

Essays on the Labor Market Effects of
Place-Based Policies

by

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To Sashi, Ishwari, and Anuradha

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Chapter 1

The Impact of Rebuilding Grants and Wage Subsidies on the Resettlement Choices of Hurricane Katrina Victims ¹

Policymakers designing disaster relief packages face a difficult tradeoff. The desire to assist disaster victims must be weighed against the fear that subsidizing people to live in dangerous areas will generate moral hazard with regard to households' location decisions. The size of the economic distortion caused by this type of moral hazard depends on the extent to which individuals' and households' location choices are influenced by financial incentives. If each household is nearly indifferent between many different residence locations, then disaster policies that subsidize residence in a particular location will generate large distortions. If most households strictly prefer one location, then it is possible for disaster relief programs to increase the welfare of residents in disaster-affected areas without generating large distortions. A full appraisal of the impact of disaster-relief policy on social welfare must consider the direct impact of disaster relief on victims' welfare and the extent to which the programs distort individuals' location decisions.

This paper develops and estimates a dynamic discrete choice model of New Orleans homeowners' post-Hurricane Katrina resettlement choices. In the model, households make choices regarding residential locations, home repairs, home sales, and amounts to borrow or save. The model's parameters describe households' preferences over consumption and residence locations and describe households' ability to borrow. The model is estimated using a unique dataset that combines data

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from the recently fielded Displaced New Orleans Residents Survey with administrative records from the Orleans Parish property database. The model is identified by variation across households in the financial incentive to rebuild provided by the Louisiana Road Home rebuilding grant program and by variation in the relative labor wages available in New Orleans versus away from New Orleans for workers in different occupations. I use the estimated model to measure the impact of post-Katrina relief programs on households' short term resettlement choices and to quantify a key component of the deadweight loss associated with a guarantee of future relief.

The estimated model finds that households in this population have a strong preference for living in New Orleans on average; there is substantial variation across households in the strength of that location preference; and large population subgroups face borrowing constraints. The model finds that the government's Road Home program, which paid cash rebuilding grants to individual homeowners, increased the fraction of homes rebuilt during the first four years following Katrina by 11%. This impact occurred primarily by relaxing borrowing constraints for households that would have strongly preferred to rebuild even in the absence of the subsidy if the associated costs could have been smoothed.

Estimates suggest that heterogeneity in location preferences is substantial enough that most households are far from the margin with respect to their preferred location. This pattern implies that the elasticity of location choices with respect to financial incentives is low, as few households switch locations in response to a location subsidy. For this reason, simulations find that a guarantee of relief in the event of a future disaster generates a deadweight loss that is at most 3% of the policy's expected cost.

Several recent studies have estimated explicit dynamic behavioral models to measure the responsiveness of migration choices to financial incentives. Kennan and Walker (2011) estimate a dynamic discrete choice model of optimal migration and find that labor flows are responsive to variation in wages across space. Several related studies have considered variations on Kennan and Walker's model to investigate particular aspects of migration.² This paper contributes to that literature in several important ways.

This paper is the first to estimate a dynamic structural model of migration that explicitly includes the asset accumulation choice and allows for the possibility of borrowing constraints. Borrowing constraints will affect the migration choices of any population that faces large up-front costs to moving. In the disaster context, those costs reflect the price of home repairs. The model would be equally well-suited to investigate migration choices, for example, among homeowners with negative mortgage equity, or other populations that face up-front costs to moving.³

This paper also contributes to this literature by estimating a dynamic structural migration model using directly observable sources of variation in location-specific financial incentives. Other studies in this literature have relied heavily on variation in the individual-location match component of

²Gemici (2011) develops a model in which married couples must consider the potential labor earnings of both household members across locations. Bishop (2007) applies a computational innovation that allows her to model migration with enough geographic detail that migration choices become informative about preferences for spatially delineated amenities.

³It has been documented (Chan, 2001; Ferreira, Gyorko, Tracy, 2010) that declining home equity is associated with reduced mobility, but the relative importance of the several mechanisms that could account for this phenomenon is unknown.

workers' wages, which must be inferred statistically from panel wage data, to identify the influence of earnings opportunities on migration. Because I replicate several key findings of the existing literature using a different and more transparent source of identification, my study provides an out-of-sample validation for the main conclusions of the existing literature.

The responsiveness of individual migration choices to financial incentives is of general interest beyond the disaster-policy context. For example, the manner in which local or regional labor markets adjust to labor demand shocks depends critically on the willingness of affected workers to relocate (Topel 1986; Blanchard and Katz, 1992; Bound and Holzer, 2000).⁴ If workers' location choices are not strongly influenced by financial incentives, then localized labor demand shocks will have large wage effects that persist until capital re-adjusts. If workers quickly relocate in response to financial incentives, then localized labor demand shocks will have short lasting wage effects and larger effects on the size of local populations.

The responsiveness of individual migration choices to financial incentives also figures prominently in the broader literature that examines the economic distortions caused by place-based policies.⁵ If, as in classic models of spatial equilibrium (Rosen, 1979; Roback, 1982), workers are fully mobile and share homogeneous location preferences, the deadweight loss associated with a tax or subsidy to a given location depends entirely on the elasticities of local labor demand and local housing supply (Albouy, 2009). Because long-run labor demand and housing supply are quite elastic, this sort of model suggests that local policies generate large distortions. On the other hand, if workers' preferences for locations are heterogeneous, then the long-run supply elasticity of residents to locations is finite (Moretti, 2011; Busso, Gregory, and Kline 2011), and local policies will generate smaller distortions.

This study also contributes to the literature that examines the economic consequences of disasters and the more narrow literature that has examined patterns of post-Katrina migration and resettlement.⁶ Hurricane Katrina struck the Gulf Coast of the United States on August 29, 2005 and generated, in the years following, the largest disaster-relief effort in the nation's history. The city of New Orleans received some of the storm's most concentrated and costly damage when, in the days following the storm, the city's protective levees failed and flood waters covered large areas of the city. Government disaster relief to New Orleans following Hurricane Katrina included substantial compensation packages to individual homeowners, in addition to traditional public disaster relief services such as debris removal and infrastructure repair. Program evaluation studies examining the impacts of disaster-relief programs are almost entirely missing from the empirical literature on disasters, so this study's evaluation of post-Katrina relief programs fills an important gap in that literature.

The remainder of this paper is structured as follows. Section I describes the dataset. Section II provides background information about U.S. disaster relief policy and describes the policy response

⁴Blanchard and Katz (1992) consider a model in which fully mobile (in the long run) workers and firms arbitrage away shocks to local employment and wages, and the long-run employment effects of such shocks depend on the relative speed with which workers and firms relocate. Bound and Holzer (2000) consider differences across race, education, and experience subgroups in the impact of labor demand shocks on wages, focusing on differences across groups in the elasticity of migration with respect to financial incentives as the primary explanation for these differences.

⁵Glaeser and Gottlieb (2008) review the economics of place-making policies.

⁶See Groen and Polivka (2010), Zissimopoulos and Karoly (2010), Vigdor (2007 and 2008), Paxson and Rouse (2008), and Elliott and Pais (2006).

to Hurricane Katrina. Section III presents the dynamic structural model to be estimated. Section IV describes the parameterization of the model for estimation and describes the estimation routine. Section V presents the structural parameter estimates and assesses the model's in-sample fit. Section VI presents the results of simulation experiments. And, Section VII concludes.

I. Data

This study analyzes a retrospective panel dataset that provides household-level measures of location, home repair status, and home ownership status at twelve evenly-spaced points during the first four years following Hurricane Katrina. The dataset draws from a population-representative survey of pre-Katrina New Orleans residents, called the Displaced New Orleans Residents Survey (DNORS), and from the Orleans Parish Assessor's Office administrative property database. The DNORS data (RAND, 2010) contribute information about demographic background traits, storm related home damage, insurance coverage, and migration. The Assessment data contribute records of post-Katrina home sales and provide information from annual property appraisals, which I use to construct measures of the timing of home repairs. I restrict attention to households that prior to Katrina owned a single-family home in New Orleans, either free-and-clear or with a mortgage,⁷ and I exclude working-aged households in which neither head was employed during the year prior to Katrina.⁸ The combined dataset contains 560 households.

DNORS was fielded by RAND and the Survey Research Center at the University of Michigan. Survey staff randomly selected dwellings from the universe of dwellings in New Orleans prior to Katrina, located the pre-Katrina occupants of selected dwellings regardless of the occupants' resettlement choices, and conducted interviews between July of 2009 and April of 2010. The resulting survey data provide a rich account of the post-Katrina experiences for a representative sample of the pre-Katrina New Orleans population.

The Orleans Parish Assessor's Office property database provides an appraised land value and an appraised improvement value (the value of structures) for each property for calendar-years 2004-2009 and provides a record of all home sales.⁹ I use changes over time in each property's appraised improvement value to construct binary measures of whether repairs had yet occurred in each model period using a procedure described in Appendix I.

I obtain additional information on prices across locations from the 2005-2009 American Community Survey public use microdata files (Ruggles et al., 2010). Measures obtained from these

⁷Studying the behavior of homeowners is natural, because the vast majority of the disaster-relief programs targeted to individual households go to homeowners. Home-ownership patterns in pre-Katrina New Orleans differed somewhat from those of the U.S. as a whole. For instance, in the 2,000 Decennial Census, only about 54% of New Orleans households owned their home, either with a mortgage or free-and-clear, compared to about 68% nationally. Also, New Orleans exhibited smaller disparities in home ownership rates between demographic groups than the U.S. as a whole. For example, the home-ownership rate blacks in New Orleans was about 77% that of non-blacks compared to 67% nationally.

⁸I define a household as working aged if a male head younger than 65 is present or if there is no male head and the female head is younger than 65. I apply this restriction so that pre-Katrina occupation may be treated as a source of variation in post-Katrina wages.

⁹The 2004-2009 appraisals were used to determine residents property taxes for years 2005 through 2010. Orleans Parish bills home owners in advance of the relevant tax year and, therefore, conducts the appraisals for year tax year t during the summer and fall of year $t - 1$.

data include occupation-by-location-specific mean wages over time and a set of rental housing price indices. Appendix *II* and appendix Table *A.1* describe my method for constructing household-specific housing variables using these indices and direct measures from the two primary data sources.

Finally, I use data from the 2005 Panel Study of Income Dynamics to compute, for each DNORS household, an estimate of the distribution of liquid assets among Southern urban homeowners with similar background traits.¹⁰ The pre-Katrina liquid asset holding is an important state variable in the dynamic model that the DNORS interviews did not collect. During the estimation routine, I condition this unobserved initial condition out of the likelihood function by computing each household's likelihood contribution at a range of values for the initial liquid asset holding and then integrating with respect to the estimated conditional asset distribution.

Table 1 provides descriptive statistics for this study's sample of households that owned homes in New Orleans prior to Katrina. Not surprisingly, a sample of homeowners is more affluent on average than the pre-Katrina New Orleans' population as a whole. About 60% of homeowners earned more than \$40,000 in the year prior to Katrina, compared to about 46% of all households.¹¹ About 48% of homeowners had a head with a bachelor's degree, compared to about 41% of all households. About 45% of homeowners were couple-headed, compared to only 34% of all households.

II. U.S. Disaster Relief Policy and the Policy Response to Katrina

U.S. Disaster Relief Policy

Federal disaster policy in the United States consists of a government-run flood insurance program and an apparatus for coordinating and delivering relief in the event of a disaster. This section describes the main provisions of the programs that provide relief services directly to individuals and households.¹²

Flooding is the most common form of natural disaster, and the sole provider of flood insurance in the U.S. is the government-run National Flood Insurance Program (NFIP).¹³ NFIP typically offers premiums at a discount to the actuarially fair rate for each specific location. To encourage localities to perform flood-plain maintenance and flood mitigation activities, NFIP provides discounts of between 5% and 45% to residents' premiums based on a locality's flood mitigation activities.¹⁴

¹⁰In the PSID, I consider liquid assets to be the sum of a household's checking account balance, savings account balance, money market account balance, and the balance of non-retirement investment accounts. Appendix *III* describes the method for computing conditional asset distributions in more detail.

¹¹The summary statistics for the full population of pre-Katrina New Orleans reported in this paragraph come from the 2005 public use microdata ACS files (Ruggles et al., 2010).

¹²This section does not describe the federal government's other primary role of coordinating clean-up activities and repairing infrastructure. The federal policy for coordinating these tasks is governed by the Stafford Act of 1988, which modified the Disaster Relief Act of 1974

¹³For a comprehensive description of the program, see Federal Emergency Management Agency and Federal Insurance and Mitigation Administration (2002).

¹⁴This process is known as NFIP's Community Rating System.

Also, NFIP offers substantial subsidies to the premiums of properties built during or before 1974 when NFIP first estimated actuarially fair premiums.¹⁵

The federal government's post-disaster relief apparatus is triggered when the President officially declares an area to be a major disaster area. This designation permits federal spending on ordinary clean-up activities such as removing debris and repairing infrastructure. Also, once an area has received this designation, homeowners and businesses become eligible for Disaster Relief Loans through the Small Business Administration (SBA), and property owners become eligible for small assistance grants from the Federal Emergency Management Administration (FEMA) to offset the cost of minimal repairs or safety improvements to damaged properties that were not covered by existing insurance arrangements.

In addition to these standing programs, a significant portion of government relief following major disasters is allocated on an ad hoc basis through block grants to local and state governments. Localities have used these types of grants in many ways, including; to purchase damaged homes, to provide cash grants for repairs, to provide subsidized loans for rebuilding, and to provide grants for relocating away from unsafe areas. Another variety of ad hoc disaster relief that has gained popularity in the past decade uses spatially targeted business subsidies to encourage capital investment in disaster areas and to stimulate demand for the labor of local residents.

Program evaluation studies examining the impacts of disaster-relief programs are almost entirely missing from the empirical literature on disasters, so the literature provides little evidence on the effects of these programs on resettlement choices.¹⁶ Methodological challenges provide a likely explanation for the absence of this sort of study from the literature. Quasi-experimental research designs that are common in the program evaluation literature rely on the presence of an otherwise similar control group with which the group exposed to the program may be compared. In a given disaster area the entire affected population often receives the policy's treatment. The structural modeling approach adopted in this study allows these programs' effects to be estimated even if the programs' effects are not directly identified by a quasi-experiment.

Hurricane Katrina's Impact and the Policy Response

Hurricane Katrina struck New Orleans on August 29th, 2005. In the days following the storm's initial impact, the levees protecting the city failed and flood waters covered roughly 80% of the city (McCarthy et al., 2006). The storm and subsequent flooding left two thirds of the city's housing stock uninhabitable without extensive repairs. In addition to damaging property, Katrina displaced nearly all of New Orleans' 460,000 pre-storm residents, and many spent a considerable amount of time away from the city (or never returned). Appendix Table A.4 provides a time line of some key events.

¹⁵The National Flood Insurance Program refers to its set of estimated fair insurance rates as its Flood Insurance Rating Map (FIRM). Subsidies are provided to so-called pre-FIRM properties. By the time of Hurricane Katrina, the fraction of NFIP policies receiving this discount had fallen to about one in four nationally. However, for areas like New Orleans with declining populations and aging housing stocks, the fraction of properties with pre-FIRM designation remained substantially higher than the national average when Katrina struck.

¹⁶One exception is a paper by Kamel and Loukaitou-Sideris (2004) that examined differences across groups in access to disaster relief following the 1994 California Northridge earthquake. The paper finds that zip codes with a lower ratio of relief spending to earthquake damage experienced larger declines in population and housing units.

Table 2 describes the distribution of Katrina-related damage among homeowners and describes the resources that were available to households for repairs. About three out of every four home owning households experienced flooding, and about 70% of homes were rendered uninhabitable.¹⁷ Figure 1 provides kernel density estimates of repair costs for households with different pre-Katrina income levels.

A majority of households with severely damaged homes faced some repair costs that were not covered by insurance, and many households, especially those with lower income, faced substantial insurance shortfalls. Although New Orleans had one of the highest rates of flood insurance coverage in the nation prior to Katrina, some households did not have any coverage. In some cases policies were not large enough to cover the full cost of rebuilding.¹⁸ Finally, many have alleged that insurance companies refused some valid homeowners insurance claims, citing uncertainty about the cause of property damage.

Several government relief programs increased the resources available to households and altered households' financial incentives for rebuilding versus relocating. The remainder of this section describes these programs.

The **Louisiana Road Home program** was a large scale government¹⁹ grant program designed to assist pre-Katrina Louisiana homeowners by providing cash grants for rebuilding that did not need to be repaid. The program was advertised as the largest single housing recovery program in US history, and during the first four years following Katrina, the Road Home program disbursed more than nine billion dollars to Louisiana homeowners.

The Road Home program addressed two policy objectives. First, the program compensated homeowners for their losses. The Road Home program made grant payments to participating homeowners irrespective of whether they chose to rebuild or to relocate. Second, the program created an incentive to rebuild by providing more generous grant packages to those who rebuilt than to those who relocated.

Participating households selected from among three available Road Home benefits packages, known as options 1, 2, and 3. Each Road Home option required participating households to meet certain obligations with regard to their rebuilding and resettlement choices.

The vast majority of New Orleans participants in the Road Home program selected option 1, the option that paid the most generous benefits. Option 1 participants agreed to repair and reside in the pre-Katrina home within three years and to purchase any required flood insurance.²⁰ Option 1 grants

¹⁷Appendix Table A.3 provides a cross-tabulation of flood exposure and home damage categories and finds that both the self-reported measure of home damage from DNORS and a measure based on changes in appraised home values from the Assessor's data are highly correlated with flood exposure. As one would expect, the two independent measures of home damage are also highly correlated with one another.

¹⁸The reasoning of Glaeser and Gyourko (2004), discussed in the context of pre-Katrina New Orleans by Vigdor (2008), provides a rational explanation for this type of shortfall. Because housing is a durable commodity and because New Orleans' population had been steadily declining during the years prior to Katrina, many homes' market values prior to Katrina were substantially less than the home's replacement cost. As a result, even if a home was insured for its full market value, as many mortgage agreements required, insurance payments could fall short of the full cost of rebuilding.

¹⁹The Road Home program was funded through a U.S. Department of Housing and Urban Development Community Development Block Grant and was administered by the Louisiana Office of Community Development

²⁰Rebuilding households were also required to meet a minimum set of building standards.

paid the estimated cost of repairs minus the value of any insurance payments already received, up to a maximum of \$150,000.

Under options 2 and 3, participants received grant compensation but turned their properties over to a public land trust. Option 2 paid a grant equal to the size of the option 1 grant and required the homeowner to purchase another home in Louisiana within three years. Option 3 provided a grant that was 40% smaller than the option 2 grant but did not require the grant recipient to purchase another home or to remain in Louisiana.

The Road Home program provided a financial incentive to return home by paying the most generous overall package to those who agreed to rebuild (option 1). Although the size of the grant payment itself was the same under options 1 and 2, option 2 participants lost any as-is value of their properties, because option 2 required participants to turn their properties over to the Road Home Corporation. Homeowners, of course, also had the option to sell their homes privately.

Define the Road Home program's financial incentive to rebuild to be the difference between what a household's net worth would be under Road Home option 1 and what the household's net worth would be under the more lucrative of Road Home option 2 and a private home sale. Figure 2 illustrates how this financial incentive to rebuild depended on the level of home damage and the fraction of repair costs covered by insurance. The horizontal axis plots the cost of needed repairs as a fraction of the home's value if it is repaired. The upward-sloping line plots total proceeds (grant plus insurance settlement) that the household would receive if it accepted a Road Home option 2 grant. The downward sloping lines plot the total proceeds (insurance settlement plus proceeds from the sale) that the household would receive if it sold its home privately (assuming different levels of insurance coverage). The financial incentive to rebuild is the difference between full compensation under option 1 and the upper envelope of these two lines. The financial incentive to rebuild is higher for households with less insurance coverage, and, unless insurance coverage is nearly complete, the financial incentive to rebuild is larger for households with intermediate levels of damage.

Table 3 describes Road Home program participation patterns among households with homes rendered uninhabitable by Katrina. About three quarters of households with initially uninhabitable homes participated in the Road Home program. Only about 10% of participants selected option 2 or 3. Consistent with the program's incentive structure, program participants with less comprehensive insurance and with moderate home damage were more likely to select option 1 than options 2 or 3.

About 18% of households with an initially uninhabitable home sold the home during the first four years following Katrina. As expected, more households sold their homes privately than accepted a Road Home option 2 or 3 grant if the home was moderately damaged or if the household had comprehensive insurance. More households accepted an option 2 or 3 grant if their home was destroyed or if a significant fraction of their losses were not covered by insurance.

The Road Home program was announced in February of 2006, about six months after Hurricane Katrina, and similar grant programs figured prominently in the policy debate prior to that formal announcement. The program's implementation was plagued by long delays in multiple stages of the application process. After submitting an application to the program, applicants were required to

meet with a “program housing advisor” in order to provide documentation of identity, home ownership, and the home’s initial value.²¹ Those living in Louisiana attended in-person meetings at “Housing Assistance Centers” around the state. Those living out of state could conduct their meetings by telephone or at one of several Housing Assistance Centers opened in out-of-state locations with large evacuee populations, including in Houston, Dallas/Fort Worth, San Antonio, and Atlanta. Applicants then awaited a grant offer, after which the applicant formally selected one of the Road Home options, signed a corresponding “covenant,” and awaited disbursement of the grant. The Road Home program’s application deadline was July 31, 2007, almost two years after Katrina. The median date of grant payments for New Orleans participants occurred near the second anniversary of Katrina, and the standard deviation of grant payment dates was about four months.

Estimating a dynamic model with forward-looking agents requires making assumptions about agents’ expectations. For the model that I consider in this study, agents’ expectations about the Road Home program’s eventual existence and about the timing of Road Home grant payments are especially important. In the model, I assume that households know immediately after Katrina that the Road Home program will eventually be available. This assumption greatly simplifies the model’s solution, allowing me to ignore the manner in which households might learn about government programs. And while actual households could not have been certain about the program’s eventual availability prior to its formal announcement, this particular model assumption should not significantly alter the choice problem of model households, because quick rebuilding was nearly impossible during the first few months following Katrina. Following the program’s announcement, knowledge of the program was indeed widespread. Public service announcements advertised the program throughout Louisiana on television and radio and in print. The program’s high participation rate itself also suggests that households were aware of the program. Without information about the timing of grant payments to individual households, I further assume that all grants became available on Katrina’s second anniversary, and I assume that households foresaw that timing for payments. This assumption should provide a reasonable approximation to reality if households anticipated considerable bureaucratic inefficacy in the program’s implementation.

For a household that did not have savings sufficient to cover the cost of needed repairs, the ability to borrow was crucial if the household wanted to repair its home without a long delay. For many households, the **Small Business Administration’s (SBA) Disaster Loan Program** would be the most easily accessible lender. The SBA Disaster Loan Program is a standing program that provides loans to individual homeowners in federally declared disaster areas to cover the cost of home repairs (less any insurance payments) of up to \$200,000. The terms of Disaster Loans are determined on a case-by-case basis based on an assessment of each borrower’s ability to repay. Approved applicants who do not have access to other credit receive an interest rate that is no more than 4%, and approved applicants who could obtain credit elsewhere receive an interest rate that is no more than 8%. SBA’s creditworthiness standards are marginally more lenient than a bank’s standards, but not all applicants are approved.

