.4-4.0]	[1.4-7.9]	[---]	.	.	[0.8-7.6]	[1.2-8.2]	[---]
First stage F test: neighbor drought = 0	.	.	10.33	7.58	33.19	.	.	7.14	6.39	33.73
Prob > F	.	.	0.00	0.00	0.00	.	.	0.01	0.00	0.00
Wald test: ODA = 1, p-value	0.00	.	0.77	0.06	0.00	0.00	.	0.42	0.14	0.00
Hausman test, p-value	0.02	.	.	.	.	0.00	.	.	.	.
Sargan-Hansen test, p-value	.	.	.	0.00	0.00	.	.	.	0.00	0.00

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Net ODA and trade value are in constant (1995) USD and are divided by mean GDP 1-3 years before in annual data and 1 period before in 4-year average data. Population growth (approx.) is the change in population divided by mean population 1-3 years before in annual data and 1 period before in 4-year average data. Own disaster exposure measures are actual exposure. Regressions have year or period fixed effects, recipient clustered standard errors and, where indicated, recipient fixed effects. Instrument in IV specifications (3) and (8) is contemporaneous neighbor disaster exposure measure for droughts (proportional deviations from recipient long-run median rainfall). Alternative instruments in 2SLS specifications (4), (5), (9) and (10) are log(GDP per capita) and log(population). AR confidence intervals are from the Anderson-Rubin test, which is robust to weak instruments and was constructed here to be robust to a clustered error structure. Hausman test of endogeneity and Sargan-Hansen test of overidentifying restrictions are likewise constructed to be cluster-robust.

Table 3.13: Impact of foreign aid on economic growth: medium- to long-run

Dependent Variable:
Per Capita GDP Growth Rate

Explanatory Variable	8-year average OLS (1)	8-year average IV (2)	12-year average OLS (3)	12-year average IV (4)	24-year average OLS (5)	24-year average IV (6)
Net ODA as fraction of GDP	-0.003	0.519	0.137	-0.282	-0.032	0.023
	(0.063)	(1.324)	(0.087)	(0.832)	(0.013)**	(0.234)
Own exposure: Droughts, prop. deviations from median rainfall	0.053	0.007	-0.099	-0.019	-0.018	-0.011
	(0.035)	(0.125)	(0.067)	(0.196)	(0.047)	(0.058)
Trade value (goods & services) as fraction of GDP	0.047	0.040	0.012	0.034	0.007	0.007
	(0.010)***	(0.035)	(0.010)	(0.042)	(0.001)***	(0.001)***
Population growth (approx.)	-0.383	-0.311	-0.190	-0.283	-0.173	-0.181
	(0.636)	(0.638)	(0.184)	(0.315)	(0.038)***	(0.055)***
Recipient and period fixed effects	yes	yes	yes	yes	no	no
R^2	0.73	0.50	0.87	0.73	0.51	0.41
Root MSE	0.02	0.03	0.02	0.02	0.01	0.01
Number of observations	343	343	227	227	92	92
Standard 95% C.I. (ODA)	[-0.13-0.12]	[-2.08-3.11]	[-0.03-0.31]	[-1.91-1.35]	[-0.06- -0.01]	[-0.44-0.48]
AR 95% C.I. (ODA)	.	[---]	.	[---]	.	[---]
First stage F test:						
neighbor drought = 0	.	0.21	.	0.75	.	0.33
Prob > F	.	0.65	.	0.39	.	0.57
Wald test: ODA = 1, p-value	0.00	0.72	0.00	0.13	0.00	0.00
Hausman test, p-value	0.66	.	0.48	.	0.80	.

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes Net ODA and trade value are in constant (1995) USD and are divided by mean GDP 1 period before. Population growth (approx.) is the change in population divided by mean population 1 period before. Own disaster exposure measures are actual exposure. Regressions have recipient and period fixed effects and recipient clustered standard errors except in specifications (5) and (6), which are cross-sections. Instrument in IV specifications (2), (4) and (6) is contemporaneous neighbor disaster exposure measure for droughts (proportional deviations from recipient long-run median rainfall). AR confidence intervals are from the Anderson-Rubin test, which is robust to weak instruments and was constructed here to be robust to a clustered error structure. Hausman test of endogeneity is likewise constructed to be cluster-robust.

Table 3.14: Assessing monotonicity of neighbor drought impact on aid inflows

Dependent Variable:
Net ODA as fraction of GDP (1-6, 8) and Per Capita GDP Growth Rate (7)

	Annual							4-year average
			OLS, Quartiles of Neighbor Drought Exposure (Z)					
Explanatory Variable	OLS	OLS	Q1	Q2	Q3	Q4	IV	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Net ODA as fraction of GDP							1.186	
							(0.625)*	
Neighbor Exposure, year or period t								
Droughts: prop. deviations from median rainfall	0.056	0.054	0.033	0.034	0.414	0.069		0.055
	(0.021)***	(0.023)**	(0.090)	(0.231)	(1.104)	(0.153)		(0.049)
Droughts: (prop. deviations from median rainfall)^2	-0.012	-0.011	-0.023	-0.005	-0.135	-0.016		-0.007
	(0.007)*	(0.008)	(0.100)	(0.108)	(0.405)	(0.039)		(0.018)
Own Exposure, year or period t								
Droughts: prop. deviations from median rainfall	0.014	0.015	-0.018	-0.003	0.052	0.028	-0.032	0.068
	(0.013)	(0.013)	(0.025)	(0.019)	(0.021)**	(0.028)	(0.022)	(0.031)**

Trade value (goods & services) as fraction of GDP	0.018	0.018	0.035	-0.016	0.042	0.016	0.081	0.022
	(0.018)	(0.019)	(0.026)	(0.031)	(0.012)***	(0.013)	(0.027)***	(0.024)
Population growth (approx.)	-0.215	-0.214	-0.541	-0.102	0.063	-0.262	0.307	-0.195
	(0.089)**	(0.090)**	(0.353)	(0.310)	(0.216)	(0.151)*	(0.314)	(0.154)
R^2	0.81	0.81	0.87	0.82	0.87	0.85	.	0.89
Root MSE	0.04	0.05	0.05	0.06	0.03	0.03	0.07	0.04
Number of observations	2,853	2,821	706	706	704	705	2,821	716
Standard 95% C.I. (ODA)	.	.	.	.	.	.	[-0.04-2.41]	.
AR 95% C.I. (ODA)	.	.	.	.	.	.	[0.4-3.9]	.
Support of Z: [Z_{min} Z_{max}]	[0.05-2.59]	[0.05-2.33]	.	.	.	.	.	[0.09-1.90]
Implied turning point, Z^*	2.33	2.45	0.71	3.47	1.53	2.20	.	3.74
	(0.52)	(0.80)	(1.48)	(52.26)	(0.50)	(0.89)	.	(5.85)
Wald test: $Z^*_{all} = Z^*_q$, p-value	.	1.00	0.31	0.89	0.33	0.65	.	.
First stage F test:								
neighbor drought = 0	.	.	.	.	.	.	10.42	.
Prob > F	.	.	.	.	.	.	0.00	.
Wald test: ODA = 1, p-value	.	.	.	.	.	.	0.77	.

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Net ODA and trade value are in constant (1995) USD and are divided by mean GDP 1-3 years before in annual data and 1 period before in 4-year average data. Population growth (approx.) is the change in population divided by mean population 1-3 years before in annual data and 1 period before in 4-year average data. Own disaster exposure measures are actual exposure. Regressions have year or period fixed effects, recipient fixed effects, and recipient clustered standard errors. Instrument in IV specification (7) is contemporaneous neighbor disaster exposure measure for droughts (proportional deviations from recipient long-run median rainfall) = Z. Specification (1) is estimated for the entire sample, while specification (2) is estimated for the subsample where Z < Z^* of specification (1). Specifications (3)-(6) are estimated for quartiles of Z from the subsample. Z^*_{all} refers to Z^* from the subsample, while Z^*_q refers to Z^* from quartile q. The support of Z is given only for OLS, non-quartile samples. Standard errors for Z^* are computed using the delta method. AR confidence intervals are from the Anderson-Rubin test, which is robust to weak instruments and was constructed here to be robust to a clustered error structure.

Table 3.15: Mechanisms of foreign aid's impact on economic growth

	Dependent Variable						
	Household consumption as fraction of GDP	Gross capital formation as fraction of GDP	Government consumption as fraction of GDP	Net exports as fraction of GDP	Secondary school enrollment as fraction of pop.	R&D expenditure as fraction of GDP	Health expenditure as fraction of GDP
	4-year average, IV Specifications						
Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Net ODA as fraction of GDP	4.671	-1.768	0.592	0.267	0.061	0.198	0.193
	(1.570)***	(0.764)**	(0.423)	(0.891)	(0.269)	(0.434)	(0.453)
Own exposure: Droughts, prop. deviations from median rainfall	-0.252	0.124	-0.066	0.007	-0.015	0.002	-0.003
	(0.257)	(0.113)	(0.049)	(0.085)	(0.037)	(0.017)	(0.018)
Trade value (goods & services) as fraction of GDP	0.141	0.277	0.025	-0.140	-0.016	-0.003	0.018
	(0.083)*	(0.031)***	(0.011)**	(0.039)***	(0.014)	(0.010)	(0.015)
Population growth (approx.)	1.574	1.688	-0.111	0.992	0.356	0.220	-0.130
	(1.386)	(0.981)*	(0.300)	(0.891)	(0.128)***	(0.119)*	(0.288)
R^2	0.53	0.69	0.82	0.85	0.87	0.98	0.96
Root MSE	0.18	0.09	0.04	0.08	0.02	0.00	0.01
Number of observations	568	583	575	716	596	84	271
Standard 95% C.I. (ODA)	[1.59-7.75]	[-3.27--0.27]	[-0.24-1.42]	[-1.48-2.01]	[-0.47-0.59]	[-0.65-1.05]	[-0.69-1.08]
AR 95% C.I. (ODA)	[2.0-9.4]	[-3.7--0.1]	[-0.1-2.0]	[-3.8-2.0]	[-0.2-6.8]	[-]	[-]
Wald test ODA = 1, p-value	0.02	0.00	0.34	0.41	0.00	0.07	0.08
Wald test ODA = -1, p-value	0.00	0.32	0.00	0.16	0.00	0.01	0.01
OLS: ODA coefficient	1.102	-0.239	0.082	-0.387	-0.034	-0.003	0.022
OLS: ODA coeff. standard error	(0.418)***	(0.233)	(0.097)	(0.250)	(0.033)	(0.035)	(0.019)

Testing Equality of ODA IV Coefficient (Table 10, column 8) with Sum of National Accounts IV Coefficients (Current Table, columns 1-4)

IV Coefficient (current Table)	
C [column 1]	4.67***
I+GI [column 2]	-1.77**
GC [column 3]	0.59
NX [column 4]	0.27
Sum = Y	3.76*
Var(Y)	4.02
IV Coefficient (Table 10)	
ODA	1.69*
Var(ODA)	0.75
Wald test $\beta_{oda} = \beta_Y$, p-value	0.67

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Net ODA and all dependent variables except for secondary school enrollment are in constant (1995) USD and are divided by mean GDP 1 period before. Secondary enrollment is divided by mean population 1 period before. Health expenditure is the sum of private and public health expenditures. Population growth (approx) is the change in population divided by mean population 1 period before. Own disaster exposure measures are actual measures are actual exposure. Regressions have recipient and period fixed effects, as well as recipient clustered standard errors. Instrument in all IV specifications is contemporaneous neighbor disaster exposure measure for droughts (proportional deviations from recipient long-run median rainfall). AR confidence intervals are from the Anderson-Rubin test, which is robust to weak instruments and was constructed here to be robust to a clustered error structure. Variance of Y, the sum of the national accounts coefficients, assumes independence of the coefficient terms. The primary sampling unit is based on the number of recipients / clusters (144) from estimation of the ODA coefficient in Table 10, column 8, since some observations are lost in national accounts columns 1-3 (but not 4) due to missing values of the dependent variable.

Table 3.16: Heterogeneity in the impact of foreign aid on economic growth

Dependent Variable:
Per Capita GDP Growth Rate

Explanatory Variable	4-year average						
	Region Interactions OLS (1)	Region Interactions IV (2)	Excluding EUR IV (3)	Excluding AFR IV (4)	Excluding LAC IV (5)	Aid Squared OLS (6)	Aid Squared IV (7)
Net ODA as fraction of GDP	0.058	-0.624	0.995	2.523	1.693	0.165	1.724
	(0.058)	(0.421)	(0.695)	(1.601)	(0.904)*	(0.079)**	(1.610)
(Net ODA as fraction of GDP)^2						-0.163	-0.159
						(0.103)	(5.141)
(Net ODA as fraction of GDP) x (Africa recipient dummy)	0.006	0.787					
	(0.073)	(0.417)*					
(Net ODA as fraction of GDP) x (Asia recipient dummy)	0.227	2.605					
	(0.298)	(2.289)					
(Net ODA as fraction of GDP) x (Latin American & Caribbean recipient dummy)	0.003	1.023					
	(0.102)	(4.926)					
(Net ODA as fraction of GDP) x (Europe recipient dummy)	2.151	14.468					
	(0.676)***	(12.642)					
Own exposure: Droughts, prop. deviations from median rainfall	0.009	-0.013	-0.047	-0.084	-0.113	0.010	-0.096
	(0.023)	(0.035)	(0.053)	(0.083)	(0.077)	(0.023)	(0.093)
Trade value (goods & services) as fraction of GDP	0.078	0.066	0.052	-0.027	0.046	0.078	0.038
	(0.007)***	(0.044)	(0.030)*	(0.075)	(0.050)	(0.008)***	(0.046)
Population growth (approx.)	-0.276	-0.253	-0.028	-0.077	0.182	-0.276	0.055
	(0.584)	(0.636)	(0.617)	(0.843)	(0.701)	(0.583)	(0.684)
R^2	0.55	0.03	.	.	.	0.54	.
Root MSE	0.03	0.05	0.05	0.08	0.07	0.03	0.07
Number of observations	716	716	666	461	528	716	716
Standard 95% C.I. (ODA)	[-0.06-0.17]	[-1.45-0.20]	[-0.37-2.36]	[-0.61-5.66]	[-0.08-3.46]	[0.01-0.32]	[-1.43-4.88]
AR 95% C.I. (ODA)	.	.	[0.2-5.6]	[0.9-31.4]	[0.7-9.3]	.	.
First stage F test:							
neighbor drought = 0	.	.	6.69	4.76	6.18	.	.
Prob > F	.	.	0.01	0.03	0.01	.	.
Cragg-Donald statistic	.	0.22	.	.	.	.	0.82
Kleibergen-Papp rk statistic (H_0: rank (r) = K-1)	.	0.07	.	.	.	.	1.62
Wald test: ODA = 1, p-value	0.00	0.00	0.99	0.34	0.45	0.00	0.65
OLS: ODA coefficient	.	.	0.073	0.069	0.105	.	.
OLS: ODA coeff. standard error	.	.	(0.039)*	(0.070)	(0.050)**	.	.

