

**HOW IMMIGRANTS AND STUDENTS
RESPOND TO PUBLIC POLICIES:
EVIDENCE FROM WELFARE REFORM, THE
MINIMUM WAGE AND STAFFORD LOANS**

by
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A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Economics)
in The University of Michigan
2008

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to my parents and grandparents
for all their sacrifices on my behalf

ACKNOWLEDGEMENTS

I would like to thank the members of my committee — Charles Brown, Sheldon Danziger, Rebecca Blank and John DiNardo — for all their help, guidance, and timely advice throughout my graduate school experience. I would also like to thank Kerwin Charles for his friendship and excellent mentorship. In addition, several graduate students helped keep me sane and motivated. I would especially like to thank Soren Anderson, Ben Keys, Brian Kovak, Alex Resch and Jim Sallee for fostering an environment of encouragement and camaraderie. Their support was essential to my success, both personally and academically.

In addition to those already mentioned, I would like to thank John Bound, Mike Elsby, Jeff Smith, Gary Solon and several seminar and conference participants for helpful comments on the first essay. Ethan Lewis graciously provided code to create consistent geographic boundaries across census years.

The third essay was co-written with Ben Keys. I truly appreciate his partnership in that mutual endeavor. The project benefited greatly from comments by Rebecca Blank, John Bound, Charlie Brown, Kerwin Charles, Paul Courant, Sheldon Danziger, John DiNardo, Mike Elsby, David Gunter, Miles Kimball, Pat Kline, Jens Ludwig, Karen Pence, Alexandra Resch, Jim Sallee, Dan Silverman and Jeff Smith, as well as participants at the NBER Higher Education Working Group Meeting and the University of Michigan Labor Seminar.

I am also grateful for financial support from the National Science Foundation and

Rackham Graduate School throughout my graduate career. The faculty and staff of the National Poverty Center provided me not only with office space, but with a collegial environment and an opportunity to engage with the research frontier. I am truly grateful for the resources they invested in me.

Finally, I would like to thank my family for all of their support over the past five years. I owe a special debt of gratitude to my wife, Candace, for her willingness to join me in whatever journey comes next.

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CHAPTER I

Introduction

This dissertation consists of three distinct essays, although the first two are closely related. In the first two chapters, I present evidence that low-skilled newly arrived immigrants help keep the economy in geographic equilibrium by differentially selecting destinations that provide better labor market prospects. Many models of local economies predict that the presence of a mobile factor will reduce wage and unemployment disparities across geography. Immigrants, as a self-selected group of highly mobile workers, are a natural though understudied candidate to serve in this capacity. Given the large disparity in mobility by skill among natives (low-skilled workers are especially immobile), immigrants' ability to respond to such differences is especially important.

In the first essay, I demonstrate that immigrants select labor markets with smaller welfare-reform created native supply shocks. Reforms to the welfare system in the 1990s dramatically increased the labor market participation of native low-skilled women. Welfare leavers and newly arriving low-skilled immigrants work in much the same occupations, and thus are competing for the same set of job vacancies. Simple labor market theories predict that increases in native supply will either reduce the probability that searching workers find employment, decrease their wages once they

are hired, or some combination of the two. Immigrants who are willing to alter their chosen destination for an improvement in expected earnings have a clear incentive to choose labor markets experiencing smaller policy-driven supply shocks. Using a linearized version of a discrete choice model, I demonstrate that immigrants do exactly this. Even after netting out differences due to the changing set of immigrants' origins, the distribution of destination cities within the US shifts markedly away from cities with high welfare participation prior to reform and toward cities with lower participation. In fact, changes in immigrants' chosen destinations "undo" nearly all of the difference in labor supply that would have resulted had they continued to go to more traditional locations.

The second essay demonstrates that changes in states' minimum wage policies also affect the destinations immigrants choose. Unlike the first chapter, in which the policy change creates a clear incentive for immigrants to select one location over another, an increase in the minimum wage has a theoretically ambiguous effect on a job seeker's expected earnings. The wage a worker will earn if he/she is hired increases, but the probability of securing a position falls. I apply a classic two-sector labor market model to a geographic context, and demonstrate how the labor demand elasticity and turnover rate combine to determine whether expected earnings rise or fall. I then use native teenagers, a highly immobile group, to determine that the labor demand is sufficiently elastic that immigrants should prefer states that do not increase their minimum wage. I then show that newly arriving immigrants do exactly this. I find that immigrants choose not to go to states with large increases in the minimum wages, instead selecting states with smaller increases or a fixed minimum. The results are strong and statistically significant even after accounting for unobserved fixed state characteristics, changes in the size of the immigrant population

over time and differential growth rates of the immigrant population across states.

In sum, the first chapters provide evidence that newly arrived immigrants act as labor market arbitrageurs, moving to destinations that provide better labor market prospects and preventing negative labor market shocks from remaining concentrated in a few geographic areas.

The final essay, which is co-authored with Ben Keys, investigates another form of arbitrage. We propose an explanation for a surprising borrowing phenomenon: nearly one fifth of undergraduate students who are offered interest-free loans turn them down. In doing so, they are foregoing a significant government subsidy worth up to \$1,500. We suggest that the recent advances in behavioral economics can explain students' failure to accept this "free money." We discuss a burgeoning literature under which economic actors may actually prefer to limit their future choices rather than expand them. As evidence that students are acting to limit their own liquidity, we demonstrate a differential rejection rate based on the level of direct access that students would have to the money. Some students who live off-campus will receive refund checks if they accept the loan, exposing them to the temptation of easy-to-spend cash. Students with the exact same family income and aid package, but who live on-campus, will have their aid funds applied directly to their housing expenses, partially mitigating the temptation of additional liquidity. Using a difference-in-differences strategy, we find that students who would receive a refund check are six to seven percentage points more likely to reject the loan than are similar students living off-campus. We interpret this finding as evidence for the behavioral explanation.

CHAPTER II

How Do Immigration Flows Respond to Labor Market Competition from Similarly-Skilled Natives?

2.1 Introduction

In the policy debate over immigration reform, no issue is as controversial as the extent to which recent waves of immigration have affected the labor market opportunities of native workers. Economic theory clearly predicts that an exogenous increase in low-skilled labor in a closed labor market will reduce the wages paid to these workers. Yet many empirical studies have found very little effect of increased immigration on natives' labor market outcomes. Some authors have argued that this disparity could arise if immigrants endogenously choose labor markets with better earnings and employment prospects. In this paper, I present direct evidence of this type of selection by demonstrating that immigrants arriving during the 1990s avoided cities experiencing larger increases in native labor market participation as a result of reforms to welfare policy. The results reveal a substantial degree of selection: for each native woman who begins to work as a result of welfare reform, 0.83 immigrant women choose to live and work in alternative locations.

This finding has important implications for interpreting the two strands of an expansive literature that has examined the effect of immigration on natives' labor market outcomes. The first branch of the literature compares changes in wages and

employment for native workers in cities receiving large immigrant inflows to those in cities receiving smaller inflows. Card's (1990) influential paper studied the effect of the Mariel Boatlift on Miami's labor market. He found that despite the large influx of predominantly low-skilled immigrants, subsequent wage and employment growth among natives followed much the same pattern as in comparison cities that received few Marielitos. Several subsequent papers find similar results - native wage and employment growth is not significantly different across locations experiencing substantially different inflows of immigrants.¹

Critics of this "area analyses" approach have outlined two alternatives to explain this lack of a spatial correlation. The theoretical prediction of lower native wages requires both a closed labor market and exogenous immigration. The first criticism questions whether local labor markets defined at the state or city level are actually closed. The alternative hypothesis has been referred to as the "skating rink model" (Card and DiNardo 2000). According to this view, each new immigrant locating in a city bumps a native or pre-existing immigrant off the ice, i.e. causes them to move to an alternative location. The resulting internal migration spreads the increase in low-skilled labor throughout the nation, limiting the power of area studies analyses to detect the effects of increased immigration.

The second critique focuses on the requirement that immigration to a city be effectively random. The primary concern is that immigrants may disproportionately choose areas where demand for low-skilled labor is increasing. If this selection occurs, comparing natives' wage growth in cities with differently-sized immigrant inflows may understate the negative effects of immigration relative to those that would be obtained in a closed market.

¹Additional examples include Altonji and Card (1991), Lalonde and Topel (1991) and Schoeni (1997). A careful review of these studies finds that, although the estimates vary from study to study, they are clustered around zero (Smith and Edmonston 1997).

The “factor shares” approach responded to these potential shortcomings by explicitly treating the market for labor as integrated at the national level. The first study of this type combined data on immigration flows with externally-estimated demand elasticities across skill groups to simulate the effect of increased low-skilled immigration on native wages and employment (Borjas, Freeman and Katz 1997). These simulations suggest that immigrants have significantly lowered wages for low-skilled natives. The results are perhaps not surprising: in a model with only two factors, an increase in the supply of one factor must necessarily reduce the wages paid to that factor, and this approach essentially fits this model using census data.

More recent studies have addressed this shortcoming by considering a national market for labor that has multiple factor groups based on education and experience (Borjas 2003, Borjas, Grogger and Hanson 2006). Younger, less-educated workers’ wages have grown significantly less rapidly over the past four decades than have those of their older and more-educated counterparts. These skill groups have also received larger immigrant inflows over the same time period, and the authors of these studies attribute the differential wage growth to differences in competition from immigrants. In contrast to the area analyses approach the factor shares approach yields much more negative effects of immigration on natives’ labor market outcomes.

Given that the two approaches yield such contradictory results, recent studies have directly examined the original criticisms of the areas analyses method. Several studies examine whether internal migration patterns effectively “undo” increases in the low-skill population due to the arrival of new immigrants (e.g. Card and DiNardo 2000, Card 2001). Most find that internal migration does very little to offset changes in the skill distribution due to immigration. A recent paper demonstrates both that internal migration failed to mitigate changes to the skill mix of local economies

and that nevertheless differential immigrant inflows did not lead to differential wage changes across locations (Card and Lewis 2005).

Designing a study to evaluate the “skating rink” alternative is relatively straightforward. One simply has to determine whether cities receiving more immigrants actually become less skilled than cities with few new immigrants. Determining whether immigrants select locations that offer better labor market prospects has proved significantly more difficult for a number of reasons. First, immigrants may select areas with higher wages and wage growth even if geographic differences in these attributes do not directly affect immigrants’ location decisions. For example, wages and growth may be correlated with other local amenities important to immigrants, including the location of previous immigrants, the local blend of taxes and public goods or different costs of living. More importantly, testing the extent to which immigrants select promising labor markets requires overcoming a simultaneity problem. Even if immigrants do choose areas with tight labor markets, their decision to enter the market will reduce this tightness, making any selection mechanism difficult to establish empirically. Existing studies do not attempt to overcome this simultaneity issue and have found mixed results. Some give evidence consistent with highly mobile responsive immigration flows while others find that ethnic networks and other city characteristics dwarf any impact of labor market opportunities (Bartel 1989, Borjas 2001, Jaeger 2007).

How can these challenges to identification be overcome? The ideal research design would randomly assign increases in the demand for low-skilled labor across the country, and examine the resulting changes in where immigrants choose to locate. Lacking this experiment, one requires a source of plausibly exogenous geographic variation in the labor market opportunities available to new immigrants. This paper

argues that the increase in the number of low-skilled native women in the labor force due to the 1996 federal welfare reform provides this type of policy-driven variation.

This policy change provides an ideal environment to determine whether immigrants select cities with better labor market prospects. Suppose that immigrants and natives are substitutes in production, but that any consequences of this competition are diffused throughout the country because immigrants choose locations that offer better returns to their labor. Under this hypothesis, the native supply increases created by welfare reform had a similar effect, from a potential immigrant's perspective, as a decrease in demand for her type of labor. Moreover, the effect of welfare reform on the labor supply of natives was not equally distributed across cities. Instead, the increase in native female employment within a local labor market was primarily a function of the size of the population affected by the policy changes. With different benefit levels across states and important demographic differences across cities and states, the size of welfare caseloads varied dramatically prior to reform.

This paper contributes to the literature, therefore, by identifying exogenous policy-driven variation in the labor market prospects faced by newly-arriving low-skilled immigrants and evaluating the effect of these differences on immigrants' resulting location choices. I use decennial census data from the 1980, 1990 and 2000 five percent samples of the Integrated Public Use Microdata Series (IPUMS) to create a sample of immigrants who arrived during welfare reform, as well as in the decades prior to reform. I estimate regressions motivated by a discrete choice random utility model, allowing for unobserved city attributes that potentially differ among immigrants according to sending country. I find that low-skilled immigrants arriving in the US during the 1990s were significantly less likely to locate in the labor markets most

affected by welfare reform compared to immigrants arriving in the decade prior.

I also present several additional pieces of evidence that support interpreting this relationship as evidence of immigrants responding to labor market competition. I include a number of additional control variables, including measures of potentially offsetting changes in demand and variables that allow for general dispersion of immigrants across the country. Each of these variables enters the model in the expected direction, yet the negative estimate of the effect of the reform-induced supply shocks remains strong and significant. Additionally, I repeat the analysis using waves of the census immediately prior to welfare reform. In a period with no major changes to the welfare system (the 1980s), welfare caseloads are unrelated both to changes in native employment and to changes in immigrants' location decisions. Finally, I demonstrate that the negative relationship is strongest among immigrants who made their most recent move after the implementation of welfare reform.

The remainder of the paper is organized as follows: the next section describes the policy changes and presents evidence of welfare reform's disparate geographic effect on native labor supply; Section 2.3 presents a discrete choice model and motivates the appropriate empirical methodology for estimation; Section 2.4 discusses the data sources, and provides a descriptive analysis of new immigrants and the extent to which they compete for jobs with welfare leavers; Section 2.5 presents the main empirical results and additional robustness checks; the final section discusses the implications of these findings both for interpreting the previous literature and for policymakers.

2.2 Welfare Reform and Labor Supply

In this section, I discuss the changes to welfare policy over the 1990s and how each was designed to increase the employment of former recipients. I then present two facts that are essential to the later formal empirical analysis. First, I provide evidence that these reforms substantially increased the labor supply of the target population. I further demonstrate that geographical differences in welfare participation rates prior to reform reliably predict differential increases in native female employment over the reform period. Taken together, these results suggest that pre-reform participation rates can serve as an instrument for changes in native female labor supply from 1990 to 2000. These exogenous supply shocks allow for direct evaluation of whether immigrants choose labor markets with better returns to employment.

The federal cash welfare system, first implemented in 1935, was originally designed to provide for the material needs of widows with dependent children. The program was designed so that women could stay at home to care for their children. By the late 1970s, the demographic makeup of the welfare rolls had changed dramatically. Widows were covered by social security and rising rates of divorce and nonmarital childbearing meant that most recipients were in families headed by divorced and never-married mothers. As greater numbers of married women worked, there was political pressure to increase employment among mothers on welfare. In the early 1990s, states were given expanded authority to secure federal waivers from AFDC program rules and in 1996 the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) ended the AFDC program.

After reform, cash assistance was no longer a federal entitlement program; seeking or participating in employment became a pre-condition for benefit receipt. Welfare

offices implemented work support and “work first” programs to move women into the workforce. The culture of local welfare offices changed - caseworkers were now expected to provide supportive employment services rather than simply determining eligibility for benefits. Welfare recipients are now subject to a sixty month lifetime limit (fewer at state discretion), giving potential recipients an incentive to delay benefit receipt and search more intensely for employment opportunities before applying.

² Most states also reduced the rate at which benefits are taxed away as a recipient earns income, thus reducing disincentives to work. Concurrent expansions in the federal Earned Income Tax Credit gave single mothers additional financial incentives to work. Each of these policies was designed to increase employment and raise the return to work among women who, in the absence of reform, might have relied mainly on public assistance (Ellwood 2000). The cumulative effect of these reforms created the textbook definition of a labor supply shift - policy changes resulted in more low-skilled women willing to work at any given wage.

The empirical literature evaluating welfare reform supports the conclusion that these reforms increased employment among the target population. While any credible study of the effect of these changes includes essential controls for the role of the strong macroeconomy over the period in which reform was implemented, most studies (and especially evaluations of demonstration projects using random assignment) find that the policy changes had a significant effect on the labor force attachment of low-skilled women.³ Figure 2.1 uses data from the annual March Supplement to the Current Population Survey (CPS) and shows trends in the employment rate for women with at most a high school degree between the ages of eighteen and fifty-four living in large Metropolitan Statistical Areas (MSAs). They are classified according

²Grogger, Haider and Klerman (2003) find that as much as half of the decline in caseloads resulted from a decrease in the entry rate.

³For a careful review of the employment effects of welfare reform, see Blank (2002, pp.1139-1142).

to marital status and parenthood.⁴ Employment among single mothers increased dramatically from 61 percent in 1993 to 79 percent in 2000. There was no similar increase among women in either of two comparison groups - married mothers or single women without children. In 1993, single mothers had an employment rate that was 11 percentage points lower than that of single women without children; by 2005, their employment rate was 6 points higher.

Even though many of the policy changes were implemented at the national level, pre-existing characteristics of MSAs led to significant geographic variation in the extent to which this reform affected employment. Local labor markets whose welfare recipients represented a greater fraction of potential participants experienced larger increases in employment. As evidence of this relationship, consider Figure 2.2, constructed with the same annual CPS data as Figure 2.1. To create this figure, I rank each MSA based on the fraction of all low-skilled women receiving cash welfare from 1988 to 1992. I then select women from all years who live in MSAs that fall into the top or bottom quartile of this ranking.⁵ Although the levels are different, the time pattern of employment in both quartiles is quite similar prior to the mid-1990s. After that point, however, employment increases significantly for women in high participation cities. Employment among women in low participation cities, in contrast, is roughly flat. By the end of the decade the employment gap (on average eight to nine percentage points prior to reform) had essentially disappeared. Thus, the degree of welfare participation prior to reform reliably predicts increases in employment over the reform period.

Having established that welfare reform had differential effects on the employment

⁴As closely as possible, this set of cities matches the consistent geographic areas described in Section 2.4.

⁵ This figure takes the average over all women living in any city within a quartile. Each quartile contains 33 MSAs, and captures a roughly equal fraction of the sample population. A similar, though somewhat noisier pattern emerges when each MSA average contributes one observation to the quartile mean. The cutoffs for the bottom and top quintiles are around 5 and 10 percentage points, respectively.

rate of women by geographic area, it is worth examining whether these increases represent substantial labor market competition for newly-arriving low-skilled immigrants. Several descriptive factors suggest that this is the case. First, the population affected by welfare reform and the flow of new immigrants are of similar magnitudes. Welfare rolls fell by 2.3 million adults (from 3.8 to 1.5 million) between 1990 and 2000 (US Department of Health and Human Services 2007).⁶ Over that same time period 4.4 million low-skilled immigrants (male and female) entered the country (US Bureau of the Census 2007).

In addition, newly arriving immigrants and welfare leavers tend to work in similar jobs. Welch's (1999) index of congruence provides a useful measure of the degree of overlap in occupations and industries for various groups. This methodology has been used in the immigration literature to determine which groups of natives and immigrants should be considered close substitutes (e.g. Borjas 2003). The index is similar to a correlation coefficient and is calculated for two groups of workers k and l as

$$C_{kl} = \frac{\sum_j (p_{kj} - \bar{p}_j)(p_{lj} - \bar{p}_j)/\bar{p}_j}{\sqrt{\left(\sum_j (p_{kj} - \bar{p}_j)^2/\bar{p}_j\right) \left(\sum_j (p_{lj} - \bar{p}_j)^2/\bar{p}_j\right)}} \quad (2.1)$$

with p_{kj} and p_{lj} representing the fraction of the two groups working in occupation j and \bar{p}_j representing the fraction of all workers in that occupation. This index has a range from negative one to positive one. A value of positive one indicates complete occupational overlap. Whenever one group is overrepresented in an occupation the other group is similarly disproportionately likely to work in that same occupation. A value of negative one implies the opposite relationship. Values near zero mean that the two groups overlap about as much as each group overlaps with the average

⁶Note that these figures may understate the number of women affected by reform as the caseload reached a peak of 4.4 million adults in 1994.

worker.

Table 2.1 presents this index comparing single native women who are currently working but received welfare in the past year to eight other groups of workers based on gender, education and nativity. The data source and geographical selection criteria are the same used in the first two figures, although I only use years in which nativity questions were asked (post-1993). I classify both occupations and industries at the two-digit level.

These results suggest that women entering the labor force in response to welfare reform are very close substitutes for newly arriving female immigrants. The index for occupation is +0.73, while the index for industry is +0.54. This is the highest degree of overlap among all groups in occupation and second only to other low-skilled native women in industry (+0.76). Although these results reveal significant occupational sorting by gender, low-skilled immigrant men compete with welfare leavers about as much as the average worker (industry and occupation values of -0.03 and -0.11 respectively). Overall, increases in labor supply among welfare leavers represent significant competition to newly arriving immigrants (and vice versa).

In addition to the supply shock due to welfare reform, federal programs that encouraged employers to hire welfare leavers potentially increased demand for welfare recipients relative to those of other low-skilled workers, such as less-skilled native men and immigrants. The Welfare to Work Partnership, formed by the Clinton administration, recruited businesses to pledge to hire welfare leavers in entry level jobs. Charter members included United Airlines, Burger King, and United Parcel Service, companies in industries common for low-skilled immigrants. The Welfare to Work Tax Credit refunded a portion of the earnings employers paid to recent welfare leavers, with maximum credits over \$8000 for hiring a “long-term” welfare recipient.

Hiring a newly arrived immigrant would provide neither the public relations boost nor the wage subsidy that came with hiring a recent welfare recipient.

If immigrants avoid labor markets with larger numbers of new native job seekers, this fact could explain outstanding questions in the literature. For example, Card and Lewis (2005) document the diffusion of newly arriving Mexican immigrants over the 1990s. In contrast to prior cohorts, these immigrants tended not to cluster as tightly in traditional locations (i.e. California and Texas) and instead located in several cities that had previously seen very few Mexican immigrants. The cities that they identify as having the largest “surprising” inflows of Mexicans tend to be cities where there were smaller relative numbers of welfare recipients.⁷ Additionally, there is little evidence that welfare leavers had any detrimental effect on the employment and wages of other potential labor market substitutes, such as low-skilled men (see, for example, Blank and Gelbach 2006). These somewhat surprising results are consistent with immigrants intentionally locating in areas with fewer women leaving the welfare rolls for employment.

One final concern with the plausibility of the labor market competition explanation is the degree to which newly arriving immigrants are informed about the labor market opportunities awaiting them in the US. McLaren (2006) demonstrates that border apprehensions are a reliable leading indicator of US economic growth, implying that undocumented immigrants have access to information about the labor market and do not undertake the risky venture of crossing the border unless they are reasonably confident that they will find work. Selection across geography requires only a minor extension to this model where potential immigrants have network contacts in different cities, each providing this type of information.

⁷The cities mentioned in the text as surprising are Houston, Dallas, Atlanta, Phoenix, Las Vegas, New York, Denver, Portland, Salt Lake City, Washington, DC, Seattle, Raleigh-Durham, Greensboro and Charlotte. Only New York, Seattle and Raleigh-Durham had pre-reform participation rates above the median.

Taken together, this descriptive evidence suggests that welfare leavers represent a substantial exogenous increase in labor market competition for newly arriving low-skilled immigrants. Further, geographic variation in pre-reform welfare participation induces substantial differences in the degree to which local labor markets were affected. These facts lead to a clear hypothesis: if immigrants are sufficiently sensitive to geographic variation in the relative demand for their labor, newly arriving immigrants will tend to select locations with fewer women entering the labor market as a result of welfare reform. The next section provides a theoretical model of location choice, and suggests empirical specifications that can be used to test this hypothesis.

2.3 Empirical Model

This section provides a theoretical framework for understanding how differential labor supply shifts across the country affect newly arriving immigrants' location choices. Suppose that each metropolitan area in the United States offers an immigrant a level of utility from settling there, U_{isdt} . In this notation, i indexes individuals, s indexes the source region, d denotes destination MSAs and t indexes time periods (census decades).⁸ This setup is a standard discrete choice problem, and utility maximization simply requires that an immigrant select the location that provides the highest level of utility. The decision rule can be expressed in a straightforward way: an immigrant chooses to move to location j if and only if $U_{isjt} > U_{isdt} \quad \forall d \neq j$.

In the context of this model, the hypothesis that immigrants select locations that offer better labor market prospects means that natives' labor market attachment will be an important component of the utility an immigrant receives from each city.

Locations with fewer natives willing to work at any given wage will offer better

⁸Because the data I use do not provide information on potential immigrants who choose not to migrate, I model only the decision of where to locate conditional on deciding to move to the US. This simplification implicitly assumes that the relative utility of cities within the US is independent of the value of remaining at home.

employment and wage prospects, and thus immigrants will be more likely to select these cities. The alternative hypothesis contends that the value of other factors, such as the location of networks, is so large that differences in employment prospects are unlikely to determine which location an immigrant ranks the highest. From the researcher’s perspective, however, it is not possible to measure the utility of each location for each individual, so one cannot directly investigate the effect native supply shocks on utility. Instead, we can observe some attributes of each potential choice as well as the location each immigrant selected. We can model the utility levels as a common function of these observed attributes and an individual-specific unobserved component. Suppose that the observable portion of the utility function can be represented as a linear combination of the location’s attributes. Then the total utility of a location with observed characteristics X_{sdt} is given by

$$U_{isdt} = X_{sdt}\beta + u_{isdt}. \quad (2.2)$$

In order to estimate the parameters of this model, one needs to make an assumption about the distribution of the error terms. McFadden (1974) demonstrated that if each u_{isdt} is independently and identically distributed Type I extreme value, β can be estimated consistently by running maximum likelihood conditional logit models on the individual-level data. This approach is commonly adopted by other authors investigating the location choices of new immigrants (e.g. Bartel 1989, Kaushal 2005, Jaeger 2007). Yet, the required assumption almost surely fails. In particular, there are most likely unobserved city attributes that have similar value to all immigrants from the same source region ($u_{isdt} = \eta_{sdt} + \epsilon_{isdt}$). Maintaining the assumptions required for conditional logit estimation thus creates two problems. First, assuming i.i.d. errors in the presence of these grouped unobserved compo-

nents will vastly understate the standard errors and lead to faulty inference. More importantly, if these unobserved factors are correlated with the observed attributes, estimates of the value of these attributes will be inconsistent.

My empirical approach improves upon previous studies by explicitly allowing for these unobserved components of the error term and taking steps to remove their influence on the parameter estimates. I adopt a similar methodology to the one presented in Scanlon, Chernew, McLaughlin and Solon (2002), and my exposition of the econometric model closely follows theirs. I first derive an expression relating the share of immigrants selecting a particular city to the observed and unobserved components of utility in any given time period. I then demonstrate how using data on immigrants' choices from multiple time periods can net out the influence of any unobserved factors that are constant over time. Finally, I discuss how an instrumental variables approach allows for consistent estimation of the effect of native competition on immigrants' location decisions even in the presence of time-varying unobserved attributes that are correlated with native employment.

Allowing for common unobserved city attributes yields a new representation of the utility offered by a city

$$U_{isdt} = X_{sdt}\beta + \eta_{sdt} + \epsilon_{isdt}. \quad (2.3)$$

Note that this general framework nests the possibility that $\eta_{sdt} = \eta_{dt} \quad \forall s$, i.e. the city-specific attributes have similar value to immigrants from all source regions. I will estimate models under both assumptions, but I use the most general form for exposition. In order to estimate β , I make the much less restrictive assumption that the ϵ_{isdt} are distributed i.i.d. Type I extreme value. In other words, conditional on the observed attributes *and* any common omitted factors, the remaining individual-level errors are well-behaved. Given this assumption, the probability that an immigrant

selects a given destination in time period t is

$$\pi_{sdt} = \frac{e^{X_{sdt}\beta + \eta_{sdt}}}{D_{st}}. \quad (2.4)$$

with

$$D_{st} = \sum_j e^{X_{sjt}\beta + \eta_{sjt}}. \quad (2.5)$$

This expression closely parallels the probability arising in a conditional logit model with the addition of the unobserved group effects in both the numerator and denominator. In expectation, the share of immigrants selecting each destination will be equal to these choice probabilities. In practice, the observed shares will differ from the actual choice probabilities due to random sampling error. Let S_{sdt} represent the observed share of immigrants from source s selection location d in year t . Then

$$S_{sdt} = \pi_{sdt} + \nu_{sdt} \quad (2.6)$$

$$S_{sdt} = \frac{e^{X_{sdt}\beta + \eta_{sdt}}}{D_{st}} + \nu_{sdt} \quad (2.7)$$

Here ν_{sdt} is a mean-zero error term with variance that is inversely proportional to the number of observations within an st cell. Taking logs of both sides yields

$$\ln(S_{sdt}) = \ln(e^{X_{sdt}\beta + \eta_{sdt}} + D_{st}\nu_{sdt}) - \ln(D_{st}). \quad (2.8)$$

Taking a first-order Taylor Series approximation around $\nu_{sdt} = 0$ gives

$$\ln(S_{sdt}) \approx X_{sdt}\beta + \eta_{sdt} - \ln(D_{st}) + \frac{\nu_{sdt}}{\pi_{sdt}}. \quad (2.9)$$

This equation demonstrates that an appropriately transformed version of the share of immigrants selecting a city will be approximately linear in the observed and un-

observed attributes. If η_{sdt} were uncorrelated with native labor supply, β could be estimated consistently directly from this cross-sectional specification, but this is unlikely to be the case. More likely, some of these unobserved city attributes that are attractive to immigrants will be correlated with native employment. To the extent that many of these city attributes are fixed over time, using data from multiple time periods can overcome part of this difficulty. The grouped error components can be partitioned into factors fixed over time γ_{sd} and factors specific to each time period ψ_{sdt} .

$$\eta_{sdt} = \gamma_{sd} + \psi_{sdt} \quad (2.10)$$

Taking time differences of equation (2.9) eliminates the γ_{sd} term.

$$\Delta \ln(S_{sd}) \approx (\Delta X_{sd})\beta - \Delta \ln(D_s) + \Delta \psi_{sd} + \Delta \frac{\nu_{sd}}{\pi_{sd}} \quad (2.11)$$

Even after taking time differences, however, OLS estimates of the effect of a change in native employment on the change in immigrant share are likely to be biased. These changes most likely do not represent exogenous changes in the labor market competition new immigrants will face. For example, increases in demand for low-skilled labor will increase native employment while at the same time attracting more immigrants, biasing the coefficient upward.