The demand for SBA Disaster Loans following Hurricane Katrina was high, and by the end of 2005, about 276,000 Gulf Coast homeowners had submitted applications. However, nearly 82% of

²¹ Applicants were instructed to bring personal identification, documentation for any FEMA assistance received, proof of home ownership (property tax bill, title, mortgage documents, etc.), proof of insurance, any SBA loan documents, home appraisal information, proof of income for all adult household members, and a utility bill (Road Home Program, 2006).

the applications were rejected due to insufficient income or a poor credit history by the applicant (New York Times, 2005). This rejection rate was higher than the rejection rates following other recent disasters, reflecting the fact that Gulf Coast homeowners on average had lower incomes and poorer credit histories than homeowners in less economically depressed regions. These figures corroborate the findings of the estimated model that many homeowners could not easily borrow to finance repairs.

The **Gulf Opportunity Zone (GO Zone)** initiative provided a package of investment subsidies and tax credits targeted to firms operating in areas impacted by the storm. This approach to disaster relief drew precedent from the use of Liberty Zones in New York City following the September 11, 2001 terrorist attacks. Spatially targeted business subsidies have been used increasingly over the past thirty years (including state Enterprise Zones and federal Empowerment Zones, Enterprise Communities, and Renewal Communities) to target transfers to areas with chronic poverty and low economic activity. The Liberty Zones and GO Zone expanded the original scope of these earlier programs by using business subsidies to cushion negative shocks caused by man-made and natural disasters.

The package of GO Zone benefits to zone businesses included both subsidies for the hiring and retention of workers and subsidies to capital investment. Specifically, the GO Zone program provided an employee retention credit or a Work Opportunity Tax Credit (WOTC) of 40% of the first \$6,000 paid to a retained or newly hired employee who lived in the GO Zone on the day before Katrina struck. Existing research suggests that spatially targeted hiring subsidies positively impact local employment and wages (Busso, Gregory, and Kline, 2011). My simulation experiments consider how these subsidies might have affected households' resettlement choices through their impact on New Orleans wages.²²

The model considered in the remainder of the paper considers the influence of these policies on households' resettlement choices. Figure 3 plots trends in home repairs and home sales during the first four years after Katrina. Few home repairs occurred in Katrina's immediate aftermath, and on Katrina's second anniversary only about one in five initially uninhabitable homes had been repaired. Substantially fewer black households than nonblack households repaired homes during the first two years after Katrina. By Katrina's fourth anniversary, about three in five households with an initially uninhabitable home had repaired the home and the racial disparity in repair rates had closed. An additional 12% of homes had been repaired by someone who purchased the home from the pre-Katrina owner.

Figure 4 plots trends in residence-location outcomes during the first four years following Katrina. Panel A plots the fractions of black and nonblack households residing in New Orleans and

²²The GO Zone initiative also included provisions that altered the tax treatment of capital investment in ways that were favorable to businesses. These provisions altered the time frame over which businesses could deduct various spending on clean-up, demolition, and acquiring property, allowed for the use of tax exempt bonds to raise capital, and provided tax credits to offset rehabilitation expenses. The literature studying the employment impacts of programs that rely primarily on tax breaks and capital subsidies (mainly state Enterprise Zones) finds little evidence of an aggregate impact on job creation or on wages (Papke, 1993, 1994; Boarnet and Bogart, 1996; Bondonio, 2003; Bondonio and Engberg, 2000; Elvery, 2009). Bartik (1991) provides a comprehensive review of earlier evidence on the effects of place-based policies. For this reason, I restrict attention to the effects of the GO Zone's wage subsidies.

residing in the pre-Katrina home. Nonblack households returned to New Orleans and to their pre-Katrina homes much more quickly, but these disparities closed by Katrina's fourth anniversary. Panels B and C illustrate that racial disparities in the initial resettlement of New Orleans are explained in large part by differences by race in Katrina-related storm damage. Black households with homes that were still inhabitable following Katrina returned more quickly than nonblack households. Black households with severely damaged homes returned more slowly than nonblacks, but a larger fraction had returned to the pre-Katrina home by the fourth anniversary of the storm. These descriptive findings, particularly regarding initial resettlement patterns, corroborate previous research (Groen and Polivka, 2010; Zissimopolous and Karoly, 2010; Vigdor, 2008; and Paxson and Rouse, 2008) that finds that blacks returned to New Orleans more slowly than non-blacks, with differences in flood exposure accounting for some but not all of this disparity.

III. Model

To better understand how post-Katrina subsidies influenced rebuilding and resettlement patterns, I develop a dynamic discrete choice model of households' resettlement choices. The main goal of the model is understand the factors shaping the prevalence and timing of three broad resettlement outcomes; rebuild and return to the pre-Katrina home, relocate to another (potentially less flood-prone) location within New Orleans, or resettle away from the New Orleans. In the empirical implementation, I allow for the possibility that rebuilding in heavily-flooded areas is less attractive than rebuilding in less heavily-flooded areas by allowing the benefit of returning to the pre-Katrina home to depend on the fraction of nearby homes that were damaged severely by Katrina. The model's parameters describe households' access to credit (for financing home repairs) and describe households' preferences over location amenities and consumption.

A number of techniques have been adopted in the existing literature to incorporate limited credit access into dynamic discrete choice models. Some studies consider models that explicitly impose limits to credit.²³ The approach that I adopt is closer in spirit to that of studies that allow for the possibility that credit access is limited and attempt to infer from observed choices *whether or not* agents face binding borrowing constraints (Evans and Jovanovic, 1989; Cameron and Heckman, 2001; Keane and Wolpin, 2001). Studies in this vein commonly estimate their models using data in which asset accumulation choices are observed, identifying borrowing constraints from the relationship between current wealth and the choice to make an investment of a given expected return. The data that I analyze do not contain information on non-housing assets, so I adopt an alternative approach to identifying borrowing constraints that more closely resembles a strategy from the education literature (Cameron and Taber, 2004) and a strategy from the macro literature testing the permanent income hypothesis (Shea, 1995; Souleles, 1999; Stephens, 2003).

The estimated model allows me to assess the impact of the various post-Katrina programs on

²³Many studies (i.e. Keane and Wolpin, 1997; Rust and Phelan, 1997; Keane and Wolpin, 2002a&b; Todd and Wolpin, 2006; and Kennan and Walker, 2011) have considered models in which agents consume all of their income each period. This approach provides substantial computational savings and is often a useful approximation when factors other than credit constraints are the primary interest. Other studies of environments in which borrowing is known to be constrained (i.e. Rosenzweig and Wolpin, 1993; French and Bailey, 2011) have considered models in which agents are free to save but are explicitly forbidden from holding negative net assets.

households' welfare and households' behavior and to assess the extent to which disaster-related location subsidies more generally distort households' location decisions.

Framework, Timing, and Preferences

I model the dynamic problem facing home owning households in the aftermath of Hurricane Katrina using a finite horizon, discrete time framework. Periods are indexed by $t = 0, \dots, T$. Each period is four months long. An asset holding $A(t)$ and a vector $X(t) = [L(t), H(t), D(t)]$ characterize the state facing the household at time t ; with $L \in \{1, 2, 3\}$ denoting location (1 indicates residence in the pre-Katrina home, 2 indicates residence in another New Orleans residence, and 3 indicates residence elsewhere), $H \in \{0, 1\}$ indicating ownership of the pre-Katrina home, and $D \in \{0, 1\}$ indicating that the home damage caused by Hurricane Katrina is yet to be repaired.

Hurricane Katrina occurs at $t = 0$, and T is the period in which the household reaches age 80. At $t = 0$ the household is endowed with an initial state $X(0)$ and an initial asset holding $A(0)$. Each household owns its home at $t = 0$. Initial housing damage and initial location are exogenously determined. Each period the household observes its current state $X(t)$ and must select the subsequent period's state $X(t + 1)$. The household must hold non-negative assets at retirement, assumed to occur at age 65. The household may not re-purchase its home if it has been sold. The household may not reside in the pre-Katrina home if the home has been sold or if it is damaged.

The household chooses a consumption level $C(t)$ each period and derives constant relative risk aversion consumption utility. The household derives utility from the amenities associated with its residence location $B(L(t), t)$, and the household suffers utility costs χ^M and χ^R from moving or rebuilding. These utility costs capture the difficulty of relocating (χ^M) and the logistical and regulatory hurdles associated with rebuilding (χ^R).

Households are heterogeneous in their preference for living in New Orleans. Each household draws a random variable $\eta \sim N(0, \sigma_\eta)$ at time $t = 0$ that permanently characterizes the strength of its attachment to New Orleans relative to the average among pre-Katrina New Orleans homeowners.

Finally, each period households receive a set of i.i.d. transitory shocks $\epsilon_t(X(t + 1))$ to the payoffs associated with each of the available choices. These transitory shocks capture idiosyncratic deviations of households' benefits from the various choices relative to the average benefit from those choices. Examples of factors captured by these ϵ -shocks include a delayed or faster-than-expected permit for home repairs, situations like a child's school enrollment status or a family member's health condition that influences a household's ability to move at a particular time, and especially optimistic or pessimistic expectations about the fraction of friends and family who will have returned to New Orleans at different times.²⁴

Incorporating all of these factors, a household's utility each period is,

²⁴No doubt, some of these factors are somewhat persistent over time. The model approximates the role of unobservables using only a permanent component η and a set of i.i.d. components ϵ . The role of factors that are partially persistent will be approximated in this framework by those factors "loading" partially on the persistent component and partially on the i.i.d. components.

$$\begin{aligned}
u(t) = & \frac{1}{\alpha} \frac{C(t)^{1-\omega}}{1-\omega} + B(L(t), t) + \epsilon_t(X(t+1)) + \eta(X(t+1)) \\
& - \chi^M \mathbf{1}(L(t+1) \neq L(t)) \\
& - \chi^R \mathbf{1}(D(t+1) < D(t))
\end{aligned} \tag{1}$$

Each period t households observe the current state $(X(t), A(t))$ and choice specific shocks $\epsilon_t(X(t+1))$ and choose the next period's state $(X(t+1), A(t+1))$. Households continue this process until reaching age 65, at which time the state becomes fixed and the household is retired. Households that are 57 or older when Katrina occurs continue adjusting states X throughout the first eight years following Katrina. Once the state $X(t)$ is fixed at retirement, the household derives utility during retirement until age 80 (period T). Retirement utility is the sum of amenity utility derived from the household's residence location and the consumption utility associated with a \$4,500 per period transfer payment plus an annuity based on the household's asset holding at retirement net of expenses (any mortgage or rent payments associated with the chosen residential location).

The household's objective is to maximize the present discounted value of future per-period utilities, denoted by U .

$$U = \sum_{t=0}^T \beta^t u(t) \tag{2}$$

where β is a subjective discount factor.

Prices and Budget Constraint

An intertemporal budget constraint requires that each household's consumption plus net asset accumulation equals its income (wage earnings plus the proceeds from home sales or grant payments) minus expenses (home repair costs and rent or mortgage payments) each period. Households are assumed to foresee the path of relevant prices over time and locations.

Each household head that worked during the year prior to Katrina receives the market wage for his or her human capital level and occupation each period in the household's chosen labor market $W(L, t)$. Residence in locations $L = 1, 2$ places the household in the New Orleans labor market and residence in $L = 3$ places the household in a pooled "other metro South" labor market.

The household derives housing services from the pre-Katrina home if $L(t) = 1$. The household must rent an equivalent flow of housing services at the market rate $RENT(L(t))$ if $L(t) = 2$ or $L(t) = 3$. The household makes a mortgage payment $M(t)$ each period until the mortgage is paid off (30 years after the home is purchased) or until the home is sold.

The household pays a repair cost K if it chooses to repair its home. The household may not make partial repairs over multiple periods, but the household may self-finance by saving a portion of the total repair cost over several periods before purchasing repairs.

If the household sells its home at time t , it receives proceeds equal to $(P^H - PRINC(t) - D(t)K)$, the home's market value in post-Katrina New Orleans if it were fully repaired minus any principal remaining on the home's mortgage and the cost associated with any needed repairs.

The household's budget constraint incorporates an approximation to the Road Home program's actual eligibility rules. A household that repairs or sells its home during the first two years following Katrina is reimbursed for uninsured repair costs by a Road Home option 1 grant G_1 in period 7, the first period after the second anniversary of Katrina. If the household purchases home repairs between periods 7 and 15 (during the third, fourth, and fifth years after Katrina), the household is reimbursed by a Road Home option 1 grant G_1 at the time the repairs are made. If the household sells its home between periods 7 and 15, the household receives either a Road Home option 2 grant G_2 or the market value of the home, whichever is larger.

$$G_1(t) = \begin{cases} \min [\$150,000, K - INS] & \text{if } t=7 \text{ and } D(t) = 0 \text{ or } H(t) = 0 \\ \min [\$150,000, K - INS] & \text{if } t \in [7, 15] \text{ and } D(t-1) < D(t) \\ 0 & \text{otherwise} \end{cases} \quad (3)$$

$$G_2(t) = \begin{cases} \min [\$150,000, K - INS] & \text{if } t \in [7, 15] \text{ and } H(t) < H(t-1) \\ 0 & \text{otherwise} \end{cases} \quad (4)$$

The household's intertemporal budget constraint is,

$$C(t) = \overbrace{W(L(t), t)}^{\text{wage income}} + \overbrace{\left(\max(G_2(t), P^H - D(t)K) - PRINC(t) \right) (H(t) - H(t+1))}^{\text{proceeds from home sale}} + \overbrace{G_1(t)}^{\text{Option 1 grant}} \quad (5)$$

$$- \underbrace{H(t)M(t)}_{\text{mortgage payment}} - \underbrace{RENT(L(t))}_{\text{rent payment}} - \underbrace{K(D(t+1) - D(t))}_{\text{repair costs}} - \underbrace{(A(t+1)/(1+r) - A(t))}_{\text{change in asset holding}}$$

The possibility of a borrowing constraint is incorporated by allowing the interest rate faced when borrowing to differ from (exceed) the interest rate faced when saving. That is,

$$A(t+1)/(1+r) = \begin{cases} A(t+1)/(1+r^S) & \text{if } A(t+1) \geq 0 \\ A(t+1)/(1+r^B) & \text{if } A(t+1) < 0 \end{cases} \quad (6)$$

$$r^B \geq r^S$$

Dynamic Programming Representation

Given the separability of the transient utility shocks ϵ , the solution to the household's problem may be expressed as a dynamic programming problem. Define the value function $V(X, A, \eta, \epsilon, t)$ as a mapping from each state to the expected present discounted value of the subsequent utility associated with an optimal choice policy. By the principle of optimality, this value function must satisfy the Bellman equation,

$$V(A(t), X(t), \eta, \epsilon) = \max_{A(t+1), X(t+1)} \left\{ u\left(X(t), X(t+1), A(t), A(t+1), \eta, \epsilon(X(t+1))\right) + \beta \bar{V}\left(A(t+1), X(t+1), \eta\right) \right\} \quad (7)$$

$$\bar{V}\left(A(t+1), X(t+1), \eta\right) = \text{Emax}_\epsilon V\left(A(t+1), X(t+1), \eta, \epsilon\right) \quad (8)$$

Because the choice specific ϵ shocks vary with $X(t+1)$ but not $A(t+1)$, the optimal asset accumulation policy is a deterministic function A^* of the current state and the chosen state, and Equation (7) may be rewritten as,

$$V(A(t), X(t), \eta, \epsilon) = \max_{X(t+1)} \left\{ u\left(X(t), X(t+1), A(t), A^*(X(t), \eta, X(t+1), A(t), t), \eta, \epsilon(X(t+1))\right) + \beta \bar{V}\left(A^*(X(t), \eta, X(t+1), A(t), t), X(t+1), \eta\right) \right\} \quad (9)$$

where $A^*(X(t), \eta, X(t+1), A(t), t)$ is given by,

$$A^*(X(t), \eta, X(t+1), A(t), t) = \arg \max_{A(t+1)} \left\{ u\left(X(t), X(t+1), A(t), A(t+1), \eta, \epsilon(X(t+1))\right) + \beta \bar{V}\left(A(t+1), X(t+1), \eta\right) \right\} \quad (10)$$

This representation is convenient for estimation, because it allows for households' financial assets (which are not observed in the data) to be conditioned out of the likelihood function. In practice, I discretize the asset space, so for any current state and chosen state the definition of A^* in Equation (10) requires finding the maximal element in a finite set.

The assumption that the ϵ shocks are drawn from the type I extreme value distribution allows for a closed form representation of the expected maximal continuation value from any state (McFadden, 1975; Rust, 1987),

$$\bar{V}(A(t), X(t), \eta) = \ln \left\{ \sum_{X(t+1)} \exp\left(\bar{u}(X(t), \eta, X(t+1), A(t), A^*(X(t), \eta, X(t+1), A(t), t)) + \bar{V}(A(t+1), X(t+1), \eta))\right) \right\} + \gamma \quad (11)$$

where $\gamma \approx 0.577$ is Euler's constant. Also, the conditional choice probabilities take the multinomial logit form,

$$P(X(t+1) | A(t), X(t), \eta) = \frac{\exp\left[\bar{u}(X(t), \eta, X(t+1), A(t), A^*(X(t), \eta, X(t+1), A(t), t)) + \bar{V}(A(t+1), X(t+1), \eta))\right]}{\sum_{X'(t+1)} \exp\left[\bar{u}(X(t), \eta, X'(t+1), A(t), A^*(X(t), \eta, X'(t+1), A(t), t)) + \bar{V}(A(t+1), X'(t+1), \eta))\right]} \quad (12)$$

Using these simplifications, it is straightforward to numerically solve the value function for any given parameterization of the model using backward induction from the time T boundary condition.

IV. Model Parameterization and Estimation

The parameters of the model to be estimated include a wage equation, a set of household preferences, and parameters describing the interest rate at which households with different demographic traits may borrow. I estimate the model sequentially. The first step estimates the wage equation. The second step takes the first step’s estimates as a known input and estimates the parameters describing households’ preferences and the borrowing interest rate.

Wages

Each period, each household head who was employed during the year prior to Katrina receives a wage that reflects the value of his or her skills in the worker’s residence location. That wage is determined by,

$$\ln w_{jkl t} = \ln \bar{w}_{klt} + x'_j \beta + \mu_j \quad (13)$$

where j indexes workers, k indexes two-digit occupations, l indexes the labor market in which the worker resides, t indexes the period, \bar{w}_{klt} is a period-location-specific mean occupation wage, x_j is a vector of worker human capital variables, and μ_j is a worker fixed effect.

I estimate this equation using pre-Katrina annual earnings records from DNORS²⁵ and annual earnings records from the New Orleans MSA respondents to the 2005 ACS. Pre-Katrina New Orleans mean occupation wages \bar{w}_{klt} come from the 2005 ACS. I obtain estimates of the worker fixed effects $\hat{\mu}_j$ by computing the residuals from this regression for all DNORS records used to estimate equation (13).

Estimation of the structural model requires an estimate of the labor wages that each household faces in each location in each period. To construct these quantities, I compute the sum of the wages predicted by equation (13) for each household’s working head or heads. I compute the location-specific mean occupation wages that enter the right-hand side of equation (13) using the *ACS*. In the model, each location’s mean occupation wages follow their observed paths in the *ACS* during the first four years following Katrina and remain at their 2009 levels in each location in all later years.²⁶ Households with just one head who was employed during the year prior to Katrina receive that worker’s wages, and couple-headed households with two working heads receive the sum of both workers’ wages.

²⁵I estimate the wage equation using DNORS workers living in both renter-occupied and owner-occupied dwellings.

²⁶With an earlier specification of the model, I re-estimated the model under the assumption that relative New Orleans wages “decay” back to their pre-Katrina values in the out years as a sensitivity analysis. The spatial wage elasticity implied by the model increased very slightly as a result of this change, the intuition being that same variation in location choices is attributed to a smaller amount of variation in relative wages, but the change does not have any significant impact on the policy conclusions of the study. Subsequent versions of this paper will present robustness checks of this sort for the model specification presented in the paper.

Parameterizing household's preferences and the borrowing interest rate

Two parameters describe the consumption component of utility $\frac{1}{\alpha} \left(\frac{C^{1-\omega}}{1-\omega} \right)$, the coefficient of relative risk aversion ω and a parameter α that scales the importance of consumption utility relative to that of the unobserved utility shocks ϵ .

I normalize the borrowing interest rate for a group of relatively affluent households (non-black households in which at least one household head holds a bachelor's degree and pre-Katrina with annual income exceeded \$40,000) to the risk-free rate ($1/\beta$). Affluent households were likely to be eligible for the SBA Disaster Loan program or private loans. The borrowing interest rate for other households differs from the risk-free rate according to,

$$\ln(1 + r^B) = \ln(1/\beta) + \gamma_1 \mathbf{1}(black) + \gamma_2 \mathbf{1}(nocoll) + \gamma_3 \mathbf{1}(Inc < \$20k) + \gamma_4 \mathbf{1}(\$20k < Inc < \$40k) \quad (14)$$

where $\mathbf{1}(black)$ indicates that a household has a black household head, $\mathbf{1}(nocoll)$ indicates that no household head has a bachelor's degree, and $\mathbf{1}(Inc < \$20k)$ and $\mathbf{1}(\$20k < Inc < \$40k)$ indicate that pre-Katrina annual household income fell in the indicated range. A positive value for any of the γ parameters indicates that on average the corresponding group faced borrowing constraints.

When estimating the location preference parameters, I normalize the payoff to living away from New Orleans $B(L = 3)$ to zero. The payoff $B(L = 2)$ and the difference $B(L = 1) - B(L = 2)$ are parameters to be estimated. I allow these parameters to depend on a small set of household and neighborhood characteristics. This parameterization captures differences in the benefits to living in neighborhoods with different levels of flood damage and captures systematic differences in attachment to place across groups.²⁷

Following earlier structural migration studies (Kennan and Walker, 2011; Bishop, 2007), the utility cost to moving χ^M depends on the distance and timing of the move.²⁸ The utility cost to repairing one's home χ^R depends on whether the home was destroyed or the home was damaged but not destroyed.

When discussing the estimation algorithm, I jointly refer to the set of model parameters described here with $\theta = [B(L), \chi^M, \chi^R, r^B, \alpha, \sigma]'$.

²⁷Specifically, I allow $B(L = 2)$ to follow a linear time trend during the first five years following Katrina. I allow $B(L = 1, t) - B(L = 2, t)$ to depend on the fraction of owner-occupied homes on the same block segment (the DNORS sampling unit) that were rendered uninhabitable by Katrina. I group this continuous measure into three groups; 0%–50%, 50%–90%, and 90%–100%. I allow $B(L = 1, t) - B(L = 2, t)$ to follow linear time trends during the first five years following Katrina within the two higher damage categories. This parameterization allows for the possibility that living in a neighborhood that was heavily flooded might have been especially unappealing shortly after Katrina but may become more attractive as time passes. I also allow $B(L = 1, t) - B(L = 2, t)$ to depend on the 2000 poverty rate in the household's Census block group, when the household purchased its home, and whether either head was born outside of Louisiana.