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Net ODA and trade value are in constant (1995) USD and are divided by mean GDP 1 period before. Population growth (approx.) is the change in population divided by mean population 1 period before. Own disaster exposure measures are actual exposure. Regressions have recipient and period fixed effects and recipient clustered standard errors. Instruments in IV specification (2) are contemporaneous neighbor disaster exposure measure for droughts (proportional deviations from recipient long-run median rainfall), as well as its interaction with recipient dummy variables for four of the five recipient regions represented in the estimation sample (i.e., Africa [AFR], Asia, Latin America and the Caribbean [LAC], and Europe [EUR]). Oceania [OCE] is the omitted endogenous ODA interaction and thus also the omitted instrument interaction. Specifications (3) to (5) omit observations from the region noted for estimation, and so the instrument in each case is simply neighbor drought exposure. Instruments in IV specification (7) are neighbor drought exposure and its quadratic. AR confidence intervals are from the Anderson-Rubin test, which is robust to weak instruments and was constructed here to be robust to a clustered error structure. Cragg-Donald statistic here is for weak identification (not underidentification) of multiple endogenous regressors, and so was constructed from the minimum eigenvalue of the matrix analog of the first stage F-statistic. F-statistic version of the Kleibergen-Papp rk statistic (adjusted from the Wald version by dividing the statistic by the number of excluded instruments), which is robust to a clustered error structure (Cragg-Donald statistic assumes i.i.d. errors), is also reported. The null hypothesis H_0 here is that the rank r of the test matrix is K-1, where K is the number of endogenous regressors. Rejection of the null indicates rank >= (r+1), which here implies full rank = K. While the Kleibergen-Papp rk statistic is more appropriate given estimation, the Cragg-Donald statistic better corresponds to Stock and Yogo (2005) weak instrument identification critical values, which are constructed assuming i.i.d. errors.

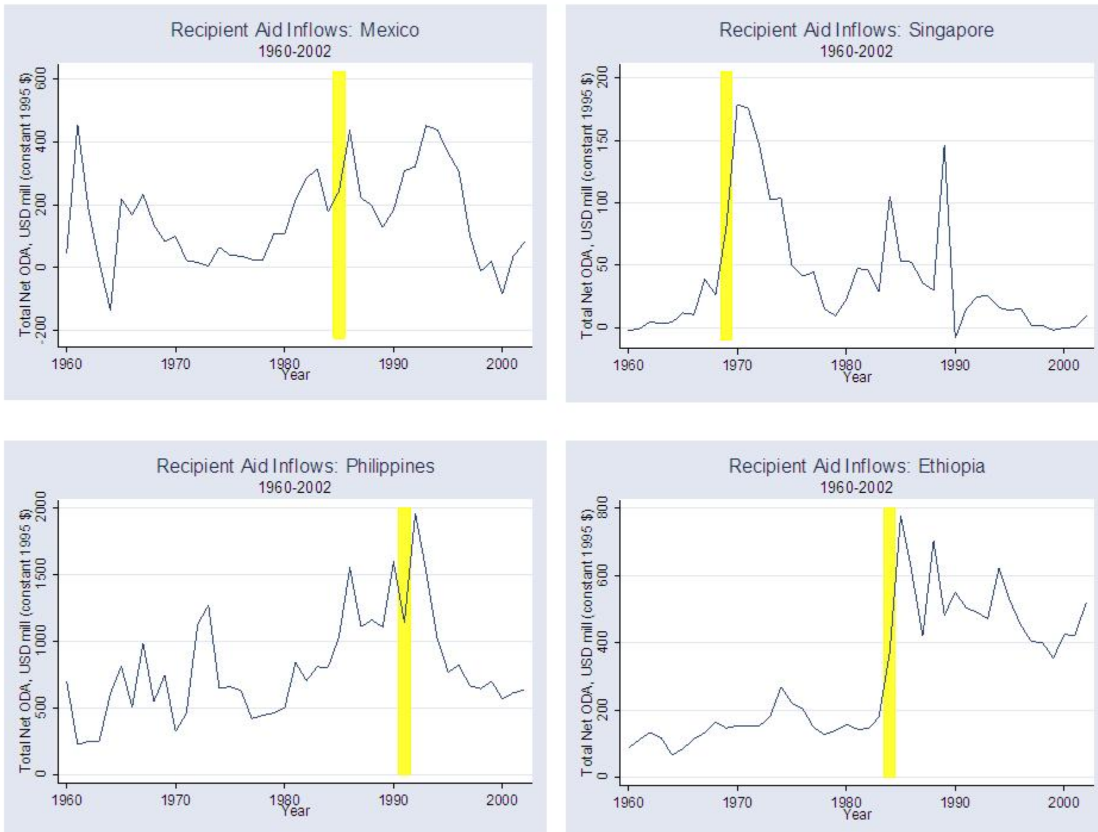
Table 3.17: Sensitivity analyses: impact of foreign aid on economic growth

	Dependent Variable: Per Capita GDP Growth Rate	
	Annual	4-year average
Alternative Specification	IV	IV
	(1)	(2)
1. Unweighted aid neighbor drought exposure	1.040	1.525
	(0.606)*	(0.784)*
	[10.26]	[7.31]
2. Non-negative aid inflows only	1.354	1.691
	(0.632)**	(0.836)**
	[11.29]	[7.53]
3. Including aid inflows from multilateral donors	0.713	0.934
	(0.345)**	(0.417)**
	[12.34]	[9.30]
4. Including aid neighbor earthquakes as an instrument	0.978	1.356
	(0.629)	(0.613)**
	[4.89]	[4.57]
5. Including aid neighbor earthquakes as an instrument, 2SLS in first differences	-36.647	1.179
	(366.700)	(0.638)*
	[0.01]	[3.95]
6. Including aid neighbor earthquakes as an instrument, GMM estimation, in first differences	-40.077	0.942
	(320.358)	(0.599)
	[0.01]	[3.95]
7. Including inflation as a control	1.129	1.639
	(0.644)*	(0.947)*
	[9.99]	[5.52]
8. Including inflation, civil and interstate wars, and M2/GDP as controls	0.524	2.455
	(0.694)	(1.161)**
	[5.46]	[5.27]
9. Including inflation, civil and interstate wars, trade openness, M2/GDP, budget balance/GDP, and food price inflation as controls	-1.000	0.571
	(5.175)	(3.153)
	[0.34]	[0.24]
10. GMM estimation using Arellano-Bond inspired procedure, specification (8) controls	0.341	-2.302
	(0.928)	(5.395)
	[0.09]	[0.01]
11. GMM estimation using Arellano-Bond inspired procedure, specification (9) controls	4.103	0.971
	(18.353)	(1.787)
	[0.02]	[0.17]

* significant at 10%; ** significant at 5%; *** significant at 1%

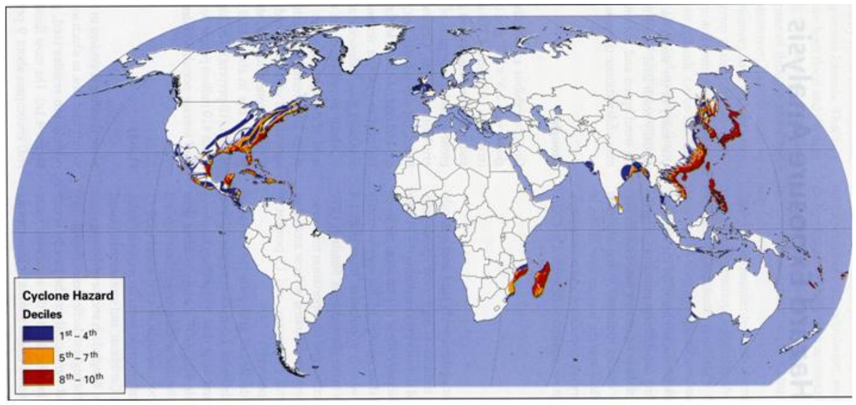
Notes: This table reports estimates from 22 separate regressions. All coefficients reflect Net ODA as fraction of GDP. Net ODA is in constant (1995) USD and is divided by mean GDP 1-3 years before in annual data and 1 period before in 4-year average data. Table 10 is baseline specification before each numbered alternative is applied - column (3) for annual data and column (8) for 4-year average data. Therefore all regressions include controls for own drought exposure, trade value (goods & services) as fraction of GDP, and population growth (approx.). Also, all regressions have year or period fixed effects, recipient fixed effects, and recipient clustered standard errors (in parentheses). Unless otherwise noted, instrument is contemporaneous neighbor disaster exposure measure for droughts (proportional deviations from recipient long-run median rainfall). First stage F-statistics or F-statistic version of Kleibergen-Papp rk statistics (robust to a clustered error structure, and adjusted from the Wald version by dividing the statistic by the number of excluded instruments) are in brackets. Note that for rk statistic, H_0: rank (r) of test matrix = K-1, where K is the number of endogenous regressors. Rejection of the null indicates rank >= (r+1), which here implies full rank = K. Civil and interstate war data is from Correlates of War (COW) project, 2002 (Penn State University, http://www.correlatesofwar.org/). Trade openness is from Sachs and Warner (1995). All other additional variables are from World Development Indicators (WDI) 2004. Generalized method of moments (GMM) estimation is run in first differences rather than levels due to weighting matrix inversion problems stemming from the number of regressors and clustered standard errors. Arellano-Bond (1991) inspired procedure runs GMM estimation in first differences, with lagged levels of potentially endogenous control variables (i.e., variables added for specification 8 or 9) as instruments for their first-differenced analogs, and first-differenced neighbor drought exposure as instrument for first-differenced net ODA as fraction of GDP. In annual data, the 9th lag of each contemporaneous endogenous control is used as an instrument, while in period data, the 3rd lag of each contemporaneous control is used. Unlike traditional Arellano-Bond estimation, however, there is no dynamic aspect to these specifications.

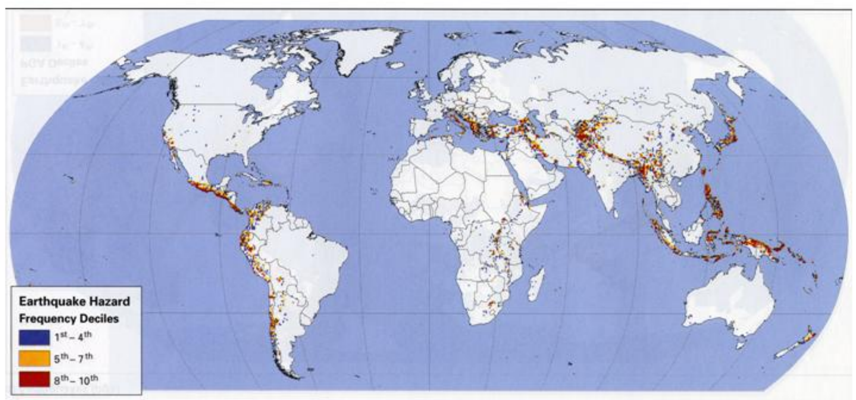
Figure 3.1: Foreign aid inflow response to disasters



Notes: OECD DAC data and authors' calculations. Yellow bars indicate disaster occurrences in each country: specifically, a 1985 earthquake in Mexico (killed 6,500 - 30,000 estimated), a 1969 flood in Singapore ($4.5m damage = 0.00274% GDP at the time, 4 deaths, 3,100 homeless), a 1991 hurricane in the Philippines (killed thousands) and a 1984 drought in Ethiopia (combined with the impact of two other droughts from late 1970s, resulted in close to 8 million people becoming famine victims in some way and over 1 million deaths).

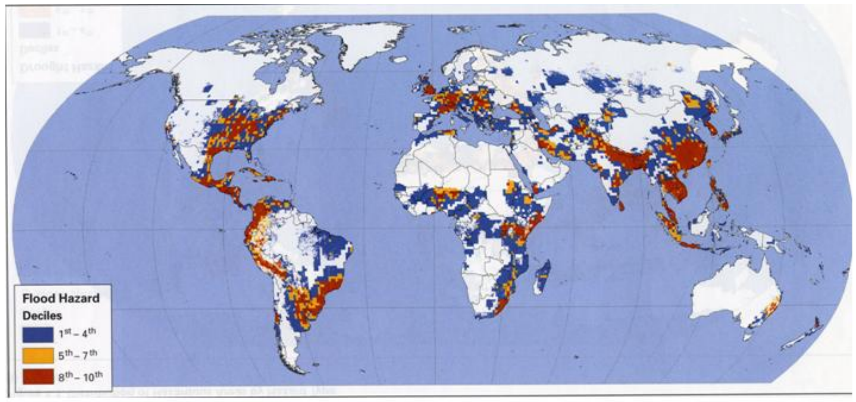
Figure 3.2: Distribution of hazardous areas by disaster type: storms (cyclones) and earthquakes

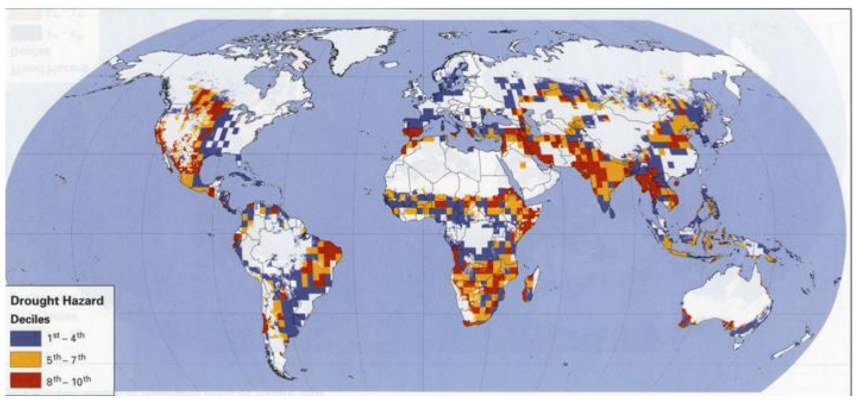




Source: Dilley et al. 2005. Cyclones noted occurred from 1980-2000, while earthquakes noted occurred from 1976-2002.