I instead estimate equation (2.11) by instrumental variables, using the welfare participation rate prior to reform as the excluded instrument. I include source-specific intercepts to account for the $\Delta \ln(D_s)$ terms. Notice that in this specification I cannot estimate the effect of attributes of a destination or source-destination pair that are fixed over time, including factors commonly considered such as distance and climate differences. Additionally, parameter estimates for attributes with little

variation over time would not be well-identified. Many other covariates used routinely in the literature fall into this latter category, including the location of previously-arriving immigrants and the geographic distribution of potential ethnic group-based network contacts. The inability to include these variables should not be considered a limitation of the model. Instead, the primary advantage of this approach is that it removes the influence of any unobserved aspect of a destination or source-destination pair that is roughly constant across time.

I have motivated this estimation procedure as the appropriate methodology under the assumptions of a particular discrete choice model. It is worth noting, however, that previous work has used a very similar reduced-form specification even without this structural derivation. Borjas (2001) used the ratio of the share of newly arriving immigrants to the share of earlier arriving immigrants as the dependent variable in his analysis of whether new immigrants respond to state differences in wages. Both his dependent variable and the one suggested by the discrete choice model roughly represent proportional differences in cities' immigrant share. Thus, even if the assumptions underlying this particular discrete choice model are violated, my chosen specification is very comparable with previous work. Yet there are distinct advantages to the approach I use. First, if the assumptions of the discrete choice model hold, then the parameters I estimate have more than a reduced form interpretation. More importantly, the need for a source of exogenous variation in labor market opportunities does not depend on the underlying structural or reduced-form motivation. This paper represents an empirical advancement, therefore, as the first to provide a source of such variation and to exploit it using instrumental variables. The next section provides information on the data I use to estimate these models, and the following section reports the results.

2.4 Data

The five percent Public Use Microdata Samples of the 1980-2000 decennial censuses provide the majority of the data for the analysis.⁹ I consider the location of newly arriving adult immigrants ages 18-54, with at most a high school degree, not living in group quarters. I classify a respondent as an immigrant if he/she is foreign-born and is either a non-citizen or a naturalized citizen. New immigrants are those who arrived in the US during the ten years prior to survey.¹⁰ I restrict the analysis to immigrants from the eleven source regions listed in Table 2.2.¹¹ This table shows the distribution of sources across all three waves of the census. This distribution has remained somewhat stable over the sample period with two exceptions: immigration from Mexico increased, while immigration from European countries decreased.

Table 2.3 provides some basic descriptive statistics for this population. In each census year, the total number of new immigrants is split almost evenly between women and men. Most new immigrants are married and very few live alone as household heads. These variables are quite similar across the different waves of the census, suggesting that changes in the locations these immigrants choose are unlikely due to household composition changes.

I consider the 156 largest MSAs within the continental US with a nonzero immigrant population in all three census years as potential locations for newly-arriving immigrants, . These cities had an adult population (18-54) of at least 150,000 in 1990.¹² For the basic specification, I treat the η_{sdt} terms as constant across all source

⁹I obtained the data from the iPUMS project at the University of Minnesota Population Center <http://usa.ipums.org/usa/>

¹⁰These immigrants may have previously lived in the United States, but the census question asks when the respondent arrived in the US “to stay”.

¹¹This eliminates less than three percent of the sample.

¹²The geographic boundaries of the MSAs change somewhat across waves of the census. I follow Card and Lewis (2005) and use state and county group codes to create consistent areas across the three census years. Ethan Lewis graciously provided programs to do so.

regions. The dependent variable in these specifications is the natural logarithm of the share of all new immigrants living in each city, calculated separately for each census decade. I use person-level weights to calculate these shares, which I calculate separately by gender.

I then match these city-level shares with a number of locational attributes (listed in Table 2.4) immigrants may consider when deciding where to locate. The primary variable of interest is the native female employment rate: the fraction of all women working positive weeks over the past year. The excluded instrument in the IV specifications is the welfare participation rate: the fraction of all women who received positive welfare benefits during the year prior to the survey. In addition to other variables directly calculated from the PUMS, I include information from two external data sources. First I include decade averages of the annual growth rate in employment as measured in the County Business Patterns data from 1980-2000.¹³ This variable serves as a measure of the overall strength of the local labor market. Additionally, I have information on the welfare generosity of the state in which each MSA is located. PRWORA instituted a five-year waiting period for federally-funded welfare benefits for all immigrants arriving after the enactment of the law in August of 1996. Some states chose to use additional state funding to restore benefits to this group (the full list is available in Zimmermann and Tumlin (1999)). Although previous work has found that immigrants do not choose locations in order to take advantage of these differential benefit restorations (Kaushal 2005), I include these variables in some specifications for robustness.

Later specifications allow for the η terms to differ across each of the source regions listed in Table 2.2. For this set of results, I calculate the share of new immigrants

¹³I aggregate these data from the county level to match the consistent geographic boundaries.

in a city separately by source region. I eliminate from the entire panel any source-destination pair that contains no immigrants in any of the census years. The number of observations varies among source regions, and the share of immigrants selecting each city will be more precisely estimated for those regions with more observations. To address the resulting heteroskedasticity, I weight each source-destination pair by the square root of the total number of observations from each source country. Because native female employment and welfare participation only vary at the MSA level, I report standard errors clustered by MSA in all specifications with multiple observations per city.

2.5 Results

Figure 2.3 displays the first-stage and reduced form results of a basic instrumental variables version of Equation 2.11. Each city contributes one equally weighted observation. The left panel plots the data used to fit the first stage regression, along with the fitted values. As hypothesized, cities with higher welfare participation prior to reform experienced greater increases in native female employment over the decade. The second panel shows the reduced form, and provides evidence consistent with the selection hypothesis: Relative to immigrants arriving over the 1980s, female immigrants arriving during the 1990s were less likely to choose cities with large native populations entering the workforce as a result of welfare reform. Figures 2.4 and 2.5 show this relationship geographically. These maps demonstrate that the relationship is not driven by any particular area; instead, the relationship holds broadly across the entire country. In each figure darker areas represent MSAs with values above than the median, and lighter areas represent areas with values below the median. Areas of the country not included in large MSAs are represented as white. The negative

relationship is apparent when looking from map to map as cities turn from light to dark and vice versa.

The parameter estimates from this specification are given in the second column of Table 2.5. As expected given the figures, the first-stage is strongly significant (the F-statistic on the excluded instrument is well in excess of 10), and the resulting IV estimate is significantly negative. Interpreting the sign and statistical significance of these coefficients is straightforward. The magnitude can be interpreted as roughly the percentage change in the probability that an immigrant selects a given city.¹⁴ The coefficient in column 2 thus says that a city experiencing a one percentage point welfare-reform-induced increase in native female labor supply saw roughly a twenty percent decrease in the probability that a female immigrant chose to locate there. The magnitude of the change in the *level* of the choice probabilities depends on the base rate.

To contrast the IV results, the first column of the table shows the results from estimating this same equation without an instrument. This coefficient is significantly more positive, consistent with the hypothesis that omitted variables such as the overall strength of the labor market tend both to increase native employment and to attract newly arriving immigrants. This difference highlights the importance of using exogenous variation to determine the extent to which immigrants avoid competing with natives.

Figure 2.6 shows the results of repeating this set of regressions using data from one decade prior. Importantly, neither the first stage nor the reduced form relationships hold in a time period without a dramatic change to welfare policy (both point

¹⁴The percentage change in the choice probability resulting from a one unit change in the independent variable is $\frac{e^{\beta\Delta X}-1}{\Delta X}$, or approximately β for small changes in X . This interpretation provides the reduced form interpretation. Based on the discrete choice model, the change in the odds that a city is selected resulting from a one unit change in X is $p(1-p)\beta$. With small probabilities (the mean is 1/156), the difference between these two interpretations is minimal.

estimates are slightly negative, and neither is statistically significant). The lack of a first-stage relationship over this period rules out certain alternative hypotheses for the employment increases over the 1990s. For example, suppose high welfare participation were indicative of poor labor market conditions and that the subsequent increases in labor supply were the result of negatively serially correlated shocks. The first-stage results over the 1980s provide no support for this hypothesis. Similarly, the reduced form results rule out a pre-existing trend as an explanation for the changes in immigrant share seen over the period when the policy was implemented. This pair of results strengthens the credibility of interpreting the relationships shown in Figure 2.3 as resulting from immigrants avoiding labor market competition with welfare leavers.

The remainder of Table 2.5 adds additional control variables to help rule out alternative hypotheses. As a first alternative, suppose that high welfare participation cities also experienced larger general declines in job creation over the 1990s. In this case, these cities would have lost immigrant share even if immigrants did not react to the increases in native labor supply. Column 3 includes the change in the decade average annual employment growth rate as a means of controlling for this potentially omitted factor. This variable enters the model with the expected sign; the distribution of immigrants shifted away from cities with slowing employment growth and toward cities with improving growth. The parameter estimate for native female supply is not substantially affected, however, suggesting that changes in job growth cannot explain the relationship between pre-reform welfare participation and a city's change in immigrant share.

Alternatively, suppose that high participation cities also tended to be traditional locations for immigrants. If traditional locations became less popular for reasons

unrelated to welfare reform then these cities would have lost immigrant share even in the absence of the policy-driven labor supply increases. For example, the value of network contacts could have declined, or there could have been a simple secular diffusion of immigrants across the country over this time period for another unobserved reason. The specification in column 4 addresses this alternative hypothesis. An immigrant arriving in the 1990s faced a very similar geographic distribution of previously arriving immigrants as did an immigrant arriving in the 1980s. The coefficient on the fraction of a city that was foreign-born in 1990, therefore, gives a good estimate of the difference in the value these two groups of immigrants placed on going to a traditional location. The estimate provides support for the diffusion hypothesis as the immigrant share enters with a negative coefficient. Yet the coefficient on native employment remains negative and significant. In fact, even though the point estimate falls in magnitude, the inclusion of this variable increases the precision of the estimates substantially, yielding even stronger statistical significance.

The time differencing strategy effectively removes the influence of any unobserved city attributes that are fixed over time. Yet there may still be unobserved city-level shocks that are correlated with the reform-induced supply increases. One way to address this potential source of bias is to include a city's change in immigrant share among a group that should be less affected by welfare reform as a control variable. To accomplish this, I include the change in the city's share of female immigrants with at least some college education. Consistent with the presence of unobserved factors, this variable enters with a positive sign and strong significance (column 5), but the coefficient of interest remains strongly negative. Thus, any alternative explanation of the shift away from high welfare participation cities must explain the larger impact on low-skilled immigrants.

The final column addresses the so-called “welfare magnets” hypothesis. Previous research contends that states with more generous welfare benefits attract larger inflows of potentially eligible immigrants (Borjas 1999). Suppose that welfare reform essentially “turned off” these magnets, and as a result, cities in generous states were no longer especially attractive to immigrants. This direct policy effect offers an alternative explanation for the losses in immigrant share these cities experienced. To test this theory, in the last specification I include the maximum benefit level for a family of three in 1990. The positive coefficient on this variable is inconsistent with this alternative hypothesis. I also include dummy variables for whether a state restored each of four programs to post-reform legal immigrants using its own funds. The resulting coefficients are variable and mostly insignificant. These results are consistent with a previous study that found no effect of these policy choices on immigrants’ location choices (Kaushal 2005). With no evidence to support the welfare magnets hypothesis, the labor market competition explanation for these patterns becomes even more likely.

Table 2.6 repeats each of the previous specifications using changes in the log of the low-skilled male immigrant share as the dependent variable. The broad pattern is quite similar to the results for women - nearly all the variables enter with similar signs. Given the different degree of overlap in occupation and industry by gender seen in Table 2.1, one should expect that a given increase in employment among native women represents a greater increase in competition for female immigrants than for males. In the specifications with control variables, the point estimate of the negative effect of an increase in native female employment is somewhat smaller than for female immigrants, although the two coefficients cannot be statistically distinguished from each other.

There are several potential explanations for the similar coefficients for men and women. First, many male immigrants may make location decisions together with a spouse or other female family member. If the employment prospects are poor for one member, the entire unit may decide to go to an alternative location. Male immigrants considering marriage prospects may respond to the choices made by women even if they are not already connected as one household.

Second, although men and women on average work in different occupations, the expected male migration response to native welfare leavers depends on which jobs the marginal male immigrant is likely to take. Suppose, for example, that men first look for employment in traditional male jobs such as construction or agriculture work, and take service jobs only when these first two alternatives are unavailable. Given a sufficient number of these marginal male workers, a response similar in magnitude to female workers is not unreasonable.

Finally, if men are simply more elastic in deciding where to locate, the male response to any given supply shock will be larger. Even if welfare reform created smaller increases in competition for men, a larger proportional male response would tend to offset the difference in observed displacement. This difference in elasticity could occur if women depend more heavily on the existence of network contacts in deciding where to locate.

Table 2.7 presents a similar series of regressions for female immigrants using source-specific shares as the dependent variable. In this specification, each city may have as many as eleven observations, one for each source region identified in Table 2.2. In columns (4) through (6), I replace the generic immigrant concentration variable from Table 2.5 with a measure of whether the city was a traditional location for immigrants from the specific source region. The results through all specifications are

quite similar to the results from Table 2.5. Even controlling for unobserved source-specific city attributes, there is significant evidence that female immigrants chose cities with smaller native supply increases.

Table 2.8 lists the coefficient and standard error from running the specification in column 4 of Table 2.7 separately by source country. Although these coefficients are imprecisely measured, there is a great deal of heterogeneity in the magnitude of the point estimates. One noteworthy dimension of similarity is that the strongest results tend to be clustered in source regions whose migrants are most likely to be unauthorized. This pattern is not surprising, given the types of visa categories available to legal immigrants. Women entering the country on a family reunification visa are likely to settle wherever those family members are living, regardless of differences in employment prospects. Similarly, immigrants sponsored by employers are unlikely to respond to differences in generalized employment prospects because they already have an offer for employment in one particular area. An increase in native labor supply should have a muted effect on these populations. While the statistical imprecision does not allow for precise conclusions on the effects across different source countries, the pattern of the point estimates suggest that the most flexible populations were the most responsive to the labor supply shocks.

Table 2.9 presents a final specification check. The native supply increases created by welfare reform occurred primarily in the latter half of the decade. If the labor market competition explanation is correct, women arriving early in the decade should be less affected. To create this table, I estimate the specification from column 4 of Table 2.5 separately for three different groups. The first group consists of women who arrived prior to 1995 and who are currently living in the same MSA as in 1995. The second group includes all women who made their most recent location

decision after 1995. This group contains both immigrants arriving in the US after 1995 and early arrivers who have subsequently changed MSAs. The final group consists only of the movers subsample. Because I do not have access to a measure of native female employment changes at this five-year interval, I report the reduced form coefficients. For reference, the reduced form coefficient from Table 2.5, column (4) that uses women arriving over the entire sample period is -0.065. Again, the results are consistent with the labor market competition explanation as the location decisions of later arrivers and internal movers are much more negatively correlated with pre-reform welfare participation.

Taken as a whole, the results in this section provide strong support for interpreting the changing distribution of immigrants' locations as resulting from immigrants avoiding competing with natives. One final question concerns the extent to which these changing location patterns effectively "undid" the labor supply shocks created by welfare reform. Figure 2.7 presents a back of the envelope calculation in response to this question, based on the IV regression results in Table 2.5, column 4. On the x-axis is the predicted increase in native female labor supply based on the first stage regression, measured as a fraction of the low-skilled female population in 1990. The y-axis displays "extra" working female immigrants as predicted by the model, also measured as a fraction of the low-skilled female native population in 1990. The "extra" working immigrants variable is the difference between the predicted number of immigrants entering a city based using actual welfare participation rates and the predicted number who would have entered if all cities had the same participation rate (the mean).¹⁵ The slope of the linear regression line is -0.83. On average, when

¹⁵Specifically, this variable is $\frac{(e^{\widehat{\Delta \ln(S)}_{actual}} - e^{\widehat{\Delta \ln(S)}_{mean}})(S_{1990})(Imm_{2000})}{Pop_{1990}}$, where Imm_{2000} is the total number of new female immigrants who are in the labor force, $\widehat{\Delta \ln(S)}_{actual}$ is the fitted value from the regression, and $\widehat{\Delta \ln(S)}_{mean}$ is the fitted value replacing the actual participation rate with the mean, but leaving the other variables at their original values.

a city experiences a native supply increase equivalent to one percent of its previous workforce, immigrants equivalent to 0.83 percent of the previous workforce choose alternative locations. This calculation suggests that changing immigration patterns diffused most of the supply shocks created by welfare reform throughout the country.

2.6 Discussion and Implications for Further Research

This paper provides evidence that immigrants function as labor market arbitrageurs, differentially selecting areas with better employment prospects. Welfare reform substantially increased the labor market participation of previous recipients, and immigrants who are likely to compete with these new labor market entrants chose to locate in areas with less affected labor markets. Additional evidence helps rule out a number of alternative explanations for this pattern, including pre-existing trends away from these cities, concurrent demand increases, a secular decline in the value of traditional locations, and other unobserved shocks that affect immigrants of all skill levels similarly. After addressing each of these concerns, the data continue to provide support for a labor market competition interpretation of these changing location patterns.

This finding has important implications beyond the specific context examined in this study. Previous papers study the effect of immigration on the native wage structure using immigration flows as a source of exogenous changes to a city's skill distribution. Despite theoretical predictions to the contrary, these studies typically find very small differences between cities receiving large immigrant inflows and comparison cities. Critics note that these comparisons are not very informative in a general equilibrium context, i.e. when local labor markets are not closed. In particular, when a mobile production factor is willing to move across cities in search

of the highest return, geographic differences in the wage structure will tend to dissipate. Although some authors suggested that immigrants, as a self-selected group of mobile workers, might serve this function, no previous study had provided a direct test of whether immigrants respond to differences in labor market opportunities using a source of plausibly exogenous variation. Future research should continue to explore the extent of immigrants' mobility in response to other exogenous labor market shocks. If the conclusions of this study hold in general, it is likely that a new empirical strategy will be required to identify the precise effect of immigration on the native wage structure.

On the other hand, these results provide new evidence of an often overlooked gain from immigration. Immigrants selectively choosing locations can quickly diffuse labor market shocks across the country. Sufficiently large immigration flows can rapidly bring the labor market back to geographic equilibrium without more costly migration by natives. This benefit of immigration is seldom discussed in the policy debate, and future research providing an estimate of its magnitude would play an important role in determining the overall implications of this line of research for policy makers.

2.7 Figures and Tables

Figure 2.1: Employment Rates 1979-2005 Women, Age 18-54, HS Degree or Less

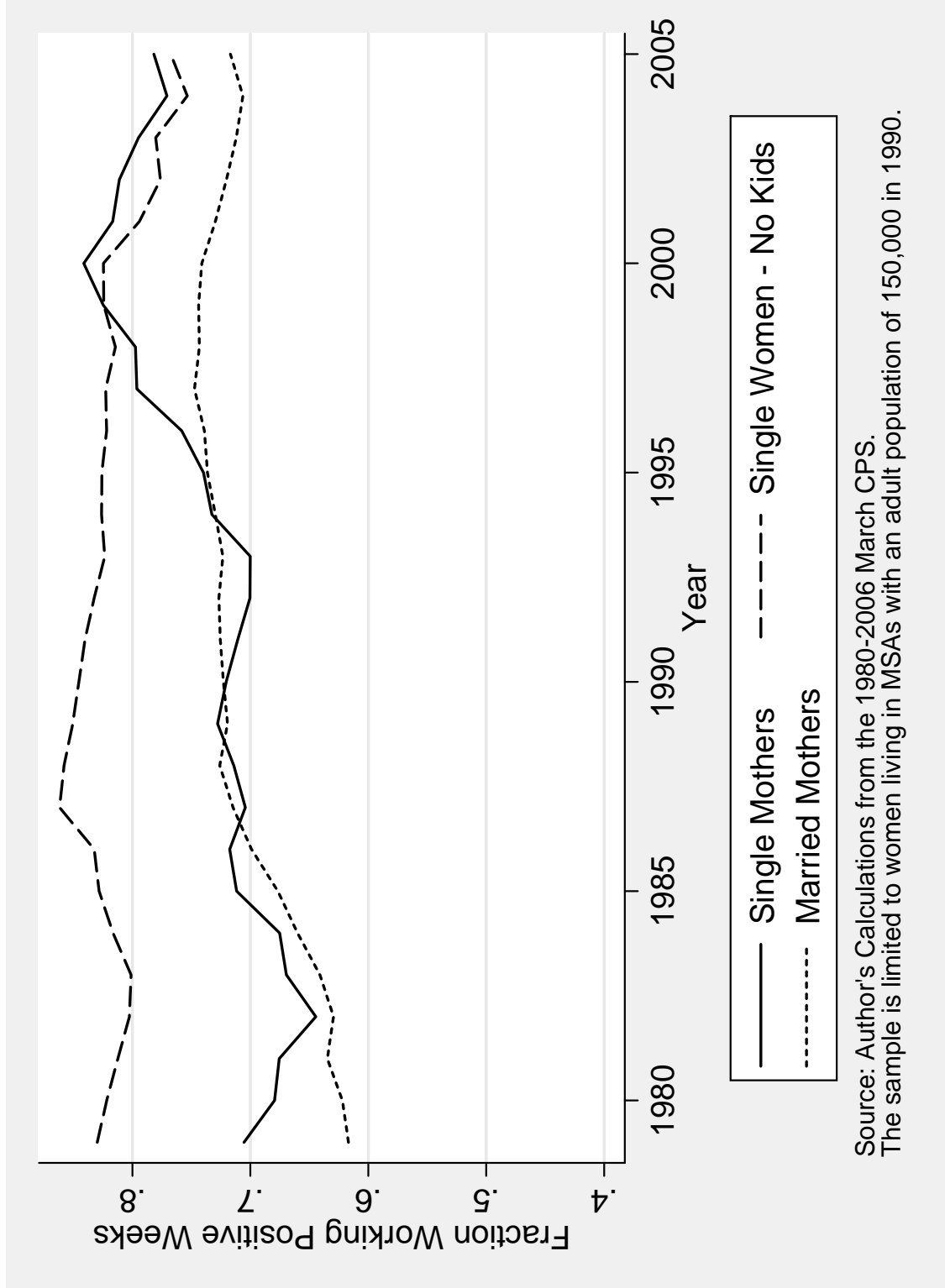
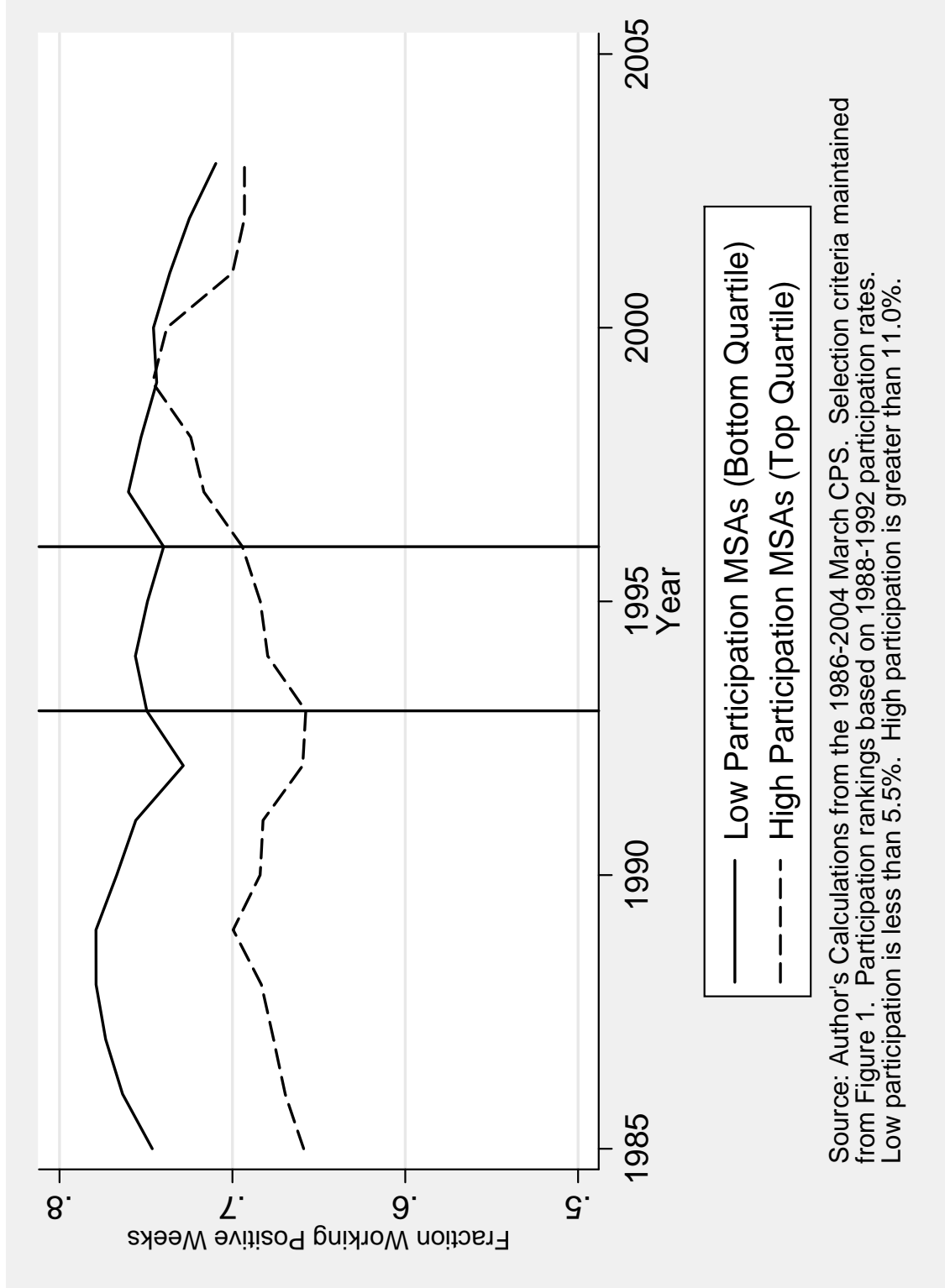
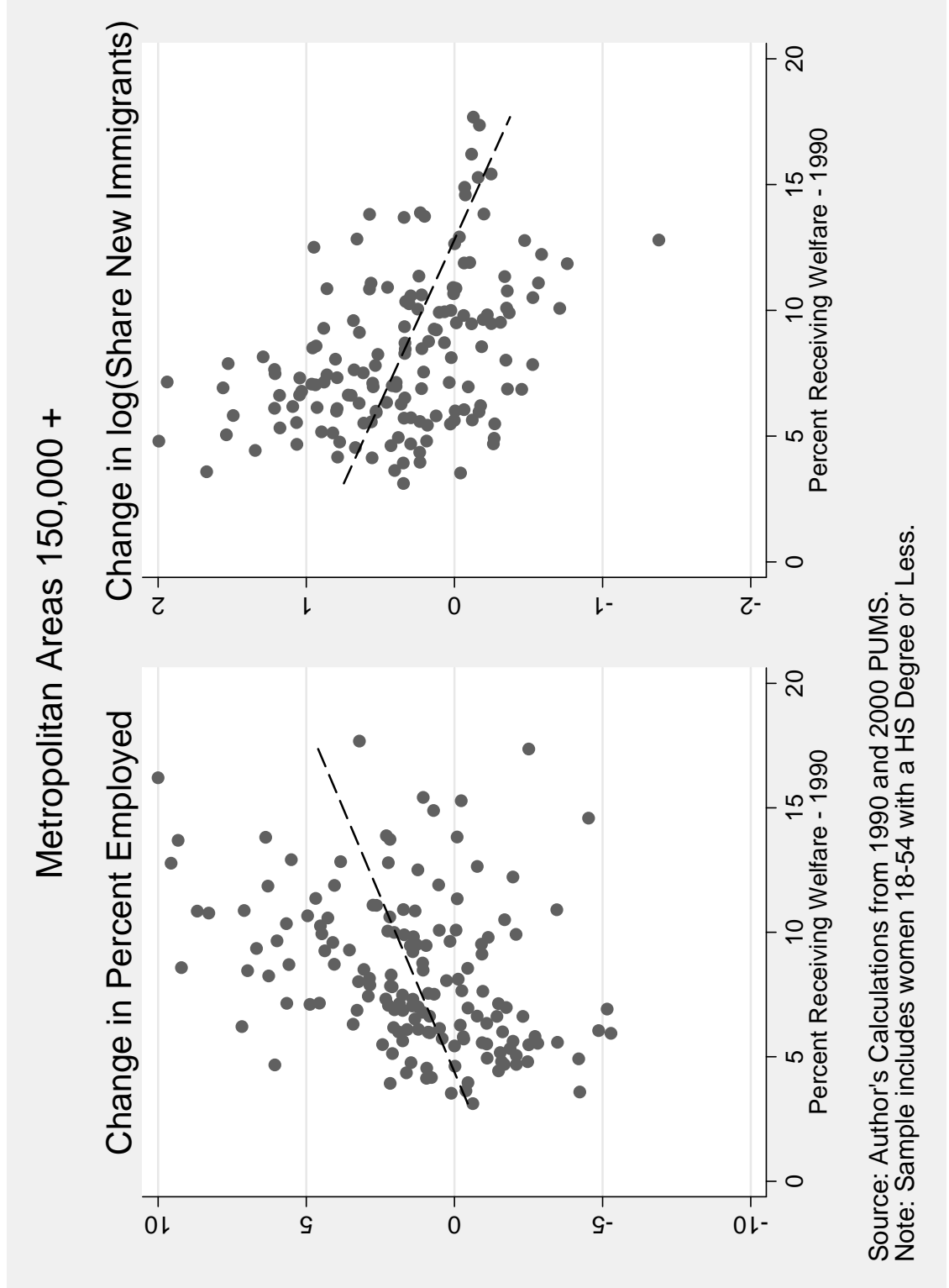


Figure 2.2: Employment Rates by Pre-Reform Welfare Participation 1979-2005 Women, Age 18-54, HS Degree or Less



Source: Author's Calculations from the 1986-2004 March CPS. Selection criteria maintained from Figure 1. Participation rankings based on 1988-1992 participation rates. Low participation is less than 5.5%. High participation is greater than 11.0%.