²⁸Specifically, the utility cost of moving depends on an indicator for any change in location, an indicator that the move was to or from New Orleans (not within the city), and an indicator that the move occurred during the first period after a home repair. This parameterization allows for the possibilities that moving is more difficult if the destination is far away, moving home is more likely immediately following a home repair, and the moving cost is different during the first period after Katrina than in subsequent periods.

I fix the values of two remaining parameters using a convention and an empirical result from the existing literature. I set the subjective discount factor to $\beta = 0.95$ annually, following earlier dynamic discrete choice studies of migration (Kennan and Walker, 2011). I set the coefficient of relative risk aversion to $\omega = 4.17$, the mean coefficient of relative risk aversion estimated by Barsky et al. (1997) using an experimental approach with Health and Retirement Study data.

Estimation

Estimation proceeds by full-solution maximum likelihood. The full-solution method possesses greater statistical efficiency than less computationally-intensive alternatives²⁹ and facilitates a novel approach to addressing the particular missing data pattern that I encounter.

The data analyzed in this study do not contain information on households' non-housing assets. I use a two-step procedure to "integrate out" this missing dimension when computing households' likelihood contributions. The first step utilizes the model's solution to collapse this missing asset data problem into a more tractable missing *initial* asset data problem.³⁰ The second step computes the likelihood of each household's observed choices at a range of assumed initial asset holdings and integrates the conditional likelihood contributions for each household with respect to the distribution of 2005 asset holdings among Southern urban homeowners with the same demographic characteristics as that household.³¹

To estimate the model, I assume that a sample of households $i = 1, \dots, N$ solve the model described in the previous section. The data contain information on these households' choices $\{X_i(t)\}_{t=1}^T$, their post-Katrina circumstances, and their demographic traits, but the households' initial financial asset holdings and the households' permanent and idiosyncratic preference shocks are not unobserved.

Using equation (10) define the model's implied asset holding policy,

$$\widehat{A}_i\left(t \mid \{X_i(\tau)\}, A(0), \eta, \theta\right) = \begin{cases} A_i(0) & \text{if } t = 0 \\ A^*(X_i(t-1), X_i(t), A_i(t-1), t-1) & \text{if } t > 0 \end{cases} \quad (15)$$

Conditional on an assumed initial asset value, the household's likelihood contribution is:

$$l_i\left(\theta \mid \{X_i(t)\}_{t=1}^T, A(0), \eta\right) = \prod_{t=0}^{T-1} \mathbf{P}\left(X_i(t+1) \mid X_i(t), \widehat{A}_i(t \mid A(0)), \eta, \theta\right) \quad (16)$$

²⁹See, for example, the Conditional Choice Probability (CCP) estimators of Hotz and Miller (1994) and Arcidiacono and Miller (2011). Aguirregabiria and Mira (2010) provide a survey of methods for estimating models of this sort.

³⁰This first step requires computing the full solution to the model. For that reason, this method precludes the use of CCP estimators, which require "finite dependence" (Arcidiacono and Miller, 2011) in order to realize computational savings.

³¹These household-specific asset distributions are modeled using 2005 PSID data with a procedure described in Appendix III.

The household's unconditional likelihood contribution is obtained by integrating this conditional expression with respect to the distribution of initial assets $F_{A(0)}^i(a_0)$ and the distribution of the unobserved heterogeneity term $G_\eta(\eta|\theta)$:

$$l_i\left(\theta \mid \left\{X_i(t)\right\}_{t=1}^T\right) = \int \int l_i\left(\theta \mid \left\{X_i(t)\right\}_{t=1}^T, \widehat{A}_i(t|a_0), \eta\right) dF_{A(0)}^i(a_0) dG_\eta(\eta|\theta) \quad (17)$$

Following Kennan (2004), I approximate the continuous distribution F of initial household asset holdings and the continuous distribution G of the heterogeneity term η using discrete distributions with ten support points and five support points respectively.³² I approximate $\widehat{F}_{A_0}^i(a)$ by assigning equal prior probability to the 5th, 15th, ..., 95th percentiles of its initial asset-holding distribution. I approximate $\widehat{G}_\eta(\eta)$ by assigning equal prior probability to the 10th, 30th, 50th, 70th and 90th percentiles of the distribution. The approximation of the integral in Equation 17 is,

$$l_i\left(\theta \mid \left\{X_i(t)\right\}_{t=1}^T\right) = \frac{1}{10} \sum_{p_a=5}^{95} \frac{1}{5} \sum_{p_\eta=10}^{90} l_i\left(\theta \mid \left\{X_i(t)\right\}_{t=1}^T, A_i(0)=F_{A(0)}^{i-1}(p_a), \eta=G_\eta^{i-1}(p_\eta)\right) \quad (18)$$

and the log-likelihood for the full panel dataset is:

$$\mathbb{L}\left(\theta \mid \left\{X(t)\right\}_{t=1}^T\right) = \ln\left(\prod_{i=1}^N l_i\left(\theta \mid \left\{X_i(t)\right\}_{t=1}^T\right)\right) \quad (19)$$

I compute the maximum likelihood estimate of θ using a nested fixed point algorithm in which an “inner loop” repeatedly computes a numerical solution to the model and obtains a sample log-likelihood at candidate values of θ , and an “outer loop” searches the parameter space for the likelihood maximizing parameter vector $\widehat{\theta}$.

I conduct inference using the asymptotic variance-covariance matrix, robust to clustering at the neighborhood level,³³

$$\widehat{COV}_{\widehat{\theta}} = H(\widehat{\theta})^{-1} \left(\sum_{k=1}^K g_k(\widehat{\theta}) g_k(\widehat{\theta})' \right) H(\widehat{\theta})^{-1} \quad (20)$$

where $H(\theta) = \partial^2 L(\theta) / \partial \theta \partial \theta'$ is the Hessian matrix and $g_k(\theta) = \sum_{\mathcal{N}(i)=k} \partial l_i(\theta) / \partial \theta$ is the sum of household scores within cluster (neighborhood) k ($\mathcal{N}(i)$ returns household i 's neighborhood and j indexes neighborhoods).

³²To approximate the distribution of initial asset holdings, I obtain an estimate of the 5th, 15th, ..., 95th percentiles of the distribution of each household's pre-Katrina asset holding conditional on the household's observed characteristics. Appendix III describes the approach in detail, which I apply to data from the 2005 wave of the PSID.

³³I cluster by official New Orleans neighborhoods. That unit of geography is larger than Census blocks or block groups, so this approach is more conservative than clustering at these smaller units of geography at which some of the model's exogenous variables are defined.

V. Identification

The assumption that the idiosyncratic shocks ϵ are drawn from the Type-I EV distribution normalizes the variance of that unobserved component. As in a standard static logit model, the values of other parameters reflect their importance relative to the importance of unobservables.

The importance of consumption utility relative to the importance of the unobserved location-preference shocks (both permanent and transitory), is identified by variation across households in the net financial benefit of residing in New Orleans or residing in the pre-Katrina home relative to the financial benefit from staying away. Variation in the net financial benefit of residing in New Orleans or in the pre-Katrina home comes from the Road Home program's provisions (see figure 2), and from variation in the relative New Orleans wage (wages in New Orleans minus wages in other Southern metro areas) across occupations and over time (see figures 5 and 6). If households' location choices are strongly related to their location-specific financial incentives, then one may infer that consumption utility receives a large weight relative to unobserved location-preference shocks. If households with dramatically different financial incentives for returning to New Orleans return at similar rates, then one may infer that the unobserved component of location preferences receives a large weight relative to consumption utility.

The flow benefit to the various residence locations is identified by the fraction of households choosing each location after accounting for the financial incentives to do so. I normalize the flow benefit to remaining away from New Orleans $B(3)$ to zero. If the fraction of households that chose to return to their pre-Katrina home exceeds the fraction predicted to do so based on financial incentives alone then one may infer that the flow benefit to residence in the pre-Katrina home $B(1)$ is positive. The flow benefit to residing "elsewhere in New Orleans" $B(2)$ is identified similarly. The parameterization of the model allows $B(1)$ to vary with neighborhood and household characteristics. The parameters that describe how $B(1)$ differs with particular pre-determined neighborhood and household characteristics are identified by cross-sectional variation in those traits.

The scale of η , the persistent unobserved heterogeneity in the flow benefit that households derive from residence in New Orleans, relative to the scale of the ϵ shocks is identified from the degree of persistence or path-dependence in observed choices. To see this, consider two households who at time t face different financial incentives to make a particular choice but who both make the same choice. On average, the household who receives the lower benefit from that choice has a draw from the unobservables ϵ plus η that more strongly favors the particular choice. If, in this situations, the two households behave similarly going forward, then the idiosyncratic shock ϵ must have a variance that is significantly larger than that of the permanent shock η . On the other hand, if choices differ substantially going forward then the persistent shock η must have a large variance relative to that of ϵ .

The model's effective borrowing rate parameters are identified in two ways. The first source of identification resembles an approach developed by Cameron and Taber (2004) who, in the context of higher education attainment, demonstrate that an effective borrowing rate is identified by comparing how an investment choice (college attendance) varies with the direct cost of investing and with a gradually accruing opportunity cost to investing. For an agent who is free to borrow, the choice of whether to make a particular investment should be similarly influenced by a change in the direct cost of the investment and an equivalent change in (the present value of) a gradually accruing opportunity

cost. For an agent who is borrowing constrained, the choice should respond more strongly to a change in the direct cost, because for a constrained agent the marginal utility of consumption will be highest in the period in which the direct cost is paid. In the case of post-Katrina rebuilding, repair costs that are not covered by insurance payments represent a direct cost that must be paid before returning to the pre-Katrina residence. The difference between expected labor earnings in the evacuation location and New Orleans represents a gradually accruing opportunity cost to returning and rebuilding.

A second source of identification involves examining the extent to which the propensity to rebuild jumps at the time that Road Home grants are dispersed.³⁴ This approach resembles an approach found in the macroeconomics literature on consumption that tests the Permanent Income Hypothesis by examining the consumption response to fully anticipated income windfalls (Shea, 1995; Souleles, 1999; Stephens, 2003). I impose that households in an affluent comparison group may borrow at the risk free rate, since households in that group would have been very likely to be eligible for government’s subsidized SBA disaster loan program. If, following the payment of Road Home grants, the rebuilding rate of a particular group changes similarly to the rebuilding rate of this (freely borrowing) comparison group, one could infer that the group also faced low borrowing costs.

Finally, I must identify utility costs to moving and rebuilding. To see the sort of variation that allows for these sorts of “transition” costs (moving costs and repair costs both reduce the payoff to particular state transitions) to be identified separately from the states’ flow benefits, consider transitions involving two states, x_1 and x_2 , which each provide a flow benefit. Optimality requires that the state transition probabilities $P(X_{t+1} = x_1 | X_t = x_1)$ and $P(X_{t+1} = x_1 | X_t = x_2)$ both increase with the flow benefit of state x_1 , but that the first quantity increases with the transition cost and the second quantity decreases with the transition costs. With knowledge of the distribution of unobservables, these two moments are sufficient to separately identify the transition cost and the difference between the flow payoffs in x_1 and x_2 .

VI. Parameter Estimates and Model Fit

Table 4 presents estimates of the labor wage equation. In the second step of estimation, the labor wages available to households across time and space are inferred from this equation and mean year-location-occupation specific wages computed from the ACS. Figure 5 depicts the change in relative New Orleans wages in the ACS from 2005 to 2008 for the two-digit occupation groupings that I use in this study. Figure 6 plots long-differences in relative New Orleans wages during the first four years after Katrina using a broader grouping of occupations. In post-Katrina New Orleans, comparatively high wages prevailed in occupations, like construction, concentrated in industries that produced the goods and services necessary for the region’s reconstruction. Comparatively low wages prevailed in occupations, like personal service providers and healthcare technicians, that are

³⁴Note that both approaches to identifying borrowing constraints differ from one common identification strategy in dynamic discrete choice models of various investment decisions (Evans and Jovanovic, 1989; Cameron and Heckman, 2001; Keane and Wolpin, 2001), namely examining the extent to which the choice to make an investment with a particular expected net return depends upon current wealth. In this study non-housing wealth is not directly observed

concentrated in industries that produce goods and services whose demand is especially dependent on a sizeable permanent population.

Table 5 provides estimates of the full model's structural parameters. The estimates find that, all else equal, households have a strong preference for living in New Orleans, and specifically in their pre-Katrina homes. This estimate is driven by several features of the underlying data. By Katrina's fourth anniversary, majorities of households had returned to New Orleans, had maintained ownership of their homes, and had repaired their homes. For most households those choices were subsidized. But similar patterns occurred even among households with a negative financial return to rebuilding — namely households with a small Road Home-induced incentive to rebuild and who worked in occupations for which New Orleans offers comparatively low wages.

The flow benefits to residing on blocks with 50% – 90% or 90% – 100% of homes initially uninhabitable follow statistically significantly positive time trends. Living in these areas was an extremely undesirable option immediately following Katrina, but the benefit to residing in these areas increased over time. The estimates find no statistically significant differences in flow location benefits between block damage categories five years or more after Katrina.

The estimated borrowing rate equation finds that the effective borrowing rate is 41 log-points higher than the saving interest rate for households with a pre-Katrina income less than \$20,000 per year, 35 log points higher for households with no bachelor's degree, and 14 log-points higher for black households. These estimates suggest that large segments of New Orleans households were constrained in their rebuilding choices in Katrina's immediate aftermath by low access to credit.

Consistent with other studies that estimate structural models of migration, I find that the utility cost to moving is large relative to income. For instance, a median-income household would be indifferent between paying the estimated baseline moving cost of 2.78 utils and suffering a one-period consumption reduction of just above 90%. One might expect that returning to New Orleans soon after Katrina would be especially difficult. The mandatory evacuation of the city lasted for more than a month, and a lack of basic city services made returning difficult even after some areas of the city were officially reopened. Indeed, I find that the moving cost is especially high during the first period following Katrina. Finally, the moving cost is higher for moves to or from New Orleans than for within-city moves.

The estimated utility cost to repairing a home is on the same order as the utility cost to moving. As expected, the utility cost to repairing a destroyed home is significantly higher than the utility cost of repairing an a home that was uninhabitable but not destroyed following Katrina.

The estimated standard deviation of η , the term capturing persistent unobserved heterogeneity in the preference for living in New Orleans, is 0.65 utils. This parameter is difficult to interpret directly, so it is useful to consider the location preferences of households with η draws one standard deviation above average and one standard deviation below average. A household with $\eta = 0$ belonging to the reference category of the location flow-benefit equations receives a long-term flow benefit to residing in the pre-Katrina home of about 0.5 utils per period. A household with η one standard deviation below average has a slight preference for living away from New Orleans, all else equal, since $0.5 + (-0.65) < 0$. A household with η one standard deviation above average has an extremely strong preference for living in the pre-Katrina home relative to living away from New Orleans,

about $0.5 + 0.65 = 1.1$ utils per period. Holding other location costs and amenities constant, a median income household with these location preference would be willing to remain in the pre-Katrina home and accept a reduction in non-housing consumption of over 95% instead of relocating away from New Orleans.

Table 6 assesses the model’s fit. I compare the model’s predicted fraction of households exhibiting four different outcomes to the empirical fraction of households exhibiting those outcome on the first four anniversaries of Katrina. I provide similar comparisons within the sample of households with initially uninhabitable homes and within three sub-samples defined by pre-Katrina household income. For each comparison, I report a cluster-corrected (clustered at the neighborhood level) chi-squared test statistic associated with the null that the predicted moment and the sample moment are equal. The model predicts the key features of the data quite well, but the model’s fit is not exact in a number of places. Among the four outcomes considered, the fraction of households having sold their homes is matched least closely, and the formal chi-squared tests reject the model’s fit to that outcome in several instances.

Figure 7 compares the model’s predicted supply elasticity of workers with respect to local wages to the corresponding elasticity predicted by Kennan and Walker (2011). Separate spatial wage elasticities are provided for households of different ages.³⁵ The spatial wage elasticity for the local New Orleans population of working age is 0.22. Among households 35 or younger, a population that is more comparable with the population of young workers considered by Kennan and Walker (2011), the spatial wage elasticity is 0.96. This estimate is on the same order as Kennan and Walker’s (2011) estimates of between 0.5 and 0.75.

VII. Policy Simulations

The remainder of this paper describes the results of simulation experiments designed to examine how several aspects of disaster-relief policy influence households’ behavior.

I conduct simulations of households’ choices under a baseline scenario, in which households face the actual post-Katrina policy environment, and under several alternative scenarios in which particular policies are changed one at a time. For each scenario, I compute 10,000 simulated panels for each household, initializing each panel using the household’s actual location L and home damage status D in the first period after Katrina. I compute 1/50th of each household’s simulated panels at each of the 50 combinations of the $A(0)$ and η used to approximate the distributions of those quantities during estimation. When computing mean outcomes from the simulated data, I weight each simulated panel by the *ex post* probability that a household falls at the particular $A(0) \times \eta$ combination conditional on the household’s actual choice sequence.³⁶

I also examine differences in policies’ impacts on household welfare. I define the expected welfare of household i under policy P using,

³⁵Again, household age is defined to be the age of the male household head if one is present and the age of the female head otherwise.

³⁶The *ex post* probability that a household is characterized by a given combination of an initial asset holding and η -draw is equal to $0.02 \times l\left(\theta \left| \left\{ X_i(t) \right\}_{t=1}^T, A(0), \eta\right) / l\left(\theta \left| \left\{ X_i(t) \right\}_{t=1}^T\right)\right)$, that is, the ex-ante weight (1/50) times the ratio of the household’s panel’s likelihood conditional on an $A(0) \times \eta$ combination to the household’s panel’s unconditional likelihood.

$$W_i(P) = 100 \times E_{A(0), \eta} \left(\frac{V(X_i(t=0) | \text{Policy} = P; A(0), \eta) - V(X_i(t=0) | \text{No Grants}; A(0), \eta)}{V(X_i(t=0) | \text{Full Reimbursement at } t = 0; A(0), \eta) - V(X_i(t=0) | \text{No Grants}; A(0), \eta)} \right) \quad (21)$$

This definition normalizes each household’s expected welfare to be zero when no grant compensation is provided and to be 100 when full reimbursement of all losses is provided at $t = 0$. The expectation in this expression is taken with respect to the *ex post* distribution of $A(0)$ and η given the household’s observed choices.³⁷ I define average household welfare under a particular policy to be the average of this quantity across households. The rationale for this approach is not that full reimbursement is the correct (in some sense) policy to which others should be compared. The rationale is simply to normalize each household’s welfare using two extreme states that are well-defined for each household so that any two policies may be compared in a manner that assigns equal weight to every household.

The Impact of the Road Home Grant Program

This section presents the results of simulation experiments that assess the Road Home grant program’s impact on households’ resettlement choices. Using the simulation methods described above, I simulate households’ choices under the actual post-Katrina policy environment (as approximated in the estimation routine) and I simulate households’ choices under a scenario in which no grant program was provided. Table 7 presents the results of these simulations.

The simulations find that, among households with initially uninhabitable homes, the Road Home program increased the fraction of homes repaired or rebuilt by the pre-Katrina owner within four years of Katrina by 5.4 percentage points (from 49.0% to 54.4%, an 11.0% increase) and generated a similar increase in the fraction of households residing in their pre-Katrina home on Katrina’s fourth anniversary.

The simulations find that the Road Home program’s impact varied substantially across population subgroups. The program generated larger increases in rebuilding rates among households with pre-Katrina annual income below \$20,000 per year (10.2 percentage point increase, 20.2% increase), blacks (6.4 percentage point increase, 13.0% increase), and those with few or none of their losses covered by insurance, (9.9 percentage point increase, 22.5% increase). Among households with a pre-Katrina annual income below \$20,000 and with few or none of their losses covered by insurance, the Road Home program increased the rebuilding rate by 14.7 percentage points (a 34.0% increase). The program generated smaller impacts on rebuilding rates among non-blacks (3.2 percentage point increase, 6.7% increase) and those with all or most of their losses covered by insurance (0.7 percentage point increase, 1.2% increase).

Table 8 compares the Road Home program’s impact on households’ resettlement choices and on households’ welfare to the impacts of counterfactual disaster relief policies. These comparisons provide a better understanding of the mechanisms through which the Road Home program

³⁷Again, I approximate this expression by computing the quantity inside the expectation operator at each of the 50 support points for $A(0) \times \eta$ used during estimation and taking a weighted average that assigns weights based on the *ex post* probability that a household falls at each particular $A(0) \times \eta$ combination.

impacted households' welfare and choices. For each comparison, the first three columns report the program's impact on the fraction of households residing in the pre-Katrina home among all households with severely damaged homes (column 1), among households with severely damaged homes and pre-Katrina income below \$20,000 (column 2), and among households with severely damaged homes and pre-Katrina income above \$40,000 (column 3). Columns 4, 5, and 6 report impacts on household welfare for these same three groups.

The first three-row group in Table 8 compares the impact of the Road Home program to the impact of a program that makes subsidized loans available to all households on Katrina's second anniversary (to approximately match the timing of the Road Home program's "treatment"). These simulations find that the expanded loan program generates an impact on the rate of residence in the pre-Katrina home that is about 70% the size of the Road Home program's impact. Among households with pre-Katrina annual income below \$20,000, the loan program's impact is 87% the size of Road Home's impact, and among households with pre-Katrina annual income above \$40,000 the loan program's impact is 55% the size of Road Home's impact. Similarly, the loan program's impact on household welfare is 40% of Road Home's impact on average, but is 69% of Road Home's impact among low-income households and just 25% of Road Home's impact among higher-income households.

This set of findings suggests that a large majority of the Road Home program's impact on re-settlement behavior occurred by relaxing borrowing constraints for households who would have preferred to rebuild even in the absence of a subsidy if the associated costs could be spread over time, and the behavioral changes associated with relaxing these constraints were welfare enhancing. A smaller fraction of the program's impact occurred by inducing "marginal" households who were close to indifferent between locations to switch locations to capture the subsidy. This latter type of behavioral change has a first order effect on program costs but does not have a first order impact on household welfare in this (partial equilibrium) framework,³⁸ creating a deadweight loss, but these results suggest that this efficiency cost of the program was relatively small.

The second three-row group compares the Road Home program's impact to the impact of a program with the same rules as the Road Home program but that paid grants at $t = 0$. The simulations find that the fraction of households having rebuilt by Katrina's fourth anniversary is very similar under the actual Road Home program, which paid grants roughly two years after Katrina, and under a grant program that paid grants immediately after Katrina. However, the welfare effects of grant payments do depend on the timing of grant payments. The average welfare effect of the immediately rolled out program is roughly one third larger than the welfare effect of the actual Road Home program. This is because the immediately rolled out program allows households with a strong preference for living in New Orleans to do so earlier. The difference between the welfare effects of the immediately rolled out program and the later rolled out program was larger among low-income households, because that group's limited ability of to borrow makes the timing of its rebuilding decisions especially sensitive to the timing of grant payments.

³⁸If social spill-over effects occur, that is one household choosing to rebuild increases the benefit that neighbors derive from rebuilding, then even this component of the program's impact that appears purely distortionary in partial equilibrium could have a positive impact on aggregate welfare. Measuring the strength of this sort of social interaction is a focus of future work.