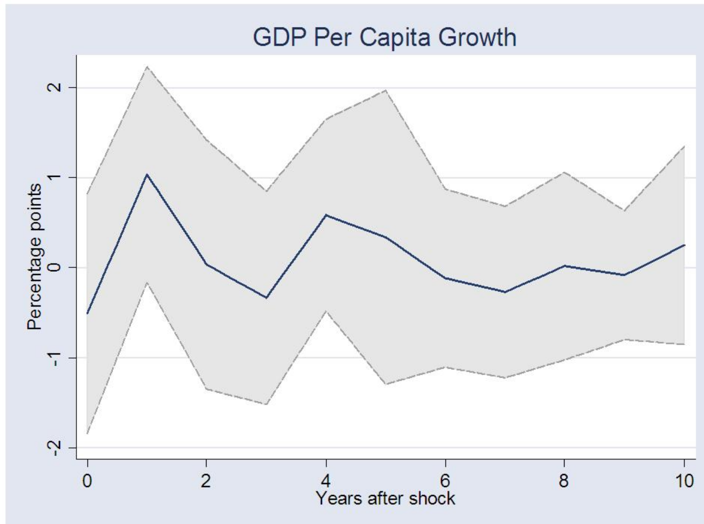
Figure 3.3: Distribution of hazardous areas by disaster type: floods and droughts

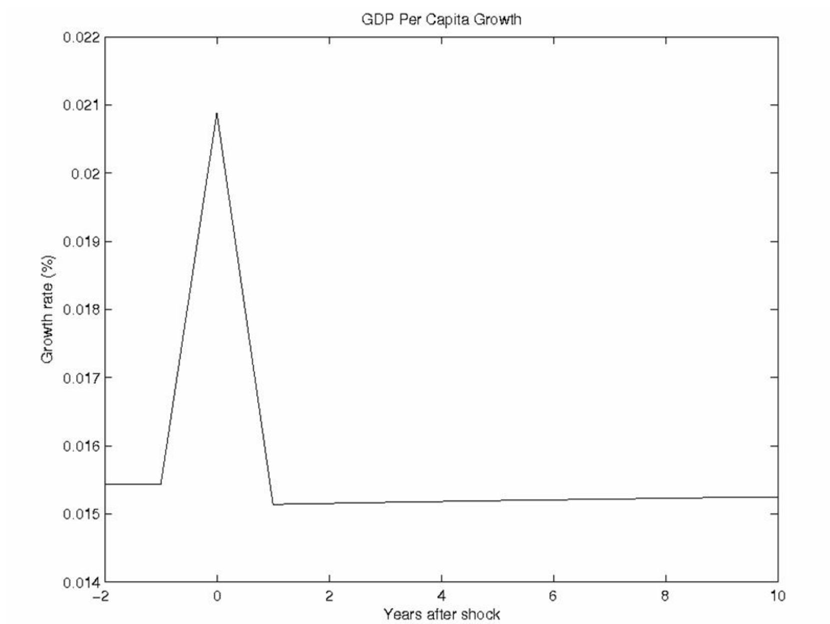




<u>Source</u>: Dilley et al. 2005. Floods noted occurred from 1985-2003, while droughts noted occurred from 1980-2000.

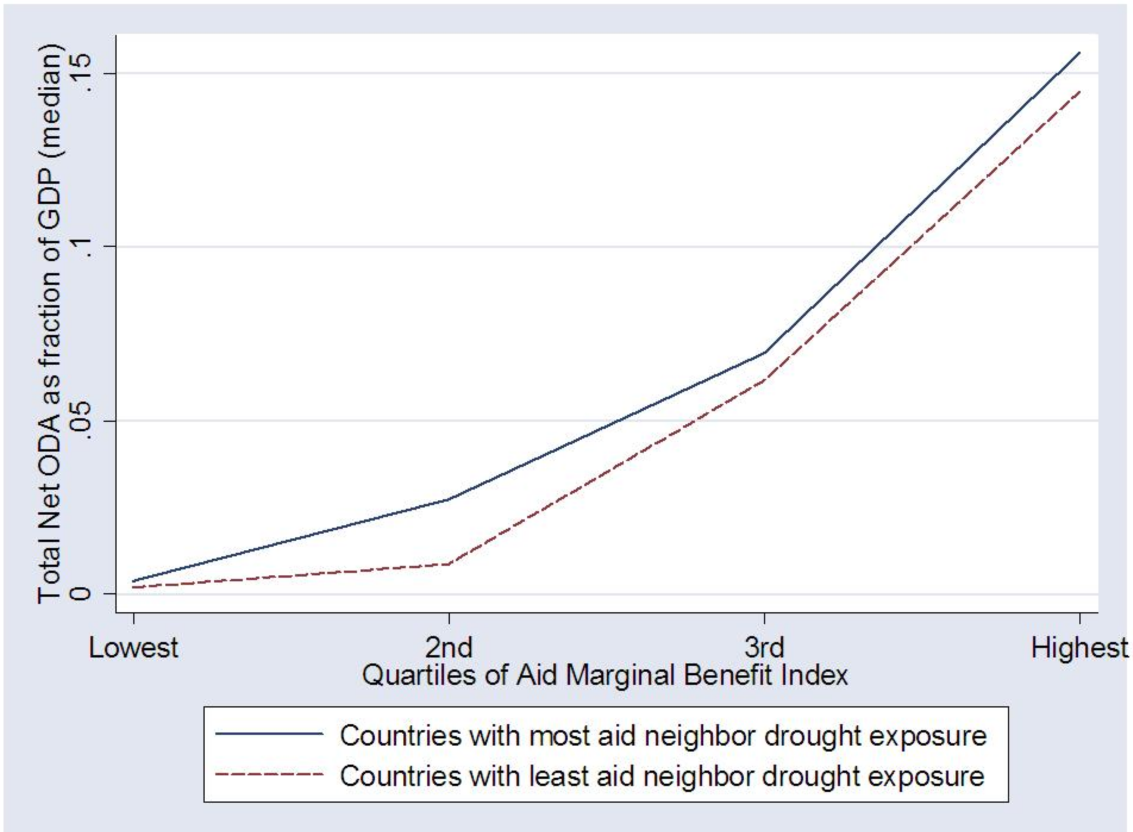
Figure 3.4: Estimated and simulated economic growth response to a one-time aid increase





Notes: (1) Estimation: From OECD DAC data and authors' calculations. Point estimates from each year after the shock are coefficients from a polynomial distributed lag (PDL) model (10 lags) analog of Table 12, column [3]. Fitted values and their lags of Total Net ODA / GDP (mean of 1-3 years before) from a first stage regression on aid neighbor drought exposure and controls (i.e., Table 11, column [2]) are used in the PDL model. Standard errors of coefficient estimates are thus adjusted for the presence of these generated regressors, following Pagan (1984). Shaded grey area represents this adjusted 95% confidence interval of estimates. (2) Simulation: Authors' calculations. Data reflect the U.S. and are from George J. Hall (http://people.brandeis.edu/˜ghall/). Initial values of capital stock, labor force and GDP are from 1959. Initial capital stock K (approximated with the net stock of nonresidential fixed private capital) is 411.7 billions USD (constant 1959 dollars), from the BEA Survey of Current Business, August 1994. Initial labor force L (approximated with the age 16+ population) is 115.3 million, from the Economic Report of the President. Finally, initial GDP Y is 506.6 billions USD (constant 1959 dollars), also from the Economic Report of the President. Parameter values for capital's share of income and its depreciation rate are set at 0.35 and 0.05, respectively, and are taken from Bosworth and Collins (2003). Meanwhile, parameter values for rates of savings, population growth, and TFP growth (approximated by the observed per capita GDP growth rate multiplied by [1 - capital's income share])are set at 0.14, 0.02, and 0.01 respectively, and are estimated across the sample of countries in our data. Aid shock is simulated via a sudden increase in capital equal to 1% of GDP at time 0 (i.e., assuming all aid is invested).

Figure 3.5: Effect of aid neighbor droughts on aid inflows, by marginal benefit quartile



Notes: OECD DAC data and authors' calculations. "Marginal benefit" quartiles are constructed following procedure outlined in Card (1995) and Kling (2001). First, for period data estimation sample (corresponding to Table 12), data is further collapsed to recipient country averages. The median value for aid neighbor drought exposure in this collapsed sample is then calculated. For countries below the median, a regression of total net ODA as a fraction of GDP (previous period) on lagged log(GDP per capita) and lagged log(population) is run. These variables correspond both to the donor allocation model in the Theory Appendix (where the income of the representative agent from each recipient country affects the marginal utility of the next aid dollar received and, hence, donor allocation decisions), as well as prior empirical research that has found recipient aid inflows to be strongly related to these variables. Because the current data is collapsed to one observation per country for interpretative ease, one can think of these variables as being implicitly included in all prior estimation via the recipient fixed effects. After the aforementioned regression is run, predicted values of total net ODA as a fraction of GDP are then generated for all countries, both below and above median neighbor drought exposure. The 25th, 50th, and 75th percentiles of these predicted values are then used to group the recipient country observations into their appropriate quartile bins, by neighbor drought exposure category. Median values of total net ODA as a fraction of GDP (as a more robust measure of central tendency) are then examined for each group of countries.

3.10 Appendix

Theoretical Framework

The paper can be thought of broadly as a two-stage static optimization problem, with stage 1 being a donor choice problem and stage 2 being a recipient choice problem. Specifically, in stage 1, each donor country determines the optimal allocation of aid flows to send to its set of recipient countries, with the recipients themselves treating these aid flows as exogenous. Then in stage 2, each recipient determines how to utilize these aid inflows, which then has implications for economic growth and development in the country.[30]

Natural disasters factor into the model as exogenous income shocks in stage 1 that alter equilibrium donor aid outflows, thereby affecting recipient aid inflows - not simply for the recipient country that receives the disaster shock, but for all of the recipients. Econometrically, this allows us to identify the effect of aid on growth in stage 2.

First Stage: A Model of Donor Aid Allocations

Setup We define the donor aid allocation problem as follows:

$$\max_{\{g_j\}} \; U(I - \sum_j g_j) + \sum_j \theta_j u_j(I_j + g_j)$$

$$s.t.\ 1)\ \sum_j g_j \leq I$$

$$2)\ -g_j \leq I_j \ \forall j.$$

[30]This setup is a convenient simplification. As discussed, there is reason to suspect the endogeneity of aid flows empirically in growth analysis, and a richer, dynamic model could reflect the temporal influence that recipient aid usage might have on future aid inflows. We also do not model any strategic interaction between donors in their respective allocation decisions, keeping rather to a single donor framework, even though such game theoretic interplay may exist (although some donors may nevertheless make their allocation decisions irrespective of others, possibly because they are extremely large or negligibly small relative to these other donors).

There is one donor country, with representative agent utility U, income $I = I(\kappa, \mathbf{I})$ available for aid and realized aid outflows $G = \sum_j g_j$. Thus, although suppressed above, donor income I available for aid is endogenous and a function of donor economic environment κ and the vector of recipient incomes $\mathbf{I}$. Meanwhile, there are three recipient countries $j = 1, 2, 3$ in the donor's recipient set R, each with representative agent utility u_j (identical in functional form to each other as well as to the donor), utility weights $\theta_j \in (0, 1]$ indicating recipient importance to the donor, income I_j and aid inflows g_j. We allow individual aid flows to recipient j, g_j, to be of any sign, subject to constraint (2), thus reflecting some occasional, small aid repayments in our data. In this model, the donor derives utility both from own income as well as from weighted recipient utility. The latter need not be due to altruism since the θ_j weights can incorporate donor self-interests. The donor thus determines its optimal allocation of aid outflows to balance the negative effect of decreased own income with the positive effect of increased recipient income.

We can form the Lagrangian for this problem as $\mathcal{L} = U(I - \sum_j g_j) + \sum_j \theta_j u_j(I_j + g_j) + \lambda(I - \sum_j g_j) + \mu(I_j + g_j)$, where λ and μ are the Lagrange multipliers for constraints (1) and (2), respectively. We incorporate disasters into this model implicitly through their effect on recipient income I_j for some j. Specifically, we assume that disaster shocks have a *negative* impact on I_j, which will be important to keep in mind for the comparative statics derived regarding the effects of a disaster shock.

Equilibrium & Comparative Statics

For an interior solution (i.e., $\lambda, \mu = 0$), which is likely to hold given the nature of the constraints, we have the following first-order conditions for each recipient j:

$$U'(I - \sum_j g_j^*) = \theta_j u_j'(I_j + g_j^*) \ \forall j, \tag{3.8}$$

where $U', u' > 0$ and $U'', u'' < 0$. There is no closed form solution for $\{g_j^*\}$ without a functional form assumption on U and u_j. However, assuming a disaster shock to recipient j, we can totally differentiate (8) to sign the individual own disaster effect dg_j/dI_j as well as the individual neighbor disaster effects dg_i/dI_j for $i \neq j$. This leads to the following empirically testable propositions:

Proposition 1. *For a disaster shock to recipient j, the sign of the own disaster effect, dg_j/dI_j, as well as the aid neighbor disaster effects, dg_i/dI_j for $i \neq j$, will depend on the sign and magnitude of dI/dI_j, the relationship between the incomes of the donor and the affected recipient j. Specifically, there are three possible cases:*

Case 1: If $dI/dI_j = 0$, then (a) $dg_j/dI_j < 0$ (i.e., own disaster shocks increase aid inflows) and (b) $dg_i/dI_j > 0$ for $i \neq j$ (i.e., neighbor disaster shocks decrease aid inflows).

Case 2: If $dI/dI_j < 0$, then (a) $dg_j/dI_j < 0$ (i.e., own disaster shocks increase aid inflows) and (b) for $i \neq j$, either $dg_i/dI_j > 0$ or $dg_i/dI_j < 0$ if $|dI/dI_j| > 1$ (i.e., neighbor disaster shocks may decrease or increase aid inflows).

Case 3: If $dI/dI_j > 0$, then (a) either $dg_j/dI_j < 0$ or $dg_j/dI_j > 0$ if $|dI/dI_j|$ is sufficiently large (i.e., own disaster shocks may increase or decrease aid inflows) and (b) $dg_i/dI_j > 0$ for $i \neq j$ (i.e., neighbor disaster shocks decrease aid inflows).