Figure 2.3: Changes in Native Female Employment and New Immigrant Locations 1990-2000
By 1990 Welfare Participation



Source: Author's Calculations from 1990 and 2000 PUMS.
Note: Sample includes women 18-54 with a HS Degree or Less.

Figure 2.4: Map of Metropolitan Areas by Fraction of Low-Skilled Female Population Using Welfare 1990

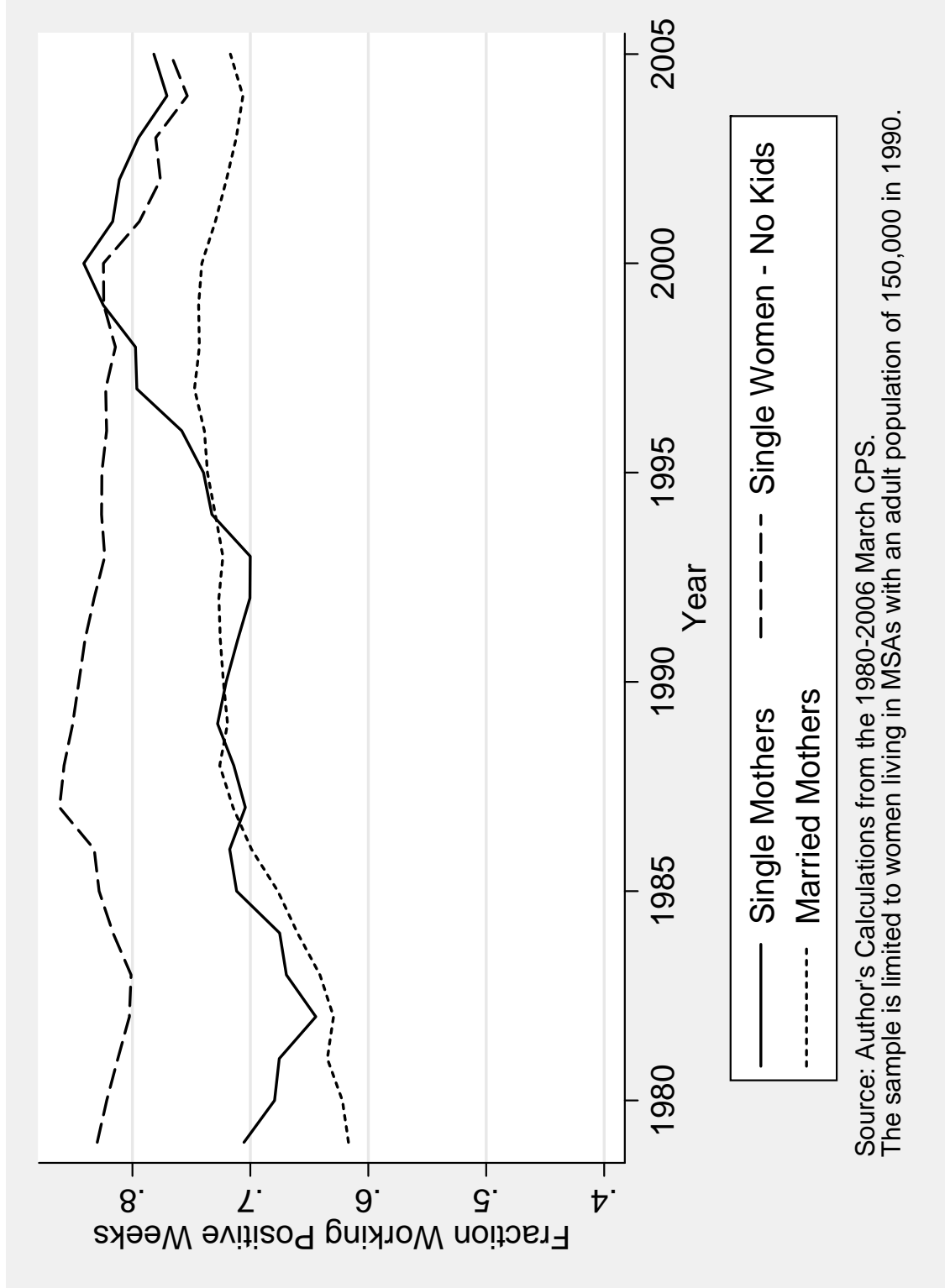


Figure 2.5: Map of Metropolitan Areas by Change in log(Low-Skilled Female Immigrant Share) 1990-2000

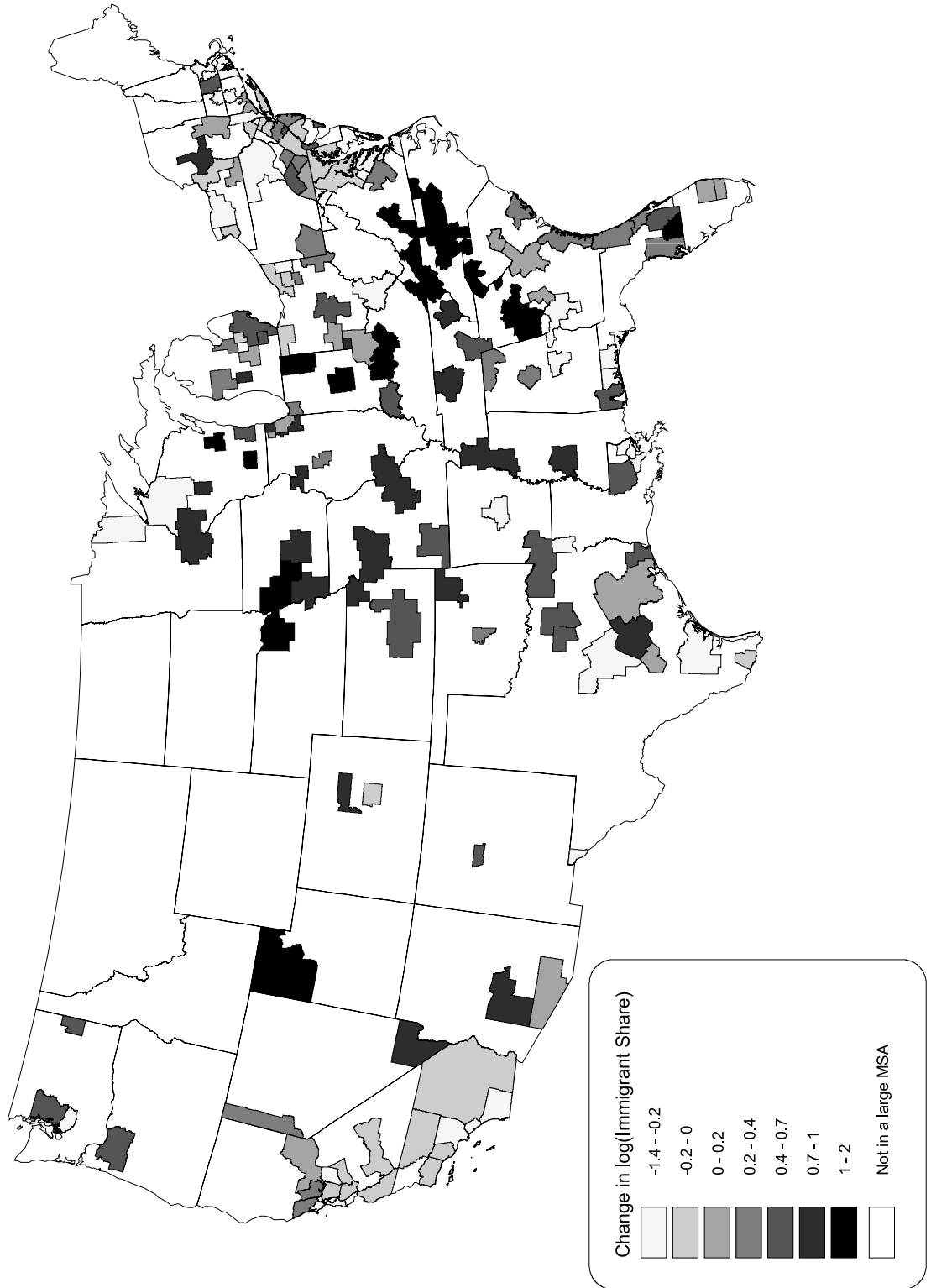
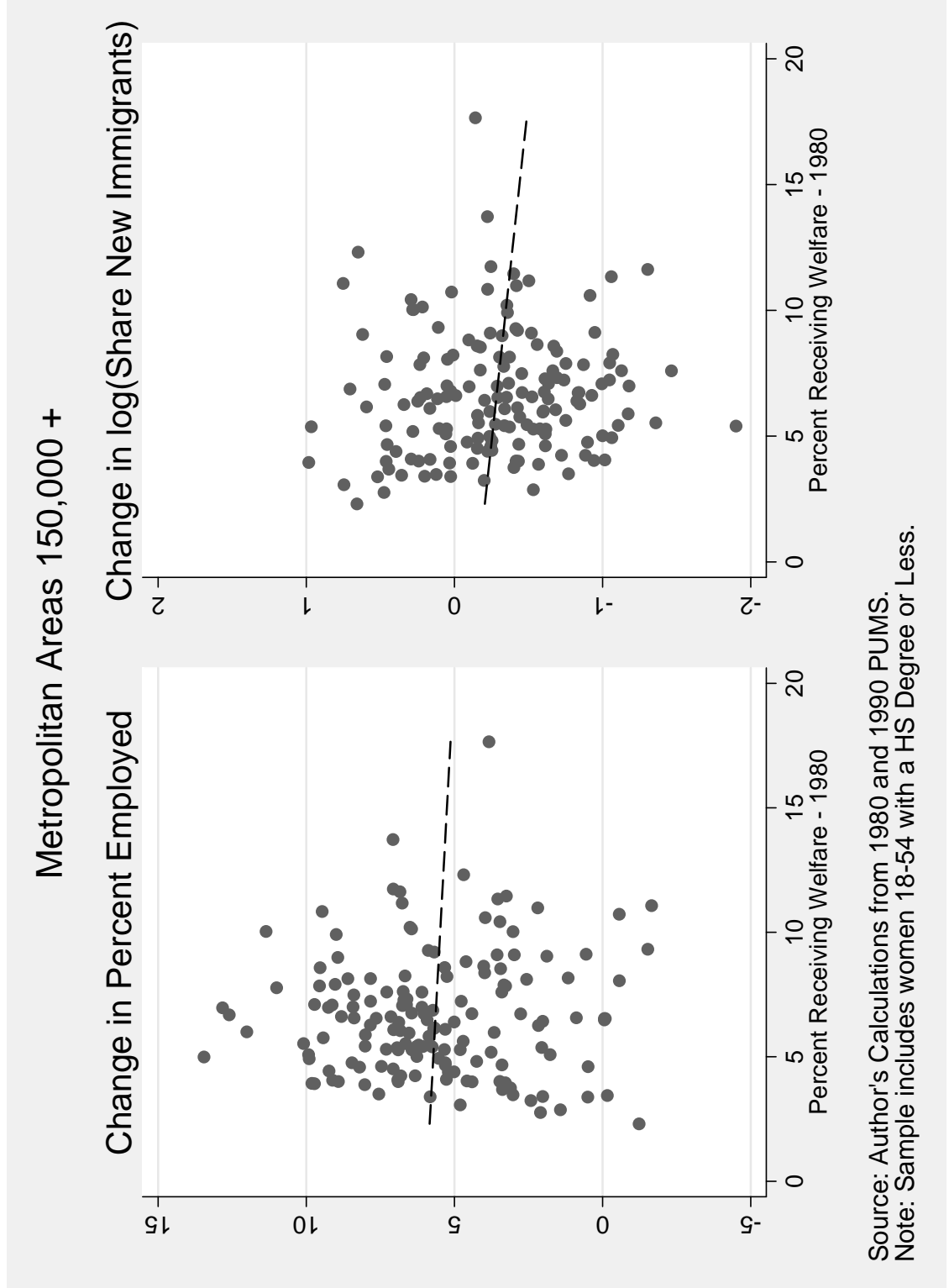


Figure 2.6: Changes in Native Female Employment and New Immigrant Locations 1980-1990
By 1980 Welfare Participation



Source: Author's Calculations from 1980 and 1990 PUMS.
Note: Sample includes women 18-54 with a HS Degree or Less.

Figure 2.7: Predicted Additional Immigrants Selecting a City By Predicted Natives Entering Labor Force

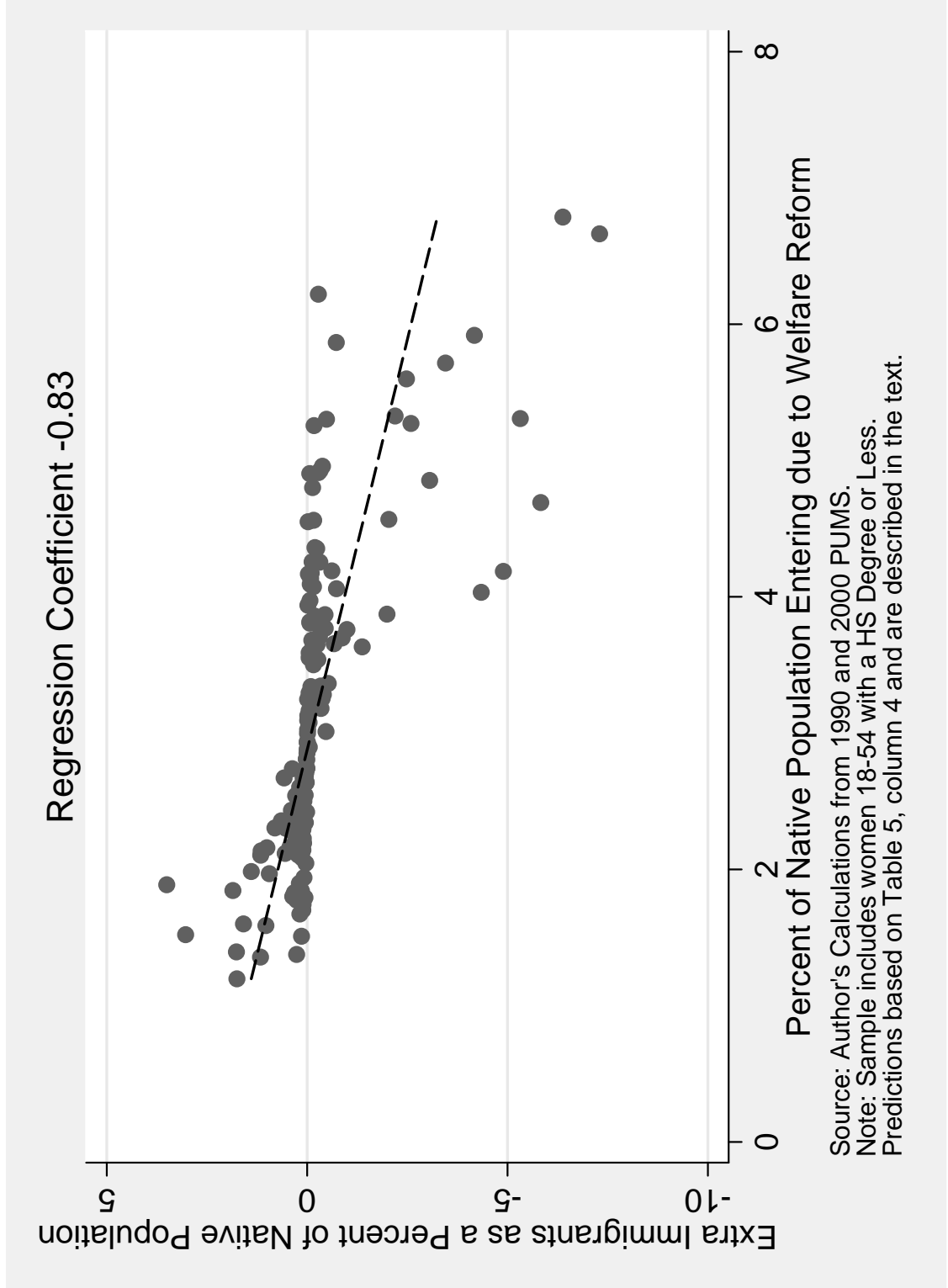


Table 2.1: Indices of Congruence in Industry and Occupation with Welfare Leavers

Group	Occupation	Industry
High Skilled Native Men	-0.58	-0.61
High Skilled Immigrant Men	-0.37	0.02
Low Skilled Native Men	-0.11	-0.25
Low Skilled Immigrant Men	-0.10	-0.03
High Skilled Native Women	-0.09	0.02
High Skilled Immigrant Women	0.43	0.31
Low Skilled Native Women - did not receive welfare	0.63	0.76
Low Skilled Immigrant Women	0.73	0.54
Low Skilled Single Native Women - received welfare	1.00	1.00

Source: Author's calculations from the 1994-2000 March CPS. The sample selection criteria are maintained from Figure 1. Definition of index of congruence found in the text. Two-digit industries and occupations used. The immigrant sample is restricted to persons arriving after 1990. Low Skilled means no more than a high school degree. High Skilled means at least some college. The index is equal to one by construction for the reference group.

Table 2.2: Distribution of Source Regions - New Immigrants 18-54, HS Degree or Less

	1980	1990	2000
Mexico	34.0%	36.6%	48.5%
Europe	16.0%	7.2%	7.2%
Caribbean	12.9%	11.6%	8.9%
Southeast Asia	8.4%	10.1%	6.2%
East Asia	7.9%	7.6%	4.5%
South America	6.7%	6.8%	6.7%
Central America	5.8%	13.5%	10.5%
Middle East	3.1%	1.6%	1.4%
Southwest Asia	2.8%	3.2%	3.1%
Africa	1.3%	1.3%	2.7%
Canada	1.2%	0.6%	0.4%
Observations	98,767	145,456	217,104
Total (Weighted)	1,975,340	3,168,171	4,819,553

Source: Author's Calculations from 1980-2000 PUMS. New immigrants are those arriving in the US less than ten years prior to the survey. Person-level census weights used.

Table 2.3: Descriptive Statistics of Immigrants Arriving Over the Previous Decade, Age 18-54, HS Degree or Less

	1980	
	Men (47.7%)	Women (52.3%)
Unmarried	39.6%	30.0%
No spouse in household	46.3%	33.8%
Household head, no spouse	10.8%	9.7%
Any children in household	45.2%	63.3%
No HS Degree	66.8%	63.1%
	1990	
	Men (52.4%)	Women (47.6%)
Unmarried	50.5%	38.8%
No spouse in household	61.6%	44.5%
Household head, no spouse	11.0%	9.8%
Any children in household	34.2%	56.5%
No HS Degree	58.6%	54.6%
	2000	
	Men (53.1%)	Women (46.9%)
Unmarried	51.9%	36.8%
No spouse in household	65.3%	43.1%
Household head, no spouse	10.6%	9.1%
Any children in household	31.2%	56.4%
No HS Degree	56.0%	51.7%

Source: Author's Calculations from the 1980-2000 PUMS. Sample selection criteria are the same as for Table 2. The numbers in parentheses give the percent of all immigrants who were of each gender. Person-level census weights used.

Table 2.4: MSA Characteristics 1980-2000

	1980		1990		2000	
	Mean	SD	Mean	SD	Mean	SD
Native Female Employment Rate, HS Degree or Less	66.75	5.81	72.36	5.96	73.71	5.57
Percent Receiving Welfare - Low Education Women	6.67	2.44	8.29	3.04	5.43	2.25
Average annual employment growth rate - over past decade	--	--	2.90	1.70	2.08	1.22
Immigrant Share of MSA Population, previous census	--	--	5.85	5.70	7.78	8.26
Maximum cash benefit, family of three (hundreds of dollars)	--	--	3.90	1.63	--	--
MSA in state that restored Food Stamp benefits	--	--	--	--	0.58	0.48
MSA in state that restored TANF benefits	--	--	--	--	0.36	0.47
MSA in state that restored Medicaid benefits	--	--	--	--	0.32	0.45
MSA in state that restored SSI benefits	--	--	--	--	0.15	0.36

Source: Author's Calculations from the 1980-2000 PUMS and 1980-2000 County Business Patterns. Selection criteria maintained from Table 2. Person-level census weights used.

Table 2.5: Percentage Change in Share of Female Immigrants Selecting a City

	OLS (1)	IV (2)	IV (3)	IV (4)	IV (5)	IV (6)
Change in Native Female Employment Rate	-0.019 (0.016)	-0.217** (0.066)	-0.241** (0.070)	-0.154** (0.034)	-0.122** (0.028)	-0.145** (0.035)
Change in decade average employment growth rate			0.258** (0.069)	0.124** (0.037)	0.090* (0.036)	0.126** (0.046)
Immigrant Share of MSA Population, 1990				-0.043** (0.006)	-0.034** (0.006)	-0.049** (0.010)
Change in log(Share of Immigrants - College Degree)					0.384* (0.164)	0.471** (0.164)
Maximum cash benefit, family of three						0.085* (0.043)
MSA in state that restored Food Stamp benefits						0.066 (0.112)
MSA in state that restored TANF benefits						-0.148 (0.110)
MSA in state that restored Medicaid benefits						-0.236 (0.131)
MSA in state that restored SSI benefits						0.523** (0.158)
Constant	0.375** (0.053)	0.650** (0.100)	0.893** (0.142)	1.000** (0.102)	0.772** (0.121)	0.619** (0.145)
Number of Cities	156	156	156	156	156	156
R-squared	0.01					
First Stage Coefficient		0.35	0.33	0.42	0.38	0.34
First Stage F-stat		13.86	14.23	33.43	22.57	18.43
Robust standard errors in parentheses						

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

Source: Authors Calculations from the 1990 and 2000 PUMS. Selection criteria maintained from Table 2. The excluded instrument is the female welfare participation rate in 1990. Immigrant shares are calculated using person-level weights. The regressions are unweighted.

Table 2.6: Percentage Change in Share of Male Immigrants Selecting a City

	OLS (1)	IV (2)	IV (3)	IV (4)	IV (5)	IV (6)
Change in Native Female Employment Rate	-0.021 (0.020)	-0.255** (0.090)	-0.279** (0.094)	-0.146** (0.038)	-0.097** (0.032)	-0.074* (0.031)
Change in decade average employment growth rate			0.289** (0.088)	0.085* (0.038)	0.040 (0.034)	-0.015 (0.043)
Immigrant Share of MSA Population, 1990				-0.066** (0.007)	-0.052** (0.008)	-0.040** (0.009)
Change in log(Share of Immigrants - College Degree)					0.552** (0.163)	0.547** (0.157)
Maximum cash benefit, family of three						-0.046 (0.046)
MSA in state that restored Food Stamp benefits						-0.343** (0.107)
MSA in state that restored TANF benefits						0.136 (0.127)
MSA in state that restored Medicaid benefits						-0.460** (0.149)
MSA in state that restored SSI benefits						0.408** (0.147)
Constant	0.608** (0.072)	0.928** (0.133)	1.201** (0.192)	1.366** (0.119)	1.051** (0.147)	1.297** (0.177)
Number of Cities	155	155	155	155	155	155
R-squared	0.01					
First Stage Coefficient		0.35	0.33	0.42	0.40	0.34
First Stage F-stat		13.57	14.09	33.21	24.68	18.66

Robust standard errors in parentheses

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

Source: Authors Calculations from the 1990 and 2000 PUMS. Selection criteria maintained from Table 2. The excluded instrument is the female welfare participation rate in 1990. Immigrant shares are calculated using person-level weights. The regressions are unweighted.

Table 2.7: Percentage Change in Share of Female Immigrants Selecting Cities. Source-Destination Differences.

	OLS (1)	IV (2)	IV (3)	IV (4)	IV (5)	IV (6)
Change in Native Employment Rate	0.006 (0.014)	-0.174* (0.075)	-0.179* (0.072)	-0.133* (0.053)	-0.105* (0.049)	-0.090 (0.053)
Change in average employment growth rate			0.179** (0.056)	0.127** (0.038)	0.086** (0.032)	0.068 (0.039)
Ethnic group members as pct of MSA pop - decade start				-0.073** (0.012)	-0.059** (0.010)	-0.051** (0.010)
Change in log(Share of Immigrants - College Degree)					0.274** (0.054)	0.253** (0.053)
Maximum cash benefit, family of three						0.053 (0.040)
MSA in state that restored Food Stamp benefits						-0.065 (0.115)
MSA in state that restored TANF benefits						0.061 (0.116)
MSA in state that restored Medicaid benefits						-0.041 (0.135)
MSA in state that restored SSI benefits						-0.461** (0.140)
Constant	-0.536** (0.090)	-0.384** (0.117)	-0.191 (0.132)	-0.253* (0.110)	-0.396** (0.111)	-0.534** (0.161)
Observations	1131	1131	1131	1131	961	961
R-squared	0.18					
First Stage Coefficient		0.28	0.28	0.30	0.29	0.32
First Stage F-stat		10.66	11.68	14.66	12.22	12.63
Standard Errors clustered by MSA in parentheses						
* significant at 5%, ** significant at 1%						

Source: Author's Calculations from 1980-2000 PUMS. Selection criteria maintained from Table 2. Destinations not selected by anyone from a source during one year are omitted from the entire sample. Pairs are weighted by the square root of the number of source observations nationwide.

Table 2.8: IV Coefficient on Change in Female Employment - By Source Region

	β	se	First Stage F-stat
Canada	-0.033	0.093	7.25
Mexico	-0.223	0.189	3.93
Central America	-0.324	0.161	8.48
Caribbean	-0.282	0.154	7.21
South America	0.021	0.082	9.34
Europe	-0.013	0.053	19.38
East Asia	-0.089	0.072	23.77
Southeast Asia	-0.124	0.069	11.58
Southwest Asia	0.008	0.066	14.47
Middle East	-0.141	0.107	11.07
Africa	-0.065	0.078	15.36

Note: The specification and sample selection criteria are identical to column (4) of Table 7.

Table 2.9: Reduced Form Estimates of Change in Selection Probability by Arrival Year

	β	se	First Stage F-stat
Canada	-0.033	0.093	7.25
Mexico	-0.223	0.189	3.93
Central America	-0.324	0.161	8.48
Caribbean	-0.282	0.154	7.21
South America	0.021	0.082	9.34
Europe	-0.013	0.053	19.38
East Asia	-0.089	0.072	23.77
Southeast Asia	-0.124	0.069	11.58
Southwest Asia	0.008	0.066	14.47
Middle East	-0.141	0.107	11.07
Africa	-0.065	0.078	15.36

Note: The specification and sample selection criteria are identical to column (4) of Table 7.

CHAPTER III

Newly Arriving Immigrants as Labor Market Arbitrageurs: Evidence from the Minimum Wage

3.1 Introduction

This paper contributes to a growing literature demonstrating that newly arriving immigrants select destinations within the United States based, in part, on differences in labor market conditions. This type of selection spreads out labor market shocks across the country and helps equalize labor market returns across geography. The existence of such a highly mobile, earnings-sensitive low-skilled population affects the way researchers should evaluate the effect of labor market policies and shocks. An often-used methodology compares outcomes in an affected city or state to similar areas unaffected by the policy. This methodology implicitly assumes that each labor market is closed, and thus that the unaffected market provides a good estimate of what would have occurred in the affected market in the absence of the policy. A large flow of earnings-sensitive immigrants, however, will tend to minimize the observed differences across geography. In the limit, sufficiently flexible labor market arbitrageurs would eliminate any predictable differences in labor market returns.

Yet most researchers do not directly take account of this possibility, perhaps because most previous attempts to determine directly the extent of immigrants' geographic selection fail to do so convincingly. Only one previous study uses iden-

tifiable exogenous variation in the earnings an immigrant can expect to earn across locations (Cadena 2008). Other studies are therefore subject to the criticism that immigrants' observed location patterns are simply a response to other unobserved locational characteristics that are correlated with favorable labor market outcomes (Borjas 2001, Jaeger 2007).