The Marginal Impact of Wage Subsidies and Direct grants

The Gulf Opportunity Zone program provided \$2,400-per-worker wage subsidies to employers who hired or retained a worker who lived on the Gulf Coast just prior to Hurricane Katrina. In this section I present the results of simulations that compare the marginal effect of \$2,400 per worker boost in the present value of New Orleans labor wages (spread evenly over the first four years following Katrina) to the impact of a \$2,400 per worker increase in the generosity of the Road Home program's option 1 grant. This comparison examines how the timing of a location subsidy to a borrowing constrained population influences the effect of the subsidy on households' location choices. I include only working households in this comparison.

Table 9 presents the results of these simulation experiments. Both policy changes cause small changes in behavior. However, the impact of the wage subsidy is only about half of the impact of an equivalent change in the rebuilding grant. The effects of grants and wage subsidies differ because, for borrowing constrained households, the benefit of a direct grant falls entirely in the period in which repairs are purchased, when the marginal utility of consumption is high. Among households with a pre-Katrina annual income less than \$20,000, the wage subsidy generates an impact that is only about 20% of the impact generated by an equivalent change in the rebuilding grant. On the other hand, among households with a pre-Katrina annual income above \$40,000, the wage subsidy generates an impact that is about 70% of the impact generated by an equivalent change in the rebuilding grant. These findings suggest that in the presence of borrowing constraints, wage subsidies are likely to have a smaller impact on households' resettlement choices than similarly sized direct grants. To the extent that borrowing constraints are concentrated in segments of the population that have few resources available for rebuilding, wage subsidies might even exacerbate disparities in rebuilding rates across groups.

One should not conclude from these results alone that subsidies to local firms are an ineffective means of providing disaster relief. To the extent that the incidence of these subsidies falls partially to firms, the subsidies may increase the number of firms finding it (weakly) profitable to operate in a disaster-affected area. Such a change may directly increase the flow benefit that a household derives from living in the area, if the presence of more firms increases the ability of households to obtain desired goods and services.

The Deadweight Loss from Disaster-Related Subsidies

Disaster-related location subsidies include up front subsidies like discounts to National Flood Insurance Program premiums and guarantees of future payments in the event of a disaster. Both types of subsidy reduce the expected cost of residing in a dangerous location. The size of the economic distortion generated by these subsidies depends on the extent to which the subsidies induce marginal households to alter their location decisions. As a final application of the model, I use the model's implied semi-elasticity of residence location with respect to housing costs to compute the deadweight loss associated with a hypothetical subsidy for living in New Orleans.

I consider the effect of a flow subsidy for living in New Orleans measured as a fraction τ of each household's home's value. For example, if $\tau = 1\%$ then a household with a \$100,000 home would receive \$1,000 per year. If the probability of a household's home being destroyed by a disaster is $\tau\%$ each year, then this subsidy provides the actuarially fair value of an insurance policy that

guarantees full compensation when a disaster occurs. Therefore, I interpret the distortionary effects of this policy as an approximation of the distortionary effects of a policy that guarantees relief in the event of a disaster.

Let $S(W - UCH)$ represent the long-run supply of households to New Orleans as a function of the real wage, annual labor earnings minus the user cost of housing. In the presence of moving costs and heterogeneity in location preferences across households, this supply curve slopes upward.³⁹ The subsidy generates a rightward shift in the supply curve, and therefore, if the labor demand and housing supply have positive price elasticities, the subsidy increases the equilibrium population of the city. To determine an upper bound on the deadweight loss of this subsidy, I consider the case in which the long run price elasticities of labor demand and housing supply are both infinite. With infinitely elastic labor demand and housing supply, none of the subsidy's incidence falls to firms or land developers (which would dampen the subsidy's distortion of households' location choices), and the elasticity of supply of residents to the city is a sufficient statistic for the deadweight loss associated with the subsidy.

The deadweight loss of this flow subsidy to residing in New Orleans may be approximated with the standard Harberger triangle,

$$DWL = \left(\frac{1}{2}\right) P_{TOT}^H \psi \tau^2 \quad (22)$$

where P_{TOT}^H is the total value of New Orleans' owner occupied housing, τ is the size of the flow subsidy expressed as a fraction of each household's home value, and $\psi = d \ln S / d \tau$ is the semi-elasticity of supply of residents to New Orleans with respect to τ .

I estimate the semi-elasticity ψ using a final set of simulations. I compare the fraction of households residing in New Orleans eight years after Hurricane Katrina (the maximum number of years for which all households are required to make location choices before some households begin to retire) under the actual post-Katrina policy regime and under a regime that pays an additional $\tau = 1\%$ subsidy to each household that owns its pre-Katrina home or rents a comparable residence in New Orleans. I estimate the semi-elasticity ψ using the percentage change in the fraction of households residing in the New Orleans from the first to the second of these scenarios.

Table 10 presents the results of these simulations and their implications for the proposed subsidy's deadweight loss. The value of the subsidy provided by guaranteed disaster relief depends on the probability that disaster relief will be needed. Computing that probability is beyond the scope of this study. I instead consider two scenarios, one in which a disaster is expected to occur once every 50 years and another in which a disaster is expected to occur every 30 years. If relief is

³⁹The classic Rosen-Roback model of spatial equilibrium (Rosen, 1979; Roback, 1982) and some applications of that model in the modern local public finance literature (Albouy, 2009, for example) assume that moving is costless and that preferences are homogeneous. That assumption immediately implies that in equilibrium, all individuals are indifferent between living in any location with population greater than zero and that the supply of residents to locations is infinitely elastic with respect to the real local wage. In such a model, spatially targeted subsidies must generate large distortions. See Busso, Gregory, and Kline (2011) for an expanded discussion of the efficiency consequences of local subsidies in the presence of location-preference heterogeneity.

guaranteed at no cost to the household, then the value of the subsidy under these scenarios is approximately 2% of the home's value annually (1/50 chance of full reimbursement) or 3.33% of the home's value annually (1/30 chance of full reimbursement). Using the simulation results, I estimate that $\psi = 0.45$. Using tax year 2011 property assessment records for all owner occupied homes in New Orleans, I estimate the value of the owner occupied housing stock to be roughly \$11 billion. Plugging these values into equation 22 finds that the deadweight loss associated with the $\tau = 2\%$ subsidy is roughly \$1 million per year, compared to the subsidy's cost of roughly \$220 million per year and finds that the deadweight loss associated with the $\tau = 3.33\%$ subsidy is roughly \$2.7 million per year, compared to the subsidy's cost of roughly \$363 million per year.

Underlying this procedure is the assumption that the elasticity of prior New Orleans residents' location decisions provides a reasonable approximation to the elasticity of location decisions among the full population "at risk" for eventually living in New Orleans. One might expect a more elastic response from young pre-Katrina New Orleans residents who were yet to be homeowners when Katrina occurred and from residents of other areas who consider whether to move to New Orleans.

To address this concern, one could turn to the existing literature and assume that among non-pre-Katrina homeowners the long-run supply elasticity of residents to New Orleans with respect to the New Orleans wage level equals an elasticity estimated by Kennan and Walker (2011).⁴⁰ Kennan and Walker (2011) present estimates of the supply elasticity of residents to many locations by simulating the effects of permanent changes to locations wages beginning when workers are 20, and each estimate falls between 0.5 and 0.75. As another option for addressing this concern, one might assume that among non-pre-Katrina homeowners the long-run supply elasticity of residents to New Orleans with respect to the New Orleans wage level is well-approximated by the model's implied elasticity for young households. I adopt this second approach with the rationale that this approach, which assumes a somewhat larger elasticity, leads to more conservative deadweight loss estimates.

Columns 3 and 4 of Table 10 present calculations of the deadweight losses associated with the same subsidies assuming a baseline under which 3/4 of households that reside in New Orleans exhibit the model's implied average location elasticity and 1/4 exhibit the higher location elasticity found for young households. This 3 : 1 ratio is roughly the ratio found in New Orleans prior to Katrina of Louisiana-native households to households with at least one head born outside of Louisiana. In this scenario, the deadweight loss associated with a guarantee of relief in the event of a future disaster is slightly less than 1% of the policy's expected flow costs if a disaster occurs with a probability of 1/50 each year, and the deadweight loss associated with a guarantee of relief in the event of a future disaster is slightly more than 1% of the policy's expected flow costs if a disaster occurs with a probability of 1/30 each year.

Columns 5 and 6 of Table 10 compute these deadweight losses under the assumption that all households exhibit the location elasticity found for young households. This scenario is perhaps appropriate when considering the policies' distortion over a very long horizon, perhaps multiple generations. However, even in this scenario that assumes the most elastic response, the deadweight loss associated with the flow subsidy to New Orleans is less than 3% of the policy's expected cost.

⁴⁰Note that Kennan and Walker report location elasticities with respect to nominal local wages, while the discussion in the previous paragraph considers location elasticities with respect to housing costs.

Substantial heterogeneity in location preferences accounts for the relatively small size of these deadweight loss figures relative to the subsidies' expected flow costs. While disaster-related location subsidies may comprise large difficult-to-justify transfers from residents of safe areas to residents of dangerous areas, the distortions associated with these transfers appear to be relatively small.

VIII. Conclusion

This paper develops and estimates a dynamic structural model of pre-Katrina New Orleans homeowners' post-Katrina resettlement choices and uses the model to investigate the immediate and long-term impacts of government disaster-relief policy. During the first two years following Hurricane Katrina, low-income households and black households rebuilt damaged homes at a much lower rate than higher-income households and non-black households. By the fourth anniversary of Katrina, disparities in rebuilding rates had closed substantially. The structural model finds that these patterns are best rationalized by a model in which pre-Katrina homeowners have a strong preference on average for residing in New Orleans, there is substantial heterogeneity in the strength of that location preference, and several large population subgroups face borrowing constraints.

Using the estimated model, I conduct a series of simulation experiments to assess the impact of the Road Home rebuilding grant program on households' resettlement choices and to assess the relative impacts of wage subsidies and rebuilding grants. The simulation experiments find that the Road Home program increased the fraction of households with severely damaged homes that had rebuilt within four years by about 11%. The program's impact occurred primarily within groups that otherwise lacked the savings or ability to borrow that would have been necessary to finance repairs or rebuilding if no grant program had been provided. A second set of simulation experiments comparing the impact of direct grants to the impact of wage subsidies finds that, on the margin, direct grants influence rebuilding rates by about twice the amount of similarly sized (in present value terms) wage subsidies.

I then use the model's implied elasticity of location choices with respect to financial incentives, in the context of a simple general equilibrium framework, to assess the deadweight loss associated with disaster-related location subsidies. The model finds that a guarantee of full compensation in the event of a future disaster generates a deadweight loss that is about 3% of the subsidy's expected cost under the conservative assumption that a devastating disaster will occur on average once per thirty years. While one may question the fairness of providing large transfers from safe locations to unsafe locations, these results suggest that the distortionary effects of disaster-related location subsidies are relatively small.

This paper considers a partial equilibrium model of households' resettlement choices, and as a consequence the policy experiments do not capture any general equilibrium price effects that the programs might have had, and perhaps more importantly, do not capture any social spill-over effects. A social spill-over would occur if, for instance, other things equal residing on a block on which 50% of one's neighbors had returned provided greater utility than residing on a block on which 40% of one's neighbors had returned. Capturing these types of effects in a dynamic model presents considerable challenges for estimation, but would lead to a more complete

assessment of program impacts and will be one focus of my future work.

TABLE 1.1. HOUSEHOLD BACKGROUND CHARACTERISTICS

Trait/Characteristic	Percentage
Household Headship	
Solo male headed	17.3
Solo female headed	29.6
Couple headed	53.1
Race	
Either head is black	57.6
Neither head is black	42.4
Education of Most Educated Head	
H.S. dropout	8.4
H.S. graduate	17.8
Some college	24.9
Bachelor's degree or higher	48.9
Household Age †	
Under 40	22.3
40-49	22.9
50-64	29.9
65 or older	25.0
Attachment to Place	
Home purchased > 25 years before Katrina	37.5
Home purchased 10-25 years before Katrina	25.8
Home purchased 0-10 years before Katrina	36.7
Either household head born outside of Louisiana	23.4
Neither household head born outside of Louisiana	76.6
Pre-Katrina Annual Household Income	
< \$20,000	17.7
\$20,000 - \$40,000	22.5
\$40,000 - \$80,000	33.6
> \$80,000	26.3
Neighborhood Poverty	
Block group poverty rate (2000 Census), <10%	21.3
Block group poverty rate (2000 Census), 10% - 25%	45.7
Block group poverty rate (2000 Census), > 25%	32.7
Observations	560

† Household age is defined to be the age of the male household head if present and the age of the female head otherwise. Note: This table reports the sample means of household background variables for the sample of pre-Katrina New Orleans households used to estimate the dynamic model. The sample includes only households that owned single-family homes (either free-and-clear or with a mortgage) at the time of Katrina. The sample also restricts attention to households in which at least one household head was employed during the year prior to Katrina or that had reached retirement age. Source: DNORS data and Assessor's data.

TABLE 1.2. STORM DAMAGE AND RESOURCES AVAILABLE FOR REPAIRS

	All Homeowners (N=560)					
	All	Pre-Katrina HH Income			Race	
		<\$20k	\$20-40k	>\$40k	Blacks	Nonblacks
Flood exposure						
No flooding	26	17	16	31	11	45
0-2 feet	13	15	14	13	12	15
2-4 feet	21	31	32	16	29	11
> 4 feet	40	37	38	40	48	29
Self-reported home damage category						
Still liveable	31	26	19	36	13	53
Unliveable	48	56	57	44	60	33
Destroyed	21	18	24	21	27	14
>30% decline in appraised structure value	71	80	74	69	86	52
Imputed repair cost (\$1000s)						
Repair costs	65	44	47	75	67	64
Households with Severely Damaged Homes (N=414)						
	All	Pre-Katrina HH Income			Race	
		<\$20k	\$20-40k	>\$40k	Blacks	Nonblacks
Imputed repair cost (\$1000s)						
Repair costs	84	51	55	103	74	108
Property damage covered by insurance						
Few/none of losses covered	25	39	32	18	28	19
Some/half of losses covered	47	46	46	48	49	45
All/most of losses covered	28	15	22	34	24	37
Percentiles of Liquid Asset Distribution						
5th percentile	0	0	0	0	0	0
25th percentile	2	0	1	2	0	5
50th percentile	7	3	4	10	2	19
75th percentile	31	14	20	40	10	78
95th percentile	219	101	145	275	101	477

Note: These figures provide sample mean outcomes for pre-Katrina New Orleans households who owned their homes prior to the storm, omitting households whose household age is less than 65 and for whom neither household head is employed. Source: DNORS and Assessor's data.

TABLE 1.3. HOME SALES AND PARTICIPATION IN THE LOUISIANA ROAD HOME AMONG HOUSEHOLDS WITH SEVERELY DAMAGED HOMES

Group	Option 1	Option 2 or 3	Private Sale	No Sale or Grant	Total
All households with damaged homes	67	8	10	15	100
Not destroyed but uninhabitable	68	5	10	17	100
Destroyed	75	14	6	5	100
Few/none of losses covered by insurance	78	12	2	9	100
Some/half of losses covered by insurance	71	9	7	13	100
All/most of losses covered by insurance	51	3	21	25	100
No flooding	0	0	18	82	100
0-2 feet	60	0	15	25	100
2-4 feet	75	5	8	12	100
> 4 feet	68	12	9	11	100
Fraction of block homes damaged: <50%	29	0	0	71	100
Fraction of block homes damaged: 50-90%	53	8	11	29	100
Fraction of block homes damaged: >90%	71	8	10	11	100
Observations					414

Note: This table describes patterns of participation in the Road Home Homeowner program within the DNORS sample analyzed in this study. A household that selected Road Home option 1 received a grant payment equal to its repair costs minus any insurance payments received and agreed to repair and reside in the home within three years of receiving the grant. A household that selected option 2 agreed to sell its home to a state land trust at a price equal to the grant that it would have been paid under option 1 and agreed to purchase another home in Louisiana within three years. A household that selected option 3 agreed to sell its home to a state land trust at a price equal to 60% of the grant that it would have been paid under option 1, but did not face any residency or home-purchase requirements. Source: DNORS, Assessor's Data, and Road Home participation data from the Louisiana Recovery Authority.

TABLE 1.4. WAGE EQUATION

Dependent variable: ln(earnings)	(1)
Ln(mean occupation wage in local labor market)	1.00 [constrained]
Age	0.137*** [0.005]
Age squared	-0.001*** [0.000]
Race	
non-Black	---
Black	-0.114*** [0.028]
Gender	
Male	---
Female	-0.291*** [0.026]
Education	
High school dropout	-0.331*** [0.044]
High school graduate	---
Some college	0.045 [0.034]
Bachelor's+	0.177*** [0.034]
Intercept	-3.375*** [0.102]
Observations	5,099

Note: This table reports estimates of a regression equation explaining the difference between individual workers' earnings and the average earnings in each worker's three-digit occupation. The sample includes all working respondents to the 2005 American Community Survey from the New Orleans metropolitan area and all DNORS respondents who worked during the year prior to Katrina. The dependent variable is the log of the workers annual earnings. The regressions imposes the restriction that the coefficient on the log of the mean earnings in the worker's two-digit industry in New Orleans (as measured in the American Community Survey) equals one. Source: Author's calculations using ACS and DNORS.

TABLE 1.5. UTILITY FUNCTION AND BORROWING RATE PARAMETERS

Parameter:	Estimate
<u>Flow benefit from residence location - B(L)</u>	
<u>Residence away from New Orleans: B(3)</u>	0.000 [normalized]
<u>Residence in New Orleans: B(2)</u>	
Long-run level	-0.068 [0.018]***
Time trend during first five years after Katrina	-0.148 [0.011]***
<u>Additional benefit from pre-Katrina home: B(1)-B(2)</u>	
Intercept	0.577 [0.063]***
Fraction of block homes damaged: 50-90%	
Intercept shift (long run)	-0.009 [0.096]
Time trend during first five years after Katrina	0.247 [0.036]***
Fraction of block homes damaged: >90%	
Intercept shift (long run)	0.072 [0.054]
Time trend during first five years after Katrina	0.538 [0.074]***
Black	-0.203 [0.057]***
Block poverty rate (2000 Census), 10% - 25%	-0.003 [0.093]
Block poverty rate (2000 Census), > 25%	0.050 [0.059]
Purchased home 10-20 years before Katrina	-0.066 [0.048]
Purchased home > 20 years before Katrina	-0.135 [0.055]*
Neither head born in Louisiana	0.002 [0.029]
<u>Moving utility cost: γ^M</u>	
Intercept: Moves in period $t=1$	3.515 [0.037]***
Intercept: Moves in period $t>1$	2.786 [0.067]***
Moves to or from New Orleans	1.240 [0.063]***
Move home in first period after home repair	-4.237 [0.098]***
<u>Repairing/rebuilding utility cost: χ^R</u>	
Intercept	3.582 [0.120]***
Additional utility cost to rebuilding a destroyed home	1.203 [0.019]***
<u>Log of Borrowing interest rate: $\ln(1+r^B)$</u>	
Intercept	$\ln(1/\beta)$ [normalized]
Black	0.141 [0.003]***
No bachelor's degree	0.357 [0.011]***
Household income before Katrina < \$20k	0.414 [0.015]***
Household income before Katrina \$20-40k	-0.012 [0.003]***
<u>Scale of shocks relative to $u(c)$: α</u>	
	5.047 [1.312]
<u>Scale of persistent unobserved heterogeneity: σ</u>	
	0.647 [0.045]
Observations - household-periods	6,720
Observations - households	560
Log-Likelihood	-2,629.3

Note: This table reports nested fixed point maximum likelihood estimates of the model's structural parameters (see Section 6 for details). Asymptotic standard errors clustered at the neighborhood level are reported in brackets. Asterisks indicate statistical significance at confidence levels of (*) 5%, (**) 1%, and (***) 0.1% -- no *s are reported for the scale parameters. Source: Author's calculations using DNORS and Assessor's data covering calendar years 2005 through 2009.

TABLE 1.6. MODEL FIT

Percentage of households	All Households											
	All Households			HH Income: < \$20k			HH Income: \$20-40k			HH Income: > \$40k		
	Data	Model	χ^2	Data	Model	χ^2	Data	Model	χ^2	Data	Model	χ^2
Home liveable												
1st Anniversary	30.7	32.6	0.06	22.7	22.6	0.00	21.2	25.7	0.57	36.7	38.1	0.02
2nd Anniversary	38.0	40.8	0.13	28.4	31.5	0.18	29.2	35.0	0.63	44.2	45.7	0.03
3rd Anniversary	47.7	53.4	0.78	30.7	46.5	4.26	36.5	49.0	2.97	56.7	57.1	0.00
4th Anniversary	69.6	66.3	0.56	67.0	61.7	0.81	67.2	63.8	0.25	71.3	68.5	0.34
Living in pre-Katrina home												
1st Anniversary	22.1	22.4	0.00	14.8	14.3	0.01	16.1	18.5	0.22	26.6	26.1	0.01
2nd Anniversary	29.1	29.7	0.01	21.6	21.9	0.00	23.4	26.4	0.22	33.4	33.1	0.00
3rd Anniversary	36.8	41.8	0.97	23.9	36.0	3.18	30.7	39.5	1.87	42.7	44.2	0.09
4th Anniversary	57.9	55.2	0.51	58.0	51.6	1.15	59.1	55.1	0.39	57.3	56.2	0.08
Living in New Orleans												
1st Anniversary	60.7	61.1	0.01	60.2	57.0	0.34	52.6	60.2	1.62	64.2	62.5	0.12
2nd Anniversary	71.4	71.3	0.00	71.6	68.1	0.57	67.2	71.3	1.32	73.1	72.2	0.04
3rd Anniversary	75.4	75.5	0.01	73.9	72.8	0.05	74.5	76.0	0.21	76.1	76.0	0.00
4th Anniversary	78.4	76.7	0.66	76.1	74.3	0.16	81.0	77.8	1.34	77.9	76.9	0.09
Sold pre-Katrina home												
1st Anniversary	3.2	4.8	4.12	2.3	5.8	4.68	1.5	4.5	9.02	4.2	4.7	0.18
2nd Anniversary	8.8	9.1	0.06	5.7	11.0	4.61	5.8	8.5	1.76	10.7	8.8	1.64
3rd Anniversary	16.1	12.8	4.64	11.4	15.4	1.28	13.9	11.9	0.39	18.2	12.4	11.78
4th Anniversary	19.6	15.9	4.15	19.3	19.1	0.00	15.3	14.9	0.02	21.5	15.5	7.13
Households	560			78			111			371		
Percentage of households	Households with an Initially Damaged Home											
	All Households			HH Income < \$20k			HH Income: \$20-40k			HH Income > \$40k		
	Data	Model	χ^2	Data	Model	χ^2	Data	Model	χ^2	Data	Model	χ^2
Home liveable												
1st Anniversary	6.3	8.9	1.26	6.9	6.7	0.00	3.6	9.1	6.16	7.4	9.5	0.46
2nd Anniversary	16.2	20.0	0.88	13.7	17.4	0.64	13.4	20.5	2.23	18.3	20.5	0.21
3rd Anniversary	29.2	37.0	3.50	16.4	35.5	11.47	22.3	37.6	7.77	36.7	37.2	0.02
4th Anniversary	58.9	54.4	1.32	60.3	53.8	0.99	59.8	55.7	0.31	58.1	53.9	1.34
Living in pre-Katrina home												
1st Anniversary	3.4	5.6	2.23	5.5	4.3	0.13	2.7	6.0	2.58	3.1	5.9	2.90
2nd Anniversary	11.8	13.6	0.34	11.0	12.2	0.09	9.8	14.2	1.19	13.1	13.7	0.05
3rd Anniversary	22.0	29.2	4.70	13.7	28.8	7.62	17.9	29.7	6.14	26.6	29.0	0.71
4th Anniversary	50.0	47.1	0.59	54.8	47.5	1.25	52.7	48.4	0.34	47.2	46.3	0.05
Living in New Orleans												
1st Anniversary	51.9	53.4	0.20	58.9	52.6	0.89	45.5	54.3	2.15	52.8	53.1	0.00
2nd Anniversary	65.7	65.5	0.00	69.9	64.8	0.95	61.6	66.7	2.05	66.4	65.2	0.06
3rd Anniversary	71.3	71.0	0.00	72.6	70.5	0.17	69.6	72.3	0.53	71.6	70.5	0.07
4th Anniversary	74.9	73.0	0.82	75.3	72.6	0.28	77.7	74.6	1.00	73.4	72.4	0.07
Sold pre-Katrina home												
1st Anniversary	2.4	5.4	12.63	0.0	6.2	-	0.9	4.9	21.12	3.9	5.3	1.26
2nd Anniversary	8.7	10.2	0.95	4.1	11.7	10.85	5.4	9.4	3.42	11.8	10.1	0.89
3rd Anniversary	17.4	14.2	2.87	9.6	16.3	3.20	15.2	13.1	0.34	21.0	14.1	11.02
4th Anniversary	21.3	17.6	3.02	19.2	20.1	0.04	16.1	16.3	0.01	24.5	17.5	7.26
Households	414			66			91			257		

Note: This table compares the value of various sample moments to the model's predicted value for those moments. For each comparison, a cluster-robust chi-squared test statistic (clustered at the neighborhood level) is reported for a test of the null that the predicted and empirical moments are equal. The critical value for the chi-squared(1) distribution with alpha=0.05 is 3.84.