Proof of Proposition 1

Totally differentiating the first order conditions of (8), we obtain:

$$\begin{pmatrix} (1+\alpha_1) & 1 & 1 \\ 1 & (1+\alpha_2) & 1 \\ 1 & 1 & (1+\alpha_3) \end{pmatrix} \begin{pmatrix} dg_1 \\ dg_2 \\ dg_3 \end{pmatrix} = \begin{pmatrix} dI - \alpha_1 dI_1 \\ dI \\ dI \end{pmatrix}$$

where $\alpha_j = \frac{\theta_j u_j''}{U''} > 0 \ \forall j$, and $dI_2 = dI_3 = 0$.[31]

Applying Cramer's Rule to this system results in the following three expressions for the own and neighbor disaster comparative statics:

$$\frac{dg_1}{dI_1} = \frac{-(\alpha_1\alpha_2 + \alpha_1\alpha_3 + \alpha_2\alpha_3) + \alpha_2\alpha_3(dI/dI_1)}{\alpha_1\alpha_2 + \alpha_1\alpha_3 + \alpha_2\alpha_3 + \alpha_1\alpha_2\alpha_3}, \tag{3.9}$$

$$\frac{dg_2}{dI_1} = \frac{\alpha_1\alpha_3 + \alpha_1\alpha_3(dI/dI_1)}{\alpha_1\alpha_2 + \alpha_1\alpha_3 + \alpha_2\alpha_3 + \alpha_1\alpha_2\alpha_3}, \tag{3.10}$$

$$\frac{dg_3}{dI_1} = \frac{\alpha_1\alpha_2 + \alpha_1\alpha_2(dI/dI_1)}{\alpha_1\alpha_2 + \alpha_1\alpha_3 + \alpha_2\alpha_3 + \alpha_1\alpha_2\alpha_3}. \tag{3.11}$$

For a given sign and magnitude of dI/dI_1, which determines the relevant case, the proposition immediately follows.

Proposition 2. *For a disaster shock to recipient j and $-2 < dI/dI_j \leq 0$, the magnitude of the own disaster effect will be larger than the magnitude of the aid neighbor disaster effects: $|dg_j/dI_j| > |dg_i/dI_j|$ for $i \neq j$.*

Additionally, as previously noted, the derivation of the comparative statics for the aid neighbor disaster effect, dg_i/dI_j for $i \neq j$, provides a guide for determining how to construct aid neighbor disaster exposure from the objective disaster occurrence data that we have.

[31]We thank Brian Kovak for the α_j notation and aid on this proof.

Proof of Proposition 2

Follows immediately from the three expressions (9)-(11) above and $\alpha_j > 0 \ \forall j$.

Second Stage: Solow Growth Model The framework for thinking about the second stage recipient growth problem will be a standard Solow model (Solow 1956), where for concreteness we can limit ourselves to the case where the production function is of Cobb-Douglas form and reflects Hicks neutral technology:

$$Y_t = A_t K_t^{\gamma} Lt^{1-\gamma}, \ \gamma \in (0,1). \tag{3.12}$$

Here, at time t, Y_t is output, A_t is exogenous technological progress, K_t is capital input, L_t is labor input, and γ is capital's share in output, where both inputs here exhibit diminishing marginal returns. Alternatively, we could instead use as a basis the augmented Solow model analog (Mankiw, Romer, and Weil 1992), which allows for human capital investment to play a role in output alongside the physical capital investment of the standard model.[32]

[32]While we acknowledge that strictly speaking these are exogenous growth models, we simply utilize them as a theoretical foundation for the aid-growth analysis. We extrapolate from these models in our empirical work by allowing for the possibility that recipient countries may in fact be able to impact the growth rate of technological process that governs long-run output per worker growth, rather than formally turn to alternative, endogenous growth theory.

Appendix: Data

Damages and Human Losses

The data on disaster damages, people killed and injuries from EM-DAT: the CRED/OFDA International Disaster Database are collected from various sources, including United Nations agencies, national governments, non-governmental organizations, insurance companies, research institutes and press agencies (EM-DAT 2008). The damage estimates, which correspond to the year of the associated event only, are in currency units and include both direct costs (such as damage to property, infrastructure, and crops) and the indirect losses due to reductions in economic activity. As discussed in Yang (2008), active data collection for EM-DAT started in the late 1960s, while retrospective research was necessary to record disasters prior to that date, stretching back to 1900. Our EM-DAT data ranges from 1949 to 2002.

Storms

As discussed in Yang (2008), the National Oceanic and Atmospheric Administration (NOAA) Tropical Prediction Center and the Naval Pacific Meteorology and Oceanography Center/Joint Typhoon Warning Center (NMFC/JTWC) create 'best tracks' of individual hurricanes: positions (latitude and longitude) of hurricane centers at 6-hourly intervals, combined with intensity information (wind speed and barometric pressure). These best tracks are constructed from post-event analysis, and incorporate information from a variety of sources, such as reconnaissance aircraft, ships, and satellites. Our storm data from these tracks ranges from 1960 to 2002, when the data quality is likely to be higher compared to earlier best track data.

Floods and Droughts

As discussed in Miguel, Satyanath, and Sergenti (2004), the Global Precipitation Climatology Project (GPCP) rainfall data use weather station gauge measures of actual rainfall, as well as satellite information on the density of cold cloud cover (closely related to actual precipitation), to obtain rainfall estimates, at 2.5 latitude and longitude degree intervals. We follow the methodology of Miguel et al. (2004) to convert this raw data on monthly rainfall estimates at 2.5-degree intervals or "nodes" into yearly rainfall estimates from 1979 onwards for each recipient country, averaged across all nodes that are associated with a given country. Though our country sample is larger than that of Miguel et al. (2004), for the set of African countries and years for which our datasets overlap, our yearly rainfall estimates do match theirs.

Earthquakes

We obtain U.S. Geological Survey (USGS) data for each country on yearly earthquake occurrences of magnitude 4, 5, 6, and 7 or greater, available from 1973 onwards. We likewise obtain analogous earthquake data for each magnitude and country in which earthquake events 50 miles outside a given country's borders are also counted, in order to better incorporate offshore earthquakes.

Defining Disaster Exposure Across Disaster Types

Storms

Our measure of wind storm exposure comes from Yang (2008) and is an exposure index. As discussed in much more detail in his paper, Yang's storm index can be thought of as intensity-weighted storm events per capita using best track hurricane data. The storm index H_{jt} for recipient j in year t takes into account both the intensity (windspeed) of hurricanes as well as the population density of the area in which the hurricanes strike, in order to form a measure of exposure that should be directly related to likely damage from the storm. More explicitly, H_{jt} is constructed as follows:

$$H_{jt} = \frac{\sum_l \sum_s x_{lsjt}}{N_{jt}}, \tag{3.13}$$

where x_{lsjt} is a measure of how affected person l is by storm s in country j and year t. As in Yang (2008), the measure of "affectedness" is the square of the windspeed above the tropical storm windspeed threshold (33 knots[33]), normalized by the maximum of this variable, or

$$x_{lsjt} = \frac{(w_{lsjt} - 33)^2}{(w^{MAX} - 33)^2}, \tag{3.14}$$

where w_{lsjt} is the windspeed to which an individual is exposed and w^{MAX} is the maximum windspeed observed in Yang's original data, 152.3 knots. Individual affectedness is summed across all storms and individuals in a given year and country, and is then divided by population N_{jt} to obtain a per-capita measure. If all of the residents of a country were each exposed to the maximum windspeed ($x_{lsjt} = 1$ for all residents) on one occasion in a single year, $H_{jt} = 1$ for that country in that year. Similarly, $H_{jt} = 1$ if each resident were exposed twice to a storm where $x_{lsjt} = 0.5$. Because no data for individual-level storm affectedness x_{lsjt} is available, Yang approximates this number using windspeed estimates at 0.25-degree gridpoints, which are also then weighted by gridpoint subnational population estimates to incorporate the number of individuals affected by the storm at each gridpoint.

[33] A knot is one nautical mile per hour, and a nautical mile is 1.15 statute or land miles.

Earthquakes

Our measure of earthquake exposure is the number of magnitude 4 earthquakes (the lowest magnitude for which we have data) with a 50-mile buffer per square mile of recipient country j in year t. This measure is only approximately per capita, insofar as population and land area are positively correlated (correlation coefficient $\rho = 0.55$ in our annual data). Nevertheless, it does account for the likelihood that a given magnitude earthquake will be associated with more intense exposure and damage for an individual in the affected country, ceteris paribus, the smaller the land area is of the country in which the earthquake is localized.

Floods and Droughts

For our measures of floods and droughts, in each year t we use positive and negative proportional deviations of recipient country j rainfall from the country's long-run median rainfall level (measured over the estimation period, 1979 to 2002). Like our earthquake variable, the flood and drought measures are also only approximately per capita measures of exposure, again to the extent that population and land area are positively correlated. The per capita approximation stems from the fact that each annual rainfall estimate is formed from the average of all 2.5-degree nodes that are associated with a recipient country. This ensures that a given level of rainfall for a particular node, ceteris paribus, results in a lower level of average rainfall for countries with greater land mass since there are more nodes over which that average is taken.

Analysis: Assessing Monotonicity

The approach explored in Table 14 is to first specify non-linear effects in OLS estimation of the first stage regression. Given the significance of the quadratic neighbor drought instrument in column (1), we then proceed by reducing the sample size to values of this instrument, which we will call Z, that are less than or equal to the implied turning point, Z^*, of the instrument's effect on the conditional mean function of the aid treatment (here, $Z^* = 2.3$, or in other words an aggregate decrease in aid neighbors' rainfall as a proportion of their median levels of 230 percentage points). We then re-run the first stage, again allowing for non-linear effects of the instrument on the treatment. Had these effects still been significant, we would have had to once again reduce the sample and repeat estimation. However, as column (2) shows, this is no longer the case.

As a result, we then first estimate the new turning point of the conditional mean function on this subsample, Z^*_{all}, which is equal to 2.5. We then split this subsample up further into quartiles to determine whether there are any significant non-linear effects of the drought instrument on the endogenous aid treatment within these quantiles, which the table shows is not the case. Additionally, we formally test whether the implied turning point of the conditional mean function in each quartile q, Z^*_q, is significantly different from the implied turning point in the subsample as a whole, Z^*_{all}.

The approach in Table 14 does have the feature, however, of non-randomly reducing the estimation sample, which is somewhat less than desirable, especially if the aid coefficient in column (7) had turned out to be substantively different from the aid coefficient in Table 12, column (3). Appendix Table 19 eliminates the sample reduction aspect of this approach and has the following intuition: if there are significant non-linear effects of the instrument on the treatment variable, but they are non-linear *in the same way* for all observations in the sample (i.e., all observations share the same Z^*), then what would be necessary to recover monotonicity and identification of a LATE parameter in this case would be to correctly specify this non-linearity in first stage estimation and second stage instruments. Table 19 takes this alternative approach. While the magnitude of the IV aid coefficient of 1.1 percentage points when using this method is quite similar to our main estimate (Table 12, column (3)), it is no longer significant, as the neighbor drought instruments are now rather weak and the second stage coefficient is somewhat less precisely estimated.

Table 3.18: Included recipient countries

Recipient Country	Years	Recipient Country	Years	Recipient Country	Years
Albania	1989-2002	Gabon	1979-2002	Pakistan	1979-2002
Algeria	1979-2002	Gambia	1979-2002	Panama	1980-2002
Angola	1986-1994, 1996-2002	Ghana	1979-2002	Papua New Guinea	1979-1999
Antigua & Barbuda	1979-2002	Grenada	1979-2002	Paraguay	1979-2002
Argentina	1979-2002	Guatemala	1979-2002	Peru	1979-2002
Armenia	1991-2002	Guinea	1987-2002	Philippines	1979-2002
Azerbaijan	1993-2002	Guinea-Bissau	1979-2002	Poland	1991-2002
Bahamas	1979-1987	Guyana	1979-2002	Romania	1991-2002
Bahrain	1981-2002	Haiti	1979-2002	Russia	1991-2002
Bangladesh	1979-2002	Honduras	1979-2002	Rwanda	1979-2002
Barbados	1979-2002	Hungary	1991-2002	Samoa	1995-2000
Belarus	1991-2002	India	1979-2002	Sao Tome & Principe	1987-2002
Belize	1980-2001	Indonesia	1979-2002	Saudi Arabia	1979-2002
Benin	1979-2002	Iran	1979-2002	Senegal	1979-2002
Bhutan	1981-2002	Israel	1979-2002	Seychelles	1979-2002
Bolivia	1979-2002	Jamaica	1979-2002	Sierra Leone	1979-2002
Botswana	1979-2002	Jordan	1979-2002	Slovak Republic	1991-2002
Brazil	1979-2002	Kazakhstan	1992-2002	Slovenia	1994-2002
Bulgaria	1991-2002	Kenya	1979-2002	Solomon Islands	1980-1990
Burkina Faso	1979-2002	Kiribati	1979-2000	South Africa	1991-2002
Burma	1979-1998	Korea, Rep.	1979-2002	Sri Lanka	1979-2002
Burundi	1979-2002	Kuwait	1979-1989, 1993-2002	St. Kitts & Nevis	1979-2002
Cambodia	1994-2002	Kyrgyz Rep.	1991-2002	St. Lucia	1981-2002
Cameroon	1979-2002	Lao PDR	1985-1998	St. Vincent & Grenadines	1979-2002
Cape Verde	1987-2002	Latvia	1991-2002	Sudan	1979-1987, 1996-2002
Central African Rep.	1979-2002	Lebanon	1989-2002	Suriname	1979-2002
Chad	1979-2002	Lesotho	1979-2002	Swaziland	1979-2002
Chile	1979-2002	Lithuania	1991-2002	Syria	1979-2002
China	1980-2002	Madagascar	1979-2002	Tajikistan	1991, 1997-2002
Colombia	1979-2002	Malawi	1979-2002	Tanzania	1990-2002
Comoros	1981-2002	Malaysia	1979-2002	Thailand	1979-2002
Costa Rica	1979-2002	Maldives	1996-2002	Togo	1979-2002
Cote d'Ivoire	1979-2002	Mali	1979-2002	Tonga	1982-2001
Croatia	1991-2002	Malta	1979-2002	Trinidad & Tobago	1979-2002
Cyprus	1979-1999	Mauritania	1979-2002	Tunisia	1979-2002
Czech Republic	1991-2002	Mauritius	1981-2002	Turkey	1979-2002
Djibouti	1995-2000	Mexico	1979-2002	Turkmenistan	1993-2001
Dominica	1979-2002	Moldova	1993-2002	Uganda	1983-2002
Dominican Republic	1979-2002	Mongolia	1982-2002	Ukraine	1991-2002
Ecuador	1979-2002	Morocco	1979-2002	United Arab Emirates	1979-1998
Egypt	1979-2002	Mozambique	1981-2002	Uruguay	1979-2002
El Salvador	1979-2002	Namibia	1985-2002	Uzbekistan	1991-2002
Equatorial Guinea	1986-1998	Nepal	1979-2002	Vanuatu	1980-1999
Eritrea	1993-2002	New Caledonia	1990-1999	Venezuela	1979-2002
Estonia	1992-2002	Nicaragua	1979-2002	Vietnam	1986-2002
Ethiopia	1982-2002	Niger	1979-2002	Yemen	1991-2002
Fiji	1979-2001	Nigeria	1979-2002	Zambia	1979-2002
French Polynesia	1991-2000	Oman	1979-1988, 1990-2002	Zimbabwe	1979-2002

Notes: Recipient countries (144 total) and years (up to 24 per recipient) refer to estimation sample corresponding to Table 11. Thus observations are restricted to those where per capita GDP growth, net ODA as a fraction of GDP, aid neighbor drought exposure, and controls are available.