I overcome this challenge to identification by examining how the distribution of newly arriving immigrants' destinations responds to differences in states' minimum wage policy from 1994 to 2007. I begin by determining theoretically how changes in the minimum wage should affect the location immigrants prefer when they are searching for employment. I adopt a simple model with two labor markets (states), each with its own minimum wage. The model initially provides an ambiguous prediction for how immigrants should respond when one state increases its minimum wage. The effect of the policy change on expected earnings depends on the labor demand elasticity and the rate at which workers separate from their jobs. I measure the labor demand elasticity using much less mobile native teenagers and find that demand is roughly unit-elastic in the neighborhood of the observed minimum wage levels. This estimate, in combination with the model, results in a clear prediction that immigrants should prefer states with unchanged minimum wages.

The remainder of the empirical analysis then evaluates this prediction. I take advantage of variables providing information on nativity and year of arrival that were added to the core of the Current Population Survey (CPS) in 1994. This time period provides a particularly good opportunity to evaluate the effect of state policies. In response to a federal minimum wage that remained fixed in nominal terms for nearly a decade, a majority of states set higher minimums, and several enacted multiple increases. The resulting mix of policies provides significant geographic variation in

both the timing and the dollar amount of the minimum wage across multiple labor markets. This exceptional variation allows me to use an empirical framework that removes the influence of unobserved state characteristics, the general increase of the immigrant population, and state-specific trends in the immigrant population. I find strong, significant evidence in favor of the hypothesis that immigrants choose destinations that maximize expected earnings¹. A ten percent increase in a state's minimum wage leads to a seven percent decrease in the number of newly arriving immigrants who live in that state.

As a falsification test, I repeat the analysis using higher educated immigrants who generally command market wages significantly above any state's minimum wage. The location pattern of this group is uncorrelated with changes in states' minimum wage policies. The lack of a correlation for the high skilled immigrants supports interpreting the results for the low-skilled group as an optimizing response to changes in labor market conditions. Together, these findings support the idea that immigrants' endogenous location selection tends to minimize the difference in labor market outcomes across geography.

One literature in which this possibility features prominently is the empirical debate over the effect of immigration on native labor market outcomes. The potential for this type of selection is one of the primary criticisms of studies that rely on geography-based research designs to determine how the change in skill mix created by immigrants affects the wage distribution.² The extent of the selection document

¹Orrenius and Zavodny (forthcoming) study the effect of minimum wages on labor market outcomes for immigrants and conclude that immigrants experience smaller unemployment effects than do native workers. They suggest that immigrants moving away from states that increase their minimums may account for the different effects, and they present some suggestive evidence that this type of selection is occurring. In contrast to my analysis, however, their paper does not address the question of whether this type of behavior represents an optimizing choice for newly arriving immigrants. Nor do they explore the broader implications of such selection on local and national labor markets and on researchers' attempts to use geographic variation to evaluate the response to labor market policies and shocks.

²See Card (1990) and Card and Lewis (2005) for examples of studies that use this type of variation, and Borjas et al. (1997), Borjas (2003) and Borjas et al. (2006) for studies that allege this criticism.

in this paper casts doubt on the usefulness of geographic comparisons in determining the effect of immigration on the native wage distribution.

Additionally, this paper contributes to the local labor markets literature by providing new evidence that the geographic flexibility of low-skilled immigrants serves as an especially important mechanism in restoring geographic equilibrium following labor market shocks. This finding is especially important given the increased inequality that can result from the relatively low mobility rates of low-skilled natives (Bound and Holzer 2000).

The remainder of the paper is organized as follows: Section 3.2 presents the model with an emphasis on the empirical parameters necessary to form a prediction; Section 3.3 contains the empirical work, and Section 3.4 concludes and discusses policy implications.

3.2 Model

This section adopts the two-sector model of Mincer (1976) to determine whether a binding increase in one state's minimum wage will create incentives for newly arriving immigrants to flow toward or away from that state's labor market. The analysis assumes that this policy change will generate unemployment in addition to raising wages. These two changes in labor market conditions will have opposing effects on that state's desirability as a destination for newly arriving immigrants. The model demonstrates that predicting which state immigrants should prefer requires an estimate of two labor market parameters: the labor demand elasticity and the turnover rate. I estimate these parameters in the empirical section, and the results imply that an increase in the minimum wage will cause expected earnings for new entrants to fall, and thus immigrants will prefer destinations with a fixed minimum

wage.

3.2.1 Basic Model

The basic model begins with a fixed set of workers who inelastically supply one unit of labor in one of two states. Workers are free to move from state to state to maximize their expected earnings, but each worker must choose one and only one state in which to search for employment. Mincer originally motivated the model as describing the equilibrium wage and employment dynamics of covered and uncovered sectors within the same geographic area. The assumption that workers cannot simultaneously search for employment in both sectors is difficult to justify in this original setting and alternatives have been proposed (Brown 1999). In the present geographic context, however, this assumption more closely reflects reality as workers need to move to another state in order to search for employment covered by the higher minimum wage.³

A geographic equilibrium requires equal expected earnings for new entrants in both markets. If one state has a higher minimum wage, searching workers in that state must have a lower probability of finding employment. When differences in the probability perfectly offset differences in wages, no worker has an incentive to move. My analysis begins at such an initial equilibrium and then determines the resulting migration incentives when only one state increases its minimum wage.⁴ I begin by determining whether expected earnings would rise or fall in the state with the increased minimum wage if no workers were to move. This answer generates a straightforward migration prediction: workers will flow from the state with lower expected earnings toward the state with higher expected earnings. In doing so, they

³Todaro (1969) describes a similar dynamic in the context of a developing country where the jobs covered by the minimum wage are located in an urban area that is geographically distant from the rural uncovered jobs.

⁴This analysis differs slightly from Mincer's presentation as his assumed that the other market was entirely uncovered by any minimum wage.

alter the probabilities of finding employment in each state until the expected earnings equalize.

Formally, a worker's expected income is the probability of finding employment multiplied by the wage paid conditional on finding employment. The wage is simply the binding minimum wage in each state.

$$E[I] = p(w) \cdot w. \quad (3.1)$$

Here I denotes income, w is the minimum wage, and p is the probability of finding employment. I also adopt Mincer's (1976) version of the probability of finding employment. Job vacancies are created through exogenous separations. In each period, there are δE vacancies, where E denotes the number of employees firms demand at the minimum wage and δ represents the fraction of employees who lose their jobs in any period. The set of searching workers includes both those who are recently separated, and workers who were unemployed over the previous period, U . Because the minimum wage binds, there will be unemployed workers in the pool of job seekers. With more searching workers than vacancies, I assume that each job opening is randomly allocated. The resulting probability that a searching worker successfully finds a job is simply the ratio of vacancies to searching workers.

$$p(w) = \frac{\delta E}{\delta E + U}. \quad (3.2)$$

To determine whether a state's increase in the minimum wage will increase expected earnings for workers searching in that state, I take the derivative of expected earnings with the respect to the minimum wage and evaluate the resulting expression at the previous minimum.

$$\frac{\partial E[I^m]}{\partial w^m} = p(w_0^m) + w_0^m \left[\frac{\partial p}{\partial w^m} \Big|_{w^m = w_0^m} \right], \quad (3.3)$$

Here, the superscript m denotes the state that raised its minimum wage, and w_0^m represents the minimum wage prior to the increase.

When this derivative is positive, expected earnings increase and workers will have an incentive to migrate toward the state that increased its minimum. Workers will flow toward the other state when this expression is negative. The following proposition summarizes how the change in expected earnings depends on both the labor demand elasticity (η) and the turnover rate (δ).

Proposition III.1. *In the absence of mobility, and with inelastic labor supply, an increase in one state's minimum wage will increase expected earnings and attract workers from the other state whenever*

$$\delta \frac{E^m}{E^m + U^m} + \frac{U^m}{E^m + U^m} > \eta. \quad (3.4)$$

The proof involves simple algebraic manipulation of the derivative of expected earnings and is provided in the appendix for the interested reader.

This expression says that, for a given increase in the binding wage floor, labor markets with smaller demand elasticities and higher turnover rates are more likely to experience an increase in expected earnings, and thus to attract geographically mobile workers. The elasticity result fits well with intuition. A less elastic demand curve means a smaller fall in desired employment, leading both to a smaller decrease in the number of vacancies in each period and a smaller increase in the number of new unemployed workers joining the pool of searchers. Together, these effects result in a smaller decrease in the probability of finding employment.

The intuition for the separation rate is less straightforward. Conditional on the same changes in employment and unemployment, a higher separation rate leads to a larger decrease in vacancies but also results in a smaller increase in job seekers. The latter effect always dominates, however, and the interested reader is directed to a formal proof in the appendix (Section 3.6.2).

Figure 3.1 provides a graphical summary of how these two parameters jointly determine the effect on expected wages. The upward sloping line shows the values of η and δ such that expected earnings in the state with the increased minimum wage are unchanged. Along this line, no workers need to move in order to restore geographic equilibrium. For parameter pairs above the line, equilibrium requires that workers flow away from the state with the higher minimum wage. The opposite flow is necessary for parameter pairs below the line. This figure is drawn for a particular value of pre-change unemployment. A larger unemployment level in the initial equilibrium would increase the intercept and decrease the slope. The point $(\delta = 1, \eta = 1)$ will always lie on the line.

This general result nests the particular situation examined by Mincer (1976) when neither sector has a minimum wage in the initial equilibrium. In that case, there is no unemployment prior to the implementation of the minimum wage, and workers will flow to the minimum wage state whenever $\delta > \eta$. Also of note, when turnover is complete in every period ($\delta = 1$), workers will be attracted to an increase in the minimum wage whenever labor demand is inelastic. However, when workers currently holding jobs have a higher probability of employment in the next period than do unemployed workers, an increase in the minimum wage will only lead to inflows for a restricted range of elasticities less than one (in absolute value).

3.2.2 Possible Extensions

The preceding analysis was developed under a number of restrictive assumptions. Before proceeding to the empirical analysis, I briefly discuss how relaxing a few key assumptions would affect the relationship found in Equation 3.4. The basic search model I present has only one period. Introducing multiple periods could easily increase the importance immigrants place on the probability of finding employment beyond its role in expected earnings. If, for example, immigrants face liquidity constraints, failing to find employment quickly will have an especially high cost. Additionally, if immigrants intend to return home after a short spell of work abroad, they may be especially unwilling to risk a long period of unemployment. Each of these additional considerations would move the cutoff line from Figure 3.1 vertically down, resulting in a smaller set of parameters under which minimum wage increases will attract immigrants.

I have also assumed that the turnover rate is unaffected by the minimum wage. If an increase in the minimum wage reduced turnover, the effect of the minimum wage on the probability of finding employment would be more negative and the cutoff line would again shift vertically down.

Additionally, I have assumed an inelastic total labor supply and thus that an increase in the minimum wage does not induce any state residents to enter or exit the labor force. If workers instead have a range of reservation expected earnings levels, new workers already living in a state could join the labor force if expected earnings rose as a result of the minimum wage. Similarly, workers with the highest reservations wages could exit the labor force if expected earnings fell. Assuming that these workers respond more quickly than potential migrants from the other state, an elastic labor supply within a state would result in a larger set of parameters for

which no workers would want to move across state lines. The line of indifference in Figure 3.1 would be replaced by a larger zone of indifference surrounding the line above and below. Relaxing this assumption, however, will not reverse the sign of the prediction.

The above discussion has assumed that immigrants maximize earnings, rather than utility. Suppose that instead of supplying labor perfectly inelastically, immigrants value their leisure time. Then immigrants may actually prefer a state with lower expected earnings, if those earnings are accompanied by lower expected work effort. Thus, a given decrease in a state's employment probability will not have as large of an effect on that state's attractiveness as a destination. This modification would move the cutoff line vertically upward, increasing the range of elasticities for which a minimum wage increase makes a state more attractive.

The first portion of the empirical section reveals a near unit-elastic labor demand elasticity. Earnings maximizing immigrants should flow away from states that increase their minimum wage, although the prediction is somewhat less clear for immigrants who value leisure. Further analysis at the end of the section confirms that they respond in accordance with the earnings maximization prediction.

3.3 Empirical Analysis

This section begins by evaluating a key assumption from the model; I demonstrate that the minimum wage binds on the wages of a significant fraction of newly-arrived low-skilled immigrants. I then discuss how numerous policy changes enacted by the states during the period for which immigration status is available in the CPS (1994-2007) provide an excellent environment for estimating the effect of these policies. I first obtain an estimate of the labor demand elasticity in the minimum wage sector

by examining changes in native teen employment and wages in response to minimum wage increases. Teens are a fairly immobile group, and thus their results provide a rough estimate of the labor demand elasticity under no mobility. The estimated demand elasticity is sufficiently large to suggest that for any empirically reasonable rate of turnover immigrants should prefer locating states with unchanged minimum wages. I then provide direct evidence that immigrants alter their location decisions in accordance with the theory's prediction and test the robustness of this finding.

3.3.1 Immigrants' Wages Are Bound By the Minimum Wage

The model in the preceding section relies on the assumption of a binding minimum wage. Thus, the minimum wage must affect the wages of a significant fraction of newly arriving immigrants' wages in order for the theoretical predictions to have empirical relevance. Figure 3.2 displays kernel densities of wage distributions within a narrow window around minimum wage increases and provides straightforward evidence that this condition is satisfied.

To estimate these distributions, I select from each month of the CPS Outgoing Rotation Group (ORG) all hourly workers who live in a state with a minimum wage that will increase within six months or with a minimum wage that increased fewer than six months ago. I pool observations from all effective minimum wage changes – changes that increase the maximum of the state or federal minimum – from 1994 to 2007, the period over which nativity information is included. I then limit the sample to recently arriving immigrants (fewer than ten years in the US) and native teenagers (16-19). For each worker, I calculate the difference between the log of his/her hourly wage and the log of the new minimum hourly wage. I then estimate separate kernel densities of the distribution of this difference before and after the minimum wage

increase.⁵ In each panel, the solid line shows the distribution for workers in months prior to the minimum wage increase, and the dotted line represents workers in the six months following the increase.

The distributions change exactly as one would expect under a binding minimum wage. Comparing the new to the old distribution among newly arriving immigrants, there is a pronounced spike at the new minimum with “missing” density below the new minimum. While minimum wage jobs make up a smaller fraction of the immigrant wage distribution when compared to native teens, the magnitude of the spike created by the minimum wage is comparable. Figure 3.3 provides an additional point of reference and demonstrates that the minimum wage has only a modest effect on the wages of native adult workers without a high school degree.

It is clear from this analysis that the wages of many newly arriving immigrants are affected by the wage floor. In fact, the change in the immigrant wage distribution closely mirrors the results for the group on which most empirical research has focused. It is reasonable to expect, therefore, that immigrants will respond to any changes in expected earnings created by minimum wage policies.

3.3.2 State Minimum Wage Policies Provide Excellent Variation

The goal of the empirical analysis is to determine whether immigrants act as expected earnings maximizers in selecting destinations. In the ideal experimental context, a researcher would first observe both the probability of finding employment and the expected wage conditional on employment across a number of potential destinations. Then, one could randomly assign changes in both attributes to each destination and measure the resulting change in the geographic distribution of new immigrants.

⁵I use an Epanechnikov kernel and a bandwidth of 0.05 log points.

State minimum wage policies over the past fifteen years provide a sufficiently close approximation to this ideal context. As discussed in the model, minimum wage policies will, in general, manipulate expected earnings. Additionally, although minimum wage increases are not random events, sufficient variation both in the timing and in the magnitude of minimum wages will allow for an identification strategy that eliminates the influence of unobserved location characteristics that are fixed over time as well as characteristics that are changing similarly over time across all locations. Eliminating these potential confounding influences provides a closer approximation to the ideal experimental approach.

Figure 3.4 summarizes state and federal minimum wage policy from 1979-2007. The solid line displays the inflation-adjusted level of the federal minimum wage. The graph also displays the dollar amount and effective month of each new state minimum that is higher than the federal level, represented by the state's two-letter abbreviation. All wage levels are adjusted to December 2007 dollars using the CPI-U. Although data limitations preclude examining the effect of the minimum wage on immigrants' behavior prior to 1994, the figure reveals that most of the geographic variation in policies occurs when immigrants are identifiable. By the late 2000s, the range of minimums across geography rivals the time-series variation in the federal minimum between 1979 and the mid-1990s, the time period often studied in the canonical minimum wage and employment literature.⁶ Yet because each of these policy decisions was made at the state level, much less of this variation comes from the slow, predictable decline over time in the value of the federal minimum wage due to inflation. Using geographic variation is therefore less likely to attribute to the minimum wage changes in employment that are simply the result of unobserved

⁶Brown (1999) has a list of such studies on p. 2116.

gradual shifts in the nation's economy.

Table 3.1 provides additional evidence that these policies are varied and substantial. The first column displays the percentage of months between January 1994 and December 2007 that each state's minimum exceeded the federal minimum. The majority of states (31) had higher minimums than the federal level for at least part of the period. The second column shows the average gap between the state and federal minimums in months when the the state minimum was binding. These differences are sizable, with most 15 to 25 percent higher. The final column indicates the number of times the effective minimum wage increased in a state. Over this time period, there were a total of 212 increases in the effective minimum wage, 106 of which were created by state policy changes. The large number of changes allows for precise estimates of the effect of the minimum wage, even when using an estimation strategy that accounts for the influence of unobserved state attributes, overall time trends and state-specific time trends.

3.3.3 Estimation Strategy

My initial panel data specification is commonly used in the minimum wage and employment literature.

$$Y_{st} = \alpha_0 + \log(\text{RealMW}_{st})\beta_1 + X_{st}\gamma + \tau_t + \delta_s + \delta_s * t + \epsilon_{st}. \quad (3.5)$$

Observations are at the state-month level (s denotes states, t denotes states), and the cell means are calculated using CPS weights, while the regressions are unweighted.⁷ Standard errors are clustered at the state level, allowing for heteroskedasticity and state-level serial correlation of unknown form. X_{st} is a vector of time-

⁷Weighted versions of the regressions are quite similar to the unweighted versions, and none of the substantive conclusions are altered.

varying covariates, τ_t are time(month) dummies, δ_s are state dummy variables and $\delta_s * t$ are state specific linear time trends. I use this specification to estimate the effect of the minimum wage on a number of outcomes (Y_{st}), which are discussed in more detail below. In addition, Table 3.2 gives a complete description and descriptive statistics for each dependent variable and covariate.

The real minimum wage is measured monthly and is the maximum of either the state or federal minimum wage, unadjusted for coverage rates. I include the log of the state's average wage as a separate control variable, rather than using it to form a ratio with the minimum wage. Card and Krueger (1995, pp.208-239) provide compelling arguments that these choices are the preferred specification. A version of this specification figures prominently in the debate between Neumark and Wascher (1992) and Card, Katz and Krueger (1994). In contrast to these previous studies, I run this specification over a time period with much more variation in minimum wage policies (see Figure 3.4), and I run the analysis on the full set of monthly data rather than on annual or once per year monthly data (the May CPS supplement). Subsequent studies have also used this specification including Burkhauser, Couch and Wittenburg (2000). These authors argue against the use of time dummies, primarily because doing so eliminates the contribution of the federal minimum to the variation in the effective minimum wage thus eliminating most of the variation. They run auxiliary regressions of the minimum wage variable on several sets of dummy variables and find that, during the period they study (1979-1992), nearly 90 percent of the variation in the minimum wage can be explained by the year dummies alone whereas state and month dummies account for less than five percent of the variation. The analysis presented here uses policies from 1994-2007, and suffers much less from this concern. Year dummies can explain only fifteen percent of the variation, and the

inclusion of state, year and month dummies explains only 65 percent. Additionally, previous studies omit the state-specific time trends. In the discussion surrounding each specification, I discuss reasons to prefer the results that include the trends, although I present results both with and without them for completeness.

Recall that the model provided a potentially ambiguous prediction for whether an increase in the minimum wage will attract or repel immigrant workers. The direction of the effect depends on both the elasticity of labor demand and the turnover rate as shown in Figure 3.1. I first use this specification, therefore, to measure how the minimum wage affects employment and wage variables, which I then use to calculate an estimated labor demand elasticity.

To approximate the elasticity that would be observed without a migration response (as required by the model), I examine the effect of the minimum wage on a group of workers unlikely to move for employment: native teenagers (16-19). Table 3.3 tabulates data from the 2000 census to show the relative mobility of several demographic groups. Native teens and native adults without a high school degree are very unlikely to move across state lines. More than 90 percent of each of these two groups was living in the same state in 2000 as in 1995 (93 and 92 percent respectively). In addition, it is difficult to imagine a household relocating based on a teenager's employment prospects. In contrast, newly arriving immigrants are a much more mobile portion of the labor force. Newly arriving immigrants have, by definition, recently selected a new destination. Even among immigrants arriving more than five years ago, only 85 percent were living in the same state in 2000 as they were in 1995.

Thus, the employment and wage effects from the immobile native teenagers can provide a good estimate of how changes in the minimum wage affect the earnings the more mobile immigrants can expect across destinations. The measured elasticity is

sufficiently large to imply that earnings maximizing immigrants should prefer states with smaller increases or fixed minimums. Using the same specification, and thus the same portion of the policy variation, I demonstrate that minimum wage increases lead to immigrant outflows.

3.3.4 The Demand Elasticity Using Native Teens

To form an estimate of the demand elasticity, I estimate Equation 3.5 for a number of teen outcome variables, and the results are given in Tables 3.4-3.7. Table 3.4 provides the results from the most common dependent variable used in the minimum wage literature, the employment to population ratio. The primary parameter of interest is β_1 , the coefficient on the minimum wage variable. The first three columns give results for native teens, while columns 4-6 provide similar results for native adult high school dropouts. Column 1 includes only state and time dummies as controls. Column 2 adds controls and the third column adds the state-specific trends.

The trends are potentially quite important in the analysis of teen employment. Over the relevant time period, the participation of teens in the labor market fell dramatically (Aaronson, Park and Sullivan 2006). If this trend occurred at different rates in states with different minimum wage policies, an empirical specification lacking state-specific trends may erroneously attribute differences in employment to differences in minimum wage policies.

Across all specifications, the measured effect on employment is small. In my preferred specification (column 3), the estimated coefficient of -0.029, while statistically insignificant, implies that a ten percent increase in a state's minimum wage leads to a 0.0029 decrease in the teen employment to population ratio. With the mean of the dependent variable at 0.44, the implied elasticity is -0.06.⁸ Thus a ten percent

⁸Calculating this elasticity directly as a regression coefficient using the log of the employment to population ratio

increase in the minimum wage leads to a 0.6 percent decrease in the employment to population ratio. This estimate is slightly smaller than typical results using time series variation, although it is solidly within the widely varying estimates that come from panel data specifications (Brown 1999). Results for native adults are similarly clustered around zero.

At first blush, these results suggest that state minimum wage increases will attract immigrant workers, given the negligible measured effects on employment. However, using this outcome measure is unlikely to provide an appropriate estimate of the actual labor demand elasticity, a well-known limitation despite this variable's widespread use.⁹ First, not all teen workers' wages are affected by the minimum wage. As a result, this specification understates the employment decreases for the affected population and overstates the wage increases. Brown (1999, p. 1214-1215) discusses how these complementary measurement errors can easily lead to an estimated wage elasticity of employment that is five to nine times too low. In addition, employers are likely to adjust employment on both the intensive and extensive margins. Therefore, a more appropriate measure of employment is hours per teenager rather than the fraction of teenagers who are employed. Nevertheless, the conventionally measured employment effects in column 1 are fully consistent with conventional estimates, implying that there is nothing abnormal about this particular time period or policy environment. The next set of results use alternative dependent variables that allow for a better estimate of the actual labor demand elasticity in the neighborhood of implemented minimum wages.

Table 3.5 continues to use estimate versions of the specification in Equation 3.5.

as the dependent variable yields a similar result.

⁹Despite this limitation, this specification may provide an appropriate measurement of a policy relevant parameter. Policymakers often must decide whether to raise the minimum wage, and knowing that few workers will lose their jobs is likely of direct interest.

This table explicitly allows for adjustment by employers on both the intensive and extensive margin. The dependent variable is the log of the mean hours per worker, including zeros. Including workers with zero hours in these averages accounts for the fact that positions may disappear entirely as employers scale back in response to the minimum. These results show a larger elasticity for teenage workers across all specifications, with the largest results in the third column that includes state trends.

The coefficient of -0.230 on the log of the minimum wage in the third column implies that a ten percent increase in the minimum wage will lead to a 2.3 percent decrease in the average hours worked by teenagers. This hours elasticity reveals a much more substantial adjustment than the employment measure alone. Rather than cutting employees, employers respond to increases in the minimum wage by decreasing the hours of some employees.

The elasticities in Table 3.5 would correctly measure the labor demand elasticity if the minimum wage were always binding for all teen workers. Although there is a pronounced spike in the teen wage distribution at the minimum (see Figure 3.2), many teens command a market wage above the minimum while others can legally work below the minimum. Table 3.6 measures the extent of this discrepancy by regressing teens' hourly wages on the minimum wage. The coefficient of 0.213 (column 3) implies that a ten percent increase in the minimum wage leads to only a 2.13 percent increase in the average teen worker's hourly wage.

The hours and wage elasticities for teens are of roughly equal magnitude and opposite sign, suggesting that employers respond to changes in the price of teen labor by reducing total hours by enough to leave their wage bills unchanged. Table 3.7 demonstrates this fact directly using the log of average weekly earnings (including zeros) as the dependent variable. The point estimate is essentially zero across all

specifications, consistent with the hours and wages results. Taken together, the results for native teenagers imply a roughly unit-elastic labor demand curve.

These results are consistent with the findings from a previous study that used the alternative identification strategy of comparing year to year changes for workers at different points in the wage distribution around the time of minimum wage increases (Neumark, Schweitzer and Wascher 2004). That methodology also revealed opposing effects on wages and hours with a net result of either no change or a slight decrease in total earnings. That study included lagged levels of the minimum wage to allow for a delayed response to the change in wages. As a specification check, Table 3.8 repeats each of these regressions including the minimum wage variable lagged one quarter. I report results from specifications including the state-specific trends (column 3 of Tables 3.4-3.7). The “cumulative” effect, the sum of the coefficients on the contemporaneous and lagged terms, agrees nearly exactly with the results using only the contemporaneous measure.

The labor demand elasticity revealed by this analysis, therefore, is sufficiently large to eliminate most of the ambiguity from the model in Section 3.2. Figure 3.5 adds a reference line at $\eta = 1$ to Figure 3.1. The entire line lies in the range of parameters under which earnings-maximizing workers will flow away from states increasing their minimum wages in order to restore the geographic equilibrium across labor markets.¹⁰

¹⁰There is a slight disconnect between the hours-based elasticity and the η and δ framework used in the model that assumed unitary labor supply. One might argue that the expected earnings for a job seeker are roughly unchanged because most of the labor adjustment occurs on the intensive margin and average earnings are unchanged. As a result, minimum wage increases would not affect immigrants’ location choices. There are at least two plausible modifications to the model that would restore the sharp prediction even when total employment and average earnings are roughly unchanged by the minimum wage. First, if the increase in wages reduces churning by lowering the quit rate and thus the hiring rate, the probability of finding employment will fall in the state that increased its minimum wage. Alternatively, if the increase in hourly wage draws more workers into the labor force, the probability that a searcher will find employment will also fall. Additionally, the measured response by immigrants is sufficiently large to overcome this objection.

3.3.5 Minimum Wage Increases Lead to Immigrant Outflows

Table 3.10 presents regression results demonstrating that newly arriving immigrants respond in exactly this way. The dependent variable in each regression is the log of the number of newly arriving (within the past ten years) immigrants without a HS degree living in a state in a given month.¹¹ These are the same sample criteria used to create Figure 3.2, which demonstrated that the minimum wage was binding on a significant share of this group. The regression specification in the first column omits covariates but includes all of the state and time dummies. Subsequent columns add state-specific trends and additional controls.

The dummies and state-specific trends are essential to this analysis because of the role they play in creating the counterfactual. The state dummies remove the influence of any unobserved fixed state attributes that affect which destination newly arriving immigrants select. The observed negative relationship, therefore, is not simply driven by higher minimum wage states lacking other amenities immigrants value. Similarly, the time dummies take account of the fact that successively larger cohorts of immigrants were arriving over this time period.

The state trends are especially important in this specification in light of broader patterns in immigrant settlement during this time period. Over the 1990s, the newly arriving immigrant population became much less concentrated among traditional destination states and cities compared to previous waves (Card and Lewis 2005). To the extent that this diffusion occurred for reasons unrelated to policy-driven differences in labor market prospects, this pattern presents an empirical challenge to the analysis. Without the inclusion of state trends, one might be concerned that the estimated negative relationship resulted simply from traditional destination

¹¹These counts are weighted sums using the census-provided weights.

states implementing earlier and larger increases in the minimum wage. In fact, the specification in column 1 that omits state trends provides a substantially more negative estimate of the effect of the minimum wage. Instead, the inclusion of state trends ensures that the measured outflow of immigrants in response to a minimum wage increase represents a deviation from the general increase or decrease in a state's popularity over time.

The results that include state trends are clustered around -0.7 and measure the elasticity of the newly arrived immigrant population with respect to the minimum wage. These estimates are strongly statistically significant, even after allowing for heteroskedasticity and serial correlation within state panels of unknown form ($p < 0.05$). The elasticity implies that when a state's minimum wage increases by ten percent, its newly arriving immigrant population falls by seven percent relative to the counterfactual. Here the counterfactual is the level that would have been predicted using the state's average new immigrant population, the overall growth of the new immigrant population over time and the state's specific trend in the newly arriving immigrant population.