TABLE 1.7. THE IMPACT OF THE ROAD HOME PROGRAM ON HOUSEHOLDS' RESETTLEMENT CHOICES

Group	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Resettlement Outcomes -- Measured on Katrina's 4th Anniversary							
	Home Repaired by Original Owner		Living in pre-Katrina Home		Living in New Orleans		Sold Home	
	No Grant Program	Road Home Program	No Grant Program	Road Home Program	No Grant Program	Road Home Program	No Grant Program	Road Home Program
All Households	49.0	54.4	42.7	47.1	70.8	73.0	19.9	17.6
Household Income:								
< \$20k	43.7	53.8	38.6	47.5	69.2	72.6	22.9	20.1
Between \$20k and \$40k	49.5	55.7	43.4	48.4	72.4	74.6	18.9	16.3
> \$40k	50.4	53.9	43.7	46.3	70.6	72.4	19.4	17.5
Race:								
Black	49.4	55.8	43.7	48.9	73.2	75.7	17.7	15.4
Non-black	48.0	51.2	40.5	43.1	65.8	67.1	24.7	22.5
Insurance coverage:								
Few/none of losses covered	43.8	53.7	39.0	47.5	68.6	73.4	21.3	17.8
Some/half of losses covered	46.3	52.2	40.7	45.2	69.9	71.9	21.2	18.5
All/most of losses covered	58.1	58.8	49.4	49.9	74.4	74.5	16.5	16.1
Other illustrative subgroups:								
HH income < \$20k and few/none of losses insured	43.1	58.1	39.3	53.0	69.5	75.9	20.6	17.0
Bachelor's degree and all/most of losses insured	56.9	58.0	47.3	48.4	72.7	72.9	17.8	17.1

Note: This table provides the results of simulation experiments designed to assess the partial equilibrium impact of the Road Home program. The first four pairs of columns (columns 1-8) report pairs of simulated outcomes, the first of each pair reporting the outcome under a scenario in which no Grant Program was in place and the second reporting the outcome under the scenario in which the Road Home program was available. The final columns (column 9) computes the Road Home program's impact households' expected welfare. Expected welfare is computed at time t=0. For each household, welfare in the scenario in which no grant program is in place is normalized to zero. For each household, expected welfare under the scenario in which all home repairs not covered by insurance are reimbursed at t=0 is defined to be one hundred. The welfare impact of the Road Home program is reported as a fraction of the welfare impact of immediate full reimbursement. Source: Author's calculations using the estimated model.

TABLE 1.8. DECOMPOSITION OF THE ROAD HOME PROGRAM'S IMPACT ON HOUSEHOLDS' RESETTLEMENT CHOICES AND HOUSEHOLDS' WELFARE

Scenario	Living in pre-Katrina home on Katrina's 4th anniversary			Household Welfare (0%= no-grant program, 100% = full reimbursement at t=0)		
	All	Pre-Katrina HH Income < \$20k	Pre-Katrina HH Income > \$40k	All	Pre-Katrina HH Income < \$20k	Pre-Katrina HH Income > \$40k
	Households			Households		
Access to Credit						
No grant program (baseline)	49.0	43.7	50.4	0.0%	0.0%	0.0%
Loans available to all households (no grant program)	+3.8	+8.2	+2.1	+23%	+36.1%	+13.7%
Actual Road Home program	+5.4	+10.1	+3.5	+58.2%	+63.5%	+58.6%
Timing of Road Home Program						
No grant program (baseline)	49.0	43.7	50.4	0.0%	0.0%	0.0%
Actual Road Home program	+5.4	+10.1	+3.5	+58.2%	+63.5%	+58.6%
Road Home program rolled out at t=0	+5.2	+10.5	+3.2	+79.3%	+90.3%	+76.3%

Note: This table provides the results of a set of simulations that decompose the Road Home program's impact by examining possible avenues through which the program could influence households' choices and welfare. The first set of three rows compares the impact of an expanded loan program to the impact of the Road Home program. The second set of three rows compares the impact of the Road Home program to the impact of a program with the same rules but that is rolled out at t=0. Source: author's calculations using the estimated model.

TABLE 1.9: THE RELATIVE IMPACT OF DIRECT GRANTS AND
SIMILARLY SIZED WAGE SUBSIDIES

Group -- among population of working-aged households	Home repaired by pre-Katrina owner by Katrina's fourth Anniversary
All households with home damage not covered by insurance	
Actual policy environment	57.601
Direct grant reduced by \$2,400	-0.056
Present value of New Orleans wages \$2,400 lower	-0.027
Households with pre-Katrina annual income < \$20k	
Actual policy environment	59.017
Direct grant reduced by \$2,400	-0.188
Present value of New Orleans wages \$2,400 lower	-0.039
Households with pre-Katrina annual income > \$40k	
Actual policy environment	57.465
Direct grant reduced by \$2,400	-0.043
Present value of New Orleans wages \$2,400 lower	-0.032

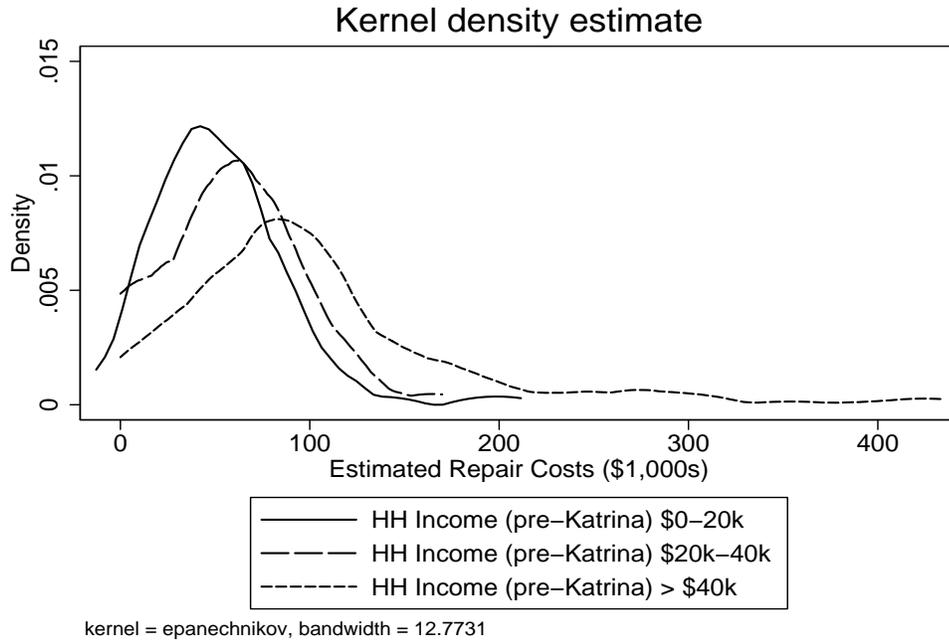
Note: This table provides the results of a set of simulation experiments that assess the relative impact of direct grants and similarly sized wage subsidies on households' resettlement choices. These experiments restrict attention to working age households (head < 65 years old). For each set of results (three-row groups), the first row provides the fraction of homes repaired by the pre-Katrina owner under a baseline simulation that imposed the actual post-Katrina policy environment. The second row provides the impact of removing \$2,400 per worker from the size of each household's Road Home option 1 grants. The third row provides the impact of removing \$2,400 per worker from the present value of New Orleans labor wages (spread evenly over the first four years following Katrina). Source: author's calculations using the estimated model.

TABLE 1.10. THE DEADWEIGHT LOSS ASSOCIATED WITH A FLOW DISASTER-INSURANCE SUBSIDY

Spatial elasticity used in DWL calculation: Per-year probability of a disaster:	Average Households' Elasticity		75% Average Elasticity 25% Young Elasticity		Young Households' Elasticity	
	1/50	1/30	1/50	1/30	1/50	1/30
	Baseline value of housing stock	\$11 B	\$11 B	\$11 B	\$11 B	\$11 B
Subsidy: τ (as a % of home value)	2.00%	3.30%	2.00%	3.30%	2.00%	3.30%
Elasticity: $\psi = d\ln(\text{N.O. households}) / d[\text{subsidy } (\tau)]$	0.450	0.450	0.450	0.450		
Elasticity: ψ among young households			1.737	1.737	1.737	1.737
Flow cost of subsidy ([Value of Housing Stock] $\cdot \tau$)	\$220 M	\$363 M	\$220 M	\$363 M	\$220 M	\$363 M
DWL ($\approx 1/2 \cdot [\text{Value of Housing Stock}] \cdot \psi \cdot \tau^2$)	\$1 M	\$2.7 M	\$1.7 M	\$4.6 M	\$3.8 M	\$10.4 M

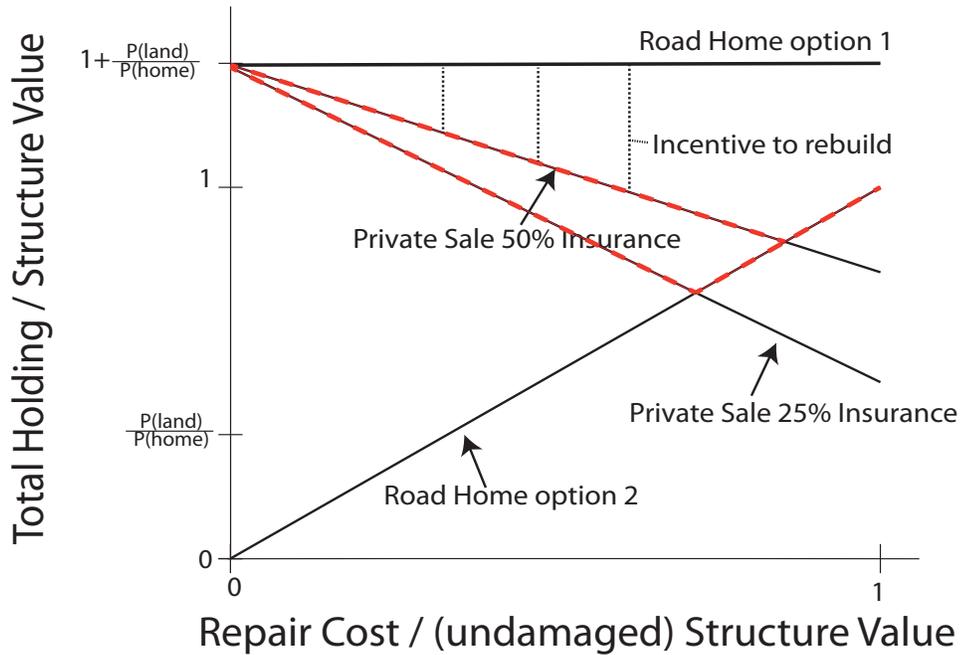
Note: This table provides the quantities necessary to compute the deadweight loss associated with a hypothetical flow subsidy to residence in New Orleans. Sources: The baseline value of the New Orleans housing stock is computed from Orleans Parish Property Assessment data. The semi-elasticity of the New Orleans homeownership population with respect to a flow subsidy to residence in New Orleans (an annual subsidy as a fraction of a household's home's value) is computed using simulations with the estimated model. The quantity labeled average households' elasticity is computed using the change in the fraction of simulated panels for all model households in which the household is living in New Orleans eight years after Katrina. The quantity labeled young households' elasticity is computed using a similar calculation among households that were age 35 or less when Katrina occurred.

FIGURE 1.1: REPAIR COSTS AMONG HOUSEHOLDS WITH INITIALLY UNINHABITABLE HOMES



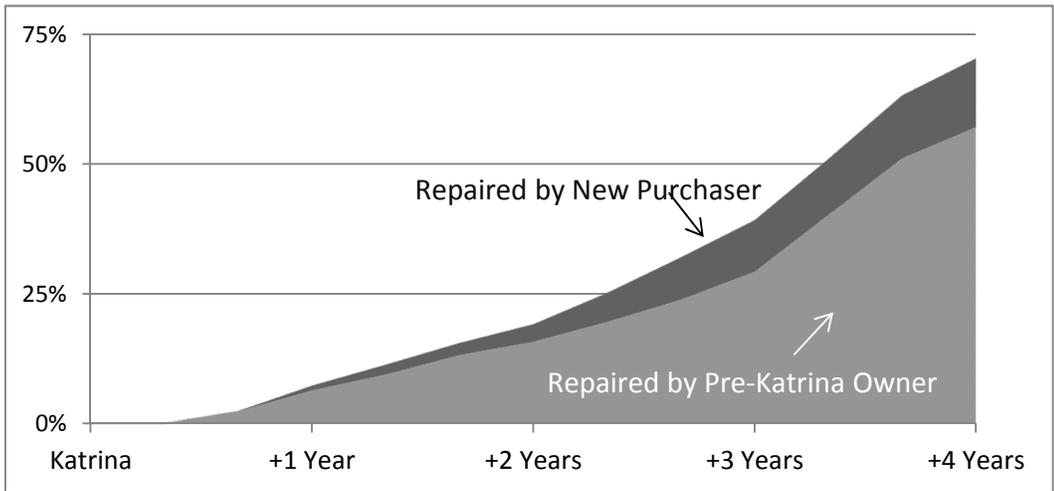
Note: This figure presents kernel density plots of repair costs for households with low, middle, and high household income during the year prior to Hurricane Katrina. Source: DNORS and Orleans Parish Assessor's Office property database.

FIGURE 1.2. THE FINANCIAL INCENTIVE TO REBUILD ASSOCIATED WITH THE ROAD HOME PROGRAM

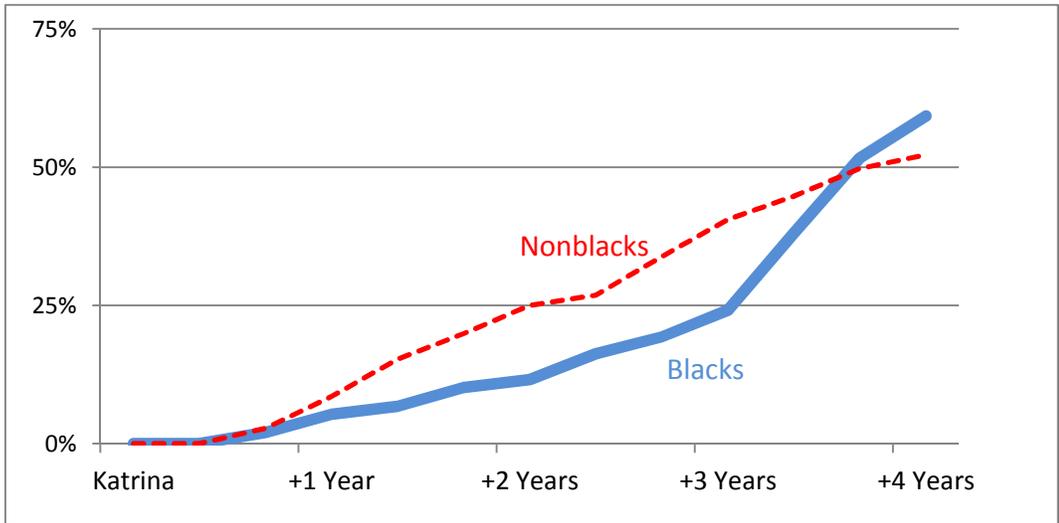


Note: If a household accepts an option 2 Road Home rebuilding grant, it holds cash approximately equal to the cost of repairs (the sum of insurance payments and the grant payment). If a household sells its home privately, it holds cash approximately equal to the as-is value of the structure following Katrina (the sale price) plus any insurance payments. This figure plots these two quantities as fractions of the home's value if it was fully repaired. The financial incentive to rebuild is the difference between the value of the home if fully repaired, which the household owns under Road Home option 1, and the upper envelope of these two quantities. The financial incentive to rebuild is highest for households with little insurance and for households with intermediate levels of home damage.

FIGURE 1.3: TIMING OF HOME REPAIRS



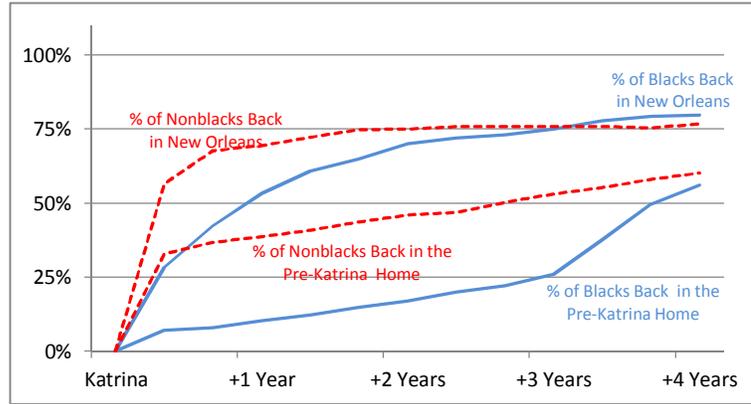
A. HOME REPAIRS BY ORIGINAL OWNER OR NEW PURCHASER



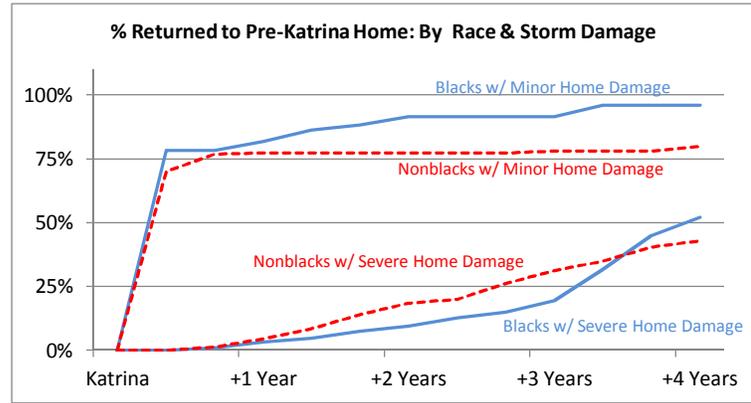
B. HOME REPAIRS BY ORIGINAL OWNER: BY RACE

Source: DNORS and Orleans Parish Assessor's Office property database.