Table 3.19: Assessing monotonicity: quadratic neighbor drought alternative

	Dependent Variable: Net ODA as fraction of GDP (1-5) and Per Capita GDP Growth Rate (6)					
	Annual					
		OLS, Quartiles of Neighbor Drought Exposure (Z)				
Explanatory Variable	OLS	Q1	Q2	Q3	Q4	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Net ODA as fraction of GDP						1.051
						(0.646)
Neighbor Exposure, year or period t						
Droughts: prop. deviations from median rainfall	0.056	0.009	0.090	-0.599	0.058	
	(0.021)***	(0.090)	(0.231)	(1.099)	(0.098)	
Droughts: (prop. deviations from median rainfall)^2	-0.012	0.004	-0.030	0.230	-0.015	
	(0.007)*	(0.099)	(0.106)	(0.400)	(0.023)	
Own Exposure, year or period t						
Droughts: prop. deviations from median rainfall	0.014	-0.019	0.003	0.052	0.026	-0.029
	(0.013)	(0.025)	(0.019)	(0.021)**	(0.028)	(0.020)

Trade value (goods & services) as fraction of GDP	0.018	0.035	-0.015	0.037	0.016	0.084
	(0.018)	(0.026)	(0.032)	(0.012)***	(0.013)	(0.024)***
Population growth (approx.)	-0.215	-0.544	-0.121	0.071	-0.269	0.276
	(0.089)**	(0.353)	(0.307)	(0.216)	(0.151)*	(0.315)
R^2	0.81	0.87	0.82	0.88	0.85	.
Root MSE	0.04	0.05	0.06	0.03	0.03	0.07
Number of observations	2,853	714	713	713	713	2,853
Standard 95% C.I. (ODA)	.	.	.	.	.	[-0.22-2.32]
AR 95% C.I. (ODA)	.	.	.	.	.	[0.0-9.0]
Support of Z: [Z_{min}, Z_{max}]	[0.05-2.59]	.	.	.	.	.
Implied turning point, Z^*	2.33	1.01	1.50	1.31	1.93	.
	(0.52)	(32.97)	(1.57)	(0.13)	(0.61)	.
Wald test: $Z^*_{all} = Z^*_q$, p-value	.	0.83	0.46	0.24	0.47	.
First stage F test: neighbor vars = 0	.	.	.	.	.	5.57
Prob > F	.	.	.	.	.	0.00
Wald test: ODA = 1, p-value	.	.	.	.	.	0.94

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Net ODA and trade value are in constant (1995) USD and are divided by mean GDP 1-3 years before in annual data and 1 period before in 4-year average data. Population growth (approx.) is the change in population divided by mean population 1-3 years before in annual data and 1 period before in 4-year average data. Own disaster exposure measures are actual exposure. Regressions have year or period fixed effects, recipient fixed effects, and recipient clustered standard errors. Instruments in IV specification (6) are contemporaneous neighbor disaster exposure measure for droughts (proportional deviations from recipient long-run median rainfall) = Z, as well as its quadratic = Z^2. Specification (1) is estimated for the entire sample, while specifications (2)-(5) are estimated for quartiles of Z from the sample. Z^*_{all} refers to Z* from the entire sample, while Z^*_q refers to Z* from quartile *q*. The support of Z is given only for OLS, non-quartile samples. Standard errors for Z* are computed using the delta method. AR confidence intervals are from the Anderson-Rubin test, which is robust to weak instruments and was constructed here to be robust to a clustered error structure.

CHAPTER IV

Educational Quality, Asymmetric Information, and Self-Selection in High-Skilled Migration

4.1 Introduction

The flow of migrants across borders has notable affects for the markets that those individuals depart as well as the markets in which they enter. Because individuals generally choose whether or not to migrate from their home country, the observed migratory patterns are of great interest in order to examine the extent to which they are not random. This paper examines the nature of the self-selection of migrants in the presence of cross-country differences in educational quality and informational asymmetries. I seek to build on the existent migration literature in this area which has not explored the simultaneous influence of these phenomena when individuals are determining both where to obtain their education and employment. I develop a simple model of educational quality and migration to form and test predictions on the nature of self-selection, and then further extend the model to incorporate informational asymmetries.

Numerous studies have examined, both theoretically and empirically, the nature of the self-selection of migrants, in large part because of the implications for earnings differentials and the growth of source and destination countries. One segment of this expansive literature gains insights by applying a model by Roy (1951) to the context of migration. This model examines the self-selection into occupations that can occur when individuals have unobserved skills in different occupational sectors and those skills are correlated. Borjas (1987), in constructing the first parametric representation of the Roy model, claims that the nature of immigrant selection into some destination country will be governed by the variance of earnings in the source country relative to the destination country, as well as the correlation between those countries of the value of the immigrant's unobserved skills.

Chiquiar and Hanson (2005) extend the work of Borjas (1987), in order to try to explain why, in the context of Mexico, Borjas' prediction of negative selection - namely, that any Mexican immigrants to the United States would be from the lower tail of the earnings distribution in Mexico - does not seem to hold, according to U.S. and Mexican Census data. According to this data, there is actually intermediate or positive selection of Mexican immigrants. By focusing on observable skill in the form of schooling, as well as making an alternative assumption than Borjas regarding the nature of the migration costs that individuals in the source country face,[1] Chiquiar and Hanson (2005) are able to generate the theoretical prediction from their model that intermediate or even positive immigrant selection from Mexico *is* possible, as they claim is observed in the data.

However, not all of the migration literature focuses on the partial equilibrium Roy (1951) model in order to theorize the determinants of non-random immigrant sorting between countries. Notably, Kwok and Leland (1982) argue that firm-level informational asymmetries alone can theoretically explain self-selection amongst immigrants. In their paper, individuals from the source country have already traveled abroad to the destination country for education, and are determining whether to

[1] Chiquiar and Hanson (2005) assume that the time-equivalent migration costs - i.e., the number of labor hours that would be needed to overcome the costs - are decreasing in schooling, rather than constant as Borjas (1987) assumes, although this is similar to later work by Borjas (1991).

remain abroad for employment or return home. The firm-level asymmetries are such that firms in the destination country pay all immigrants a wage equal to their productivity but firms in the source country pay all returning migrants a wage equal to the *average* productivity of that group as a whole. The authors show in this case that "brain drain" sorting will occur, where higher productivity immigrants remain abroad and lower productivity immigrants return home. Moreover, this general equilibrium result holds even if all immigrants have some preference for their country of origin over the foreign country.

Thus, while there has been research examining separately the ways in which the return to skill (observable and/or unobservable) or informational asymmetries can influence migration decisions, there has been a lack of exploration of the simultaneous effects of these two phenomena, both of which are likely to matter for the nature of self-selection and the observed migratory patterns and labor market outcomes. Existing research such as Friedberg (2000) has shown that the location where one obtains education and work experience plays a role in determining the return to that acquired human capital. Cross-country educational quality differences and informational asymmetries may help to explain such findings.

Additionally, although papers such as Chiquiar and Hanson (2005) do examine how the *level* of schooling an individual has may affect her migration decisions, neither they nor many other researchers have examined, in an international context, the impact that the *quality* of schooling would have on those decisions. Given that there is numerous evidence from recent literature that educational quality matters for later labor market outcomes in the U.S. (Turner 1998, Dale and Krueger 2002, Black and Smith 2004, Black, Daniel and Smith 2005), it becomes all the more relevant to examine the return to increased educational quality in an international setting, where differences across countries in such quality may be more stark, and where those differences may also affect migratory patterns.

Both theoretically and empirically throughout the paper, focus is restricted to individuals seeking high levels of schooling in the form of a college education. One justification for doing so is the concern that, for lower levels of schooling, the migration decision may not be made by the individual in question but rather by her parents or jointly as a household. Although parents undoubtedly often play an influential role in the college location decision of their children, so long as the child is the only one migrating, the incentives may still be aligned between all involved parties so as to nevertheless treat it as the individual maximization problem of the child. In contrast, for lower levels of schooling, a child might obtain education in the destination country solely because of a household or parental migration decision. The incentives between the child and the parents may actually be quite different, as the parents may, for example, place a larger weight on the local labor market conditions relevant to their own employment than on the local educational quality for their child.

Another reason for focusing on the highly-skilled (in terms of observable skill, schooling) is that the net emigration of such individuals may be of particular interest to developing countries, given the important role human capital accumulation can play in growth (Becker 1964). Moreover, according to data from the New Immigrant Survey, the schooling distribution of legal immigrants to the U.S. has heavy tails (NIS 2007). Thus, the flows of such highly-educated individuals to more developed countries from developing countries are likely to be of nontrivial size.

Thus, the model derives predictions on the nature of non-random sorting that will occur amongst these highly-skilled individuals for each of the four potential schooling/career paths that an individual may choose (given that an individual is choosing between the source and destination countries for both schooling and employment). The baseline model is then further extended to incorporate potential informational asymmetries that may exist between countries. The paper builds upon the work by Kwok and Leland (1982) by similarly including firm-level asymmetries regarding the capacity of firms to observe individual ability, but makes somewhat less restrictive assumptions on the nature of these asymmetries. These asymmetries are modeled in a similar fashion to much of the literature on statistical discrimination (Phelps 1972, Aigner and Cain 1977, Altonji and Pierret 2001), but yield new insights when applied here in the context of migration, as well as relative to the predictions of the baseline model.

Empirically, conditional on foreign employment, the model implies that as domestic educational quality increases, fewer migrants will acquire college education abroad. In contrast, however, as informational asymmetries become more pervasive, more migrants will acquire college education abroad. Using U.S. Census microdata from 2000 as well as proxy data on cross-country educational quality and informational asymmetries, I explore the implications of the model. I find no evidence that college quality or informational asymmetries significantly influence the share of high-skilled immigrants acquiring college education in the United States. Despite potential measurement error in the proxy measures, I interpret this finding as evidence against the model's explanation for their influence in migrant self-selection.

The paper is organized as follows: section 2 describes the model and its implications. Section 3 outlines the data, while section 4 explains the methodology and results. Finally, section 5 concludes.

4.2 A Model of Educational Quality and Migration

4.2.1 Baseline

Setup

Individuals from the source country, indexed by 0, jointly choose where to get their education as well as where to locate for employment, in order to maximize their log wages. For simplicity, this decision is modeled in a static framework, with any inherent dynamic elements only being represented implicitly. In the case of both choice dimensions, these individuals can select their home/source country, or a foreign country alternative, indexed by 1 (and again, which will represent the United States).

In this paper, as the focus is on the highly-skilled, it is assumed that all individuals are determining where to get their college education, as well as how many years of college education, s, to obtain. Moreover, focusing on college education and not lower levels of schooling works towards ensuring that both the theory and empirics are accurately representing individual migration decisions, rather than those of an entire household.

In order to maximize real wages w, individuals sort themselves into one of four mutually exclusive categories, represented by the following log wage equations

$$\ln(w_{i0,0}) = \mu_0 + \delta_{i0,0} s_i + \epsilon_{i0} \tag{4.1}$$

$$\ln(w_{i1,0}) = \mu_0 + \delta_{i1,0} s_i + \epsilon_{i0} \tag{4.2}$$

$$\ln(w_{i0,1}) = \mu_1 + \delta_{i0,1} s_i + \epsilon_{i1} \tag{4.3}$$

$$\ln(w_{i1,1}) = \mu_1 + \delta_{i1,1} s_i + \epsilon_{i1}. \tag{4.4}$$

More generally, given her choices, individual i receives $\ln(w_{ijk}) = \mu_k + \delta_{ijk} + \epsilon_{ik}$, where $j \in \{0,1\}$ represents the country where the individual obtains her schooling, and $k \in \{0,1\}$ represents the country where the individual works. The parameter μ_k represents the mean or base wage in country k, δ_{ijk} represents the demeaned value of return to schooling s_i, and ϵ_{ik} represents the demeaned value of unobserved characteristics, with $\epsilon \sim (0, \sigma_\epsilon^2)$. Because information is symmetric for now across countries, firms (who are implicitly in the model via their wage payments to individuals) have mechanisms to assess ϵ values for all individuals, but these values are unobservable *to the researcher*. Thus, individual wages are decomposed into parts due to observable socioeconomic variables (μ_k), observable skill ($\delta_{ijk}s_i$), and unobservable skill (ϵ_{ik}).[2]

Return to Schooling

The parameter δ_{ijk} is jointly determined by three factors: a) educational quality q_j in country j (if $q_a > q_b \Leftrightarrow \delta_{ia,k} > \delta_{ib,k}$); b) labor market conditions via the supply of unskilled to skilled labor $L_k \equiv L_k^u/L_k^s$ in country k (if $L_a > L_b \Leftrightarrow \delta_{ij,a} > \delta_{ij,b}$); and c) the value in country k of an individual's endowment of unobserved skill, ϵ_{ik}, which has some correlation $\rho_{\epsilon\delta}$ with δ_{ijk}, which will be assumed to be positive. In the case of (a), it is theoretically assumed for now that there is one institution in each country, and that $q_1 > q_0$, and in the case of (b), it is theoretically assumed that $L_0 > L_1$.