3.3.6 Falsification Test Using Higher-Education Immigrants

Despite the inclusion of these dummies and trends to remove the influence of unobserved factors, one might still be concerned that these geographic shifts in the immigrant population would have occurred even in the absence of the differential labor market prospects created by the minimum wage. I address this concern by repeating the analysis using immigrants who have at least some college education. These immigrants should, in general, command market wages above the minimum wage, and thus the minimum wage should have a minimal effect on their expected earnings. Figure 3.6 repeats Figure 3.2 for this group, displaying wage distributions

before and after minimum wage increases. In contrast to the wage distributions of low-skilled immigrants, these results do not reveal any spike at zero to suggest that the minimum wage binds. The wage distribution does shift slightly to the right in the range just about the new minimum, opening the possibility that wage increases among lower-paid workers may result in some small spillover wage increases for higher-paid workers. Nevertheless, if the results in Table 3.10 truly reflect immigrants responding to the labor market incentives created by the minimum wage, the geographic distribution of higher educated immigrants should respond to a much smaller degree. If instead the negative correlation for low-skilled immigrants results from changes in unobserved state attributes that affect all potential residents, minimum wage increases will also be associated with outflows of higher skilled immigrants.

The analysis passes this specification check, as shown in Table 3.11. The point estimates are all near -0.1 and are substantially smaller than the corresponding estimates in Table 3.10. None of the estimates can be statistically distinguished from zero. Thus, the null that changes in the location pattern of higher-skilled immigrants are unrelated to changes in the minimum wage cannot be rejected. Additionally, a confidence interval using standard significance levels does not include the strong negative elasticity for the high school dropout immigrants. These results provide even more support for interpreting that empirical relationship as evidence that low-skilled immigrants are highly responsive to differences in labor market opportunities across geography.

3.3.7 Newer Arrivals Are More Responsive

One remaining question is whether the geographic immigrant population shifts occur primarily through a reshuffling of immigrants who are already in the United

States or through changes in where the most recent arrivals initially settle. Immigrants who arrived more recently might have fewer ties to a particular community, and thus may be more responsive to differences in labor market prospects between destinations.

Table 3.12 addresses this question. I classify immigrants by the recency of their arrival (greater or fewer than five years ago), and the observations in these regressions are state-month-recency cells. The first column restricts the effect of the minimum wage to be constant for both types of immigrants, and the estimated coefficient is quite similar to the results using only one cell per state-month. The positive coefficient on the uninteracted dummy for recency reflects the fact that in any given month, there are more immigrants who arrived in the past five years than immigrants who have been in the country between six and ten years.

This restricted model, however, masks considerable heterogeneity. The specification in column 2 includes the interaction of the minimum wage variable with the dummy variable for having arrived in the past five years. Recent arrivals are nearly twice as responsive to changes in the minimum wage, although these results do show some reshuffling of immigrants who had been in the country for longer periods of time. This finding supports a similar result in Cadena (2008) that more recent arrivals are more sensitive to changes in labor market prospects. The lower mobility response for immigrants who have been in the country longer implies that the continuous flow of new immigrants entering the country is an essential mechanism through which the national labor market adjusts to geographically disparate shocks.

3.4 Conclusions and Policy Implications

This paper demonstrates that newly arriving immigrants serve as very elastic marginal workers, willing to move across geography to access better labor market prospects. Traditional models of local labor markets predict that such a mobile factor will tend to equalize the returns to factors across space. Although low-skilled natives are quite immobile, this paper demonstrates that immigrants are much more earnings-sensitive and geographically flexible. This result has a number of implications for policymakers and for future research.

First, state policymakers should realize that policies designed to affect their own labor market will, through immigrants' responses, affect other states as well. In the particular case of the minimum wage, the flow of immigrants away from minimum wage increases will lessen the negative employment consequences that would have resulted within a closed system. Similarly, a program designed to increase employment or raise wages is likely to attract more immigrants, potentially muting the benefit of those programs to previous residents.

In addition, future research in several literatures needs to take account of this result. As immigrants select labor markets that provide the highest returns, their altered destination choices will tend to undermine the usefulness of geographic comparisons in determining the effect of labor market policies and shocks. As one example, critics of a geographic approach to identifying the wage and employment effects of increased low-skilled immigration allege that the type of selection demonstrated in this paper undermines the usefulness of geographic comparisons. If immigrants are close to perfect arbitrageurs, no comparison between areas receiving large immigrant inflows and those receiving smaller inflows will properly estimate the effect of

increased immigration on the native wage structure, even if such an effect would be substantial in the case of exogenous immigration and a closed labor market.

Finally, this paper suggests that the low mobility rates of low-skilled natives will not necessarily lead to large wage differentials across geography. Endogenous destination selection among immigrants can therefore prevent native workers from bearing potentially higher relocation costs as negative shocks in one state will quickly diffuse throughout the country. Future research should attempt to quantify this often-overlooked benefit to immigration.

3.5 Figures and Tables

Figure 3.1: Predicted Worker Flows as a Function of the Labor Demand Elasticity (η) and the Separation Rate (δ)

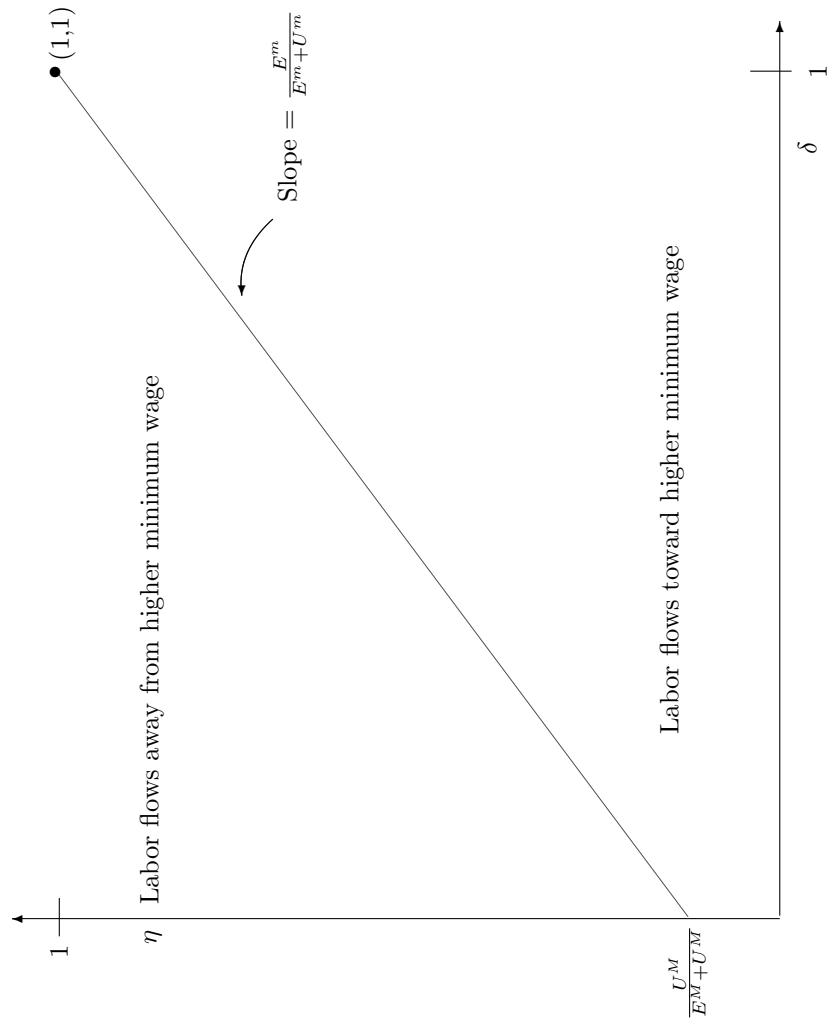


Figure 3.2: Kernel Density Estimates of Wage Distributions Before and After Minimum Wage Changes

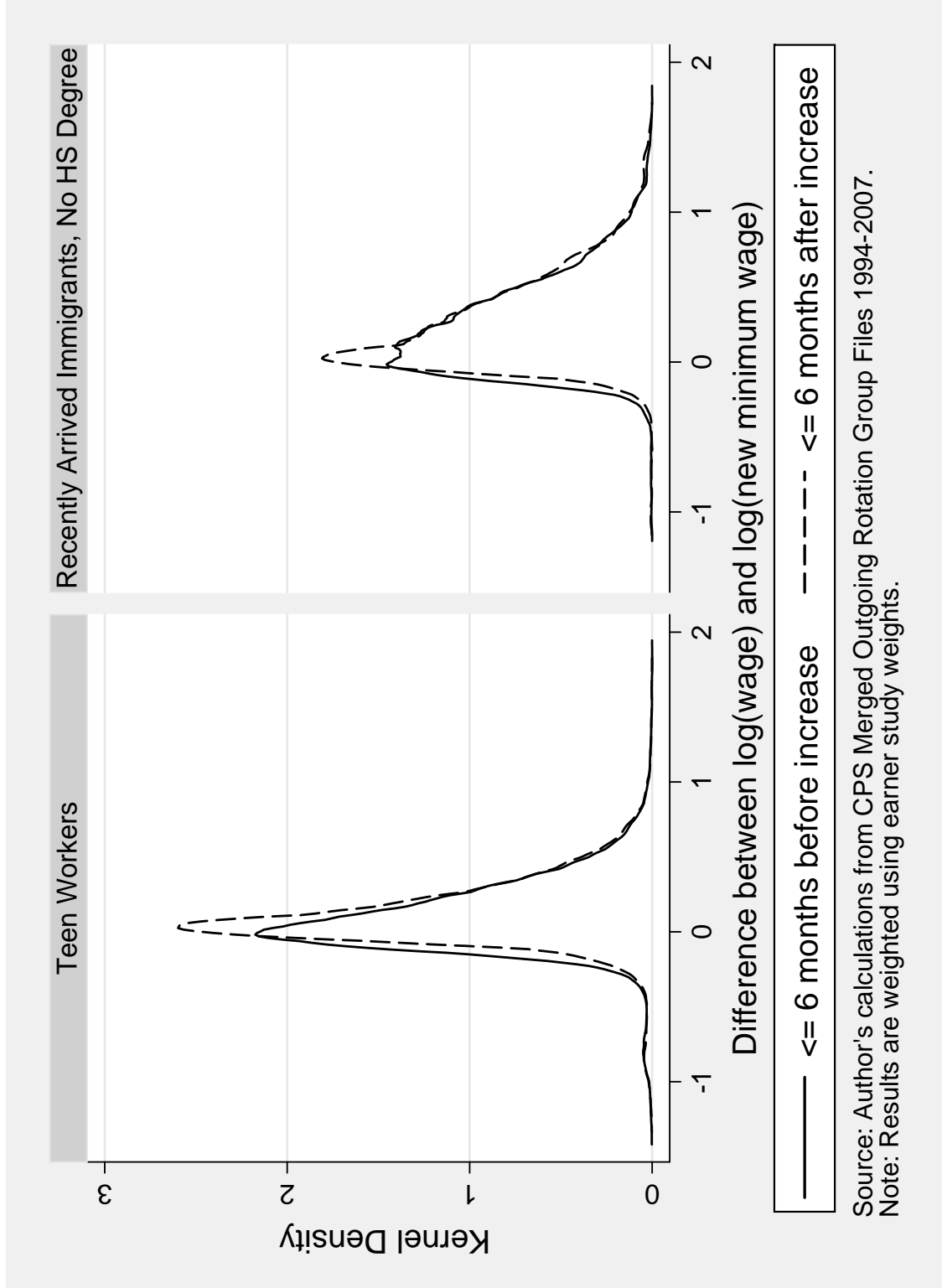


Figure 3.3: Kernel Density Estimates of Wage Distributions Before and After Minimum Wage Changes

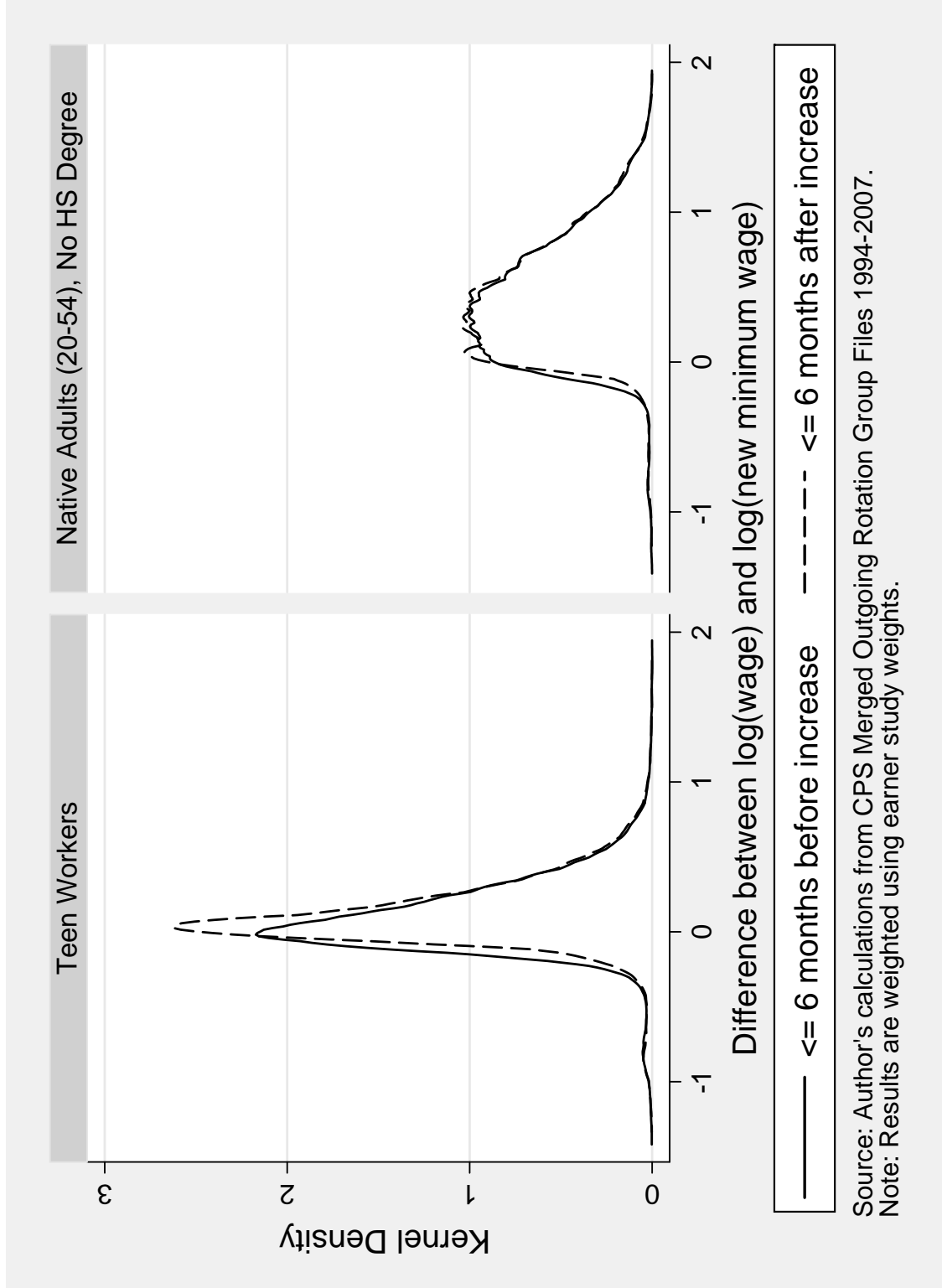
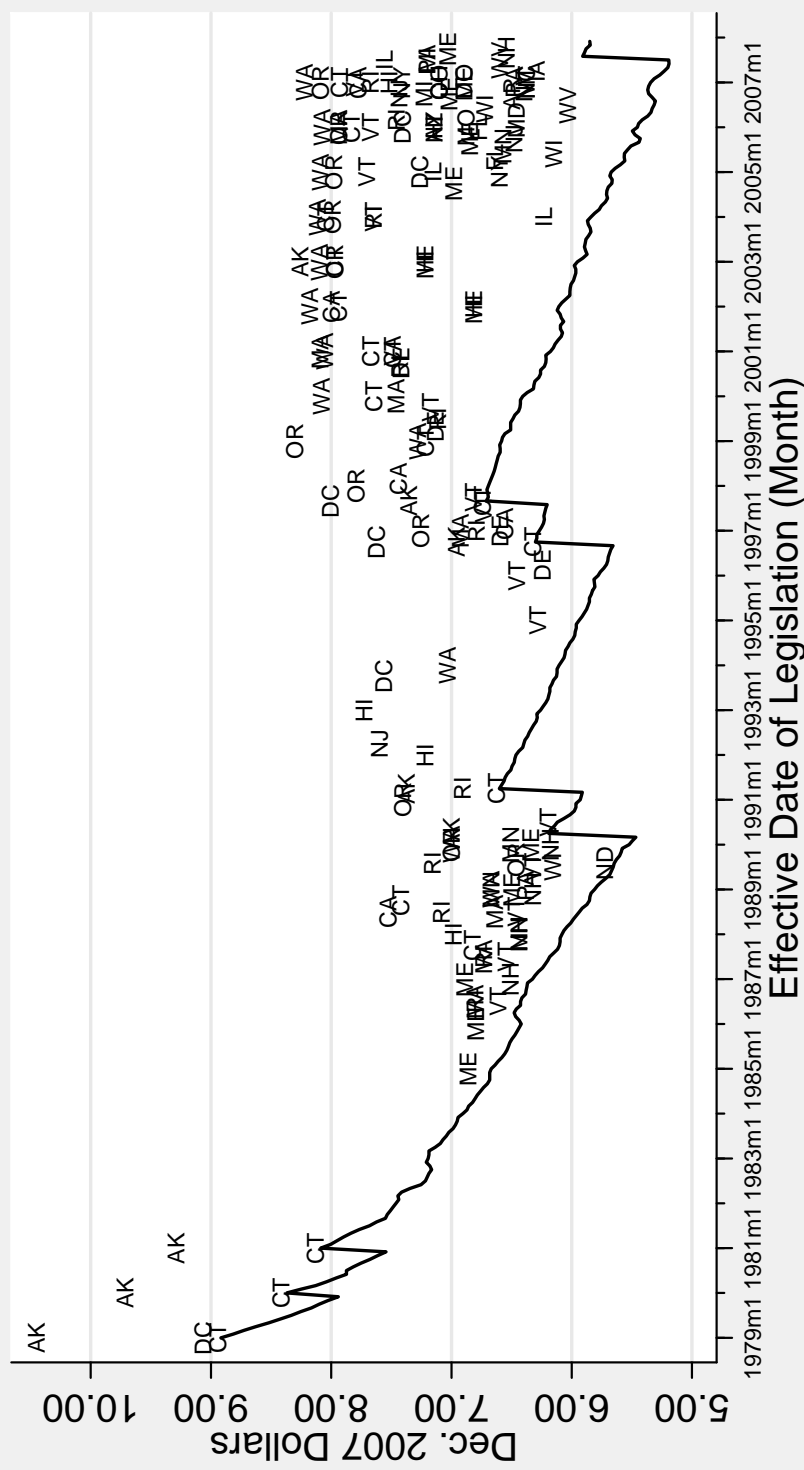


Figure 3.4: State and Federal Minimum Wage Policy 1979-2007



Note: State abbreviations represent the level and effective date of state minimum wage legislation.

Source: UKCPR data (http://www.ukcpr.org/EconomicData/UKCPRNationalDataSet_08-05-07.xls) and January Monthly Labor Review 1980-2008.

Figure 3.5: Estimated Labor Demand Elasticity Creates A Clear Prediction

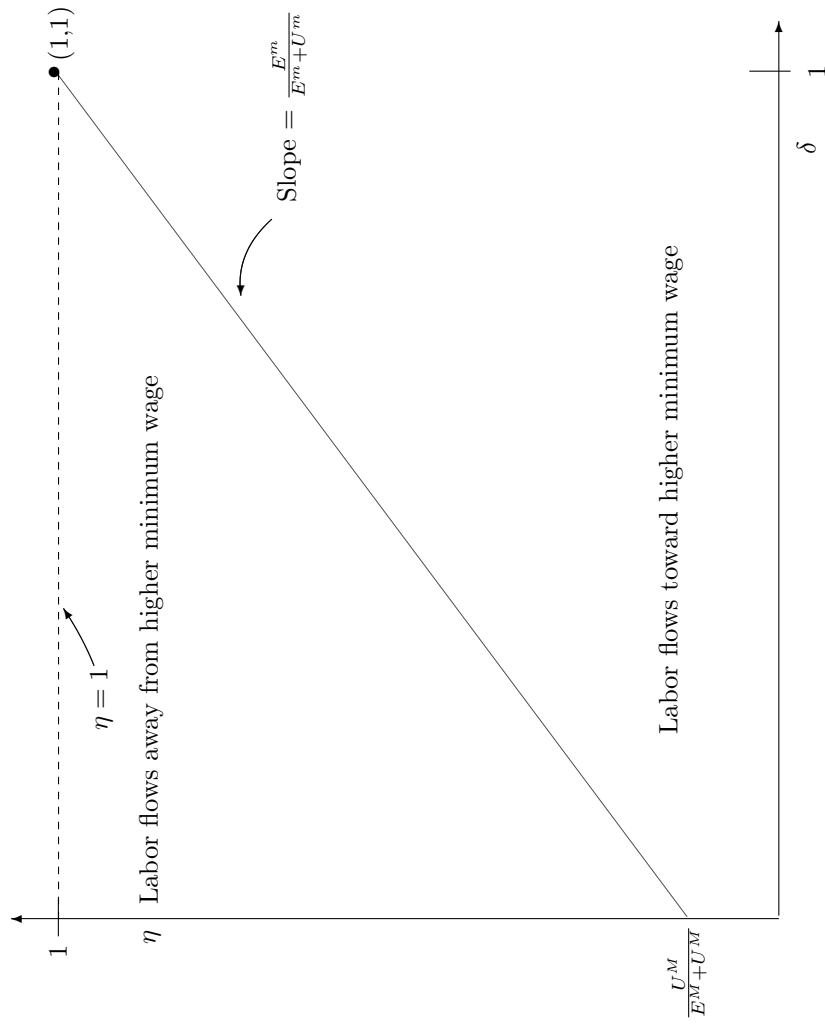


Figure 3.6: Falsification Test: Kernel Density Estimates of Wage Distributions Before and After Minimum Wage Changes

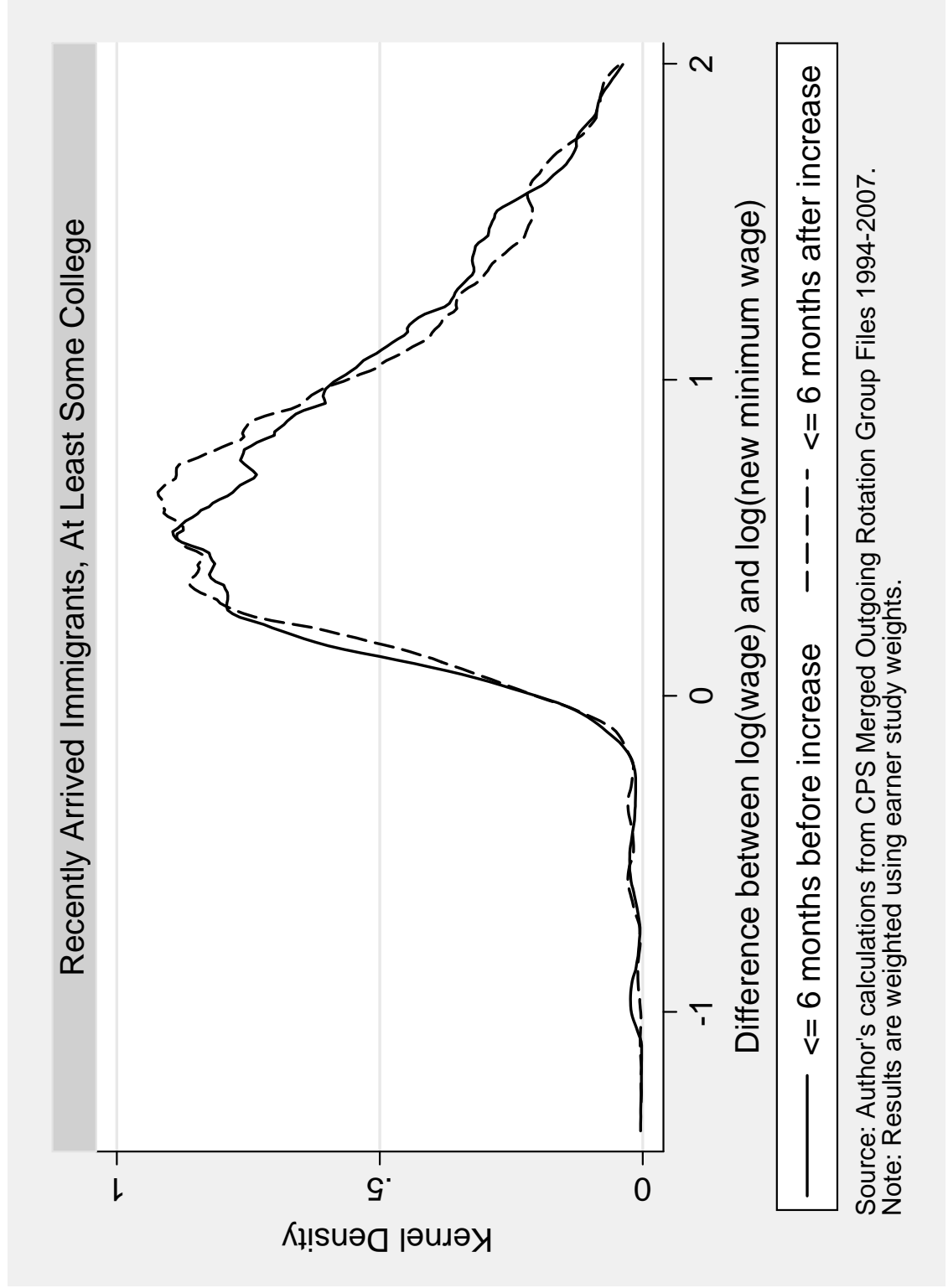


Table 3.1: State Minimum Wage Policies 1994-2007

State	Percent of Months Above Federal Minimum	Average Difference (when higher)	Number of Changes in Effective Minimum
Alaska	100%	18%	3
Alabama	0%	n/a	3
Arkansas	9%	15%	3
Arizona	14%	24%	3
California	74%	21%	7
Colorado	7%	23%	3
Connecticut	100%	17%	10
DC	100%	20%	4
Delaware	70%	15%	7
Florida	19%	19%	4
Georgia	0%	n/a	3
Hawaii	100%	14%	4
Iowa	5%	11%	3
Idaho	0%	n/a	3
Illinois	29%	20%	5
Indiana	0%	n/a	3
Kansas	0%	n/a	3
Kentucky	0%	n/a	3
Louisiana	0%	n/a	3
Massachussets	79%	21%	5
Maryland	13%	15%	3
Maine	43%	20%	8
Michigan	9%	27%	4
Minnesota	17%	16%	3
Missouri	14%	22%	4
Mississippi	0%	n/a	3
Montana	7%	12%	3
North Carolina	7%	12%	3
North Dakota	0%	n/a	3
Nebraska	0%	n/a	3
New Hampshire	2%	11%	4
New Jersey	42%	18%	3
New Mexico	0%	n/a	3
Nevada	8%	13%	3
New York	21%	23%	5
Ohio	7%	23%	3
Oklahoma	0%	n/a	3
Oregon	98%	23%	8
Pennsylvania	7%	21%	4
Rhode Island	85%	17%	7
South Carolina	0%	n/a	3
South Dakota	0%	n/a	3
Tennessee	0%	n/a	3
Texas	0%	n/a	3
Utah	0%	n/a	3
Virginia	0%	n/a	3
Vermont	87%	18%	10
Washington	90%	24%	11
Wisconsin	18%	16%	4
West Virginia	11%	13%	4
Wyoming	0%	n/a	3

Table 3.2: Variable Definitions and Descriptive Statistics

Variable Label	Variable Definition	Mean	SD
Native Outcomes			
Emp/Pop	State's employment to population ratio, teenagers	0.44	0.15
Log Hours	Log of average weekly hours worked (incl. zeros), teenagers	2.11	0.51
Log Hourly Wage	Log of hourly wage, December 2007 dollars, teenage hourly workers	2.03	0.12
Log Earnings	Log of average weekly labor earnings (incl. zeros), December 2007 dollars, all teen workers	4.40	0.50
Immigrant Outcome			
Log Immigrant Count	Log of number of recent arrival immigrants (in US <10 years), no HS degree	9.21	1.46
Independent Variables			
Log Real Minimum Wage	Log of the higher of the state or federal minimum wage, December 2007 Dollars	1.83	0.10
Log Real Native Adult Wage	Log hourly wage, native workers 20-54 paid by the hour, December 2007 Dollars	2.57	0.11
Unemployment Rate	Statewide unemployment rate, Adults 20-54, CPS	4.50	2.75
Teen Population Share	Percent of state population 16-54 that is 16-19	10.87	2.34
State Dummies	Dummy variables for each state	n/a	n/a
Date Dummies	Dummy variables for each month, Jan 1994-Dec 2007	n/a	n/a
State Trends	State-specific linear monthly time trends	n/a	n/a

Note: All variables calculated from monthly CPS Outgoing Rotation Group January 1994-December 2007 unless otherwise noted.