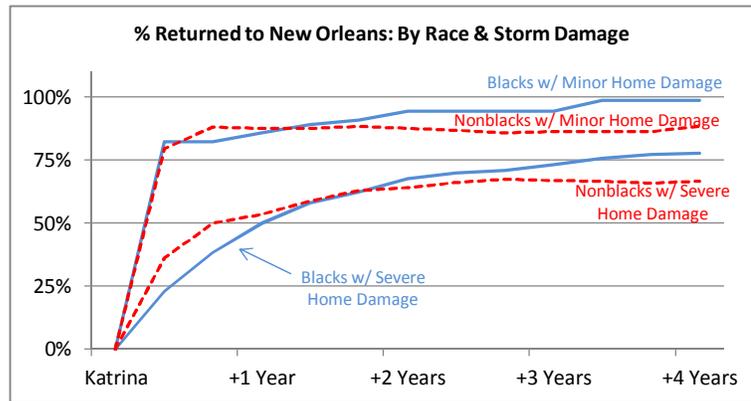
FIGURE 1.4: TIMING OF RETURNS TO NEW ORLEANS AND TO HOUSEHOLDS' PRE-KATRINA HOMES



A. FRACTION LIVING IN NEW ORLEANS AND FRACTION LIVING IN THE PRE-KATRINA HOME BY RACE



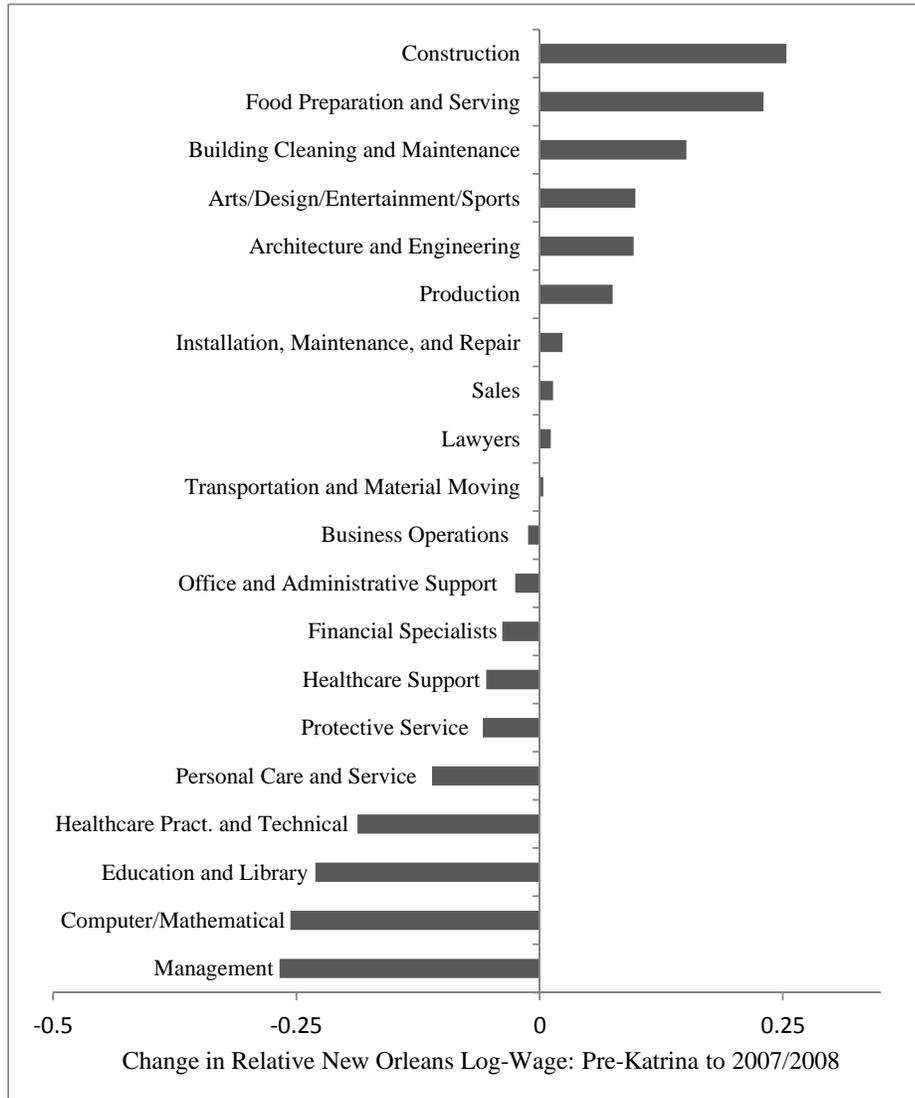
B. FRACTION LIVING IN PRE-KATRINA HOME BY RACE AND PROPERTY DAMAGE



C. FRACTION LIVING IN NEW ORLEANS BY RACE AND PROPERTY DAMAGE

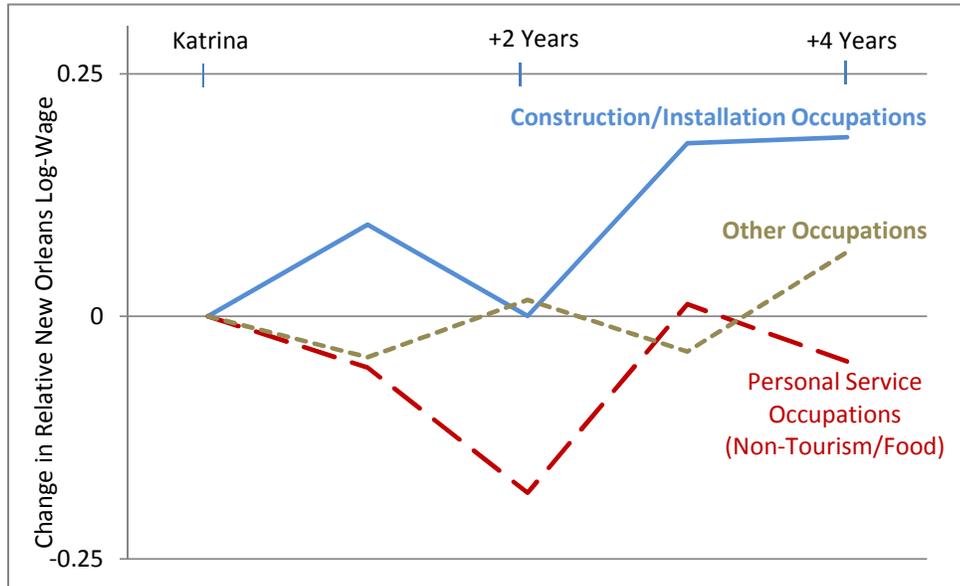
Source: DNORS and Orleans Parish Assessor's Office property database.

FIGURE 1.5: CHANGES IN RELATIVE NEW ORLEANS WAGES FROM PRIOR TO KATRINA TO 2007/2008: BY OCCUPATION



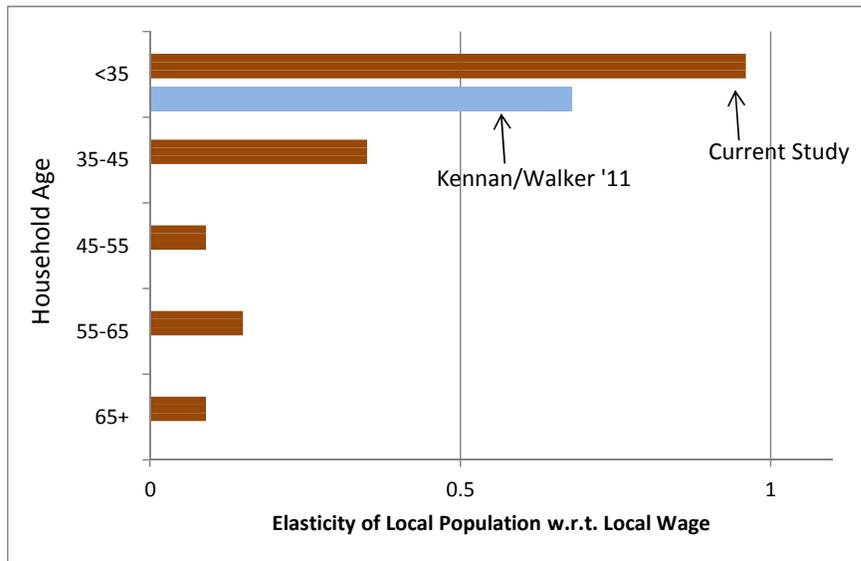
Note: The "relative New Orleans log-wage" is the difference between the log of mean annual earnings for workers in the occupation in New Orleans minus the log of mean annual earnings for workers in the occupation in other Southern metropolitan areas. This figure presents changes in the relative New Orleans log-wage from the 2005 ACS to the 2008 ACS for each two-digit occupation that comprised at least 1% of the workforce in pre-Katrina New Orleans. The ACS annual earnings questions asks about earnings during the 12 months prior to the ACS interview, so 2005 responses describe earnings during a period almost entirely before Katrina, and 2008 responses describe earnings that occurred roughly half in 2007 and half in 2008. Source: American Community Survey, 2005 and 2008.

FIGURE 1.6: CHANGES IN RELATIVE NEW ORLEANS WAGES AFTER HURRICANE KATRINA BY OCCUPATION



Note: The "relative New Orleans log-wage" is the difference between the log of mean annual earnings for workers in the occupation in New Orleans minus the log of mean annual earnings for workers in the occupation in other Southern metropolitan areas. This figure presents changes in the relative New Orleans log-wage from the 2005 ACS to the relative New Orleans log-wage in later years for three broad occupation classifications. Source: American Community Survey, 2005-2009.

TABLE 1.7. SPATIAL WAGE ELASTICITIES IMPLIED BY THE ESTIMATED MODEL



Note: This figure plots the elasticity of local population with respect to the local wage. Separate elasticities are provided depending on households' ages at the time of the permanent wage change. The dark bars plot the elasticities implied by the model estimated in this study. The light bar plots the elasticity estimated by Kennan and Walker (2011) using a sample comprised exclusively of younger workers. Source: Author's calculations using the estimated model and Kennan and Walker (2011).

TABLE I.A1. CONSTRUCTING HOUSING-RELATED PRICE VARIABLES

Variable	Method Used to Create Variable	Data source
Monthly mortgage payment for pre-Katrina home	Standard 30-year mortgage formula: inputs include the home's purchase date, purchase price, and an assumed 20% down payment	-Assessor's data
Monthly rent for a different New Orleans residence	Step 1: impute the home's rental value in pre-Katrina New Orleans $0.0785 \times$ (appraised pre-Katrina value) / 12. Step 2: adjust that for differences in rental prices between pre-Katrina New Orleans and post-Katrina New Orleans using regression adjusted price indexes (see Appendix II for details on computing rental price indexes)	-Pre-Katrina appraised home values come from Assessor's data -Housing price indices are computed using information on rental prices and building characteristics from the American Community Survey
Monthly rent for a residence in another Southern metro	Step 1: impute the home's rental value in pre-Katrina New Orleans $0.0785 \times$ (appraised pre-Katrina value) / 12. Step 2: adjust that for differences in rental prices between pre-Katrina New Orleans and the post-Katrina market in other Southern metro areas using regression adjusted price indexes (see Appendix II for details on computing rental price indexes)	-Pre-Katrina appraised home values come from Assessor's data -Housing price indices are computed using information on rental prices and building characteristics from the American Community Survey
Cost of repairing home damage	-If home was destroyed, repair cost is imputed to be the appraised pre-Katrina improvement value multiplied by a price index that reflects the difference in housing prices between pre-Katrina and post-Katrina New Orleans (this assumes that post-Katrina housing prices more accurately reflect building costs than pre-Katrina prices (Vigdor, 2008)) -If the home was uninhabitable but not destroyed, repair cost is imputed to be the difference between the appraised pre-Katrina improvement value and the appraised improvement value immediately following Katrina multiplied by a price index that reflects the difference in housing prices between pre-Katrina and post-Katrina New Orleans	-Appraised home values come from Assessor's data -Housing price indices are computed using information on rental prices and building characteristics from the American Community Survey
Insurance payment	Imputed by scaling the household's repair costs by a fraction based on the household's categorical response to the DNORS question asking what fraction of losses were covered by insurance (all or almost all, 1.0; most, 0.75; about half, 0.5; some 0.25; very few, none, or had no insurance, 0.0)	-DNORS
Sale price of pre-Katrina home if it is repaired	Imputed by multiplying the home's appraised pre-Katrina value using regression adjusted price indexes (see Appendix II for details on computing rental price indexes)	-Pre-Katrina appraised home values come from Assessor's data -Housing price indices are computed using information on rental prices and building characteristics from the American Community Survey

TABLE 1.A2. HOUSING PRICE INDEX REGRESSIONS

	(1)	(2)
<hr/>		
Housing Market Indicators		
Pre-Katrina New Orleans	---	---
Post-Katrina New Orleans	0.383*** [0.015]	0.352*** [0.015]
Elsewhere in Metro South	0.333*** [0.014]	0.233*** [0.013]
Constant	6.142*** [0.013]	6.142*** [0.013]
Controls for building characteristics: centered around 2005 New Orleans means ($\bar{X}_i - \bar{X}$)	No	Yes
Observations	706,073	706,073

Note: These regressions were computed using all renting households that lived in the New Orleans MSA in 2005-2009 or in another Southern metro from 2006-2009. The estimates were computed by regressing the log of rent on a constant, an indicator that an observation came from post-Katrina New Orleans, an indicator that an observation came from another southern metro, and (in the second column) a set of building characteristic variables centered around their mean values in the 2005 New Orleans sample. The housing market dummies should be interpreted as the mean difference in the log of rents between the indicated housing market and pre-Katrina New Orleans. Source: American Community Survey.

TABLE 1.A3. DISTRIBUTION OF STORM DAMAGE BY FLOOD EXPOSURE

Flooding	Self-reported home damage				Appraised structure value declined >30%		
	Still	Uninhabitable but		Total	Still		
	Inhabitable	not Destroyed	Destroyed		Inhabitable	Uninhabitable	Total
None	92	8	0	100	84	16	100
0-2 feet	31	60	9	100	24	76	100
2-4 feet	11	69	19	100	4	96	100
> 4 feet	1	59	39	100	0	100	100
Observations	560				560		

Note: this table describes the relationship between the depth of flooding on a household's block and the damage to the household's home. Source: flood depth comes from maps produced by FEMA. Self reported home damage comes from DNORS interviews. The property appraisal based measure classifies a property as uninhabitable if its appraised improvement value declined by more than 30% from the last appraisal prior to Katrina to the first appraisal after Katrina (conducted during the Fall of 2005).

TABLE 1.A4: POST-KATRINA POLICY TIMELINE

Date	Event
August 27, 2005	<ul style="list-style-type: none"> • New Orleans Mayor Ray Nagin announces a state of emergency and suggests a voluntary evacuation of the city.
August 28, 2005	<ul style="list-style-type: none"> • Mayor Nagin announces a mandatory evacuation of New Orleans, and the Louisiana Superdome is opened as a refuge for those unable to leave the city.
August 29, 2005	<ul style="list-style-type: none"> • Hurricane Katrina makes landfall on the Gulf coast.
August 29, 2005	<ul style="list-style-type: none"> • President George W. Bush declares much of the Gulf coast (including New Orleans) to be a major disaster area. This designation allows residents who suffer storm-related damages to seek assistance through standing federal disaster relief programs, such as FEMA's Disaster Assistance Grants and SBA's Disaster Assistance loans.
September 6, 2005	<ul style="list-style-type: none"> • Mayor Nagin orders a forced evacuation of New Orleans, and National Guard troops enter the city to enforce the order. The city's population of pre-storm residents falls to nearly zero, from the pre-storm level of about 460,000.
September 28, 2005	<ul style="list-style-type: none"> • Residents of least damaged areas of the city are first permitted to return, but, with few city services yet restored, very few return this early.
December 17, 2005	<ul style="list-style-type: none"> • Congress passes the Gulf Opportunity Zone initiative into law.
February 20, 2006	<ul style="list-style-type: none"> • Louisiana officially announces the Road Home rebuilding grant program .
March 2, 2007 (week of)	<ul style="list-style-type: none"> • The first Road Home rebuilding grant is paid to a New Orleans homeowner.
July 31, 2007	<ul style="list-style-type: none"> • The deadline for submitting Road Home grant applications occurs.
December 7, 2007 (week of)	<ul style="list-style-type: none"> • About 32,000 Road Home grants have been paid to New Orleans homeowners, about 75% of the total number of grants eventually dispersed.

Appendix I: Imputing Home Repair Status

I construct measures of home repair status using a three step procedure. The procedure is to:

1. Use annual appraised improvement values to infer repair status on 1st, 2nd, 3rd, and 4th anniversaries of Katrina.
2. Estimate a Weibull hazard model of the time until home repair for households with initially uninhabitable homes, following the approach of Grummer-Strawn (1993) for fitting such a model to “current-status” data.
3. Use the fitted model and the measures of repair status on anniversaries of Katrina to stochastically impute a repair status for the periods that do not fall on anniversaries of Katrina.

Step 1: I classify a home as initially uninhabitable if the property’s improvement value in the Orleans Parish Assessor’s Office property database declined by more than 30% between the 2004 appraisal and the 2005 appraisal or the household self-reported its home having been rendered uninhabitable by Katrina. The 2005 appraisal occurred in the months after Katrina in advance of the 2006 tax year and reflects Katrina-related home damage.

If a home is classified as liveable (not uninhabitable) immediately following Katrina, classify the home as liveable in all subsequent periods. For homes classified as uninhabitable immediately following Katrina, classify the home as liveable on the 1st, 2nd, 3rd, and 4th anniversaries of Katrina if, during the 2006, 2007, 2008, and 2009 appraisals respectively, the appraised improvement value exceeds the 2005 appraised improvement value.

One might fear that blanket appreciations applied by the Assessor’s Office would cause some still-damaged homes to be classified by this procedure as liveable following a small increase in the appraised value. There are two reasons to think that blanket appreciations are unlikely to confound this classification rule. First, in communications with the Assessor’s Office, I was informed that blanket appreciations were not applied to still-damaged properties. Second, I find very few instances in the data in which a home classified by this procedure as “still damaged” in year t experiences a positive change in assessed improvement value that does not exceed 25%.

Step 2: Among households with homes classified as initially uninhabitable, I estimate a Weibull accelerated failure time model of the time until home repair. Because I have data on repair status at particular points in time instead of duration data (time until home repair), I follow Grummer-Strawn (1993) and estimate the model by maximum likelihood in its “current status” form using the complementary log-log regression,

$$\ln \left(-\ln \left(S(t_i) \right) \right) = \alpha + p \ln t_i + Z_i' \beta \quad (23)$$

where $S(\cdot)$ is the survivor function, t_i is a time at which repair status is observed, and Z_i is a vector of household and neighborhood characteristics.⁴¹

⁴¹The explanatory variables embedded in the vector Z_i in this model include; an indicator that a home was destroyed by Katrina, an indicator that a household is black, an indicator that a household is above age 65, an indicator that a household is solo-female headed, an indicator that a household is solo-male headed, an indicator that a household’s more educated head is a high school dropout, an indicator that a household’s more educated head is a high school graduate, an indicator that a household’s more educated head attended college but did not attain a bachelor’s degree, an indicator that at least one head was born outside of Louisiana, an indicator that the household purchased its home before 1980, an indicator that the household purchased its home between 1980 and 1995, an indicator that the household’s block received 2 to 4 feet of flooding, an indicator that the household’s block received greater than 4 feet of flooding, an indicator that 50% – 90% of the owner-occupied homes on a household’s block segment were rendered uninhabitable by Katrina, an

Step 3: For each household observed with its home still damaged at anniversary an t and its home repaired by anniversary $t+1$, I stochastically impute a repair date between those two anniversaries using the estimated hazard model. Periods in this paper’s structural model each span four months, so between any two anniversary periods there are two non-anniversary periods. If an imputed repair date falls during the first four months following the earlier anniversary, the home is classified as repaired at both intermediate periods. If an imputed repair date falls during the fifth through eighth months following the earlier anniversary, the home is classified as not repaired at the first intermediate period and repaired at the second intermediate period. If an imputed repair date falls during the first last months leading up to the later anniversary, the home is classified as not repaired at both intermediate periods.

Appendix II: Computing Housing-Related Price Variables

Table A1 describes how I construct each housing-related price that I use in the analysis.

Constructing several of these variables requires a set of rental-price indices that relate the cost of rental housing in pre-Katrina New Orleans to the cost of rental housing in post-Katrina New Orleans and the cost of rental housing in the pooled group of other Southern metro areas. I compute these rental-price indices using a regression to adjust rents across markets for observable differences in the housing characteristics.

$$\ln \text{rent}_i = \gamma_0 + Z'_i \gamma + \gamma_{\text{post-K-N.O.}} + \gamma_{\text{post-K-South}} + e_i \quad (24)$$

I use ACS data on rents and building characteristics for three distinct housing markets; the market in pre-Katrina New Orleans, the market in post-Katrina New Orleans from 2006 to 2009, and the market in a pooled sample of “other Southern metro” areas from 2006 to 2009. I regress the log of rent on a set of building characteristics and housing market fixed effects. The housing-market fixed effects describe the difference between the log-rental-price level in the indicated market and the log-rental-price level in pre-Katrina New Orleans.

Appendix Table A2 presents estimates of these rental-price indices. After adjusting for building characteristics, I find that rental price levels in post-Katrina New Orleans exceeded rents in pre-Katrina New Orleans by 35 log points, and rental price levels in other Southern metros during the post-Katrina period exceeded rents in pre-Katrina New Orleans by 23 log points.

Appendix III: Imputed Asset Distributions

I approximate the distribution of possible asset holdings for each sample household using the discrete approximation method suggested by Kennan (2004). Kennan shows that the best n -point finite approximation to a continuous distribution assigns equal weight to each of the percentiles $(2i - 1)/(2n)$ for $i = 1, \dots, n$. I approximate the distribution of pre-Katrina asset holdings for each household using 10 support points that assigns equal probability to the household holding the 5th, 15th, ..., and 95th percentiles of the distribution of liquid assets among households sharing the given household’s observable characteristics.

For each sample household, I must therefore estimate $F_{A(0)}^{-1}(p)$ for $p = 0.05, 0.15, \dots, 0.95$ and where $F_{A(0)}(\cdot)$ is the CDF of the distribution of liquid non-housing assets conditional on the household’s observable charac-

indicator that 90% – 100% of the owner-occupied homes on a household’s block segment were rendered uninhabitable by Katrina, an indicator that the household’s income during the year before Katrina was less than \$20,000, and an indicator that the household’s income during the year before Katrina was between \$20,000 and \$40,000.

teristics. To accomplish this, I model the conditional distribution of liquid assets using responses to the 2005 wave of the Panel Study of Income Dynamics (PSID).

I define each PSID household's liquid asset holding to be the sum of the household's of non-IRA stock holdings, bond holdings, and holdings in checking accounts, savings accounts, money market accounts, and CDs. I then estimate a model of each quantile of the liquid asset distribution conditional on observable household characteristics using a two step procedure. First, I model the probability that a household has zero liquid assets using a logistic regression of an indicator for zero assets on a large set of household covariates.⁴² Denote with $p(x)$ the predicted probability of having zero assets conditional on a particular combination of these covariates x . I set $\hat{F}_{A(0)}^{-1}(p|x) = 0$ for each $p < p(x)$.

Second, I estimate the remaining values of $\hat{F}_{A(0)}^{-1}(p|x)$ using a sequence of quantile regressions of the log of liquid assets on the same set of covariates among households that hold positive assets. For a given value of the covariate vector x , I estimate $\hat{F}_{A(0)}^{-1}(p|x)$ to be the estimated $\left(\frac{p - p(x)}{1 - p(x)}\right)^{th}$ quantile of the distribution of assets among those with positive assets. For example, if $p(x) = .25$ then $\hat{F}_{A(0)}^{-1}(p = .5|x)$ is the fitted 33 1/3 percentile, conditional on x , of the distribution of assets among those positive assets.

⁴²The list of covariates used in this model includes; indicators for solo-female headed household, solo-male headed household, the more educated household head being a high school dropout, the more educated household head having attended college but not received a bachelor's degree, the more educated household head having a bachelor's degree, a household head being black, the household residing in an urban area, the household residing in the south, an interaction of southern and urban, indicators for each of the four highest housing value quintiles, the age of the male head if present and the female head's age otherwise, and the square of the age of the male head if present and the square of the female head's age otherwise. When linking these estimates back to DNORS households, all DNORS households are classified as Southern and urban. The other inputs depend on the household's survey responses.

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Chapter 2

Assessing the Incidence and Efficiency of a Prominent Place Based Policy ⁴³

A growing class of “place based” policies explicitly target transfers towards particular geographic areas rather than groups of individuals.⁴⁴ Economists have traditionally expressed little support for such programs, fearing they will generate large distortions in economic behavior.⁴⁵ Indeed, standard models of spatial equilibrium suggest mobile workers and firms will arbitrage the benefits associated with local policies by relocating across the boundaries of targeted areas. Local land prices ought then to rise and offset any welfare gains that might otherwise accrue to prior residents.

We critically examine this conjecture by conducting an evaluation of Round I of the federal urban Empowerment Zone (EZ) program – one of the largest place based policies in the United States. Using rejected and future applicants to the EZ program as controls, we find that EZs generated jobs in targeted communities and raised local earnings without generating large increases in population or housing rents. Our findings build on an active literature on smaller state level “enterprise zones” which, perhaps because of heterogeneity in methods and programs studied, has found

⁴³This chapter was written with Pat Kline and Matias Busso.

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⁴⁴See Bartik (2002) and Glaeser and Gottlieb (2008) for reviews. Nichols and Zeckhauser (1982) provide a useful general discussion of the welfare economics of targeted transfers.

⁴⁵Kain and Persky (1969) provide an early critique of proposals for “gilding the ghetto”. Glaeser and Gottlieb (2008) exemplify the conventional view, stating that “the rationale for spending federal dollars to try to encourage less advantaged people to stay in economically weak places is itself extremely weak.” See Greenstone and Looney (2010) for an opposing view.

mixed evidence on the effectiveness of these programs at generating jobs.⁴⁶ Our estimates also inform the recent literatures on spatial bias in national tax policies (Albouy, 2009), local environmental policies (Greenstone and Gallagher, 2008), and industrial and regional policies (Wren and Taylor, 1999; Criscuolo et al, 2007; Bronzini and de Blasio, 2006), the efficiency consequences of which all depend upon the mobility of workers and firms. Our work extends these literatures by conducting the first microfounded equilibrium welfare evaluation of a large scale place based policy using geographically detailed microdata on firms, workers, and commuting patterns.