Migration Costs

Let C be the migration costs associated with an individual moving between country 0 and country 1. As in Borjas (1987), one can also specify migration costs in

[2] In one depiction of this model, normality functional form assumptions are made on the parameters δ_{jk} and ϵ_k (as well as time-equivalent migration costs π, to be discussed shortly). However, as these assumptions do not prove necessary for some of the broad predictions of the model, they are omitted for brevity.

time-equivalent units (i.e., the number of labor hours needed to pay the cost C), such that $\pi = C/w_{0,0}$. Additionally, I assume that migration costs borne later for employment (in contrast to immediately for education) are discounted by some discount factor $\beta \in (0, 1)$. Lastly, for simplicity, unlike Chiquiar and Hanson (2005), I do not specify that π is decreasing in the level of schooling s (or alternatively the quality of schooling q or unobserved skill ϵ, for that matter) due to the likelihood that some component(s) of migration costs are fixed or are easier for more highly-skilled individuals.

Selection

An immigrant's choice of a particular schooling and career path (i.e., one of the equations (1)-(4)) will be based on the joint probability of three events. Specifically, the joint probability that an individual chooses a given schooling/career path over each of the three alternative paths.[3]

Figure 1 summarizes predictions from the model in a simplified manner. In the figure, individual i subscripts and ϵ terms have been suppressed. As a result, the figure should be interpreted as the mean behavior of immigrants given their options. First, note that the earlier assumption regarding educational quality abroad vs. domestically, $q_1 > q_0$, implies that $\delta_{1,0} > \delta_{0,0}$ and $\delta_{1,1} > \delta_{0,1}$. Moreover, the earlier assumption regarding labor market conditions abroad vs. domestically, $L_0 > L_1$, implies that $\delta_{1,0} > \delta_{1,1}$ and $\delta_{0,0} > \delta_{0,1}$. Thus, it only remains to determine the relationship between $\delta_{1,1}$ and $\delta_{0,0}$, and I assume that $\delta_{1,1} < \delta_{0,0}$. Lastly, regarding

[3] More formally, for example, the probability that an individual obtains schooling domestically and also works domestically will be equal to $\Pr(I_{0,0}^{1,1} < 0, I_{0,0}^{0,1} < 0, I_{0,0}^{1,0} < 0) = \int_{-\infty}^{0} \int_{-\infty}^{0} \int_{-\infty}^{0} g(\mathbf{I}_1) d\mathbf{I}_1$, where $\mathbf{I}_1 = (I_{0,0}^{1,1}\ I_{0,0}^{0,1}\ I_{0,0}^{1,0})'$. Note that if normality is assumed, $\mathbf{I}_i$ has a trivariate normal distribution and a specific form for $g(\mathbf{I}_p)$ is established, where p represents one of the four possible school/career paths for an individual. Let $I_{j,k}^{j',k'} \equiv$ a variable indicating the case of schooling in country j' and employment in country k', compared to alternative schooling location j and employment location k.

migration costs, I also assume that $\mu_1 - \mu_0 > \pi$.

Figure 1 shows the average impact on immigrant self-selection of educational quality differences across countries. The upper envelope of the four selection equations, thick and in bold, illustrates the chosen path for individuals of a given observable skill (i.e., schooling) level. The least skilled individuals in group I, below schooling s_L, obtain their schooling domestically but then migrate abroad for employment where the distribution of wages across schooling is more compressed. The next most skilled individuals in group II, above s_L and at or below schooling s_M, go abroad for both schooling and employment. Group III individuals, above schooling s_M and at or below s_H, remain domestically for both schooling and employment and never immigrate. Lastly, the most skilled individuals in group IV, above schooling cutoff s_H, choose to go abroad for schooling where educational quality is higher but then return home for employment where the return to that acquired skill is higher.

Thus, s_M separates individuals who immigrate for employment (i.e., long-term immigration) from those who remain domestically for employment. Meanwhile, conditional on long-term residency abroad or domestically, s_L and s_H, respectively, determine the mix of those individuals that are educated domestically as opposed to abroad. Additionally, As Chiquiar and Hanson (2005) make clear, the nature of migrant selection in terms of observable skill that Figure 1 implies depends on the distribution of schooling in the source country. For instance, if the support of the schooling distribution goes from some value above s_M to some value above s_H, then there are no long-term migrants (i.e., no group I or II individuals), and the short-term migrants for foreign education will be positively selected. However, in the case where the support of the schooling distribution spans s_L, s_M, and s_H, then the nature of selection depends on the relative mass of the distribution in each of the four groups.

Impact of Foreign Education: Access and Quality

As discussed, Kwok and Leland (1982) examine the impact that informational asymmetries could have on migrant selection for employment, conditional on having already migrated for education. The model in this paper affords the opportunity to examine how immigrant access to foreign education and its relative quality affects the size and productivity of migrant flows.

Comparing Figure 2 and Figure 1 shows that the availability of relatively high quality foreign institutions of higher education increases the magnitude of both short-term migration abroad for education, nonexistent in Figure 2, as well as long-term migration abroad for employment, as $s'_L < s_M$. The former sets the stage for Kwok and Leland's model, while the latter raises the possibility that "brain drain" (in terms of, say, GDP per capita and its growth) for the source country could occur from resident access to foreign education, even without informational asymmetries. However, it should also be noted that the mean skill level and productivity (as reflected through wages, assuming competitive labor and product markets) of long-term migrants increases for *both* the source and destination country when individuals have access to foreign education. This makes any potential "brain drain" implications of such foreign institution access ambiguous and outside of the current model's scope without further extensions (though empirical evaluation would remain a possibility).

Figure 3, meanwhile, displays the impact of a decrease in domestic educational quality (or analogously, an increase in the relative quality of foreign education). Compared to Figure 1, both regions I and III have diminished in size, while regions II and IV have grown in size. Thus, short-term migration for education definitively increases. Also, long-term migration for employment increases, as $s'_M > s_M$. Intuitively, this is because some of the group III individuals who used to remain domestically for both education and employment now choose to go abroad for both (although the figure shows that there are nevertheless some individuals who now choose to go abroad only for education and then return home). In terms of productivity and wages, the lower level of educational quality available in the source country is harmful. Mean wages of individuals as a whole fall, as evidenced by the upper envelope in Figure 3 lying weakly below its former value in Figure 1.

4.2.2 Extension: Informational Asymmetries

In the spirit of and expanding upon Kwok and Leland (1982), it is also of interest to examine how informational asymmetries on the part of firms might alter the nature of selection.

Kwok and Leland (1982), in modeling such asymmetries, assume that a group of migrants employed in a country which differs from their country of education will be paid a wage equal to the average productivity of the group as a whole. In other words, firms cannot perfectly observe the true productivity of foreign-educated individuals, due for instance to a lack of familiarity with the foreign education system.

To incorporate a similar assumption into this paper's framework, I first assume now that there exist distributions of educational institution quality q in country 0 and country 1 each, such that $q_0 \sim (\bar{q}_0, \sigma^2_{q_0})$ and $q_1 \sim (\bar{q}_1, \sigma^2_{q_1})$. Analogous to previously discussed theory, it can still be assumed that $\bar{q}_1 > \bar{q}_0$. While firms know all moments of the domestic quality distribution q_0, it is assumed that for the foreign quality distribution, they only know the first moment, $\bar{q}_1$. This generates the aforementioned imperfect observation by firms of foreign-educated individuals' productivity.

I utilize the framework of the statistical discrimination literature that explores the nature of wage differentials between different groups (e.g., Phelps, 1972; Aigner and Cain, 1977), and also focus on individual unobserved skill rather than overall productivity. I now allow for the possibility that firm assessments of individual skill are noisely "measured" when individuals were educated abroad.

This assumption results in unobserved skill ϵ, both indirectly (via return to skill δ) and directly, no longer having an effect on log wages. More formally, it is assumed that there exists some variable $\theta = f(\bar{q}, L)$ (where $f_{\bar{q}} > 0$ & $f_L > 0$). Let $\epsilon^* = \epsilon + v$, where ϵ^* is the distribution of firms' *perceptions* of individuals' unobserved ability who were educated in a country that differs from the firm, and v is some mean zero error term, such that $v \sim (0, \sigma_v^2)$. Thus, conditional on their perceptions of individuals' unobserved ability, firms' expectations of actual unobserved ability are are a weighted sum of the mean actual ability and the perceived ability. In other words,

$$\mathrm{E}(\epsilon|\epsilon^*) = (1-\omega)\bar{\epsilon} + \omega\epsilon^* \tag{4.5}$$

where $\omega = \mathrm{Var}(\epsilon)\ /\ [\mathrm{Var}(\epsilon) + \mathrm{Var}(v)]$, or the "reliability ratio," which here refers to the reliability of firms' assessments of foreign-educated individuals' ability. Thus, the noisier a firm's perceptions are of actual unobserved individual ability, as noted by v, the less weight said firm places on those perceptions and the more weight it places on mean ability. This implies the following adjustments to the log wage equations (2) and (3) from earlier:

$$\ln(w_{i1,0}) = \mu_0 + (1-\omega)(\theta_{1,0}s_i) + \omega(\delta_{i1,0}s_i + \epsilon_{i0}) \tag{4.6}$$

$$\ln(w_{i0,1}) = \mu_1 + (1-\omega)(\theta_{0,1}s_i) + \omega(\delta_{i0,1}s_i + \epsilon_{i1}). \tag{4.7}$$

As the variance of v increases relative to the variance of ϵ, ω approaches 0 and the wages are identical of all individuals educated in a different location from their employment and with the same amount of schooling, as in Kwok and Leland (1982), with no other idiosyncratic component. However, as the variance of v is small relative to the variance of ϵ and ω approaches 1, wages of individuals educated in a different location from their employment are determined as before.

In the most extreme case of asymmetric information, the initial wages that firms pay individuals who were educated in a country that differs from the location of the firm are determined with $\omega = 0$, such that no weight is placed on firm perceptions of unobserved ability. Figure 4 displays the model's implications in such a case for individuals on average, again suppressing i subscripts and ϵ terms. Compared to Figure 1, both regions I and IV have diminished in size, while regions II and III have grown in size. For both employment locations, individuals are less likely to be educated in a different location as a result of the informational asymmetries. Similar to Kwok and Leland (1982), *conditional* on individuals going abroad for education, they are now less likely to return home (region IV smaller both absolutely and relative to region II) and more likely to remain abroad (region II larger both absolutely and relative to region IV).

However, whether individuals are more or less likely to actually go abroad for education, unconditionally, is unclear, as the change in short-term migration is ambiguous. It increases (decreases) if and only if $s_L - s'_L$ is greater (less) than $s'_H - s_H$. Unambiguously, however, there is no change in long-term migration due to the asymmetries, as s_M has not moved. This shows that one may come to incorrect conclusions about how informational asymmetries affect long-term migration if examining only how the asymmetries affect the employment locations of foreign-educated individuals. One needs to also determine how the asymmetries alter the employment locations of domestic-educated individuals.

In terms of productivity and wages, as with diminished educational quality, the informational asymmetries are harmful. Mean wages of individuals as a whole fall, as evidenced by the upper envelope in Figure 4 lying weakly below its former value in Figure 1. Note that, in contrast to Kwok and Leland (1982), for a given distribution of observable skill, it is the *more* skilled individuals that return home after acquiring foreign education and the *less* skilled individuals that remain abroad, even in the presence of asymmetric information. However, this is generally the case in the current model. To determine the extent of any "brain drain" resulting from the asymmetries in the current framework, one would need to evaluate whether mean wages fall by more for those employed in the source country (regions III and IV) or the destination country (regions I and II). Although this appears to be the case in Figure 4, more formal evaluation of the wage mass lost abroad vs. domestically would be necessary and could not be done without knowledge or an assumption about the distribution of schooling.[4]

These firm-level informational asymmetries could also have implications for the nature of wage dynamics of the migrants for whom they are relevant. Specifically, over time, one could imagine that the firm-level asymmetries related to the imperfect observation of an individual's ability should not persist (e.g., Altonji and Pierret (2001)). Thus, ω may actually be a function of time t, such that $\omega = \omega(t)$, and $\omega'(t) > 0$ (in other words, more weight is placed on firm perceptions of individual unobserved ability over time rather than group characteristics). Although in reality,

[4]For instance, assuming the distribution of schooling goes from some value below s'_L to value $s_U > s'_H$, the expression for the loss in log wage mass in region IV alone is $\int_{s'_H}^{s_U} [(\mu_0 + \theta_{1,0}s - (1+\beta)\pi) - (\mu_0 + \delta_{1,0}s - (1+\beta)\pi)]ds$.

firms may still initially place some nonzero weight on their perceptions such that $\omega(0) \neq 0$, the general wage dynamics would remain the same.[5]

4.3 Data

The analysis uses a population sample (5 percent) of immigrants from the Integrated Public Use Microsamples (IPUMS) of the decennial U.S. census in 2000 (Ruggles et al. 2009). The sample consists of working-age individuals ages 18 to 64 not living in group quarters unless those quarters are schooling-related (e.g., boarding school). Only individuals who immigrated when they were at least 18 years-old are analyzed. All fifty U.S. states are included (Washington, D.C. is excluded). Individuals with no less than some college education and no more than a college degree are classified as high-skilled and are the focus of analysis, all based on census information on the highest grade attended (Jaeger 1997).[6] Individuals with more than a college degree are excluded, as their incentives and timing of migration might differ from the framework of the model.

Empirically, an immigrant is defined as an individual born abroad who is currently either a non-citizen or a naturalized citizen. Exceptions are: a) individuals born in U.S. territories or possessions (e.g., Puerto Rico, American Samoa); b) individuals born in countries where they are granted automatic U.S. citizenship due to political unions with the U.S. if not already deemed natives under exception (a) (e.g., Northern Mariana Islands); and c) individuals born abroad of American parents. There are 241,473 immigrant observations in the underlying sample, consisting of 71,294

[5]Although not examined currently, one potential testable implication of this result is that the wage variance of a given group of foreign-educated immigrants, conditional on other relevant wage factors and relative to US-educated immigrants, should increase over time.

[6]Jaeger's (1997) recommendations for coding are of particular importance here, since it is this margin of high-skilled labor where the differences exist between the census coding and his. Specifically, in the census consistent recode of educational attainment, respondents who are attending their first year of college or who did not complete that first year are identified with '12th grade' as their highest attended grade of education, whereas I categorize the highest grade attended for these individuals as 'some college'.

immigrants that were college-educated in the U.S. and 170,179 immigrants that were college-educated in their home countries, of which 71 are represented. These observations are then collapsed into source-country level means to create the cross-section dataset.