Table 3.3: Place of Residence Five Years Ago

	Same State	Different State	Abroad
Native Teens	93.0%	6.2%	0.8%
Native Adults, No HS Degree	92.0%	6.7%	1.3%
Immigrant Adult, No HS Degree, Arrived to stay 6-10 years ago	85.8%	8.5%	5.7%
Immigrant Adult, No HS Degree, Arrived to stay 1-5 years ago	31.3%	3.8%	64.9%

Source: Author's Calculations from 2000 IPUMS.

Table 3.4: Effects of the Minimum Wage on the Employment to Population Ratio 1994-2007

	Teens			Native Adult HS dropouts		
	(1)	(2)	(3)	(4)	(5)	(6)
Log Real Minimum Wage	-0.036 (0.032)	-0.033 (0.032)	-0.029 (0.039)	0.031 (0.038)	0.029 (0.038)	-0.050 (0.038)
Log Real Native Adult Wage		-0.027 (0.025)	-0.029 (0.026)			
Unemployment Rate		-0.003 (0.001)	-0.002 (0.001)			
Teenager Population Share		-0.000 (0.001)	-0.000 (0.001)		0.000 (0.000)	0.000 (0.000)
Constant	0.396 (0.063)	0.512 (0.070)	0.504 (0.092)			
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Date Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State Trends	No	No	Yes	No	No	Yes
Number of State-Months	8568	8568	8568	8568	8568	8568
R-squared	0.49	0.49	0.50	0.23	0.23	0.24

Standard Errors clustered by state in parentheses.

Source: Author's Calculations from Merged Outgoing Rotation Group CPS files, January 1994-December 2007

+ p<0.10, * p<0.05, ** p<0.01

Table 3.5: Effects of the Minimum Wage on Log(Hours) 1994-2007

	Teens			Native Adult HS dropouts		
	(1)	(2)	(3)	(4)	(5)	(6)
Log Real Minimum Wage	-0.086 (0.102)	-0.098 (0.104)	-0.230 (0.122)	-0.006 (0.117)	-0.007 (0.116)	-0.205 (0.115)
Log Real Native Adult Wage		-0.167 (0.083)	-0.216 (0.084)	*		
Unemployment Rate		-0.011 (0.002)	-0.008 (0.002)	**		
Teenager Population Share		0.004 (0.002)	0.005 (0.002)	+		
Constant	1.862 (0.206)	2.562 (0.244)	2.905 (0.295)	**	2.318 (0.232)	**
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Date Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State Trends	No	No	Yes	No	No	Yes
Number of State-Months	8518	8518	8518	8443	8443	8443
R-squared	0.41	0.41	0.42	0.19	0.19	0.21

Standard Errors clustered by state in parentheses.

Source: Author's Calculations from Merged Outgoing Rotation Group CPS files, January 1994-December 2007

+ p<0.10, * p<0.05, ** p<0.01

Table 3.6: Effects of the Minimum Wage on Log(Hourly Wage) 1994-2007

	Teens			Native Adult HS dropouts		
	(1)	(2)	(3)	(4)	(5)	(6)
Log Real Minimum Wage	0.182 (0.040)	** 0.183 (0.039)	** 0.213 (0.058)	** 0.017 (0.062)	0.017 (0.062)	0.067 (0.058)
Log Real Native Adult Wage		* 0.083 (0.032)	* 0.056 (0.027)			
Unemployment Rate		-0.001 (0.001)	-0.000 (0.001)			
Teenager Population Share		0.000 (0.001)	0.000 (0.001)		0.000 (0.000)	0.000 (0.000)
Constant	1.585 (0.072)	** 1.430 (0.118)	** 1.441 (0.137)	** 2.253 (0.127)	** 2.248 (0.111)	** 2.155 (0.105)
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Date Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State Trends	No	No	Yes	No	No	Yes
Number of State-Months	8540	8540	8540	8517	8517	8517
R-squared	0.33	0.33	0.34	0.17	0.17	0.19

Standard Errors clustered by state in parentheses.

Source: Author's Calculations from Merged Outgoing Rotation Group CPS files, January 1994-December 2007

+ p<0.10, * p<0.05, ** p<0.01

Table 3.7: Effects of the Minimum Wage on Log(Earnings) 1994-2007

	Teens			Native Adult HS dropouts		
	(1)	(2)	(3)	(4)	(5)	(6)
Log Real Minimum Wage	0.028 (0.155)	0.006 (0.154)	0.003 (0.192)	0.036 (0.110)	0.041 (0.109)	-0.049 (0.105)
Log Real Native Adult Wage		0.018 (0.083)	-0.058 (0.080)			
Unemployment Rate		-0.010** (0.002)	-0.007** (0.002)			
Teenager Population Share		0.007** (0.002)	0.008** (0.002)		-0.001+ (0.001)	-0.001+ (0.000)
Constant	3.977** (0.302)	4.117** (0.285)	4.292** (0.394)	5.110** (0.205)	5.366** (0.199)	5.521** (0.190)
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Date Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State Trends	No	No	Yes	No	No	Yes
Number of State-Months	8544	8544	8544	8543	8543	8543
R-squared	0.35	0.36	0.37	0.18	0.18	0.19

Standard Errors clustered by state in parentheses.

Source: Author's Calculations from Merged Outgoing Rotation Group CPS files, January 1994-December 2007

+ p<0.10, * p<0.05, ** p<0.01

Table 3.8: Employment, Wage and Earnings Effects, Lag Specification, Teens

	(1)	(2)	(3)	(4)
	Emp/Pop	Log Hours	Log Hourly Wage	Log Earnings
Log Real Minimum Wage	-0.020 (0.057)	-0.082 (0.194)	0.141 (0.062)	0.040 (0.245)
Log Real Min Wage, 1 qtr lag	-0.011 (0.054)	-0.185 (0.220)	0.090 (0.060)	-0.046 (0.227)
Log Real Native Adult Wage	-0.029 (0.026)	-0.216 (0.083)	0.056 (0.027)	-0.058 (0.080)
Unemployment Rate	-0.002 (0.001)	-0.008 (0.002)	-0.000 (0.001)	-0.007 (0.002)
Teenager Population Share	-0.000 (0.001)	0.005 (0.002)	0.000 (0.001)	0.008 (0.002)
Constant	0.508 (0.096)	2.971 (0.324)	1.409 (0.143)	4.308 (0.413)
State Dummies	Yes	Yes	Yes	Yes
Date Dummies	Yes	Yes	Yes	Yes
State Trends	Yes	Yes	Yes	Yes
Number of State-Months	8568	8518	8540	8544
R-squared	0.50	0.42	0.34	0.37

Standard Errors clustered by state in parentheses.

Source: Author's Calculations from Merged Outgoing Rotation Group CPS files, January 1994-December 2007

+ p<0.10, * p<0.05, ** p<0.01

Table 3.9: Employment, Wage and Earnings Effects, Lag Specification, Adult HS Dropouts

	(1)	(2)	(3)	(4)
	Emp/Pop	Log Hours	Log Hourly Wage	Log Earnings
Log Real Minimum Wage	-0.043 (0.065)	-0.330 (0.267)	0.002 (0.079)	-0.087 (0.222)
Log Real Min Wage, 1 qtr lag	-0.009 (0.072)	0.155 (0.289)	0.081 (0.088)	0.047 (0.231)
Teenager Population Share	0.000 + (0.000)	0.000 (0.001)	0.000 (0.000)	-0.001 + (0.000)
Constant	0.618 ** (0.078)	2.958 ** (0.219)	2.127 ** (0.115)	5.505 ** (0.197)
State Dummies	Yes	Yes	Yes	Yes
Date Dummies	Yes	Yes	Yes	Yes
State Trends	Yes	Yes	Yes	Yes
Number of State-Months	8568	8443	8517	8543
R-squared	0.24	0.21	0.19	0.19

Standard Errors clustered by state in parentheses.

Source: Author's Calculations from Merged Outgoing Rotation Group CPS files, January 1994-December 2007

+ p<0.10, * p<0.05, ** p<0.01

Table 3.10: Log Immigrant Counts as a Function of the Minimum Wage
Recent Arrivals 18-54, No HS Degree

	(1)	(2)	(3)	(4)
Log Real Minimum Wage	-1.114 (0.334)	-0.700 (0.328)	-0.702 (0.328)	-0.699 (0.327)
Unemployment Rate			0.116 (0.328)	0.109 (0.333)
Log Real Native Adult Wage				-0.062 (0.220)
Constant	10.984 (0.594)	10.227 (0.586)	10.225 (0.585)	10.373 (0.852)
State Dummies	Yes	Yes	Yes	Yes
Date Dummies	Yes	Yes	Yes	Yes
State-Specific Trends	No	Yes	Yes	Yes
Number of State-Months	5590	5590	5590	5590
R-squared	0.80	0.81	0.81	0.81

Standard Errors clustered by state in parentheses.

Source: Author's Calculations from Merged Outgoing Rotation Group CPS files, January 1994-December 2007

+ p<0.10, * p<0.05, ** p<0.01

Table 3.11: Falsification Test: Log Immigrant Counts as a Function of the Minimum Wage
Recent Arrivals 18-54, At Least Some College

	(1)	(2)	(3)	(4)
Log Real Minimum Wage	-0.102 (0.248)	-0.133 (0.182)	-0.137 (0.183)	-0.121 (0.183)
Unemployment Rate			0.002 (0.003)	0.002 (0.003)
Log Real Native Adult Wage				-0.262 (0.167)
Constant	9.186 ** (0.442)	9.240 ** (0.324)	9.237 ** (0.323)	9.859 ** (0.453)
State Dummies	Yes	Yes	Yes	Yes
Date Dummies	Yes	Yes	Yes	Yes
State-Specific Trends	No	Yes	Yes	Yes
Number of State-Months	7155	7155	7155	7155
R-squared	0.85	0.85	0.85	0.85

Standard Errors clustered by state in parentheses.

Source: Author's Calculations from Merged Outgoing Rotation Group CPS files, January 1994-December 2007

+ p<0.10, * p<0.05, ** p<0.01

Table 3.12: Differential Response in Log Immigrant Counts by Recency of Arrival

	(1)	(2)
Log Real Minimum Wage	-0.748 (0.241)	-0.497 (0.241)
Log Real Minimum Wage*Recent		-0.449 (0.175)
Recent (Arrived <5 years ago)	0.448 (0.026)	1.274 (0.327)
Unemployment Rate	-0.063 (0.156)	-0.061 (0.156)
Log Real Native Adult Wage	-0.000 (0.003)	-0.000 (0.003)
Constant	9.945 (0.617)	9.480 (0.613)
State Dummies	8843	8843
Date Dummies	0.79	0.79
State-Specific Trends	Yes	Yes
Number of State-Month-Recency Cells	8843	8843
R-squared	0.79	0.79

Standard Errors clustered by state in parentheses.

Source: Author's Calculations from Merged Outgoing Rotation Group CPS files, January 1994-December 2007
+ p<0.10, * p<0.05, ** p<0.01

3.6 Appendix

3.6.1 Proof of Proposition III.1

To prove: When total labor supply is fixed and no workers migrate between states the following inequality implies that expected earnings will increase in the state increasing its minimum wage:

$$\delta \frac{E^m}{E^m + U^m} + \frac{U^m}{E^m + U^m} > \eta. \quad (3.6)$$

Proof. As shown in the text, the derivative of expected earnings depends on the initial probability of finding employment, the initial wage, and the derivative of the probability at the initial wage.

$$\frac{\partial E[I^m]}{\partial w^m} = p(w_0^m) + w_0^m \left[\frac{\partial p}{\partial w^m} \Big|_{w^m = w_0^m} \right], \quad (3.7)$$

The proof begins by evaluating the derivative of the probability of finding employment.

$$p(w^m) = \frac{\delta E^m}{\delta E^m + U^m}. \quad (3.8)$$

$$\frac{\partial p}{\partial w^m} = \frac{-\delta E^m (\delta \frac{\partial E^m}{\partial w^m} + \frac{\partial U^m}{\partial w^m}) + (\delta E^m + U^m) (\delta \frac{\partial E^m}{\partial w^m})}{(\delta E^m + U^m)^2}. \quad (3.9)$$

Distributing through the parentheses yields

$$= \frac{-\delta^2 E^m \frac{\partial E^m}{\partial w^m} - \delta E^m \frac{\partial U^m}{\partial w^m} + \delta^2 E^m \frac{\partial E^m}{\partial w^m} + \delta U^m \frac{\partial E^m}{\partial w^m}}{(\delta E^m + U^m)^2}. \quad (3.10)$$

$$= \frac{-\delta E^m \frac{\partial U^m}{\partial w^m} + \delta U^m \frac{\partial E^m}{\partial w^m}}{(\delta E^m + U^m)^2}. \quad (3.11)$$

With inelastic labor supply and no movement from state to state, $\frac{\partial E^m}{\partial w^m}$ and $\frac{\partial U^m}{\partial w^m}$ are equal in magnitude and of opposite signs. Thus,

$$\frac{\partial p}{\partial w^m} = \frac{\delta(E^m + U^m) \frac{\partial E^m}{\partial w^m}}{(\delta E^m + U^m)^2}. \quad (3.12)$$

Plugging this expression back into the derivative of expected earnings yields

$$\frac{\partial E[I^m]}{\partial w^m} = \frac{\delta E^m}{\delta E^m + U^m} + w_0^m \frac{\delta(E^m + U^m) \frac{\partial E^m}{\partial w^m}}{(\delta E^m + U^m)^2}. \quad (3.13)$$

This expression will be positive whenever

$$\frac{\delta E^m}{\delta E^m + U^m} > -w_0^m \frac{\delta(E^m + U^m) \frac{\partial E^m}{\partial w^m}}{(\delta E^m + U^m)^2}. \quad (3.14)$$

Rearranging gives

$$1 > -\frac{\partial E^m}{\partial w^m} \frac{w_0^m}{E^m} \left(\frac{E^m + U^m}{\delta E^m + U^m} \right). \quad (3.15)$$

Letting η denote the labor demand elasticity, this simplifies to

$$\frac{\delta E^m}{E^m + U^m} + \frac{U^m}{E^m + U^m} > \eta. \quad (3.16)$$

□

3.6.2 Higher Turnover Implies Smaller Probability Effects

Here I formally demonstrate that labor markets with higher turnover rates experience smaller declines in the probability that a searching worker finds employment for a given change in the minimum wage. To show:

$$\frac{\partial p^2}{\partial w \partial \delta} < 0 \quad (3.17)$$

Proof. Equation 3.12 gives the first derivative of the employment probability with respect to the minimum wage. Taking the cross derivative with respect to δ yields:

$$\frac{\partial p^2}{\partial w \partial \delta} = \frac{-\delta^2(E^m + U^m) \frac{\partial E^m}{\partial w} (\delta E^m + U^m) + (\delta E^m + U^m)^2 \frac{\partial E^m}{\partial w}}{(\delta E^m + U^m)^4}. \quad (3.18)$$

The denominator is always positive. The sign thus depends on the sign of the numerator. It will be negative whenever

$$(\delta E^m + U^m)^2 \frac{\partial E^m}{\partial w} < \delta^2 (E^m + U^m) \frac{\partial E^m}{\partial w} (\delta E^m + U^m). \quad (3.19)$$

Dividing both sides by $\frac{\partial E^m}{\partial w} (\delta E^m + U^m)$, which is negative, yields

$$\delta E^m + U^m > \delta^2 E^m + \delta^2 U^m. \quad (3.20)$$

This inequality holds by inspection for all values of $\delta < 1$.

□

CHAPTER IV

Can Self-Control Explain Avoiding Free Money? Evidence from Interest-Free Student Loans

“Although it may be tempting to use student loan money for college football tickets, midnight pizza while cramming for finals, or a Florida spring break trip, try to resist this lure....If you receive a larger loan than you need, the temptation to spend the extra money on “fun” things can be hard or even impossible to resist.”

-Dara Duguay, “Spend Student Loans Only on College Expenses” youngmoney.com
(money management website for young adults), 2003

4.1 Introduction

This paper uses insights from behavioral economics to explain a particularly bizarre borrowing phenomenon: About one in six undergraduate students who are offered interest-free loans turn them down. The students we observe making these choices are not atypical: Our sample consists of full-time students enrolled at public or private non-profit four-year institutions who demonstrated sufficient financial need to qualify for aid sponsored by the federal government.

There are three principal reasons we should be surprised that one-sixth of eligible students turn down subsidized loans. First, these loans do not accrue interest until

six months after students leave school. These interest payments represent a direct transfer to the student, and the amount is non-trivial. If a student were to borrow the maximum each year, with an interest rate of four percent, the government subsidy would be worth more than \$1500. The “free money” aspect of below-market interest rates on student loans has long been a part of conventional economic wisdom. One classic undergraduate textbook explains the benefits of a \$1000 interest-free loan as follows: “You could at least take the money and put it in a savings bank, where you will earn at least 4 percent per year. Each year you can draw out the \$40 interest and throw a big party. Finally...you can draw out the \$1,000, plus the last year’s interest; repay the \$1,000; and have \$40 for a last party” (Alchian and Allen 1964). We are unaware, however, of any work that has tried to systematically understand why students do not take advantage of this potential \$1500 “gift” from the government.¹

Seeing students turn down interest-free loans is also surprising because government-sponsored loans help to make increasingly expensive educational costs more affordable. During a period when the return to higher education has dramatically increased, the rising costs of an undergraduate education have far outpaced the increase in the availability of grants and scholarships (Hoxby and Long 1999, Dynarski 2002, Avery and Hoxby 2003). In 1980, 41 percent of financial aid was provided through loans. Today, loans make up 59 percent of federal aid (Douthat 2005) and 45 percent of full-time students borrow to finance their education (National Center for Educational Statistics 2000, National Center for Educational Statistics 2002). In the absence of these programs, students would find it costly to borrow against their future earnings due to informational asymmetries between students and private lenders. The federal government has recognized this potential market failure and of-

¹Note that a student need not plan on “gaming the system” when she borrows for accepting the loan to be a good idea. If there is some uncertainty about the costs she will face over the school year, she may wish to borrow the money as a precautionary measure.

fers students grants and loans through large-scale programs which provided 90 billion dollars in total aid during the 2004-2005 school year (The College Board 2005).² By rejecting their government-sponsored loans, students are effectively choosing to borrow at a significantly higher cost, if at all.

Finally, student aid offers are administered under the presumption that students will accept all of their need-based aid. Students must actively reduce or reject any amount they do not wish to borrow. In fact, if a student has borrowed before, she needs to do nothing at all to receive the full amount of any subsidized loan awarded by her financial aid office. As other researchers have shown, there is a significant mental barrier to making decisions which deviate from the default, known as “default bias” (c.f. Choi, Laibson, Madrian and Metrick 2004). In the absence of competing forces, therefore, students should rarely deviate from the default of accepting all of their need-based aid, including interest-free loans.

While the benefits of subsidized student loans are seemingly unambiguous, borrowing does increase a student’s short-term liquidity. As the quotation at the beginning of this section suggests, interest-free loans are a double-edged sword in the hands of an easily tempted consumer. Despite the fact that these loans make it possible to smooth consumption over time, having such a large amount of liquidity can lead to overspending, i.e. consuming more out of current income than an agent with perfect willpower would desire.

We formalize this argument by modeling a college student choosing how much to borrow for her education. We show that a rational agent would not turn down

²Previous work on the changing nature of the financial aid system focuses on the characteristics of students who default on their loans (Knapp and Seaks 1992, Dynarski 1994). Other authors examine whether the size and type of student loans affect whether and where students enroll (McPherson and Schapiro 1991, van der Klaauw 2002, Kane 2003, Epple, Romano and Sieg 2003). Field (2004) investigates an NYU law school experiment and finds that the decision to enter public-interest law in exchange for a lower debt burden is sensitive to the timing of incurring debt. See also Orfield (1992) for a summary of the policy debate which led to the expansion of student loans in the 1990s.

interest-free student loans because doing so requires foregoing a significant government subsidy in addition to limiting future liquidity. We then discuss how rejecting the loan is consistent with models of self-control from the theoretical literature that allow rational consumers to prefer a subset of choices to the complete set. The debt-averse behavior we observe, therefore, may be the optimal choice a forward-thinking student can make knowing that in the following period she will be tempted to overspend.

There are, however, alternative reasons why a potential borrower could make the “wrong” decision. Certainly some students will reject the loan because they do not understand how the subsidy works or do not analyze the decision closely enough.³ Students may also falsely believe that borrowing through student loan programs will hurt their credit score. In fact, each month while the student is in school the lender reports that the loan account is being paid as agreed, establishing a solid credit history. Apart from these information problems, some students may reject their loans because of the hassle borrowing creates, such as having to keep track of the documents associated with a loan or being required to make a payment each month after graduation.⁴ Still others may reject the loans because they have acquired an anti-debt ethic such that indebtedness carries a psychological cost. Survey research in the United Kingdom finds that students who are uncomfortable with debt are less likely to pursue a college education, although they do not attempt to determine the source of this discomfort (Callender and Jackson 2005). Because any of these factors can potentially explain the significant fraction of students who turn down their interest-free loans, we cannot simply interpret high rejection rates as evidence

³See Dynarski and Scott-Clayton (2006) for a discussion of the sometimes overwhelming complexity of the financial aid system.

⁴Another potential reason to turn down student loans is that they are not dischargeable under current bankruptcy law.

of a self-control motive.

To determine whether self-control plays an important role, the ideal quasi-experimental setting would fix the benefits of borrowing while varying students' exposure to increased liquidity. A feature of financial aid disbursement does *exactly* this: Although the value of the subsidy is unchanged, needy on-campus students have their loans automatically applied to their educational expenses while needy off-campus students receive a portion of their aid in cash. Comparing the take-up rates of these two groups provides us with a means to test whether self-control motives are responsible for some of the failure in take-up.

However, if students who reject their loans for other reasons tend to live in off-campus housing, this comparison may incorrectly attribute differences in take-up rates to differences in liquidity. To address these selection concerns, we form a difference-in-differences estimator, using students whose liquidity is unaffected by their housing location as a counterfactual. For these students, any loan funds will be applied directly to their tuition bill regardless of where they live. Importantly, each member of the counterfactual group is also eligible for the maximum subsidized loan. If students reject their loans to avoid excess liquidity, the difference between on- and off-campus rejection rates should be much larger for the group who potentially receive their loans in cash.

Our estimates from the 1999-2000 and 2003-2004 waves of the National Postsecondary Student Aid Study support a self-control explanation: Students who would have received cash from their loans turn down the subsidized loan seven percentage points more frequently than similarly needy students who live on-campus. Importantly, there is no significant difference in rejection rates across housing locations for students who would not receive cash regardless of where they live. As a further test,

we estimate the effect of liquidity on take-up using the university's dormitory capacity (number of beds per student) as an instrument for the housing location decision. The instrumental variables specification continues to show a differential willingness to borrow across housing locations, even when controlling for differences in school quality which are correlated with housing capacity. These results are difficult to explain without self-control concerns affecting students' decisions.

We provide evidence that consumers choose to limit their available choices in a natural setting, i.e. one not generated by the researcher. While several laboratory and simulation studies have presented evidence consistent with consumers exercising self-control (Wertenbroch, Soman and Nunes 2001, Ariely and Wertenbroch 2002, Laibson, Repetto and Tobacman 2003, Fernandez-Villaverde and Mukherji 2006), studies using data and situations not generated by the researcher have tended to find evidence of consumers succumbing to the temptation of earlier consumption (Stephens 2003, Shapiro 2005, DellaVigna and Malmendier 2006). In addition, while most field experiments are explicitly designed to hold constant any differences between two choices except for the level of commitment, our results reveal that some consumers are willing to pay a substantial amount of money in order restrict their future decisions.⁵ These two features distinguish this study as particularly compelling evidence for the existence and importance of time-inconsistent preferences.

In the next section, we discuss the mechanics of financial aid and emphasize the case when impatient individuals might be most wary of taking out loans. We present a brief model of the financial aid process in Section 4.3 and show how rejecting an interest-free loan, while costly, can effectively serve as a mechanism to regulate impulsive consumption. In Section 4.4, we establish the phenomenon empirically,

⁵For example, Ashraf, Karlan and Yin (2006) are careful to note the equal interest rates paid in the experimental restricted bank account and the unrestricted account.

test the additional predictions suggested by the model, and address selection into off-campus housing. Section 4.5 concludes and discusses potential policy implications.

4.2 Overview of the Financial Aid Process

This section presents a sketch of the process that determines the financial aid a college student receives. Our discussion draws heavily from the Federal Student Aid Handbook published by the Department of Education for use by financial aid professionals (Department of Education 2003).

There are two primary components that determine a student's eligibility for all federal financial aid: a measure of the student's ability to pay, and the costs the student faces at the school where she is enrolled. A student interested in need-based financial aid must first file a Free Application for Federal Student Aid (FAFSA), which collects information on the student and her parents, including the value of their assets and incomes from the previous year.⁶ These data are then entered into a federal formula that calculates the Expected Family Contribution (EFC), the dollar amount a family could reasonably be expected to pay for the student's educational costs in the upcoming school year.

In order to qualify for need-based aid, a student must have educational expenses in excess of her EFC.⁷ Based on this level of need, the student may be eligible for grant money from the federal or state government or from the institution the student attends. The student may also receive merit-based institutional aid or private scholarships.

If these grants and scholarships do not cover the student's entire need, she will be

⁶Individuals who have previously been convicted of drug-related felonies, or males over the age of 18 who refuse to register for Selective Service, are generally not eligible for federal financial aid.

⁷The definition of educational expenses is quite broad and includes tuition and fees, room and board, books and supplies, transportation, and other miscellaneous expenses.

eligible for subsidized Stafford loans.⁸ The federal government pays the interest on these loans as long as the student is enrolled at least half-time, and for a period of six months after the student is no longer enrolled. Students can borrow through this program up to a grade-level specific cap: \$2,625 for first-year students, \$3,500 for second-year students and \$5,500 for upper-year students. A student is thus eligible for the maximum loan amount when the difference between her total costs and the sum of the EFC and other grants is greater than the loan limit for her grade level.

After filing a FAFSA, the student receives an award letter from the college or university she is attending (or from the schools to which she applied if she is a first-time student). The letter contains an itemized list of the amounts and types of aid the student has been awarded. Although students maintain the right to reject individual types of aid and even to change individual amounts if they desire, the default choice is to accept the amount of the interest-free loan awarded by the financial aid office.⁹ Thus students must intentionally choose not to receive subsidized loans for which they are qualified.

Our identification relies on a feature of the loan disbursement process. Financial aid funds must first be applied to expenses billed directly by the school, including tuition and fees, dorm room rental, and cafeteria meal plans. Any aid funding in excess of the school's direct charges is then distributed to the student through a refund check. Because aid eligibility is determined based on the entire student budget and not just tuition, these refund checks are not uncommon.¹⁰

⁸Students with exceptional need are also given access to interest-free Perkins loans. The Perkins loan program affects far fewer students than the Stafford program, and the loans are administered by each institution separately, so we choose to focus our attention on the larger federal program. The student may also receive a work-study award which is a promise from the government to pay a portion of the student's wages if she obtains employment. Because both of these awards are also need-based, we will be conservative in the empirical section by subtracting Perkins and work-study awards from need before categorizing a student as eligible or ineligible for the Stafford program.

⁹In order to receive their loans, first-time borrowers must sign a Master Promissory Note and receive loan counseling related to borrowing through student loans. A student can currently fulfill both of these requirements online. In subsequent years, the student does not need to take any additional action beyond the normal FAFSA application process to receive the entire amount of loan funds she has been offered.

¹⁰Recent statistics based on administrative data from our institution reveals that 36 percent of aid recipients were

Figure 4.1 presents the combination of circumstances required to be eligible for a refund check. Students living off-campus receive refund checks earmarked for room and board whenever the sum of their financial aid funds exceeds the cost of tuition and fees. All else equal, students who attend schools with lower tuition or who receive larger grant awards are more likely to be eligible for refunds. On-campus students will never have direct control over the money because it is automatically applied to their educational expenses, including room and board. Thus, the disbursement process creates variation in the short-term liquidity to which students are exposed, even though the financial benefits of the loan are the same.

4.3 A Self-Control Motive?

With these institutional details in mind, we explore a stylized version of the decision facing an enrolled student who receives an aid award that includes a subsidized Stafford loan. We begin by demonstrating that rejecting the loan cannot be the optimal decision for a student with stable, time-invariant preferences. We then discuss models from the literature under which rejecting a Stafford loan, and thus foregoing the government subsidy, can be utility improving if doing so serves as a constraint on the behavior of an impatient future self.

Students' borrowing and consumption decisions take place over three periods: prior to attendance, during school, and post-graduation. In the initial period, the student is offered a financial aid package, one component of which is a subsidized loan in the amount of \bar{S} . She decides whether to accept or reject the loan, and once she has chosen, no future actor can alter this decision.¹¹ We refer to the actor who

issued refund checks.

¹¹We discuss the choice as binary, despite the fact that students can choose to borrow only a fraction of the amount they are offered. While we were originally interested in students who took partial loans because they appear to exhibit sophisticated behavior, data limitations do not allow us to distinguish between volitional partial borrowers and students who failed to receive the full amount because they dropped out or graduated. In addition, the structure of the award letter often frames the choice as an all-or-none decision, with the reduction option buried in the fine

makes these decisions as the “Borrower.”