In an initial contribution, we develop a tractable spatial equilibrium model of Empowerment Zones with landlords, firms, and mobile workers who make labor supply and commuting decisions. The incidence and efficiency of local subsidies are shown to depend critically upon the distribution of agents' preferences over residential and commuting options. If most agents are inframarginal in their commuting and residential decisions, deadweight loss will be small and local workers will reap the benefits of place based interventions. If, on the other hand, agents have nearly identical preferences, as in the classic models of Rosen (1979) and Roback (1982), deadweight loss will be substantial and government expenditures will be capitalized into land rents. We show, using arguments similar to Chetty (2009), that our model allows for simple approximations to the incidence and deadweight loss of EZs via a set of reduced form elasticities quantifying the program's impact on the wages of local zone workers and commuters, the rental rate of zone housing, and the number of zone jobs for local residents and commuters.

Our empirical work centers on estimating these impacts using confidential microdata from the Decennial Census and the Longitudinal Business Database (LBD). These data provide us with two independent sources of information on local employment and allow us to adjust for changes over time in the composition of workers and firms. Crucial to our analysis, the Journey to Work component of the Census microdata allows us to separate the impacts of EZ designation on zone workers and zone residents. Because Empowerment Zones usually constitute a small fraction of a city's area, zone residents who work typically do so outside of the zone. Likewise, most zone workers are commuters who live outside the zone. EZs subsidize the employment of workers who live and work in the zone, and involve block grants which may indirectly subsidize commuters, making it critical for us to be able to distinguish between these populations across the period of our study, a task which is infeasible given publicly available data sources.

To identify the causal impacts of EZ designation we construct a set of control zones based upon previously confidential data obtained from the Department of Housing and Urban Development on the census tract composition of rejected and later round Empowerment Zones. Since these tracts were nominated for designation by their local governments, they are likely to share unobserved traits and trends in common with first round EZs which also underwent a local nomination phase. We demonstrate that, after some basic adjustments, the pre-treatment levels and trends in these controls closely mirror those of the EZs. Having demonstrated suitable balance, we assess causal impacts of the EZ program using an adjusted difference in differences estimator. To account for the clustered nature of our data, and the fact that only six EZs were awarded over our sample period, we rely on a wild bootstrap testing procedure studied by Cameron, Gelbach, and Miller (2008) to conduct inference.

⁴⁶See Papke (1993, 1994), Boarnet and Bogart (1996), Bondonio (2003), Bondonio and Engberg (2000), and Engberg and Greenbaum (1999). Peters and Fisher (2002) provide a review. More recent studies include Bondonio and Greenbaum (2007), Elvery (2009), Ham et al. (2011), and Neumark and Kolko (2010).

Point estimates from our main specifications suggest that neighborhoods receiving EZ designation experienced substantial (12% – 21%) increases in total employment relative to observationally equivalent tracts in rejected and future zones. The weekly wages paid to zone residents working inside the zone also appear to have increased significantly (by approximately 8% – 13%) relative to controls. Yet despite these improvements in the zone labor market, we find only a small insignificant influx of households to zone neighborhoods. Rental and vacancy rates appear stable over the duration of the study suggesting that most workers consider zone neighborhoods poor substitutes for residence in areas outside of the zone.

To assess whether our results are confounded by city-wide shocks, we conduct a variety of robustness checks meant to examine whether our control tracts provide a suitable proxy for the counterfactual behavior of EZs over the 1990s. We construct a set of “placebo” zones in EZ counties with pre-treatment characteristics similar to real EZs. We then compute difference in differences impacts comparing these placebo zones to rejected and future control zones, which reassuringly results in small insignificant estimated effects. We also show that our qualitative results remain when tract level outcomes are converted into percentiles in their city-wide distribution, indicating that our results are not driven by rank preserving city wide shocks.

We conclude with a quantitative assessment of the program’s incidence and a calculation of deadweight costs. Though our estimates are imprecise, we find that EZ designation generated wage increases for workers from zone neighborhoods worth approximately \$296M per year. Based upon two independent estimates of the number of zone jobs created for zone residents, we find that the tax credits associated with designation yielded relatively modest deadweight costs equal to roughly thirteen percent of the flow cost of the subsidy, though allowing for the possibility that EZ tax credits shifted workers out of jobs at firms ineligible for the credit and incorporating upper bound estimates of the marginal cost of raising the funds for the subsidy inflates this figure to as much as forty eight percent.

The remainder of the paper is structured as follows: Section I provides background on the EZ program, Section II develops a general equilibrium model of EZs, and Section III introduces our empirical strategy, Section IV describes the data used, Section V outlines our main results, Section VI tests for violations of the assumptions underlying our research design, Section VII conducts a welfare analysis and Section VIII concludes.

I. The Empowerment Zone Program

The federal Empowerment Zone program is a series of spatially targeted tax incentives and block grants designed to encourage economic, physical, and social investment in the neediest urban and rural areas in the United States. In 1993 Congress authorized the Department of Housing and Urban Development (HUD) to award Empowerment Zones to local communities via a competitive application process. Local governments were invited to submit proposals for an EZ defined in terms of 1990 census tracts subject to certain restrictions on the characteristics of each proposed tract.⁴⁷

⁴⁷All zone tracts were required to have poverty rates above twenty percent. Moreover, ninety percent of zone tracts were required to have poverty rates of at least twenty-five percent and fifty percent were required to have poverty rates of at least thirty-five percent. Tract unemployment rates were required to exceed 6.3%. The maximum population allowed within a zone was 200,000 or the greater of 50,000 or ten percent of the population of the most populous city within the nominated area.

HUD awarded EZs to six urban communities: Atlanta, Baltimore, Chicago, Detroit, New York City, and Philadelphia/Camden. Two additional cities, Los Angeles and Cleveland, received “supplemental” EZ (SEZ) designation while forty-nine rejected cities were awarded smaller enterprise communities (ECs) as consolation prizes.⁴⁸ Table 1 shows summary statistics of EZ neighborhoods by city. The average Round I EZ spanned 10 square miles, contained 113,340 people, and had a 1990 poverty rate of 48%. Most zones are contiguous groupings of census tracts, although some EZs, such as the one in Chicago pictured in Appendix Figure A1, cover multiple disjoint groupings of tracts.

EZ designation brought with it a host of fiscal and procedural benefits, the most important of which are the following:⁴⁹

1. Employment Tax Credits —Starting in 1994, firms operating in the six original EZs became eligible for a credit of up to 20 percent of the first \$15,000 in wages earned in that year by each employee who lived and worked in the community. Tax credits for each such employee were available to a business for as long as ten years, with the maximum annual credit per employee declining over time. This was a substantial subsidy given that, in 1990, the average EZ worker only earned approximately \$16,000 in wage and salary income.
2. Title XX Social Services Block Grant (SSBG) Funds —Each EZ became eligible for \$100 million in SSBG funds. These funds could be used for such purposes as: business assistance, infrastructure investment, physical development, training programs, youth services, promotion of home ownership, and emergency housing assistance.

Evidence from the General Accounting Office (1999) and Hebert et al. (2001) suggests that participation in the tax credit program was incomplete and most common among large firms who were more likely to have positive taxable income. Roughly \$200 million in employment credits was claimed over the period 1994 to 2000, with the amount claimed each year trending up steadily over time. IRS data show that, in the year 2000, close to five hundred corporations, and over five thousand individuals, claimed EZ Employment Credits worth a total of approximately \$55 million.⁵⁰

Table 2 summarizes information compiled from HUD’s internal performance monitoring system on the amount of money allocated to various program activities by source. By 2000, the first round EZs had spent roughly \$400 million dollars in SSBG funds. However, large quantities of outside capital accompanied the grant spending. The six EZs reported allocating roughly \$3 billion to local projects by 2000, with more than seven dollars of outside money accompanying every dollar of SSBG funds.⁵¹ Audits by HUD’s Office of Inspector General and the Government Accountability Office (2006) have called the accuracy of these data into question, suggesting that they should be

⁴⁸ECs were not entitled to tax credits but were allocated \$3 million in SSBG funds and made eligible for tax-exempt bond financing. SEZs were awarded block grants similar to those received by EZs but did not become eligible for the EZ tax credit until 1999.

⁴⁹See IRS (2004) for more details. Other benefits appear not to have been heavily utilized. See Hebert et al. (2001), General Accounting Office (2004), and Government Accountability Office (2006).

⁵⁰These figures come from General Accounting Office (2004).

⁵¹The most commonly reported use of funds was enhancing access to capital. One-stop capital shops were a component

interpreted as loose upper bounds on the amount of money raised, particularly since it is difficult to ascertain how any outside funds would have been spent in the absence of the program.⁵²

In sum, the six Round I EZs constitute a 60 square mile area containing less than 700,000 residents. Federal expenditures on EZ wage credits and block grants amounted to roughly \$850 per resident over the first six years of the program (1994-2000). And HUD's internal records suggest that as much as \$4,000 per resident of outside investment may also have been leveraged over this period though we suspect this figure to be a substantial overestimate.

II. Model

We turn now to the development of a spatial equilibrium model allowing a welfare analysis of the EZ program. The framework adopted is a variant of the classic equilibrium models of Rosen (1979) and Roback (1982) extended to allow for heterogeneity, labor supply decisions, commuting, elastic housing supply, and imperfect compliance in the EZ wage credit program. The decisions of workers are modeled in a discrete choice framework as in Bayer, Ferreira, and McMillan (2007) with an emphasis on the distinction between place of residence and place of work as in, for example, Baum-Snow (2007). After developing the model, we show that a set of reduced form elasticities of the sort discussed by Chetty (2009) can be used to approximate the EZ program's deadweight loss.

Assume a continuum of agents of measure one and a finite collection $\mathcal{N} = \{\mathcal{N}_0, \mathcal{N}_1\}$ of neighborhoods in which they may live or work consisting of neighborhoods inside (\mathcal{N}_1) or outside (\mathcal{N}_0) of an Empowerment Zone. Neighborhoods have fixed bundles of amenities consumed by local residents and used by local firms in production. Commuting between neighborhoods is costly. To deal with imperfect compliance with the EZ tax credit we introduce two sectors of the economy: a first sector of covered firms likely to participate in the EZ wage credit program and a second sector of firms likely to be ineligible for (or unaware of) the program. It is useful to think of sector one as consisting of large establishments and sector two as small family run businesses.

Agents choose a neighborhood to live in, whether to work, and (if so) a neighborhood and sector in which to work. Each agent inelastically demands a single unit of housing which they rent at market rates. Write the utility of individual i living in community $j \in \mathcal{N}$ and working in

of the plans of most EZs, training local entrepreneurs to develop business plans and apply for loans either from local organizations or commercial banks. The second most common use of funds was business development which involved technical and financial assistance. Some EZs developed business incubators for this purpose or invested in the physical revitalization of commercial corridors. See Hebert et al. (2001) and Appendix IV of Government Accountability Office (2006) for detailed descriptions of the projects implemented in particular zones.

⁵²See Chouteau (1999) and Wolfe (2003). Hebert et al (2001) report that "most of the leveraged dollars are accounted for by a \$1.2 billion commitment by a lending consortium of Detroit banks."

community $k \in \{\emptyset, \mathcal{N}\}$ and sector $s \in \{1, 2\}$ as:

$$\begin{aligned} u_{ijks} &= w_{jks} - r_j - \kappa_{jk} + A_j + \varepsilon_{ijks} \\ &= v_{jks} + \varepsilon_{ijks} \end{aligned}$$

where w_{jks} is the wage a worker from neighborhood j receives when working in sector s of neighborhood k , r_j is the local rent level, κ_{jk} is the cost associated with commuting to work in location k given residence in j , A_j is the mean consumption value of local amenities, and v_{jks} is the mean utility (across individuals) of each choice. The wage for nonworkers (w_\emptyset) is the dollar value of leisure which we normalize to zero without loss of generality. We likewise normalize $\kappa_{j\emptyset} = 0$. The individual and choice specific error terms ε_{ijks} represent heterogeneity in the valuation of local amenities, the value of leisure, tastes for work in the two sectors, and commuting costs.⁵³ The ε_{ijks} are independently and identically distributed across individuals and assumed to possess a continuous multivariate distribution independent of v_{jks} .

Heterogeneity is substantively important as it allows some workers to be inframarginal with respect to their residential and work location choices; thereby creating the potential for economic rents. Traditional models of spatial equilibrium are predicated upon the absence of such rents.⁵⁴ A Rosen-Roback type model, for example, would start by specifying that $u_{ijks} = \bar{u}$. Such indifference implies that the incidence of a local subsidy cannot fall on pre-existing residents. Heterogeneity weakens this knife edge result and yields stakeholders capable of differentially benefitting (or suffering) from local policies.

Define a set of indicator variables $\{D_{ijks}\}$ equal to one if and only if $\max_{j'k's'} \{u_{ij'k's'}\} = u_{ijks}$ for worker i , where $j' \in \mathcal{N}$, $k' \in \{\emptyset, \mathcal{N}\}$, and $s' \in \{1, 2\}$. Then the measure of agents in each residential/work location is $N_{jks} = P(D_{ijks} = 1 | \{v_{j'k's'}\})$. Denote the average utility of agents as $V = E_\varepsilon \left[\max_{j'k's'} \{u_{ij'k's'}\} \right]$ where the expectation operator E_ε is defined over the heterogeneity terms $\varepsilon_{ij'k's'}$. The choice probabilities N_{jks} and the average valuation V are easily shown to obey the following relationship,⁵⁵

$$\frac{d}{dv_{jks}} V = N_{jks} \tag{25}$$

⁵³It is useful to allow for the possibility that some zone residents face a higher cost of commuting to work inside the zone than outside the zone as might happen if some residents live on the border of the zone or are located near public transportation more integrated with one neighborhood than another. This will allow some zone workers to prefer working outside the zone even if wages are equalized across all neighborhoods.

⁵⁴See for example the traditional urban economics models covered in Glaeser (2008).

⁵⁵Proof:

$$\frac{dV}{dv_{jks}} = E_\varepsilon \left[\frac{d}{dv_{jks}} \max_{j'k's'} \{u_{ij'k's'}\} \right] = E_\varepsilon \left[I \left[\max_{j'k's'} \{u_{ij'k's'}\} = u_{ijks} \right] \right] = P(D_{ijks} = 1 | \{v_{j'k's'}\}) = N_{jks}.$$

We are grateful to David Card for help in simplifying an earlier version of this proof.

which amounts to a generalization of Roy's Identity for a representative agent with indirect utility function V . This relationship will prove useful in our analysis of social welfare.

We turn now to the demand side of the model. Goods are produced in each neighborhood k and sector s with a constant returns to scale technology $F(K_{ks}, B_k L_{ks}) = B_k L_{ks} f(\chi_{ks})$ where the arguments K_{ks} and L_{ks} refer to total capital and labor inputs respectively, $\chi_{ks} = \frac{K_{ks}}{B_k L_{ks}}$ is the capital to effective labor ratio, and B_k is the local productivity level which may depend upon infrastructure investments, natural features of the physical environment (e.g. access to a body of water, proximity to downtown), and crime levels.⁵⁶ Productivity differences across neighborhoods yield unequal derived demands for inputs across space. Because the supply elasticity of workers to any given location is finite in the presence of taste heterogeneity and commuting costs, these unequal factor demands result in unequal wages across neighborhoods.

Workers from different neighborhoods are assumed to be perfect (and homogeneous) substitutes in production so that $L_{ks} = \sum_{j \in \mathcal{N}} L_{jks}$ where L_{jks} is the labor input of workers from neighborhood j to firms in neighborhood k and sector s .⁵⁷ The EZ tax credit program induces a cost difference for zone firms between workers residing inside of the zone (whose wages are subsidized at rate τ) and zone commuters who are unsubsidized. Hence at any given wage, zone employers strictly prefer zone residents, which means that at an interior equilibrium zone firms must pay different wages to residents and commuters.

We assume capital is supplied at fixed rental rate ρ to all neighborhoods and sectors and that output is sold on an international market at price one.⁵⁸ Our fixed ρ assumption reflects the notion that urban neighborhoods are small in relation to global capital markets and that modern financial institutions, unlike workers, do not exhibit substantial preferences regarding the neighborhoods to which their funds flow. Define the indicator variable $\delta_{jks} = I[j \in \mathcal{N}_1, k \in \mathcal{N}_1, s = 1]$ which equals one for jobs subject to the wage subsidy and zero otherwise. Firms equate the marginal product of each factor to its corresponding after-tax cost so that:

$$\begin{aligned} B_k [f(\chi_{ks}) - \chi_{ks} f'(\chi_{ks})] &= w_{jks} (1 - \tau \delta_{jks}) \\ f'(\chi_{ks}) &= \rho \end{aligned}$$

The second of these conditions may be inverted to yield $\chi_{k,s} = \chi = h(\rho)$ where $h'(\cdot) \leq 0$. We

⁵⁶See Kline (2010) for an analysis of this sort of model when B_k exhibits agglomeration effects.

⁵⁷See Card (2009) for recent evidence on the high degree of substitutability between low skilled workers of the sort that work and live in EZ neighborhoods. In Supplemental Appendix A we derive an extended version of the model which incorporates productivity differences among workers and show that it yields similar conclusions.

⁵⁸It is straightforward to extend the model to the case where output is sold locally and prices are endogenous. Since we have no data on local product prices we omit this feature from our analysis.

may then rewrite the condition for wages as:

$$w_{jks} = \frac{B_k R(\rho)}{1 - \tau \delta_{jks}} \quad (26)$$

where $R(\rho) = f(h(\rho)) - h(\rho)\rho$ is the marginal product of a “raw” unit of labor. The fact that zone and non-zone workers are perfect substitutes implies that the tax subsidy for zone workers will be completely transferred into their wages. Zone jobs in the higher paying sector are not rationed because workers have idiosyncratic tastes for working in different sectors.

Finally, we allow for upward sloping housing supply curves in each neighborhood as in Moretti (2010, 2011) and Notowidigdo (2010). Each neighborhood has a continuum of risk neutral land owners distributed on the unit interval. Each land owner may develop a unit of housing on her plot of land in neighborhood j at a cost which is continuously distributed across owners according to the CDF $G_j(\cdot)$ with strictly positive support. These costs might include the time cost of rehabilitating a boarded up vacant unit or the pecuniary cost of creating a new structure on an open lot.

If a unit of housing is built, the owner rents the unit out and receives payoff r_j minus the cost of constructing the unit, otherwise she receives nothing. Let H_j represent the number of units rented out in community j . Optimization implies that the marginal landowner in each neighborhood breaks even on house construction so that:

$$G_j^{-1}(H_j) = r_j \quad (27)$$

To close the model we assume the housing market clears which requires:

$$H_j = \sum_k \sum_s N_{jks} \quad (28)$$

The model’s predictions for the response of zone neighborhoods to EZ designation are now easily derived. The EZ program involved two treatments – a wage tax credit (τ) and a block grant which we model as affecting local productivity (B_k) and amenity (A_j) levels. From (26) we see that the EZ wage subsidies should raise the wages of local zone workers and hence their employment at EZ firms in the covered sector. Because the tax credits have no effect on wages in the uncovered sector, employment may fall at such firms as workers switch their employment to the more lucrative covered sector. Likewise, because the wage subsidies yield no increase in the wages of nonresident commuters their employment may also be expected to fall slightly as some workers decide to move to the neighborhood to take advantage of the higher wages for residents.

Any productive effect of the block grants however, may counteract these negative employment

effects. Note that (26) implies:

$$\frac{d \ln w_{jks}}{d \ln B_k} = 1 \quad (29)$$

Thus productivity changes proportionally boost the wages of all workers in a neighborhood regardless of their place of residence. This may be expected to yield a large employment response among nonresident zone commuters who likely view most jobs within a sector with the same commuting distance as close substitutes. It may also counteract any negative employment effects at smaller firms not covered by the tax credit.

Finally, depending on the distribution of workers tastes for living in zone neighborhoods and features of the housing supply locus, the rental rate of housing in zone neighborhoods may increase as agents seek to move to the zone in order to take advantage of higher local wage levels and any possible increases in local amenity value. If workers have relatively homogeneous residential preferences and the housing stock is fixed we should see large increases in rental rates, while if housing is easily supplied we should see an increase in population and little change in rental rates. If, however, few workers are on the margin of moving to distressed neighborhoods we should see little response in either population or rental rates.

We turn now to an analysis of the model's welfare implications. Total social welfare in this economy is the sum of total worker utility and the utility of landlords which may now be written compactly as follows:

$$W = V + \sum_j \left[r_j H_j - \int_0^{H_j} G_j^{-1}(x) dx \right]$$

the first term giving the average (which is also the total) utility of workers and the second the total profits of landowners.

Consider first the block grant which we model as affecting local productivity and amenity levels. The marginal social benefit of an improvement in the local productivity level of community m may be written:

$$\frac{d}{dB_m} W \Big|_{\tau=0} = \sum_j \sum_k \sum_s N_{jks} \left[\frac{dw_{jks}}{dB_m} - \frac{dr_j}{dB_m} \right] + \sum_j \frac{dr_j}{dB_m} H_j \quad (30)$$

where we have made repeated use of the relationship given in (25). The first line gives the effect of the productivity change on workers and the second line the effect on housing producers.⁵⁹ A remarkable feature of this welfare calculation is that it does not include any terms of the form $\frac{dN_{jks}}{dB_m}$. This is a result of optimization which makes the marginal agent indifferent between alternatives

⁵⁹Note that in a Rosen-Roback model the welfare consequences of any increases in local wages would be perfectly offset by increases in the local cost of living. By assumption such a model requires $dV = 0$.

despite the fact that the micro-level decision is discrete. Thus, to first order, the welfare implications of zone grants are the same as the implications of changing prices on an immobile population.

In an economy without behavioral responses price changes simply generate transfers of wealth between market participants, which, in our framework, have no aggregate welfare implications. Substituting the market clearing conditions (27) and (28) into (30) and simplifying yields:

$$\begin{aligned} \frac{d}{dB_m} W \Big|_{\tau=0} &= \sum_j \sum_k \sum_s N_{jks} \frac{dw_{jks}}{dB_m} \\ &= R(\rho) N_{.m} \end{aligned} \quad (31)$$

where $N_{.m} = \sum_j \sum_s N_{jms}$ is the total number of jobs in neighborhood m and the second line follows from (26). Note that this is simply the total increase in output the economy would experience due to an increase in the local productivity level if the behavior of firms, workers, and landlords were unchanged.

Now consider an increase in amenities. By similar reasoning it can be shown that:

$$\frac{d}{dA_m} W \Big|_{\tau=0} = N_m. \quad (32)$$

where $N_m = \sum_k \sum_s N_{mks}$ is the total number of residents of neighborhood m . Again, the intuition is that, to first order, improving amenities in neighborhood m is equivalent to making an in-kind transfer to an immobile population.