Proxy measures for educational quality and informational asymmetries for the subset of source countries represented in the census data sample are constructed using data from the Cybermetrics Lab (2007). For 3000 higher education institutions in 2007, this data contains aspects of their websites relating to size, rich file content, scholarship, and visibility.[7] For each of those four characteristics, the institutions are ranked and the resulting rank is their assigned score for that variable. I utilize the rankings based on the visibility variable to create an ordinal, proxy measure of asymmetric information, and use rankings based on the aggregation of the remaining three website variables to create an ordinal, proxy measure of educational quality. It should be noted that the rankings for Cybermetrics' quality measure range from 1 to 3000. However, the rankings for the individual component measures may exceed 3000, as their confidential data samples over 10,000 institutions.

Although I utilize these reconfigured measures, it should be noted that the overall institution rankings originally constructed by Cybermetrics using all four variables are highly correlated with other cross-country educational quality rankings, such as the Times Higher World University Rankings (Cybermetrics Lab 2007). Additionally, it is likely that these website characteristics are highly correlated with institutional

[7]From Cybermetrics Lab (2007), 1) Size: Number of pages recovered from four engines: Google, Yahoo, Live Search and Exalead. For each engine, results are log-normalised to 1 for the highest value. Then for each domain, maximum and minimum results are excluded and every institution is assigned a rank according to the combined sum. 2) Rich Files: After evaluation of their relevance to academic and publication activities and considering the volume of the different file formats, the following were selected: Adobe Acrobat (.pdf), Adobe PostScript (.ps), Microsoft Word (.doc) and Microsoft Powerpoint (.ppt). These data were extracted using Google and merging the results for each filetype after log-normaliing in the same way as described before. 3) Scholarship: Google Scholar provides the number of papers and citations for each academic domain. These results from the Scholar database represent papers, reports and other academic items. 4) Visibility: The total number of unique external links received (inlinks) by a site can be only confidently obtained from Yahoo Search, Live Search and Exalead. For each engine, results are log-normalised to 1 for the highest value and then combined to generate the rank.

wealth/endowment, and thus would also be correlated with more traditional measures of educational quality such as the teacher-student ratio or per-pupil funding. Lastly, I rely on the typical persistence of quality rankings for postsecondary institutions to reconcile usage of the 2007 website data with the 2000 census data (Black and Smith 2004).

4.4 Methodology and Results

Figures 3 and 4 of the model form the basis for the empirical methodology to be explored. In order to assess the impact of educational quality and information asymmetries on migrant sorting, I use ordinary least squares (OLS) to estimate the following model for source country j in 2000:

$$USColl_j = \beta_0 + \beta_1 qual_j + \beta_2 asym_j + \beta_3 (qual_j \times asym_j) + \mathbf{X}'_j \alpha + \epsilon_j. \tag{4.8}$$

$UScoll_j$ is the proportion of immigrants from country j who arrived between ages 18 and 22, assumed to have thereby acquired their college education in the U.S. Measure $qual_j$ is mean educational quality from the mean institutional rankings of each country j, while $asym_j$ is a measure of mean informational asymmetries between the U.S. and country j, again based on mean institutional rankings in country j. A vector of control variables $\mathbf{X}$ are also included in some specifications to try to capture factors that differ from the model's assumptions that could also affect $UScoll_j$. Specifically, the mean age of immigrants in the source country is included to control for unobserved ability differences across immigrant cohorts, while the proportion from each country living in a metropolitan center is included to try to account for potential local market differences (unlike the model, which assumes a national market).[8] Additionally, to account from departures from the individual-level maximization problem that the model assumes, the proportion from each country with a parent living the household is also included as a control variable, to represent strong familial ties and possible parental influence in the migration decision. Lastly, ϵ_j is a mean-zero error term corresponding to unobserved skill in the model.

Because heteroskedasticity in U.S. college-educated rates is likely to occur in this cross-country dataset, heteroskedasticity-robust standard errors are employed. All specifications will also be unweighted, so that each country cell receives equal weight in estimation. The model predicts $\beta_1 < 0, \beta_2 > 0$, and $\beta_3 > 0$, as Figures 3 and 4 illustrate. However, to the extent that there is a classical measurement error problem with the proxy variables *qual* and *asym*, all coefficients would be biased toward 0. This may be a nontrivial issue given the inherent difficult in measuring cross-country educational quality and informational asymmetries, which have no formal definition.

Table 1 displays the top ten and bottom ten countries in the sample regarding the proportion of immigrants college-educated in the U.S. There does not appear to be any notable pattern amongst the top ten countries, although amongst the bottom ten there are several former countries of the U.S.S.R. There similarly does not appear in either group to be a clear relationship between the proportion college-educated in the U.S. and either educational quality or asymmetric information. Nevertheless, regressions in Tables 3-5 will explore this more formally.

Meanwhile, Table 2 displays the summary statistics for the dependent and independent variables used in estimation. There is not much variation in the immigrant proportion that is U.S. college-educated, which may further complicate analysis and the ability to discern a significant relationship between it and the independent vari-

[8] Given the small number of observations, state dummies were not utilized to capture local market differences.

ables of interest. Also now included (as dependent variables in Table 4, to be explained) are measures of the proportion U.S. college-educated by age/cohort (the two effects, even in this case due to mortality or emigration, cannot be separated with a single cross-section). The U.S. college-educated proportion generally decreases for older ages/cohorts.

The results from estimation of equation (8) are displayed in Table 3. As both columns 1 and 2 show, there is no statistically significant effect of educational quality and informational asymmetries on education location choice of immigrants. Moreover, across all specifications, the quality measure is positive and thus of the wrong sign. However, the asymmetry measure and the quality/asymmetry interaction are of the correct sign in all specifications, though again not statistically different from zero. Inclusion of controls in column 3 decreases coefficient magnitudes, particularly of the quality measure, but the qualitative results are the same. However, the model fit is, perhaps not surprisingly, now an order of magnitude larger. Lastly, because the theoretical model is for a given source country, column 4 includes region dummies to attempt to count for mean differences education location choices by groupings of source countries.

Tables 4 and 5 further explore the results by grouping of a couple of the control variables. As previously discussed, if there are significant differences in unobserved ability of immigrant cohorts, as has been hypothesized in some literature (e.g., Borjas 1987), the results might significantly by these groups. Table 4 examines this hypothesis, although the age and cohort effects cannot be strictly separated with this single cross-section. While the coefficient signs and magnitudes do vary notably across specifications, indicating perhaps some degree of age/cohort heterogeneity, in no case are any of the coefficients statistically significant, nor in any case are all of the predicted coefficient signs correct. Table 5 examines the effects of quality and asymmetry by region. Due to even smaller sample sizes in this case, the results need to be interpreted with some caution. Regardless, once again, no specification has coefficient signs that all match up with the theoretical predictions. In column 3 for the Americas and the Caribbean, an increase in mean asymmetry rank of 1 is significantly associated with a decrease in 0.07 percentage points in the mean U.S. college education rate, at the 10 percent level. However, quality and its interaction with asymmetries are both statistically significant and of the wrong sign in this specification as well, and the sample size is only 15 countries.

Thus, throughout the previous analysis, the theoretical predictions do not seem to hold. As noted earlier, while this may be due in part to measurement error in the proxy variables for quality and asymmetries, or the relatively small sample size, it seems to support a rejection of the current model of migrant self-selection. Nevertheless, further analysis is warranted to better evaluate these competing hypotheses before fully rejecting the model as invalid.

4.5 Conclusion

This paper examines the nature of self-selection in the migration of highly-skilled individuals when allowing for cross-country differences in educational quality, in the baseline model, as well as individual- and firm-level informational asymmetries across countries, in an extension of the baseline model. Conditional on foreign employment and an assumption about higher educational quality abroad, the model predicts that more highly skilled individuals will obtain education abroad. Increases in domestic educational quality are predicted to decrease the proportion of foreign-educated immigrants, while increases in cross-country informational asymmetries are predicted to increase the proportion of foreign-educated immigrants.

To test the model's predictions, I use U.S. Census microdata as well as proxy data on cross-country educational quality and asymmetric information from the websites of higher education institutions. In empirical analysis, I find no evidence that college quality or informational asymmetries significantly influence or are associated with the share of high-skilled immigrants acquiring college education in the United States. The observed results may be due in part or whole to potential measurement error in the proxy measures for quality and asymmetries, which if classical, would bias estimates toward zero. Nevertheless, in lieu of contrary supporting evidence, I interpret this finding as a rejection of the model's premise for the role of educational quality and asymmetric information in migrant self-selection.

Because of the small sample size of analysis, potential omitted variables with a cross-country cross-section, and potential classical measurement error in the proxy variables of interest, future work taking an alternative path to confirm or refute this paper's rejection of the selection model would be of aid. As direct, accurate measures of cross-country educational quality and informational asymmetries are difficulty to obtain, a focus on the static and dynamic predictions for wages between US-educated and foreign-educated immigrants might be preferable. This would also allow for estimation at an individual or cohort level using census data, thereby increasing sample sizes substantially.

4.6 Figures and Tables

Table 4.1: Proportion of Immigrants U.S. College-Educated

TOP TEN COUNTRIES				
Birthplace	*Proportion U.S. College-Educated*	*Total Immigrants*	*Mean Education Quality*	*Mean Asymmetric Information*
Cyprus	0.605	131	2,111	1,558
Kuwait	0.589	273	1,111	3,169
Malaysia	0.514	818	1,442	2,092
Lebanon	0.450	1276	1,081	2,491
Guatemala	0.443	1960	326	2,066
Saudi Arabia	0.443	228	1,168	3,087
Hong Kong	0.438	2085	2,306	1,141
Mexico	0.431	22447	1,732	2,423
Norway	0.421	359	1,445	1,582
Hungary	0.407	666	1,536	2,055

BOTTOM TEN COUNTRIES				
Birthplace	*Proportion U.S. College-Educated*	*Total Immigrants*	*Mean Education Quality*	*Mean Asymmetric Information*
Australia	0.202	872	2,393	689
Lithuania	0.202	207	1,471	2,304
China	0.195	8541	995	2,324
Romania	0.190	1159	1,732	2,714
South Africa	0.189	1035	1,976	1,449
Russia	0.170	3550	1,253	2,343
Georgia	0.166	106	127	1,908
Belarus	0.166	444	1,542	2,210
Bosnia	0.165	657	1,030	3,558
Ukraine	0.139	2996	748	2,881

Table 4.2: Summary Statistics

Variable	*N*	*Mean*	*Std Dev*	*Min*	*Max*
U.S. College-Educated	71	0.305	0.096	0.139	0.605
Education Quality	71	1,532	445	127	2,393
Asymmetric Information	71	2,113	694	689	4,107
Age	71	40.4	3.3	29.9	46.4
Living in a Metro Center	71	0.264	0.087	0.084	0.508
Parents in Household	71	0.571	0.131	0.359	0.841
U.S. College-Educated (ages 18-24)	71	0.888	0.066	0.678	1.000
U.S. College-Educated (ages 25-34)	71	0.345	0.120	0.130	0.626
U.S. College-Educated (ages 35-44)	71	0.221	0.115	0.000	0.486
U.S. College-Educated (ages 45-54)	71	0.184	0.096	0.000	0.372
U.S. College-Educated (ages 55-64)	70	0.185	0.142	0.000	0.596

Table 4.3: Impact of Quality and Asymmetries on Education Location (OLS)

	Dependent Variable: Proportion U.S. College-Educated			
Explanatory Variable	(1)	(2)	(3)	(4)
Quality	0.028	0.089	0.049	0.054
	(0.032)	(0.112)	(0.105)	(0.103)
Asymmetries	0.008	0.056	0.043	0.045
	(0.017)	(0.088)	(0.080)	(0.078)
Quality*Asymmetries		-0.030	-0.019	-0.023
		(0.050)	(0.047)	(0.046)
Controls	No	No	Yes	Yes
Region Dummies	No	No	No	Yes
R^2	0.01	0.02	0.18	0.18
Number of observations	71	71	71	71

* significant at 10%; ** significant at 5%; *** significant at 1%
Notes: Quality and Asymmetry variables are rescaled in the table above by a factor of 1000.
Controls are age, proportion living in a metropolitan center, and proportion with parents living in household.
There are three region dummies, where region 1 is Africa, Asia, Middle East, and Australia, region 2 is Europe, and region 3 is North America, Latin America and the Caribbean (countries in each region listed in Data Appendix).
All regressions include heteroskedasticity-robust standard errors.

Table 4.4: Impact of Quality and Asymmetries on Education Location, by Age/Cohort (OLS)

	Dependent Variable: Proportion U.S. College-Educated in Age Group				
Explanatory Variable	Ages 18-24	Ages 25-34	Ages 35-44	Ages 45-54	Ages 55-64
	(1)	(2)	(3)	(4)	(5)
Quality	-0.075	0.059	0.108	0.052	0.028
	(0.047)	(0.150)	(0.121)	(0.082)	(0.093)
Asymmetries	-0.051	0.024	0.071	0.024	-0.026
	(0.037)	(0.108)	(0.100)	(0.069)	(0.065)
Quality*Asymmetries	0.043	-0.015	-0.032	-0.028	-0.020
	(0.022)	(0.063)	(0.058)	(0.040)	(0.039)
Controls	Yes	Yes	Yes	Yes	Yes
Region Dummies	Yes	Yes	Yes	Yes	Yes
R^2	0.15	0.13	0.22	0.23	0.50
Number of observations	71	71	71	71	70

* significant at 10%; ** significant at 5%; *** significant at 1%
Notes : Quality and Asymmetry variables are rescaled in the table above by a factor of 1000.
Controls are proportion living in a metropolitan center and proportion with parents living in household.
There are three region dummies, where region 1 is Africa, Asia, Middle East, and Australia, region 2 is Europe, and region 3 is North America, Latin America and the Caribbean (countries in each region listed in Data Appendix).
All regressions include heteroskedasticity-robust standard errors.