In the next period, during school, the student takes her previous loan-taking decisions as given, receives financial aid and other exogenous income (e.g. parental support), and pays tuition. We denote income available after paying tuition as I_2 . The student then decides how much to consume while in school, c_2 , and how much to save until the final period. Savings earn a (nominal) interest rate of r per period. We assume that, other than her access to student loans, the student cannot access alternative credit markets.¹² We refer to the actor who makes these decisions as the “Student.”

In the final period, post-graduation, the student receives income I_3 , repays the principal on any loan she has accepted (the government pays the interest), and consumes the remainder of her income. We refer to this actor as the “Graduate.”

The decision whether to borrow is equivalent to a choice between two in-school budget sets, shown in Figure 4.2.¹³ If the Borrower chooses to reject the loan, the Student will be faced with the budget set AB . Choosing to borrow provides the Student with budget set CD . Notice that borrowing has two effects on the choice set available to the Student. First, the loan relaxes the Student’s credit constraint, allowing her to consume more than I_2 while in school by borrowing against future income, I_3 .

Additionally, the government pays the interest that would normally accrue on the loan, $r\bar{S}$. This increase in lifetime income results in a vertical shift upward in the budget set. Proving that a rational student who is not subject to problems of temptation and self-control should strictly prefer to accept the loan requires nothing

print.

¹²While allowing for students to borrow from higher-cost private lenders would add a degree of realism, the intuition underlying this section would be unchanged. We maintain the assumption for expositional simplicity.

¹³For a more general discussion of how self-control concerns cause consumers to prefer a subset of choices to the entire set, see (Gul and Pesendorfer 2001).

more than an assumption of locally non-satiated preferences as the choice set available without borrowing is a proper subset of the choice set induced by accepting the loan.

4.3.1 Behavioral motivations

For students who tend to indulge in immediate gratification, however, accepting the loan may not be the optimal choice. In order to take advantage of the government interest payment $r\bar{S}$, the Borrower must relax a constraint on in-school consumption, making it far easier for the Student to overspend.

Dating back to the pioneering work of Strotz (1955), consumer choice theory has developed models which allow for restrictions of future choices to improve lifetime utility. More recently, Laibson (1997), O'Donoghue and Rabin (1999) and Frederick, Shane, Loewenstein and O'Donoghue (2002) explore time-inconsistent preferences in which consumers consistently prefer immediate consumption, and discuss the utility improvement that commitment devices can create. The economic theory of self-control in which consumers use personal rules to regulate the impulses of their current and future "selves" began with Thaler and Shefrin (1981), and was further developed by Loewenstein and co-authors (Loewenstein and Thaler 1989, Prelec and Loewenstein 1998) and Benabou and Tirole (2004). In each of these models, some consumers are willing to pay to restrict their future consumption because they anticipate that doing so will help them avoid overspending.

The appendix contains a more formal treatment of one of these potential mechanisms, using quasi-hyperbolic discounting to create a time inconsistency in preferences. Here we highlight the main insights of that analysis. When the Student is relatively patient, it is not worth discarding the government subsidy to change her consumption behavior. The Borrower allows the Student access to the loan funds

even though she knows the Student will consume more than she would like her to. For a sufficiently impatient future actor, however, rejecting the loan can be utility improving.

Access to an additional commitment device that would lessen the temptation caused by the increased liquidity could mediate an agent's desire to reject a loan for self-control reasons. For example, as mentioned in the previous section, students who live on-campus will have their loan funds applied directly to their educational expenses. This aspect of the distribution process reduces the Student's short-term liquidity and guarantees that loan funds will be spent on "Borrower-approved" expenses. Either or both of these features may help the borrower control her consumption impulses. In contrast, Students who receive refund checks must manage these funds over the course of the semester, facing the constant lure to spend more out of a temporarily high bank account balance. Because of this increased temptation, we expect students eligible for refund checks to reject their loans more often than their on-campus counterparts.

Thus far we have argued that rejecting an interest-free loan can serve as an effective, albeit costly, method of reining in one's self-control problem and that students who would receive refund checks should be especially likely to make use of this mechanism. An important remaining concern is whether students will or should choose this method over a less-costly alternative. For concreteness, we consider the option of depositing the student loan funds into a certificate of deposit (CD).

There are several reasons to believe that such an alternative does not represent a viable option for our study population. First, depositing the money into a CD requires a significant amount of financial savvy that needy college students likely lack. We argue that these students are "sophisticated" only in that they perceive

their own personalities and habits, not that they understand the nuances of the financial system as well as readers of this journal.¹⁴ Additionally, the penalties on CD's (often only a few months' interest) are probably not large enough to deter students from accessing these accounts for perceived "emergencies." Finally, this strategy is vulnerable to the exact problem it seeks to correct. Because the money does not arrive until after school begins, the Borrower must rely upon the impatient *Student* to deposit the money into the CD. For each of these reasons, rejecting the loan represents the best choice for students aware of their own sufficiently severe self-control problems.

While we have described the model in the context of a within-person principal/agent problem rather than as a parent/child problem, the latter problem would have much the same flavor. In order to address concerns over parental influences, we directly examine parental financial support data in the empirical section. Nonetheless, the primary result is that seemingly irrational aversion to debt can be a rational response to a difference in the discount rates between the economic actor who makes the borrowing decision and the actor who makes the consumption decision.

4.4 Empirical Results

4.4.1 Data

We use the 1999-2000 and the 2003-2004 cross-sectional waves of the National Postsecondary Student Aid Study (NPSAS) to investigate the predictions of the self-control model. This unique data source combines administrative financial aid data from the school and from the National Student Loan Data System (NSLDS), information submitted by students and parents on their aid applications, and survey

¹⁴According to the 2004 Survey of Consumer Finances, only 12.7% of all households held CD's, with rates much lower for moderate-income families who would likely qualify for financial aid.

responses from the students during the school year.¹⁵ In addition, the NPSAS contains detailed institution characteristics and individual student information, such as GPA, SAT scores, school location and selectivity, and demographic characteristics.¹⁶

To focus our analysis on the individuals toward whom the financial aid system is most directly targeted, we restrict our sample to full-time, full-year undergraduate students enrolled at one four-year public or private non-profit institution for a full academic year. The sample includes only those students who applied for financial aid and whose unmet need exceeded the subsidized loan maximum.¹⁷ Therefore, within a grade level, all students are eligible for the same interest-free loan amount.¹⁸ These students are usually those considered “representative” needy college students, and those most likely to be burdened with loans upon completion of college.

The fact that about one-sixth of our sample of needy students does not accept interest-free loans is striking. Table 4.1 provides a descriptive look at the data, emphasizing that a significant fraction of students in each demographic group do not take the loan.¹⁹ The most dramatic differences in take-up rates are by race, where Hispanic and Asian students are nearly twice as likely to turn down the loan as

¹⁵One additional advantage of the NPSAS is that students make their financial aid decisions prior to being selected into the survey. Thus, there is no additional pressure to make the “correct” decision as a result of being in the study.

¹⁶We use the restricted version of the data for our analysis. A confidential data license agreement with NCES is required in order to obtain these data.

¹⁷This selection criterion introduces some heterogeneity by admitting needy students as well as more financially able students at high cost schools. To mitigate this issue, we further restrict our sample to students who, if they accepted their student loan, would owe no more than an additional \$10,000 in tuition. In addition, due to concerns regarding the quality of some responses in the NPSAS, we restrict our dataset further to exclude individuals whose values of student budget and Stafford loan amount were imputed. For similar accuracy concerns with the same variables, we also excluded individuals who were independent or lived with their parents, and students who were not born in the United States. Without these restrictions we risk making significant classification errors.

¹⁸Note that this sampling frame requires greater unmet need for upperclassmen to be included in the sample than for freshmen and sophomores. We have rerun our analysis using only students who have \$5,500 in unmet need regardless of grade level, and the results are qualitatively unchanged.

¹⁹We refer to students who applied for financial aid and who were determined to be eligible for subsidized loans according to the federal formula but who do not receive any loan funds as having rejected the loan. Because this measure is all that our data allow, we were concerned that a significant fraction of our observed rejections might be the result of measurement error where we had incorrectly classified a student as eligible. We asked a senior financial aid administrator at our own institution whether this number agreed with administrative data. She informed us that 18 percent of Stafford borrowers actively turned down their subsidized loans by logging on to the financial aid system and canceling the loans. Another significant fraction “passively” rejected the loans by failing to return the necessary paperwork for disbursement. This communication suggests that measurement error in eligibility does not comprise a large component of this descriptive statistic. Because we cannot distinguish between active and passive rejection in the data, we refer to all students who applied for aid and who qualified for loans but did not receive the loan as having turned it down.

white and African-American students. This could be due to persistent differences in distaste for debt by culture, differences in self-control, information, or other factors. These results serve as a reminder that while self-control may be an important determinant of the borrowing decision, it is certainly not the only one. Racial differences in loan rejection are not the emphasis of this paper, though we investigate these racial gaps in more detail below.

Students with high unmet need are much more likely to take the loan. This difference confirms that, on average, the loans are being used by those who need them most. Students from families that earn less than \$50,000, roughly the median in our sample, are actually more likely to turn down the loan than are students from wealthier families. Recall, however, that these families are also likely to be eligible for larger grant awards and scholarships. Because family income and need are negatively related as a result of the federal aid formula, it is difficult to determine whether either factor independently drives this result. More generally, this table reveals that students of all types reject the interest-free loans at non-trivial rates.

4.4.2 Evidence for a Self-Control Explanation

Less than full participation in the interest-free loan program is consistent with a number of hypotheses, including taste-based debt aversion, non-pecuniary “hassle” costs of borrowing, or a lack of information. The self-control discussion presented in section 4.3 provides a behavioral reason for rejecting subsidized loans. Unlike the other candidate explanations, this potential motivation provides an additional testable hypothesis: Students should be particularly unwilling to accept their loans when doing so would lead to a larger increase in short-term liquidity.

Recall from Figure 4.1 that some students living off-campus will receive financial aid funds earmarked for room and board in cash which they must manage over the

course of the semester, while others have funds applied directly to their educational expenses. If students turn down loans solely because they dislike debt or because they do not understand the benefits, the form in which the loan funds are disbursed should make no difference. In contrast, if self-control is an important factor in students' take-up decisions, they should be especially reluctant to accept the loans if doing so results in a refund check.

One approach would be to compare take-up rates between on-campus and off-campus students whose loan funds would pay for room and board. The results of this type of analysis are shown in the first column of Table 4.2. Students who live off-campus are 8.0 percentage points less likely to accept their loans than are students in the same financial situation living on campus. We are concerned, however, that living off-campus may be associated with greater loan rejection for reasons other than the "refund check" effect. To address this issue, we create a counterfactual using the difference in take-up rates between on-campus and off-campus students whose financial aid benefits, including subsidized loans, do not exceed tuition. Students in the counterfactual sample are also eligible for the maximum subsidized loan amount (see Figure 4.1). Assuming that any omitted factors affecting both housing choice and take-up are similarly distributed across these two financial aid situations, this specification controls for differences in take-up not related to the "refund check" effect.

The results of this difference-in-differences specification are presented in the second column of Table 4.2. We estimate linear probability models of the form

$$y_i = \alpha_1(OFFCAMPUS)_i + \alpha_2(ROOMBOARD)_i \\ + \alpha_3(OFFCAMPUS * ROOMBOARD)_i + X_i\beta + \nu_i.$$

The dependent variable is a dummy variable for whether a student accepted his/her interest-free loan (1=accept). The independent variables are indicators for residence (1=off-campus) and for whether loan funds, if accepted, would pay for room and board (1=yes).²⁰ The interaction of these two variables creates an indicator for whether the loans are distributed in cash (1=refund check). The coefficient on this variable, α_3 , is our primary parameter of interest and our measure of the effect of a potential increase in liquidity on loan take-up.²¹

After netting out any on/off-campus differences unrelated to increased liquidity, the resulting coefficient remains strongly negative at 7.3 percentage points (column 2). Figure 4.3 presents an important specification check for the difference-in-differences methodology. The graph plots loan acceptance rates against the amount of aid in excess of tuition (including the loan). The continuous lines represent the results of local linear regression smoothing, while the individual points give unweighted averages of bins with a \$1000 half-width. The darker lines and points represent the off-campus sample; the lighter plots represent the on-campus sample. For students whose loans pay only tuition (to the left of zero), the relationship between aid and acceptance is quite similar across housing situations. However, for students whose loans would pay for room and board, the on and off-campus acceptance rates quickly diverge as the amount of excess aid increases. These differential trends following the

²⁰We limit the sample to students who live either in on-campus housing or off-campus, but not with their parents. Students who live with their parents are typically given a much smaller housing allowance than students living off-campus independently, and thus it is more difficult to determine their eligibility.

²¹We have run linear probability models because our primary parameter of interest is the interaction term, which can be difficult to interpret in probit and other MLE models. Most of the variables we include are categorical, and, as a result, none of the predicted values are greater than one or below zero.

cutoff arise even though the local linear regression does not impose any structure on the shape of the estimated relationship.

This result supports the hypothesis that off-campus students are differentially rejecting the loans to avoid receiving large easy-to-spend refund checks. Additionally, the relatively small change in slope at zero aid for on-campus students supports using this group as a counterfactual for off-campus students' acceptance rates were they to live on-campus. Figure 4.3 demonstrates that most of the divergence in acceptance rates occurs quickly after the zero-dollar cutoff. Consequently, we continue to report categorical difference-in-differences results rather than results that include the size of the potential refund as a continuous variable.

Adding controls for race, gender and year in school (and thus indirectly for the amount of loan eligibility) in column 3 of Table 4.2 reduces this "refund check" effect only slightly, by less than one half of one percentage point. Importantly, we find little empirical evidence to support a selection across housing options argument as there is no significant difference in take-up between locations for those students whose loans pay only tuition.

These results provide strong evidence that liquidity concerns play a role in determining loan take-up decisions. There are certainly other factors affecting the borrowing choice. In particular, students with a smaller immediate need for funding (accepted loan funds cover more than tuition) are 6.5 percentage points less likely to accept the loan. Nonetheless, students with similar funding needs are an *additional* 7.0 percentage points less likely to take the loans when they would receive cash.

While we have described the student's self-control dilemma in the context of a within-person principal/agent framework rather than as a parent/child problem, the latter problem would have much the same flavor. To address parental influences, we

include indicators for whether parents help pay tuition or other financial support, which includes housing expenses, in column 4, our preferred specification. While students whose parents help pay tuition are less likely to take the loan, the “refund check” result remains even after including measures of parental assistance. All else equal, students who would be exposed to additional short-term liquidity are 7.1 percentage points less likely to take the loan.

Table 4.3 presents a series of additional robustness checks on our main result. One alternative explanation for these results is that housing decisions and neediness are serving as proxies for other characteristics of the school the student attends. The NPSAS provides a broad range of school-level characteristics, which we add to our preferred specification from Table 4.2. The results are largely unchanged; the point estimate on the “refund check” effect falls by only 0.5 percentage points and remains significant at the five percent level.

The second column of this table addresses the question of whether the “loan funds in cash” effect can best be interpreted as evidence for a self-control or a parental control explanation. We exclude school attributes but include a full battery of parental assistance measures and parental characteristics. The additional parental assistance measures are insignificant, and whether at least one parent has some college experience is insignificant as well. That our result still holds suggests that self-control concerns are independent of the role of parents, though a student’s parents may still influence her take-up decision. As a further test, we estimated our preferred specification separately on the sample of students who received parental assistance, and then using only those who did not, and generated nearly identical results.²²

The third column of Table 4.3 presents the most demanding test of the data,

²²We have also estimated similar specifications including a cubic in parental income, which does not substantively affect the point estimate. All results not reported in the tables are available from the authors upon request.

adding college-specific fixed effects. The point estimate of the coefficient of interest remains negative, but is no longer statistically different from zero ($p = 0.12$), nor statistically different from the results of previous columns. This specification uses only within-school variation in the housing locations and financial situations of students and has the potential to remove unobserved institutional characteristics that affect loan take-up decisions. However, this specification also ignores the potential endogeneity of the decision to live on or off campus. If, for example, students with self-control problems choose to live on campus to ensure that aid funds go toward appropriate expenses, this selection will tend to minimize differences in on and off campus take-up rates, especially within schools. We address this possibility in detail below. Nonetheless, even when controlling for a multitude of possibly confounding factors, our results which use both between- and within-school variation remain economically and statistically significant, and are consistent with a self-control explanation.

There is an alternative economic story consistent with the results presented thus far that does not require the existence of self-control problems. Because housing charges are traditionally due at the beginning of a semester, on-campus students (or their parents) may need to borrow more often than their off-campus counterparts in order to pay this bill on time. In order to rule out this alternative explanation we collected data on whether students' schools offered an installment payment plan.²³ We verified that 88 percent of our schools, representing 92 percent of our students, provide access to a payment plan that allows students to spread their directly-billed charges (including housing) over several installments. When we restrict our sam-

²³To do so, we began with lists of participating institutions from two private firms that provide payment plans for multiple schools. We conducted web searches to determine whether schools that did not contract with either of these providers offered similar plans, and only used the 2003/04 NPSAS data, as payment plan options may have changed since the 1999-2000 school year.

ple to only those students with access to an installment plan, the coefficient on the interaction term is -4.4 percentage points in our preferred specification, and is significant at the 10 percent level ($p < 0.09$). This robustness check implies that the “loan funds in cash” effect we are identifying is not due primarily to the immediate liquidity needs of on-campus students at the start of each semester.

4.4.3 Selection on observables and unobservables

The stability of the point estimates when controlling for a host of potentially confounding factors suggests that the distribution of these covariates is roughly equal across the four housing location/financial situation categories. Table 4.4 investigates this balance directly. Students from all demographic types can be found in each category, usually in roughly the same proportions. Most of the demographic variation across these categories can be attributed exclusively to housing location or to financial situation, rather than to the “refund check” interaction. Additionally, the table reveals that our comparison group (those whose loans pay only tuition) are only somewhat better off than the group potentially eligible for a refund check. The difference in Estimated Family Contribution is only about \$4,500. Our comparison, therefore, is not between poor and non-poor students, but rather between needy and somewhat less needy students.

There are a few cases, however, where demographic variables differed systematically by refund check status. This imbalance presents a challenge to the difference-in-differences specification. As an example, suppose that minority students are especially wary of borrowing to pay for expenses other than tuition. If more of these students live off-campus than on-campus, then the measured interaction would be negative even in the absence of any direct effect of receiving the loan in cash. An analogous argument can be made for any of the other unbalanced covariates.

To address this alternative explanation, we report a series of additional regressions that include the interaction of unbalanced covariates with both the off-campus and “loan pays room and board” dummies. The results are presented in four appendix tables. Appendix Table 1 includes interactions with students’ race and gender. Point estimates for the refund check coefficient range from -5.5 percentage points to -7.1 percentage points, and each is statistically significant at the .05 level. The second appendix table focuses on grade level. Here the coefficients of interest range from -7.1 percentage points to -8.7 percentage points and all estimates are significant at the 0.01 level.

The final two tables focus on whether differential characteristics of the schools the students attend across the four categories can explain the liquidity effect we identify. Appendix Table 3 interacts the two determinants of refund check status with the type of school, while the final table interacts these variables with measures of cost. In the third table, the interaction ranges from -6.9 percentage points to -7.9 percentage points. Each is significant at the 0.01 level. In the final table, the point estimate falls slightly, and we lose some precision, but the refund check effect is still significant at the .10 level. By helping to rule out simple composition effects, these robustness checks provide further evidence that variation in take-up across these groups is driven by exposure to different levels of short-term liquidity.

Nevertheless, it is still possible that differential selection into off-campus housing based on omitted factors drives the difference-in-differences results. To address this concern, we estimate instrumental variables (IV) regressions using the fraction of undergraduates that the student’s school could place in on-campus housing as an instrument for living off-campus.²⁴

²⁴These data do not appear in the NPSAS files. We obtained the data from the Integrated Postsecondary Education Data System (IPEDS), <http://nces.ed.gov/ipeds/>, and merged them into our dataset using the school identifiers.

Beds per student, viewed as a supply restriction for on-campus housing, is an excellent candidate for use as an instrument. Students who attend schools with smaller housing capacities are certainly more likely to live off-campus. Additionally, there is little reason to think that a school’s housing capacity *directly* affects borrowing decisions. Below, we investigate concerns that students’ enrollment decisions might be affected by factors correlated with housing capacity such as school quality or generosity.

Column 1 of Table 4.5 shows the results of the just-identified IV regressions, using housing capacity as an instrument for on-campus housing, and housing capacity interacted with whether the accepted loan funds pay for room and board as an instrument for the “refund check” effect. Both instruments clearly meet the test for relevance, with first-stage F-statistics over three hundred. These results suggest that, if anything, endogenous selection into off-campus housing *attenuates* the OLS estimates of the effect of receiving aid funds in cash.

To address concerns of instrument exogeneity, we re-estimated our IV specification including measures of institutional quality and the size of the school’s endowment. The specification presented in column 1 includes dummy variables for urbanicity and Carnegie classification, and the results are largely unchanged when we use other measures of school quality, such as the 25th or 75th percentile for incoming students’ SAT scores (column 2), or the institution’s graduation rate (column 3). Finally, a potential concern about a university’s endowment, wherein a wealthier school might both have more on-campus housing and provide more generous aid and scholarships, would bias us against finding any “refund check” effect.²⁵

While these checks on exogeneity are not exhaustive, these additional IV speci-

²⁵For a subset of our sample, we included a measure of the university’s endowment from the IPEDS data. The school’s endowment is nearly uncorrelated with the institution’s housing capacity, and the IV results for this sub-sample were unaffected.

cations suggest that housing capacity affects loan take-up only through the decision whether to live on or off campus. Taken together, the fully interacted difference-in-difference and instrumental variables results in this section indicate that endogenous selection into off-campus housing does not artificially generate differential take-up rates by housing status.

4.4.4 Evidence of Planning Ahead

Thus far we have presented evidence that students who would receive a part of their loan funding in cash are less likely to accept the loan. Viewed through the lens of a self-control model, these empirical results support the hypothesis that students are rejecting their loans as a commitment device against overspending. These results, however, provide no direct evidence that students are rejecting their loans as an optimal forward-looking strategy. Tables 4.6 and 4.7 present two separate tests to determine whether students are indeed planning ahead to reject these loans.

When filing a FAFSA in the spring, aid applicants must report whether they would like loans included in their financial aid package, as well as where they expect to live in the fall. The residential choice and the preference for loans do not directly determine the aid package offered to students in most cases.²⁶ Table 4.6 provides further evidence that the rejection of loans by potential refund recipients is intentional. These regressions replicate columns 2 and 4 of Table 4.2, but use the student's desire for loans as a part of the aid package as the dependent variable. Students who would get a refund are more likely to report that they are not interested in loans than are other groups. Their desire to avoid borrowing reveals itself not only in their eventual

²⁶Further correspondence with the financial aid administrator at our own institution confirmed this fact. The primary concern among administrators is that students will respond that they are not interested in loans in an attempt to secure more grant funding. As a result, subsidized loans are included in an aid package even if a student has reported not being interested. In addition, subsidized loans are an entitlement program, as students demonstrating eligibility cannot be denied these loans. Notably, in our sample more than half of the students who reported not being interested in loans eventually borrowed their entire grade-level maximum.

behavior but also in their stated intentions before making any borrowing decisions.

The first two columns of Table 4.7 replicate columns 2 and 4 of Table 4.2, again using whether the student accepts subsidized loans as the dependent variable, with the sample restricted to students who correctly anticipated whether they would live on or off campus. Our main result is even stronger and more significant when we restrict our sample to these individuals. Columns 3 and 4 show that the coefficient of interest for the sample who *incorrectly* predicted their housing location is approximately zero. Thus, our findings are driven primarily by students who could have correctly predicted whether they would receive a refund when making their borrowing decision. These results provide further support for the hypothesis that students' failure to receive interest-free loans is the result of the type of forward-thinking decisions made by "sophisticated" consumers aware of their self-control problems.

4.5 Implications for policy and further research

Our analysis suggests that self-control motives play a significant role in students' decisions to reject interest-free loans.²⁷ Students choose not to borrow despite government subsidies, and they are particularly less likely to borrow when doing so provides them with a large amount of easy-to-spend cash. This behavior is consistent with the optimal choices of sophisticated economic actors with self-control concerns.²⁸ Other theories can explain the descriptive results, but no competing theory predicts that students will be exceptionally averse to borrowing when the funding is distributed in cash. These empirical results provide some of the first non-laboratory evidence of consumers choosing to limit their own borrowing and consumption despite the

²⁷It is likely that we have identified only a portion of the behavior induced by self-control problems. Our estimates omit any effect resulting from students choosing not to apply for aid at all to avoid being faced with the temptation of loan funds.

²⁸It is straightforward to show that students who do not anticipate their own impatience will also accept the entire loan for the same reason the rational student does. That their impatience leads to overconsumption is a standard result, and is also easily shown.

financial costs. In doing so, these results also suggest that many types of behavior previously thought to be “irrationally” debt-averse may, in fact, result from consumers trying to constrain their own impulses.

These results also have important implications for policy decisions related to subsidizing student borrowing. First, we find that a significant fraction of students who reject their loans would have used the money for living expenses rather than for tuition and fees. This finding suggests that the loans end up going to students who actually use the money for school rather than to those who are gaming the system. However, many needy students, particularly from minority populations, do not reap the benefits of loans that our data suggest they would find unambiguously financially beneficial.

A second policy consideration concerns the efficiency of the design of the current loan system. Recent work on the optimal choice of default rules reveals that setting the default far away from decision-makers’ true optima may be welfare improving (Choi, Laibson, Madrian and Metrick 2003). Accentuating the difference between the optimum and the default can cause a greater fraction of decision-makers to reject the default and choose their personal optimum. This consideration must be balanced with a desire to set the default close to the modal optimum to minimize the total costs of switching that agents must incur. In the case of subsidized loans, setting the default to the maximum accomplishes both goals of optimal default rules by making the default the mode decision and by maximizing the difference between the default and the optimum for those who wish to deviate.

Third, potential policy solutions can directly reduce the burden of increased liquidity and increase student participation in this need-based program. For example, aid administrators could offer students access to educational spending accounts sim-

ilar to flexible spending accounts currently used for medical expenses. Schools could place any aid in excess of tuition into these accounts, and students would need to provide evidence of approved education-related expenses in order to spend these funds. Account balances would earn interest. Upon leaving school, any remaining funds could be applied directly to the student's outstanding loan balances. In this way, all students could receive the benefits of the subsidized loans without needing to manage large increases in liquidity.²⁹

The results presented in this paper are some of the first evidence that individuals are willing to pay to restrict their choices in a non-experimental setting, and suggest several avenues of additional research. While our data support an explanation for the surprisingly low take-up of interest-free student loans based on temptation and self-control, further survey work or a randomized experiment could directly confirm this channel. The shift from grant-based aid to loan-based aid may also affect educational decisions more broadly by influencing enrollment and school choice, and the extent of these effects is certainly worthy of further consideration.

Finally, by interacting hyperbolic discounting models with the particular features of this credit market, we have shown that impatience and a need for self-control can induce debt-averse behavior. Although the "rational" choice is less clearly defined in other contexts, we expect that this insight could help explain unresolved questions in similar economic situations, such as repaying car loans or home mortgages ahead of schedule. Further research is needed to determine how important a role self-control plays in other credit markets.

²⁹Alternatively, financial aid offices could offer the "excess" aid in monthly installments. When we compared schools on the semester calendar (where students receive two checks each year) to schools on the quarter calendar, we found that our results were stronger (though not significantly so) when the checks were delivered less frequently. To our knowledge, no university currently offers either educational spending accounts or monthly aid checks.

4.6 Figures and Tables

Figure 4.1: Student circumstances and refund eligibility

Housing location:	Accepted loan funds:	
	Do not pay room and board	Pay room and board
On-campus	Not eligible	Not eligible
Off-campus	Not eligible	Eligible for refund check

Figure 4.2: Choosing to borrow results in a larger choice set.

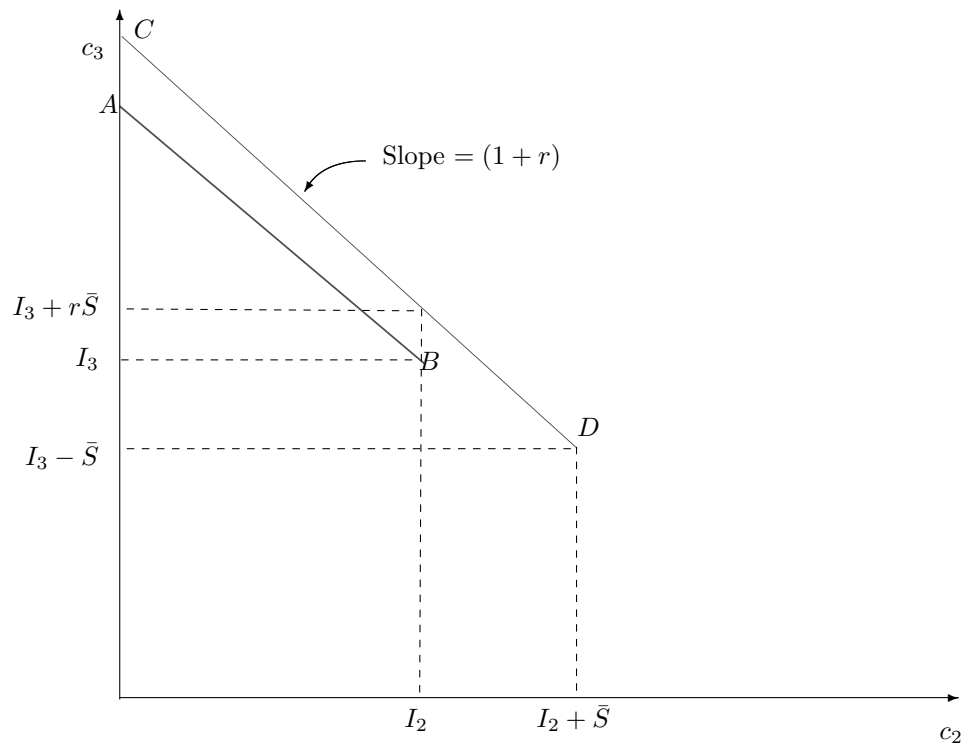


Figure 4.3: Loan Acceptance Rates by Aid in Excess of Tuition.

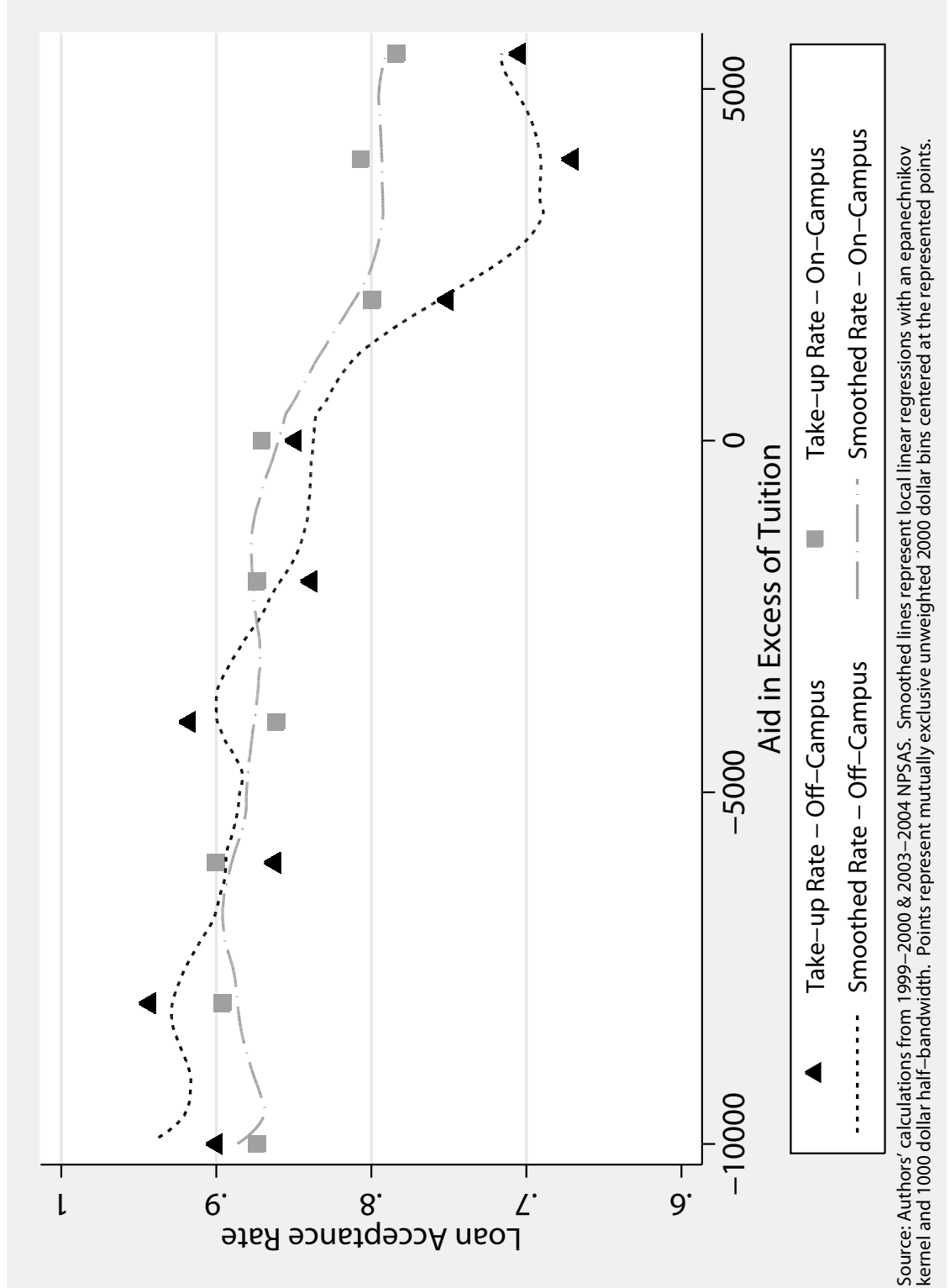


Figure 4.4: The optimal borrowing decision follows a cutoff rule.

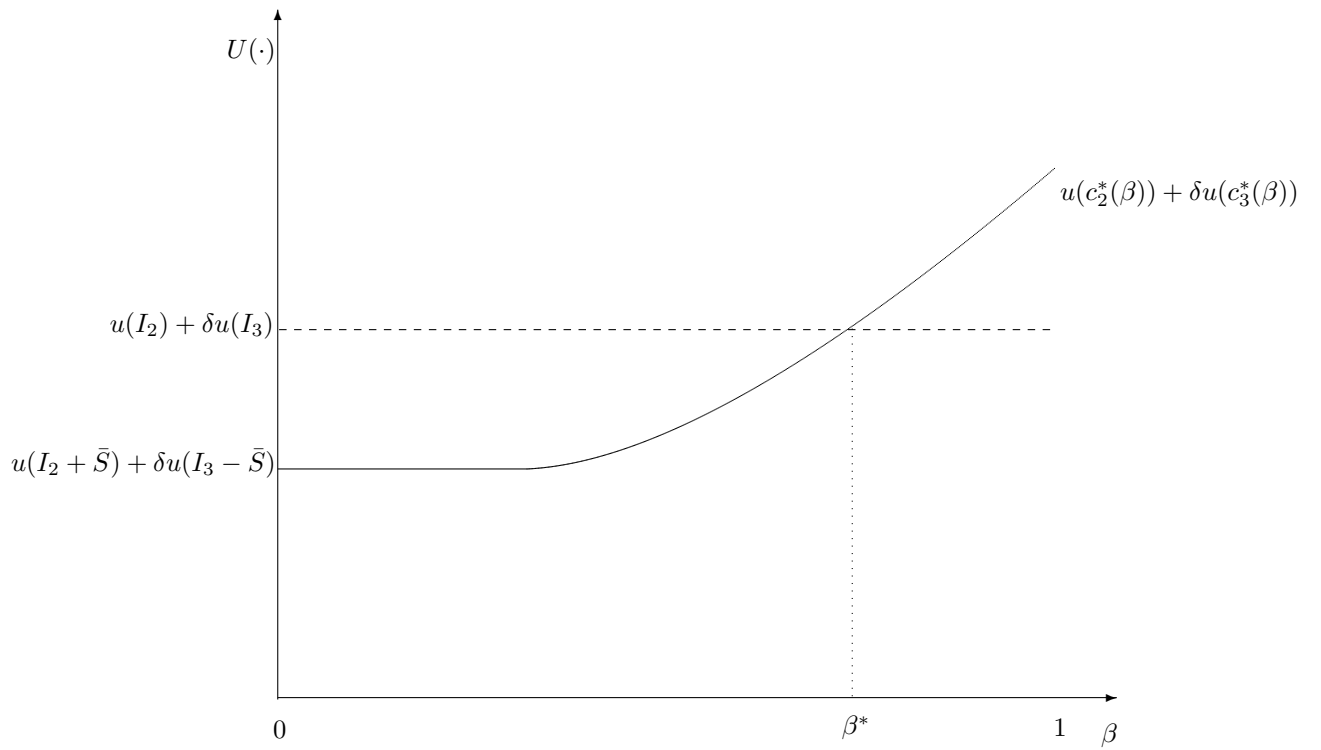


Table 4.1: Descriptive Statistics of Subsidized Stafford Loan Take-up

	% Reject loan	N
Full sample	16.9%	5531
Grade level		
Freshmen	16.0%	2469
Sophmores	20.5%	1049
Juniors	14.5%	825
Seniors	17.0%	1188
Race		
White	15.8%	3938
African-American	15.2%	834
Hispanic	27.1%	402
Asian	27.4%	179
Other (incl. multiple)	14.0%	178
Gender		
Male	17.5%	2416
Female	16.4%	3115
Parental support		
Parents do not pay tuition	16.6%	2309
Parents pay tuition	17.4%	2662
Parental income		
Below \$50,000/year	18.5%	3385
Above \$50,000/year	14.3%	2146
Cost of attendance after grants/scholarships		
Below median	20.1%	3095
Above median	12.7%	2436
Parental education		
HS degree or less	17.0%	1516
Some college or higher	16.6%	3863
Standardized test scores		
Below median SAT / ACT	14.6%	982
Above median SAT / ACT	19.4%	1008
Survey Year		
1999-2000	16.2%	2170
2003-2004	17.3%	3361

Source: Authors' calculations using the NPSAS 99/00 and 03/04.

Note: We restrict the sample to full-year, full-time, US-born, dependent, undergraduate students at four-year public or private non-profit institutions who do not live with their parents, applied for financial aid, and demonstrated financial need exceeding their grade-level specific loan maximum. We additionally exclude students whose values of student budget and Stafford loan amount are imputed.

Table 4.2: Linear Probability Models for Subsidized Stafford Take-up Rates by Direct Access

Dependent variable: Accept/reject interest-free loans	Eligible ^a	Full Sample		
	(1)	(2)	(3)	(4)
Loan funds distributed in cash (offcampus*room and board) ^b	-0.080** (0.017)	-0.073** (0.022)	-0.070** (0.022)	-0.071** (0.022)
Lives off-campus, not with parents		-0.004 (0.015)	-0.005 (0.016)	-0.011 (0.016)
Accepted loan funds pay room and board		-0.066** (0.012)	-0.065** (0.012)	-0.072** (0.012)
Female			0.013 (0.010)	0.013 (0.010)
African-American			0.017 (0.014)	0.013 (0.014)
Asian-American			-0.107** (0.033)	-0.105** (0.033)
Hispanic			-0.089** (0.022)	-0.093** (0.022)
Other race			0.027 (0.026)	0.028 (0.027)
Parents help pay tuition				-0.043** (0.012)
Financial support other than tuition				0.009 (0.012)
Constant	0.840** (0.014)	0.888** (0.010)	0.888** (0.012)	0.907** (0.014)
Controls for grade level (4 categories)	No	No	Yes	Yes
Observations	2771	5531	5531	5531
R ²	0.01	0.02	0.03	0.03

Source: Authors' calculations using the NPSAS 99/00 and 03/04.

Note: Robust standard errors in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. All models include a dummy for survey year.

a. The sample for the first column includes only students who would receive a refund check if they lived off-campus and accepted their loans. We maintain the sample restrictions from Table 1.

b. Loan funds are distributed in cash when the student BOTH lives off-campus and accepted loan funds pay room and board. See Figure 1.

Table 4.3: Subsidized Stafford Take-up Rates by Direct Access - Robustness Checks

Dependent variable:			
Accept/reject interest-free loans	(1)	(2)	(3)
Loan funds distributed in cash (offcampus*room and board) ^a	-0.066** (0.022)	-0.073** (0.023)	-0.038 (0.025)
Lives off-campus, not with parents	-0.000 (0.016)	-0.010 (0.016)	0.020 (0.018)
Accepted loan funds pay room and board	-0.065** (0.012)	-0.073** (0.012)	-0.044** (0.014)
Constant	0.934** (0.035)	0.914** (0.016)	0.863** (0.019)
Controls for grade level (4 categories)	Yes	Yes	Yes
Controls for race, gender, and parental assistance	Yes	Yes	Yes
Controls for Carnegie Classification, urbanicity ^b	Yes	No	No
Additional parental controls ^c	No	Yes	No
Institution-level fixed effects	No	No	Yes
Observations	5499	5379	5531
R ²	0.04	0.04	0.24

Source: Authors' calculations using the NPSAS 99/00 and 03/04.

Note: Robust standard errors in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. All models include a dummy for survey year.

a. Loan funds are distributed in cash when the student BOTH lives off-campus and accepted loan funds pay room and board. See Figure 1. We maintain the sample restrictions from Table 1.

b. Includes 3 selectivity dummies, 5 categories of Carnegie classification, and 7 categories for degree of urbanicity.

c. Parents' education, whether parents help pay educational expenses, whether parents pay non-housing living expenses.

Table 4.4: Balance of Control Variables

On-campus/off-campus?	On	On	Off	Off
Does loan cover some room&board expenses?	No	Yes	No	Yes
Borrower gets a refund check? ^a	No	No	No	Yes
Female	55.0%	56.8%	56.3%	58.4%
African-American	12.0%	22.7%	8.9%	12.1%
Hispanic	4.8%	7.4%	6.3%	13.0%
Asian-American	2.5%	3.7%	3.6%	3.7%
Other race	2.9%	3.6%	2.8%	3.3%
Masters U.	20.5%	22.4%	18.9%	17.8%
BA U.	20.0%	13.7%	7.6%	4.6%
Oth U.	40.3%	39.7%	55.3%	50.1%
Research U.	19.2%	24.2%	17.6%	27.2%
Highly selective	24.7%	21.1%	23.5%	19.2%
Moderately selective	71.4%	74.2%	68.5%	75.5%
Not selective	4.0%	4.7%	8.1%	5.3%
High parental education	77.5%	67.2%	71.6%	67.8%
Tuition above median	69.8%	36.4%	61.7%	11.3%
Any parental help with expenses	76.7%	67.2%	62.3%	52.8%
After grant cost of attendance above median	75.2%	10.1%	81.1%	14.4%
Parental income above median	58.6%	19.4%	55.6%	20.3%
Test scores above median	52.9%	49.1%	51.9%	48.4%
Demonstrated need above median	62.4%	28.9%	67.1%	31.6%
Has a credit card	44.2%	44.3%	56.6%	59.3%
Carries credit card balance	21.7%	26.6%	43.4%	42.6%
Average year in school	1.85	1.91	2.75	2.75
Expected Family Contribution (EFC) (\$)	5,790	1,976	5,702	2,310
Number of Observations	2152	1755	608	1016

Source: Authors' calculations using the NPSAS 99/00 and 03/04.

a. Loan funds are distributed in cash when the student BOTH lives off-campus and accepted loan funds pay room and board. See Figure 1. We maintain the sample restrictions from Table 1.

Average year in school is coded as 1= Freshman, 2=Sophomore, etc.

Table 4.5: Instrumental Variable Estimates of the Liquidity Effect

	(1)	(2)	(3)
Loan funds distributed in cash (offcampus*room and board) ^a	-0.171* (0.079)	-0.181+ (0.094)	-0.186* (0.093)
Lives off-campus, not with parents ^b	-0.039 (0.065)	-0.096 (0.083)	-0.046 (0.086)
Accepted loan funds pay room and board	-0.016 (0.023)	-0.013 (0.026)	-0.010 (0.026)
Female	0.007 (0.010)	-0.000 (0.011)	-0.001 (0.011)
African American	0.004 (0.014)	-0.015 (0.017)	-0.009 (0.017)
Asian American	-0.107** (0.034)	-0.101** (0.034)	-0.104** (0.034)
Hispanic	-0.054* (0.023)	-0.055* (0.025)	-0.057* (0.024)
Other Race	0.026 (0.027)	0.028 (0.028)	0.029 (0.028)
25th percentile for students' SAT/ACT scores		-0.001 (0.004)	-0.007 (0.005)
75th percentile for students' SAT/ACT scores		-0.007 (0.005)	-0.009+ (0.005)
Graduation rate			0.002** (0.001)
First-stage F-test for Instrument Relevance - Off Campus	374.1	228.8	195.0
First-stage F-test for Instrument Relevance - Loan in cash	296.5	216.6	209.3
Observations	5485	4997	4996

Source: Authors calculations using the NPSAS 99/00 and 03/04, and data from <http://nces.ed.gov/ipeds/>.

Note: Robust standard errors in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. Regressions also include an indicator for year of survey, and controls for urbanicity, Carnegie classification, and grade level listed in Table 3.

a. Loan funds are distributed in cash when the student BOTH lives off-campus and accepted loan funds pay room and board. Whether loan funds are distributed in cash is instrumented by the interaction of accepted loan funds paying for room and board and dorm capacity. See Figure 1. We maintain the sample restrictions from Table 1.

b. Whether student lives on campus is instrumented by dormitory capacity.

Table 4.6: Linear Probability Models of Stated Desire for Loans

On-campus/off-campus?	On	On	Off	Off
Does loan cover some room&board expenses?	No	Yes	No	Yes
Borrower gets a refund check? ^a	No	No	No	Yes
Female	55.0%	56.8%	56.3%	58.4%
African-American	12.0%	22.7%	8.9%	12.1%
Hispanic	4.8%	7.4%	6.3%	13.0%
Asian-American	2.5%	3.7%	3.6%	3.7%
Other race	2.9%	3.6%	2.8%	3.3%
Masters U.	20.5%	22.4%	18.9%	17.8%
BA U.	20.0%	13.7%	7.6%	4.6%
Oth U.	40.3%	39.7%	55.3%	50.1%
Research U.	19.2%	24.2%	17.6%	27.2%
Highly selective	24.7%	21.1%	23.5%	19.2%
Moderately selective	71.4%	74.2%	68.5%	75.5%
Not selective	4.0%	4.7%	8.1%	5.3%
High parental education	77.5%	67.2%	71.6%	67.8%
Tuition above median	69.8%	36.4%	61.7%	11.3%
Any parental help with expenses	76.7%	67.2%	62.3%	52.8%
After grant cost of attendance above median	75.2%	10.1%	81.1%	14.4%
Parental income above median	58.6%	19.4%	55.6%	20.3%
Test scores above median	52.9%	49.1%	51.9%	48.4%
Demonstrated need above median	62.4%	28.9%	67.1%	31.6%
Has a credit card	44.2%	44.3%	56.6%	59.3%
Carries credit card balance	21.7%	26.6%	43.4%	42.6%
Average year in school	1.85	1.91	2.75	2.75
Expected Family Contribution (EFC) (\$)	5,790	1,976	5,702	2,310
Number of Observations	2152	1755	608	1016

Source: Authors' calculations using the NPSAS 99/00 and 03/04.

a. Loan funds are distributed in cash when the student BOTH lives off-campus and accepted loan funds pay room and board. See Figure 1. We maintain the sample restrictions from Table 1.

Average year in school is coded as 1= Freshman, 2=Sophomore, etc.

Table 4.7: Linear Probability Models of Subsidized Loan Take-up
Among Students with Correct and Incorrect Housing Expectations

Dependent variable: <i>Accept/reject interest-free loans</i>	Correctly predicted		Incorrectly predicted	
	housing location (1)	housing location (2)	housing location (3)	housing location (4)
Loan funds distributed in cash (offcampus*room and board) ^a	-0.105** (0.028)	-0.100** (0.028)	-0.031 (0.063)	-0.035 (0.062)
Lives off-campus, not with parents	0.006 (0.020)	-0.002 (0.021)	-0.012 (0.041)	-0.009 (0.040)
Accepted loan funds pay room and board	-0.061** (0.013)	-0.067** (0.014)	-0.066 (0.048)	-0.082+ (0.048)
Constant	0.889** (0.011)	0.901** (0.016)	0.889** (0.033)	0.949** (0.049)
Controls for grade level, gender, ethnicity, and parental help	No	Yes	No	Yes
Observations	4091	4091	599	599
R ²	0.02	0.04	0.02	0.06

Source: Authors' calculations using the NPSAS 99/00 and 03/04.

Robust standard errors in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%.

a. Loan funds are distributed in cash when the student BOTH lives off-campus and accepted loan funds pay room and board. See Figure 1. We maintain the sample restrictions from Table 1.

4.7 Appendix - Formal Treatment Using Hyperbolic Discounting

This appendix considers the borrowing decision whether to accept or reject a subsidized loan in the context of a student with quasi-hyperbolic preferences. In particular, we assume that the Student has $\beta\delta$ preferences, i.e. her discount rate between any two future periods, s and t is δ^{t-s} , while her discount rate between consumption today ($s = 0$) and consumption in a future period t is $\beta\delta^t$. Both β and δ are smaller than unity.

In deciding whether to accept or reject the loan, the Borrower knows that the Student solves the following problem:

$$\max_{c_2, c_3} u(c_2) + \beta\delta u(c_3)$$

subject to

$$c_2 \leq I_2 + S \tag{4.1}$$

$$c_2(1+r) + c_3 = I_2(1+r) + I_3 + rS \tag{4.2}$$

The loan amount, S , is set by the Borrower as either \bar{S} (accept), or zero (reject), in period 1, prior to the Student's consumption decision. Consumption in each period, c_t , can take on any positive value, and I_t denotes income in period t .

We begin with a helpful proposition.

Proposition IV.1. *All else equal, a Student who is more patient consumes more later, i.e. $\frac{dc_2^*}{d\beta} \leq 0$ and $\frac{dc_3^*}{d\beta} \geq 0$, with c_t^* representing optimal consumption in period t . As a result, the Borrower always weakly prefers a more patient Student, i.e. a higher β . The Borrower strictly prefers a more patient Student whenever $\frac{dc_2^*}{d\beta} < 0$.*

Proof. When the constraint from Equation (4.1) binds, the consumption decision

does not change with small changes in β . Thus, when the constraint is binding $\frac{dc_2^*}{d\beta} = \frac{dc_3^*}{d\beta} = 0$. At interior solutions, however, the following Euler equation holds:

$$u'(c_2^*) = (1+r)\delta\beta u'(c_3^*) \quad (4.3)$$

Taking total derivatives and rearranging yields

$$\frac{dc_2^*}{d\beta} = \frac{u'(c_3^*)}{u''(c_2^*) + (1+r)u''(c_3^*)} < 0 \quad (4.4)$$

The budget constraint implies that

$$\frac{dc_3^*}{d\beta} = -\frac{dc_2^*}{d\beta}(1+r) > 0 \quad (4.5)$$

When Equation (4.1) does not bind, therefore, more patient Students consume less during school and more after graduation.

The second part of the proposition follows from this result. Letting $U(\cdot)$ denote lifetime utility, and taking the derivative of the Borrower's lifetime utility function with respect to β yields

$$\frac{dU}{d\beta} = u'(c_2^*)\frac{dc_2^*}{d\beta} + \delta u'(c_3^*)\frac{dc_3^*}{d\beta} \quad (4.6)$$

Rearranging, and plugging in $-\frac{dc_2^*}{d\beta}(1+r)$ for $\frac{dc_3^*}{d\beta}$ yields

$$\frac{dU}{d\beta} = (u'(c_2^*) - \delta(1+r)u'(c_3^*))\frac{dc_2^*}{d\beta} \quad (4.7)$$

Recall that c_2^* and c_3^* conform to Equation (4.3) whenever $\frac{dc_2^*}{d\beta} \neq 0$. Thus this can be simplified to

$$= -(1-\beta)\delta(1+r)u'(c_3^*)\frac{dc_2^*}{d\beta}$$

We have already established that $\frac{dc_2^*}{d\beta} \leq 0$; because $0 \leq \beta \leq 1$, it follows immediately that

$$\frac{dU}{d\beta} \geq 0 \quad (4.8)$$

In particular, $\frac{dU}{d\beta} > 0$ when Equation (4.1) does not hold with equality. When the constraint does bind, $\frac{dU}{d\beta} = 0$ holds with equality.

□

Before characterizing the optimal borrowing behavior, we first determine situations in which the Borrower should always accept the loan.

Proposition IV.2. *Rejecting the loan can only be optimal if doing so causes $c_2 \leq I_2$ to hold with equality.*

Proof. If Equation (4.1) does not hold with equality, then c_2^* and c_3^* follow Equation (4.3). The only effect of taking the loan in this case is that doing so increases lifetime income. Taking the derivative of Equation (4.3) with respect to income yields:

$$u''(c_2^*) \frac{dc_2^*}{dI} = (1+r)\delta\beta u''(c_3^*) \frac{dc_3^*}{dI}$$

Because $u''(\cdot) < 0$, we know that $\frac{dc_2^*}{dI}$ and $\frac{dc_3^*}{dI}$ must be of the same sign. Because they also must sum to one (by Walras' Law), we know that each is positive. Therefore, consumption (and the utility level associated with consumption) increases in both periods when the Student is given access to the loan, and the Borrower will choose to accept the loan.

□

Proposition IV.3. *If $u(I_2 + \bar{S}) + \delta u(I_3 - \bar{S}) > u(I_2) + \delta u(I_3)$, the Borrower will not reject the loan despite self-control problems.*

Proof. Proposition IV.2 implies that turning down a loan optimally for self-control reasons gives the Borrower utility of $u(I_2) + \delta u(I_3)$. Turning down the loan is therefore

optimal only when the consumption choices made by the Student will result in a lower utility. It is trivial to show that when $\beta = 0$, the Student will choose to consume all available income. When the Borrower accepts the loan, therefore, a Student with $\beta = 0$ will consume $(I_2 + \bar{S}, I_3 - \bar{S})$. Proposition IV.1 demonstrates that the Borrower (weakly) prefers the consumption choices of a Student with higher levels of β . Therefore, if $u(I_2 + \bar{S}) + \delta u(I_3 - \bar{S}) > u(I_2) + \delta u(I_3)$ the Borrower should accept the loan for any level of β . \square

The Borrower rejects the loan in order to limit the Student's consumption to I_2 . If accepting the loan would not increase in-school consumption beyond I_2 , she should accept the loan and take advantage of the government interest payment. Similarly, if the Borrower prefers shifting \bar{S} in consumption from the post-graduation period to the in-school period over consuming out of current income in both periods, she should always accept the loan. This could occur, for example, if I_3 were significantly higher than I_2 .³⁰

We can now characterize the decision rule that determines whether the Borrower accepts or rejects the loan.

Proposition IV.4. *If $u(I_2 + \bar{S}) + \delta u(I_3 - \bar{S}) < u(I_2) + \delta u(I_3)$, there exists a unique $\beta^* \in [0, 1]$ such that $u(c_2^*(\beta^*)) + \delta u(c_3^*(\beta^*)) = u(I_2) + \delta u(I_3)$. The Borrower rejects the loan optimally iff $\beta < \beta^*$.*

Proof. We prove this proposition in three parts, beginning with the first claim.

1. At $\beta = 0$, the Student will consume $I_2 + \bar{S}$ and leave $I_3 - \bar{S}$ for the second period. By assumption, this yields a lower utility than consuming (I_2, I_3) . When $\beta = 1$, there is no disagreement between the Student and the Borrower, and

³⁰There are two factors that, in practice, attenuate the gap between I_2 and I_3 . First, parental financial support after college is often significantly lower than while in school. Additionally, student loans must be repaid during the early years of a student's earnings trajectory.

the additional income of $r\bar{S}$ ensures that the Borrower prefers (c_2^*, c_3^*) to (I_2, I_3) . Combining these results with the (weak) monotonicity results of Proposition IV.1, we can draw Figure 4.4. This graph establishes the existence and uniqueness of β^* .

2. We next address the claim that if $\beta > \beta^*$, the Borrower should accept the loan. This result follows directly from Proposition IV.2. Any optimal rejection leads to the consumption pair (I_2, I_3) , which is, by the definition of β^* , inferior to the bundle selected by the Student given access to the loan when $\beta > \beta^*$.
3. Next we prove that $\beta < \beta^*$ is a sufficient condition for rejection to be optimal. To do so, we prove that if the Borrower rejects the loan the Student will consume (I_2, I_3) . Because, by the definition of β^* , the Borrower prefers (I_2, I_3) to (c_2^*, c_3^*) the optimal choice is to reject the loan.

We proceed by contradiction. Suppose this claim is false, i.e. that $c_2^* < I_2$, but that the Borrower prefers (I_2, I_3) to (c_2^*, c_3^*) . This preference implies that the Borrower would like to consume more in the first period than would the Student. Recall that for the Borrower, $\beta = 1$ and we have assumed that the Student's $\beta < \beta^* \leq 1$. A preference for more consumption in the first period with a higher β contradicts Proposition IV.1 which established that $\frac{dc_2^*}{d\beta} \leq 0$.
 $\Rightarrow \Leftarrow$

□

When the Student is sufficiently impatient, the Borrower will reject the subsidized loan. Figure 4.4 demonstrates this cutoff rule and how β^* is determined. The solid line shows the utility the Borrower receives if she accepts the loan and allows the Student to allocate consumption according to her preferences. The dotted line shows

the utility the Borrower receives if she rejects the loan and constrains the Student to consume out of current income.

CHAPTER V

Conclusion

The first two essays provide complementary evidence that low-skilled immigrants are sophisticated economic actors, differentially selecting destinations that provide superior labor market prospects. Unlike any previous study addressing this topic, each of these essays relies on identifiable, policy-driven changes in labor market conditions, allowing for estimation that relies on much less restrictive assumptions. These results show the important role immigrants play in spreading out local shocks across the national economy, an important implication for the local labor markets literature. In addition, these results provide support for a major criticism of attempts to determine how immigrants affect the wage structure by comparing cities that receive large immigrant inflows with cities receiving smaller inflows. Finally, the third essay provides some of the first evidence that consumers act in ways to limit their own future choices in an empirical setting not generated by the researcher. This finding bolsters the theoretical work being done in behavioral economics and provides an example of the type of policy variation necessary to find further empirical examples.

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