Table 4.5: Impact of Quality and Asymmetries on Education Location, by Region (OLS)

	Dependent Variable: Proportion U.S. College-Educated		
Explanatory Variable	Region 1	Region 2	Region 3
	(1)	(2)	(3)
Quality	0.103	0.026	-0.161
	(0.158)	(0.064)	(0.057)***
Asymmetries	0.116	-0.044	-0.071
	(0.117)	(0.048)	(0.041)*
Quality*Asymmetries	-0.077	0.013	0.048
	(0.082)	(0.034)	(0.026)*
Controls	Yes	Yes	Yes
R^2	0.59	0.45	0.71
Number of observations	21	35	15

* significant at 10%; ** significant at 5%; *** significant at 1%
Notes: Quality and Asymmetry variables are rescaled in the table above by a factor of 1000.
Controls are age, proportion living in a metropolitan center, and proportion with parents living in household.
There are three region dummies, where region 1 is Africa, Asia, Middle East, and Australia, region 2 is Europe, and region 3 is North America, Latin America and the Caribbean (countries in each region listed in Data Appendix).
All regressions include heteroskedasticity-robust standard errors.

Figure 4.1: Immigrant Self-Selection for Schooling and Employment

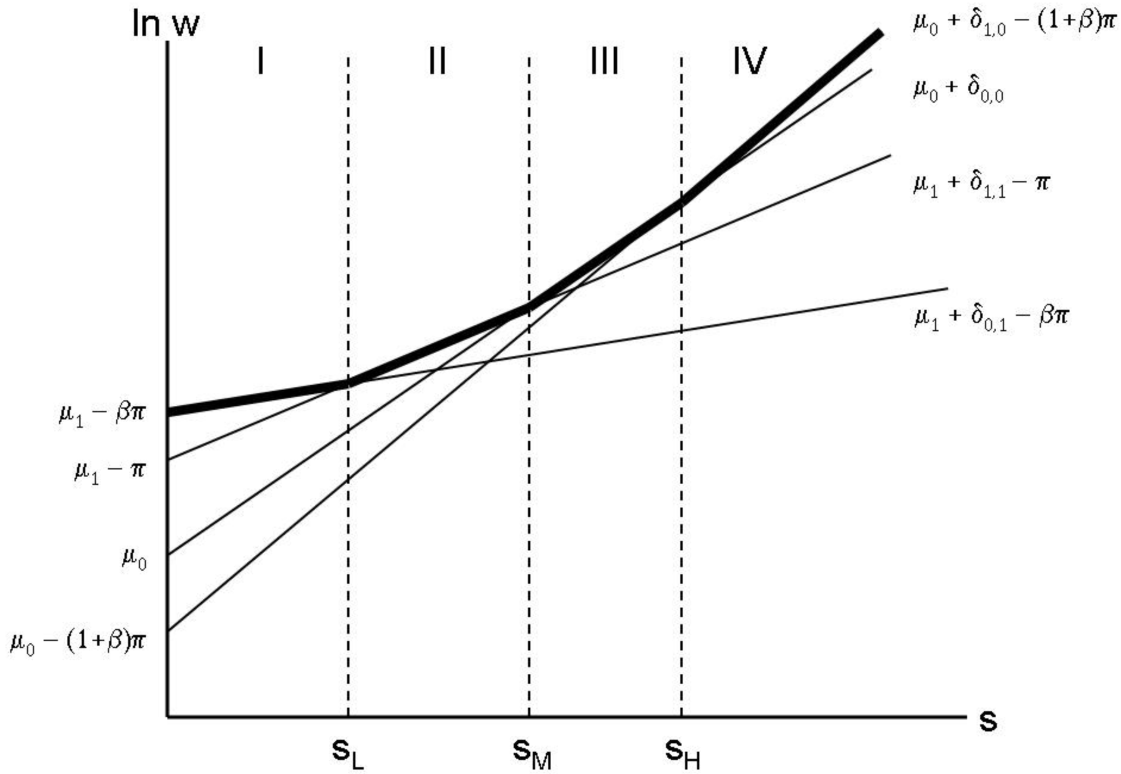


Figure 4.2: Elimination of Access to Foreign Education

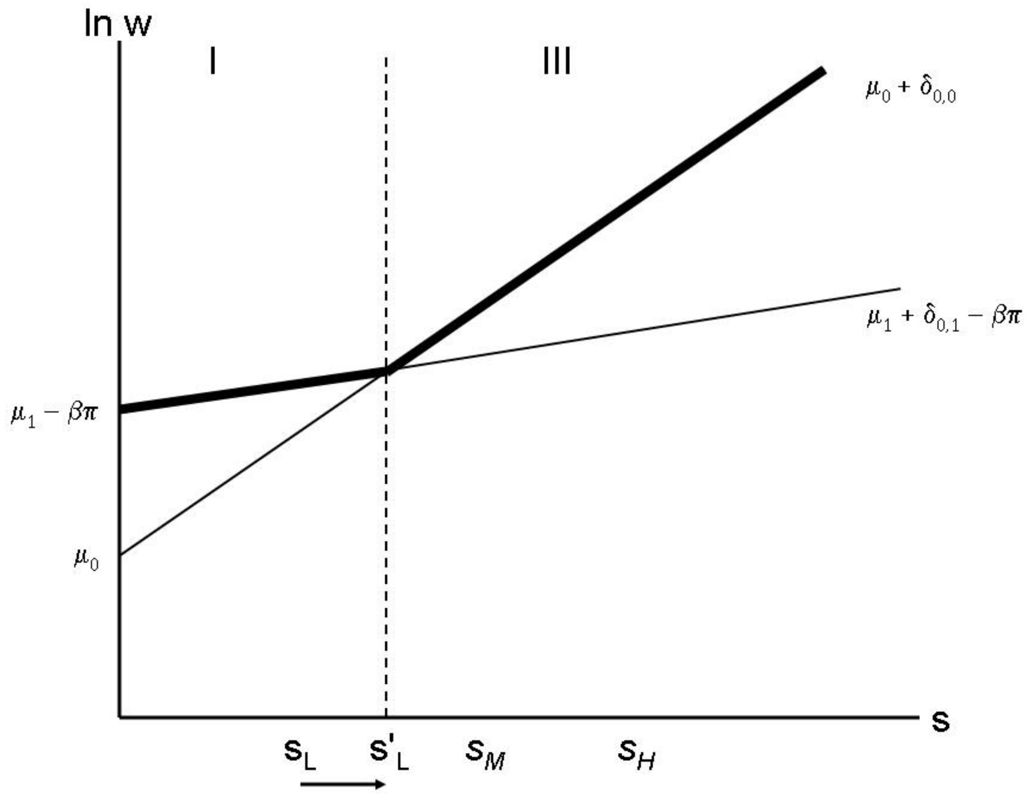


Figure 4.3: A Decrease in Domestic Educational Quality

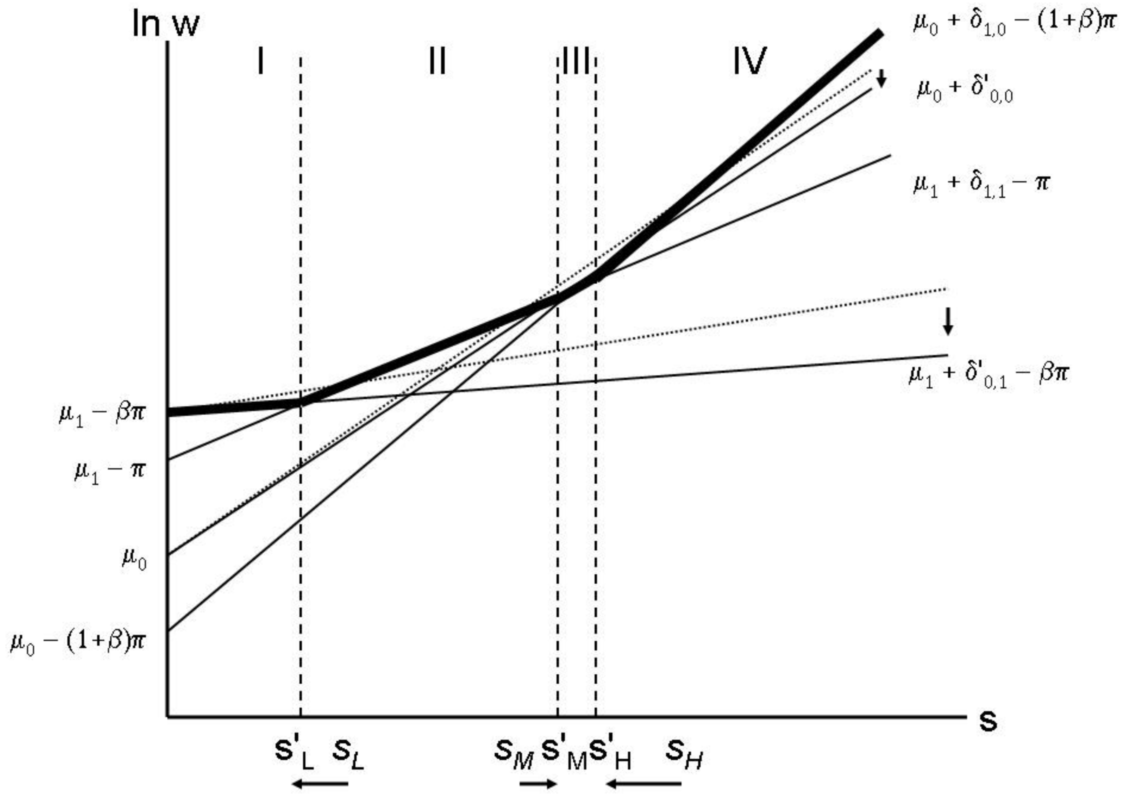
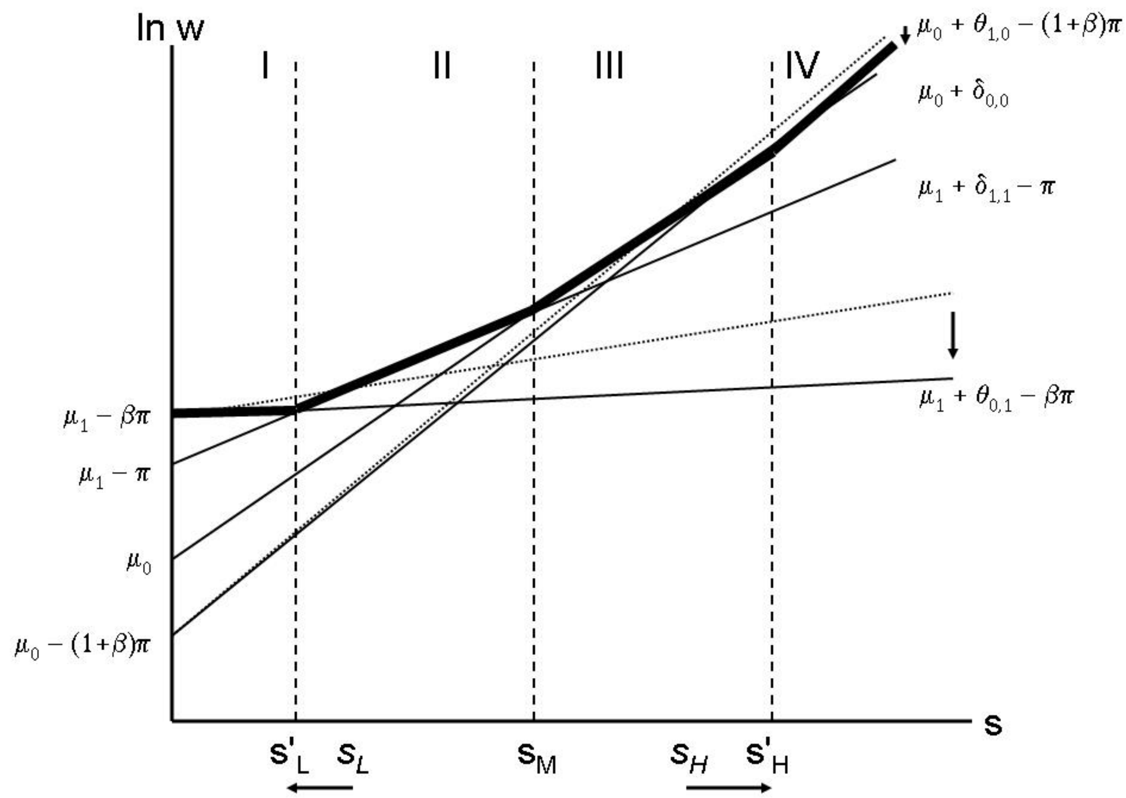


Figure 4.4: Existence of Informational Asymmetries



4.7 Appendix

Data Appendix: Sample Countries

Region 1: Africa, Asia, Australia, Middle East

China, Hong Kong, Taiwan, Japan, South Korea, Indonesia, Malaysia, Philippines, Singapore, Thailand, Vietnam, India, Iran, Kuwait, Lebanon, Saudi Arabia, Egypt, South Africa, Australia, New Zealand, Fiji.

Region 2: Europe

Denmark, Finland, Norway, Sweden, Ireland, Belgium, France, Netherlands, Switzerland, Greece, Macedonia, Italy, Portugal, Spain, Austria, Bulgaria, Slovak Republic, Czech Republic, Germany, Hungary, Poland, Romania, Croatia, Serbia, Bosnia, Latvia, Lithuania, Russia, Belarus, Ukraine, Armenia, Georgia, Cyprus, Israel, Turkey.

Region 3: North America, Latin America & Caribbean

Canada, Mexico, Costa Rica, Guatemala, Cuba, Jamaica, Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, Peru, Uruguay, Venezuela.

CHAPTER V

Conclusion

The first essay of this dissertation provides indirect evidence of how immigrants impact market prices and how natives respond to immigration by adjusting their skill level. These findings have implications for education and labor studies that have attempted to directly measure such effects with mixed results. They also contribute to the growing literature on how general equilibrium responses may play a role in the seemingly rapid absorption of immigrants into local markets, thereby mitigating direct native wage effects of immigration. The second essay demonstrates that, after credibly accounting for the endogeneity of foreign aid flows, aid only appears to have a short-run, positive effect on recipient per capita GDP growth and no significant long-run effect. This result is consistent with the finding in the essay that aid inflows seem to be predominately consumed rather than invested, which although not a stimulant of long-run growth, does at least benefit aid recipients by raising the level of GDP per capita. Finally, the third essay provides empirical evidence investigating the role of cross-country informational asymmetries and educational quality differences on immigrant self-selection, finding no significant role for either factor. While additional work would be of great benefit to explore this finding, it nevertheless serves on its own to further our understanding of which factors do and do not significantly determine immigrant location choice.

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