Finally, consider the wage tax credit. A derivation equivalent to that in (30) and (31) yields:

$$\frac{d}{d\tau} W = \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1} w_{jk1} \frac{d \ln w_{jk1}}{d\tau} \quad (33)$$

Thus, in contrast to the case of block grants, the total welfare effects of the wage subsidy depend to first order on price changes. This is because of the ad valorem nature of the subsidy which makes the size of the transfer from the federal government to zone employers contingent upon the base wage. So even if no firms or workers move, an increase in the wage will increase the total transfer to the local economy.

The marginal cost of an increase in the ad-valorem wage subsidy is:

$$\begin{aligned} \frac{d}{d\tau} \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1} w_{jk1} \tau &= \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1} w_{jk1} \left(1 + \tau \frac{d \ln N_{jk1}}{d\tau} + \tau \frac{d \ln w_{jk1}}{d\tau} \right) \\ &= \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1} w_{jk1} \left(\frac{d \ln w_{jk1}}{d\tau} + \tau \frac{d \ln N_{jk1}}{d\tau} \right) \end{aligned}$$

where in the second line we have made use of the fact that (26) implies $\frac{d \ln w_{jk1}}{d\tau} = \frac{1}{1-\tau\delta_{jk1}}$. The extra term in this expression relative to (33) constitutes the marginal deadweight loss of the wage subsidies; it reflects the fact that marginal entrants have first order effects on program cost even if they value the resulting net wage increases little.

The total deadweight loss of the tax subsidy may be written:

$$\begin{aligned}
 DWL_{\tau} &= \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1} w_{jk1} \int_0^{d\tau} t \frac{d \ln N_{jk1}}{dt} dt \\
 &\approx \frac{1}{2} \psi d\tau^2 \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1} w_{jk1}
 \end{aligned} \tag{34}$$

where in the second line we have assumed a constant semi-elasticity of local covered employment $\psi = \frac{d \ln N_{jk1}}{d\tau}$. The efficiency cost of the employment credit is proportional to ψ and the local wage bill at zone firms in the covered sector and is increasing in the square of the tax change. This formula corresponds to the standard Harberger (1964) formula for approximating deadweight loss with the number of covered sector jobs in the zone as the “good” being subsidized. It is also analogous to results found in local public finance models of between-city equilibrium (e.g. Albouy, 2009) where the local employment elasticity serves as a key input to calculations of the deadweight loss induced by local taxes. A key difference with such papers is that the present elasticity depends critically upon worker heterogeneity which generates different conclusions regarding program incidence.

Note that in the absence of heterogeneity among workers ψ will be large and the employment credits will be “wasted” on workers indifferent about the prospect of switching between neighborhoods, sectors, and labor force states. If, however, few nonzone residents are on the margin of moving to an EZ (as might be the case if EZs are perceived by most to be undesirable locations in which to live) and few EZ residents are on the margin of working (as might be the case if public assistance receipt provides disincentives to work among a large fraction of the local population) then ψ will be small and the deadweight loss of the program will be small.

The block grant investments may yield additional deadweight losses if their total cost C exceeds the value of the resulting amenity and productivity increases. Suppose every dollar of block grants proportionally raises zone neighborhood amenity levels by a factor of λ_a and zone neighborhood productivity levels by λ_b . Then we may approximate the deadweight loss associated with the block

grants by assuming marginal welfare effects are constant as follows:

$$\begin{aligned}
DWL_G &\approx C \left[1 - \lambda_a \sum_{j \in \mathcal{N}_1} \frac{dW}{d \ln A_j} \Big|_{\tau=0} - \lambda_b \sum_{k \in \mathcal{N}_1} \frac{dW}{d \ln B_k} \Big|_{\tau=0} \right] \\
&= C \left[1 - \lambda_a \sum_{j \in \mathcal{N}_1} A_j N_j - \lambda_b \sum_j \sum_{k \in \mathcal{N}_1} \sum_s N_{jks} w_{jks} \right] \tag{35}
\end{aligned}$$

where the second line follows from (31) and (32). If the block grants are wasted on unproductive investments, as is likely if the funds are mismanaged or mistargeted relative to the needs of local firms, the program's deadweight costs could be substantial. If, however, local public goods are underprovided in zone neighborhoods the social return on these local investments may dramatically exceed their cost.

III. Empirical Strategy

Our theoretical discussion highlights the point that the incidence and efficiency of EZ designation are both empirical questions incapable of being answered on prior grounds. The incidence of the program hinges critically upon the manner in which factor prices change. Wage increases in the zone will benefit workers with a preference for working in the zone while residents who prefer to take leisure will be unaffected. Rent increases will benefit zone landlords but reduce the disposable income of zone residents. Residents outside the zone may also reap some benefit from EZ designation if the productivity of zone jobs rises or rental rates for housing fall in response to any population losses. But the total economy wide gain associated with the program will be small relative to its cost if workers are highly responsive to the wage subsidies or if the block grants are wasted on unproductive investments.

Our empirical tasks, then, are threefold. First, we must identify the impact of EZ designation on local price levels in order to assess the program's incidence. Second, to compute an estimate of deadweight loss due to the program's tax credits, we need to determine ψ which corresponds to the effect of the wage subsidies on the number of covered sector zone jobs for zone residents. Third, we need to isolate the cost effectiveness of the block grants which will require determining the impact of EZ designation on the wages of nonresident zone workers, who according to (29) should experience wage increases in proportion to any productivity increases dB_k . With knowledge of $d \ln B_k$ and information on the cost C of the EZ investments we may in turn identify the productivity effect λ_b of the block grants. Note that without more assumptions the model does not allow point identification of the amenity value λ_a of the block grants from reduced form impacts alone. However, provided housing supply is not perfectly elastic, if the impact on rents of designation is nearly zero we can be assured that λ_a is small as well. We return to this issue again in Section VII.

Our research design for accomplishing these tasks will be to compare the experience over the 1990s of census tracts in Round I EZs to tracts in rejected and later round zones with similar characteristics.⁶⁰ This approach has a number of advantages. First, tracts in rejected zones, like those in winning zones, were nominated by their local governments for inclusion in an EZ proposal. If the nomination process was similar in winning and losing cities this ought to yield a set of control tracts with both observable and unobservable characteristics similar to EZs. Second, our control zones consist of contiguous clusters of poor census tracts just like real EZs. If spillovers exist across census tracts or if poor tracts surrounded by other poor tracts have important unmeasured characteristics then such agglomerated controls may be necessary for identifying causal effects. Finally, the majority of rejected and future zones are located in different cities than treated zones which reduces the sensitivity of our estimates to geographic spillover effects.

Though the use of rejected tracts as controls has many advantages, one may still be concerned that the cities that won first round EZs are fundamentally different from losing cities. A cursory inspection of Table 1 indicates that two of the three largest US cities won EZs, while the remaining winners are large manufacturing intensive cities. If large cities experienced fundamentally different conditions over the 1990s than small cities, the comparison of observationally equivalent census tracts in winning and losing zones will be biased.

To further explore this possibility we carefully examine pre-trends in EZ and control tracts for signs of imbalance after having adjusted carefully for tract and zone characteristics. We also conduct a number of robustness tests aimed at assessing the credibility of our differences-in-differences research design. First, we construct a set of “placebo zones” in treated cities with characteristics similar to real zones. If our research design is confounded by city wide shocks we should find nonzero effects on these placebo zones as well. Second, we examine how the outcomes of EZ tracts change in the city-wide distribution of tract level outcomes relative to controls. This approach, which is a nonparametric variant of the traditional differences-in-differences-in-differences (DDD) research design, is robust to arbitrary rank preserving city specific shocks.

Econometric Methods

In our comparison of EZ neighborhoods to tracts in rejected and future zones we will rely on simple generalizations of standard differences-in-differences estimators. Specifically, we estimate program impacts using tract level regressions of the form:

$$\Delta Y_{tzc} = \beta T_z + X'_{n(t)} \alpha^x + P'_c \alpha^p + e_{tzc} \quad (36)$$

⁶⁰Boarnet and Bogart (1996) take a similar approach in their evaluation of the New Jersey enterprise zone. Use of rejected applicants as controls has a long history in the literature on econometric evaluation of employment and training programs. See the monograph by Bell et al. (1995) for a review.

where ΔY_{tzc} is the change in some outcome (e.g. log population) over the 1990s in census tract t of proposed zone z in city c , T_z is an indicator for whether proposed zone z receives an EZ in 1994, P_c is a vector of mean city-level characteristics, and $X_{n(t)}$ is a distance weighted average of tract level proxies for trends in local productivity and amenities within a given radius-based neighborhood $n(t)$ of tract t . The coefficient β provides an adjusted difference in difference estimate of the impact of the EZ program on EZ tracts.

To allow for flexible patterns of treatment effect heterogeneity we also estimate interacted regressions of the form:

$$\Delta Y_{tzc} = \mu^1 T_z + (1 - T_z) \times X'_{n(t)} \alpha^x + (1 - T_z) \times P'_c \alpha^p + e_{tzc} \quad (37)$$

where $\mu^1 \equiv E[\Delta Y_{tzc} | T_z = 1]$. This specification models the mean change in outcomes among the control tracts as a linear function of $X_{n(t)}$ and P_c , but is agnostic regarding the conditional expectation function among the treated tracts. That is, least squares estimation of (37) simply yields the mean ($\hat{\mu}^1$) among the treated tracts and the coefficients ($\hat{\alpha}^x, \hat{\alpha}^p$) associated with a linear regression of ΔY_{tzc} on $(X_{n(t)}, P_c)$ in the control ($T_z = 0$) sample. Given these estimates, an estimate \widehat{ATT} of the average treatment effect on treated tracts may be formed as:

$$\widehat{ATT} \equiv \hat{\mu}^1 - \frac{1}{N_1} \sum_t T_t \left(X'_{n(t)} \hat{\alpha}^x + P'_c \hat{\alpha}^p \right) \quad (38)$$

where T_t is a tract level indicator for whether tract t is in a treated zone, and $N_1 = \sum_t T_t$ is the number of treated tracts.⁶¹ Note that \widehat{ATT} is simply the average forecast error in the treated sample of a regression model fit to the controls. Provided that the linear model for the controls is suitable, this approach will identify the average impact of EZ designation on EZ tracts in the presence of treatment effect heterogeneity arbitrarily dependent upon the covariates. The cost of this additional flexibility is that this estimator will tend to exhibit greater sampling variability than OLS estimation of (36) which assumes common regression coefficients in the treatment and control samples.

Kline (2011) shows that the estimator in (38) possesses a dual interpretation as a propensity score reweighting estimator with weights derived from a log-logistic propensity score model, leading us to term this approach a Parametric Reweighting (PW) specification. We use the implicit propensity score weights associated with this estimator in the next section to assess the extent to which our regression model is able to balance the distribution of covariates across the EZ and control samples over time. As described in Appendix I, these PW weights have the appealing property of exactly balancing the mean of any covariate included in the regression model across the treatment

⁶¹See Appendix I for details.

and control groups.

Throughout our analysis, we allow for arbitrary within city spatial correlation in the errors e_{tzc} when conducting inference. Because we have only six treated zones, standard cluster-robust variance estimation methods relying upon first order asymptotics may yield poor control over the probability of making type I errors. To deal with this problem we use a clustered wild bootstrap-t procedure explored in Cameron, Gelbach, and Miller (2008) which, under some conditions (Mammen, 1993; Kline and Santos, 2011), may yield improvements in the performance of cluster-robust methods in small samples. We conduct a Monte Carlo study, presented in Supplemental Appendix B, demonstrating that this procedure effectively controls the size of Wald tests in a variety of data generating processes mimicking the design of our data.

IV. Data

Our analysis relies upon confidential household and establishment level microdata from the Decennial Census, the Standard Statistical Establishment List (SSEL), and the Longitudinal Business Database (LBD) which we use to construct a panel of Census tract level outcomes and covariates. The bulk of our data come from the 1980, 1990, and 2000 long-form Decennial Censuses of Population and Housing. Geographic identifiers on the 1980 and 2000 files use codes pertaining to the census geographic boundaries of their vintage. We map block of residence and block of work identifiers in 2000 to 1990 Census tracts using the Census Block Relationship Files (CBRF). To map 1980 geographic identifiers to 1990 tracts we use the Census Tract Relationship Files (CTRF). We then compute quantities of interest in each year by 1990 tract of residence and tract of work, a process which relies critically upon the Journey to Work component of the Decennial Census microdata which distinguishes between place of work and place of residence. All quantities are computed using Census sampling weights and, in the case of 1980 variables, weights accounting for the imperfect correspondence between 1980 and 1990 geographies. We also adjust for nonresponse using an additional set of inverse probability weights described in Appendix II.

To supplement our Census analysis, we use establishment data from the Longitudinal Business Database (LBD) files for the years 1987-2002. The LBD provides longitudinally linked establishment-level data for all establishments with paid employees contained in the Census Bureau's Standard Statistical Establishment List (SSEL). Data contained on these files comes primarily from the Economic Census and is supplemented with tax records from the Internal Revenue Service. We coded each establishment to a 1990 census tract using an algorithm described in Appendix II based on the raw street addresses provided on the SSEL. In addition to establishments' locations, we observe each establishment's age, size (number of employees), payroll, industry, and whether the establishment belongs to a multi-establishment firm. Average characteristics in each tract are computed

adjusting for an estimated probability of being missed by the geocoding algorithm. Because the quality of the LBD data is higher in Economic Census years, we use 1992 instead of 1990 as the base year when examining changes over the 1990s in LBD based variables.

City level covariates are obtained from the County/City Databook (CCD) for the years 1980, 1990, and 2000. This yields values of city level variables such as crime rate, percentage of workers in the manufacturing sector and percentage of workers working in the government. In cases where zones span multiple cities we assign all tracts the characteristics of the largest city in the metropolitan area. We also use metropolitan housing price data from the Office of Federal Housing Enterprise Oversight (OFHEO) to control for changes in metropolitan housing market conditions in the early 1990s.

Finally, in order to construct a suitable control group for EZs, we obtained 73 of the 78 first round EZ applications submitted to HUD by nominating jurisdictions via a Freedom of Information Act request. These applications contain the tract composition of rejected zones which we merged with publicly available data on the tract composition of future zones to create a composite set of controls for use in our analysis. Appendix Table A1 details the composition of the cities in our evaluation sample, whether they applied for a Round I EZ, and the treatments (if any) they received.

Prices/Composition Adjustments

Given well known problems with the measurement of hours worked in the Census (e.g. Baum-Snow and Neal, 2009), we work with a weekly wage concept. Wages are computed by dividing annual labor income by weeks worked in the previous year. We exclude from our analysis wage observations based on allocated earnings or weeks. Owner occupied housing values and rents are self-reported in the Census as interval valued variables. We assign each response to its interval midpoint and drop allocated values.

To remove the influence of changes in demographic composition on tract level measures of behavior and prices we compute composition constant outcomes by tract for wages, housing values, and rents using fixed effects regressions. The regression specifications used to adjust tract outcomes differ slightly for individual level outcomes aggregated by residence tract, for individual outcomes aggregated by place of work tract, and for housing characteristics.⁶²

⁶²In each case, a regression model was estimated on a pooled sample of micro-data that included all observations with non-missing values of the dependent variable from 1980, 1990, and 2000. Each regression specification included a full vector of tract-year dummy variables. For individual level outcomes aggregated by residence tract and for housing characteristics, the tract-year dummy variables indicate an individual's residence tract or the tract in which a housing structure was located. For individual level outcomes aggregated by place of work tract, tract-year dummy variables indicate the tract in which an individual worked. For individual outcomes, the regression specifications included a quartic in age, dummy variables for black non-Hispanic and other race (white non-Hispanic omitted), a dummy variable for female,

Consider the adjustment of the mean of an outcome Y_{ijzt} which, in a minor abuse of notation, we take to denote the outcome of individual or housing unit i in tract j , zone z , and year t . A zone is either an EZ, a control zone, or the non-EZ, non-control portion of a county containing an EZ or control. We estimated the following regression equations separately by state on a pooled sample of individual respondents to the 1980, 1990, and 2000 long form Decennial Censuses:⁶³

$$Y_{ijzt} = \eta_{jt}^0 + X'_{ijzt}\eta_z^x + \epsilon_{ijzt}$$

where X_{ijzt} is a vector of covariates for individual or housing unit i in tract j , zone z , and year t . Note that the mean OLS residual is zero for each tract-year because of the included tract-year fixed effects η_{jt}^0 . Hence we may decompose the change in the tract level mean $\bar{Y}_{j,t}$ between 1990 and 2000 into a composition constant change and a composition effect as follows

$$\bar{Y}_{j,2000} - \bar{Y}_{j,1990} = \underbrace{(\hat{\eta}_{j,2000}^0 - \hat{\eta}_{j,1990}^0)}_{\text{composition constant change}} + \underbrace{(\bar{X}_{j,2000} - \bar{X}_{j,1990})' \hat{\eta}_z^x}_{\text{composition effect}}$$

where the $\bar{X}_{j,t}$ refer to tract by year averages of covariate values. The composition constant change $(\hat{\eta}_{j,2000}^0 - \hat{\eta}_{j,1990}^0)$ is the difference between the two estimated tract-year fixed effects while the composition effect $(\bar{X}_{j,2000} - \bar{X}_{j,1990})' \hat{\eta}_z^x$ is a linear combination of the changes in mean tract characteristics. Columns labeled “Composition Adjusted” report results using the former quantity as a dependent variable while those labeled “Composition Effect” report results using the latter quantity as a dependent variable.

Estimation Sample / Comparability of EZs and Controls

Our analysis focuses on the six original EZs which received both tax credits and block grants and restricts the sample of controls to zones containing at least 10 census tracts in cities with population greater than 100,000.⁶⁴ We also drop all control tracts with 1990 poverty and unemployment rates below the minimum thresholds specified in the EZ eligibility criteria and tracts with fewer than 200

and dummy variables for high school dropout, any past college attendance, and actively enrolled in school (non-enrolled high school graduate omitted). For housing outcomes, we included dummy variables for the number of bedrooms, the number of rooms, three building age categories, two-way interaction terms between bedrooms and rooms, and two-way interaction terms between bedrooms and building age. We computed composition constant mean outcomes by evaluating the estimated regression equation using a constant mix of included explanatory variables for each tract across the three years.

⁶³We have also experimented with more complicated specifications that allow the η_z^x coefficients to change over time by demographic group. These yield similar final results but sometimes erratic predictions for small demographic cells.

⁶⁴Census tracts in the two SEZs are dropped from our baseline analysis because they were not eligible for wage tax credits during our sample period. We also drop the Washington, DC Enterprise Zone (EnZ) from our sample because it received a wage tax credit but not block grants and hence cannot be properly characterized as an EZ or a control.

households or 500 residents in 1990.⁶⁵ This yields a baseline estimation sample of 234 EZ tracts in six cities and 1,429 controls distributed across sixty three cities.

Table 3 provides average characteristics of EZ and control tracts in 1990 along with changes in these characteristics over the period 1980-1990. To summarize this information, and to reduce multiple testing problems, we also include six indices of neighborhood quality that are linear combinations of the underlying variables scaled by their standard deviations.⁶⁶ These indices are normalized to have mean zero and standard deviation one in the EZ sample in 1990.

Columns four and nine of Table 3 provide cluster robust wild bootstrapped p-values for tests of the null hypothesis that mean pre-treatment levels and trends are equal across the EZ and control samples. While the residents of rejected and future zones are poor and have high rates of unemployment, we see from columns one, two, and four that they are not quite as poor or detached from the labor force as residents of EZ areas. In general, EZ tracts appear to be more distressed than controls. Moreover, columns six, seven, and nine of the Table indicate that while trends over the 1980-1990 period are similar between the EZs and controls, some minor differences are present. For example, trends in college share and our two measures of worker wages are slightly imbalanced, although no systematic pattern is apparent from these trend differences.

To deal with these imbalances, we rely on our parametric regression adjustments and adjust for a wide array of predesignation tract and zone characteristics.⁶⁷ All tract level covariates used in

⁶⁵Zone tracts were required to have poverty rates in excess of 20% and unemployment rates in excess of 6.3% as measured in the 1990 Census.

⁶⁶The indices are sums of the form $\frac{1}{L} \sum_{l=1}^L \frac{\bar{X}_{j,t}^l}{\sigma_{90}^l}$ where σ_{90}^l is the cross-sectional standard deviation of the covariate l in question in 1990 among the EZ tracts and the tract covariate average $\bar{X}_{j,t}^l$ has been multiplied by -1 where appropriate so as to make the sum an index of neighborhood quality. The following variables were multiplied by -1 in constructing the indices: unemployment rate, poverty rate, % of households female headed, % dropout, % black, % hispanic, vacancy rate.

⁶⁷City and Zone Level Covariates: Change in log of city population 1980-1990, Change in city employment rate 1980-1990, Proportion of city population black (1990), Total city crime / population x 100 (1990), Proportion of city employment in manufacturing (1990), Proportion of city employment in city government (1990), Log area in square miles of zone, Log OFHEO metropolitan housing price index (1991 and 1992 values).

Tract Level Covariates: Indicator for tract in central business district (1990), Indicator Tract Poverty > 25% (1990), Indicator Tract Poverty > 35% (1990), Unemployment rate (1990), Employment to Population Ratio (1990), Fraction of 1980 Adults Still Present in Tract in 1990, Change in proportion of employed tract residents commuting < 25 minutes (1980-1990), Change in proportion of tract workers with college degree (1980-1990), Proportion Hispanic (1990), Proportion Hispanic (1980), Proportion black (1990), Proportion black (1980), Proportion of structures vacant (1990), Proportion of structures vacant (1980), Mean Building Age (1990), Proportion < 18 years old (1990), Proportion < 18 years old (1980), Proportion of households female headed (1990), Proportion of households female headed (1980), Proportion \geq 65 years old (1990), Proportion \geq 65 years old (1980), Proportion of population who are high school dropouts (1990), Proportion of population who are high school dropouts (1980), Change in mean log of housing values (1980-1990), Change in mean log of rent (1980-1990), Change in log of tract population (1980-1990), Change in log of households (1980-1990), Change in mean log wage of tract residents (1980-1990), Change in mean log wage of tract workers (1980-1990), Change in mean log annual earnings of tract residents (1980-1990), Change in mean log annual earnings of tract workers (1980-1990), Change in log of tract employment - LBD (1987-1992), Change in log of average earnings per tract worker - LBD (1987-1992), Change in log # of establishments - LBD (1987-1992)

our regression adjustments save for central business district status are averaged across tracts using a spatial kernel method.⁶⁸ Because some of these covariates are lagged values of outcomes we wish to investigate via regression based methods, we construct our kernel weighted spatial averages $X_{n(t)}$ omitting the actual tract level outcome X_t in order to reduce the threat of division bias (Borjas, 1980) in our later results.

The third and seventh columns of Table 3 use the regression based weights, described in Kline (2011) and in Appendix I, to reweight the controls to mimic the covariate distribution of the treated observations using the same covariates. After reweighting, both pre-treatment levels and trends in tract characteristics exhibit dramatically improved balance despite the fact that the majority of these variables were not included in the reweighting procedure.

For example, reweighting moves the 1990 mean among control tracts of each element (the employment rate, the poverty rate, and unemployment rate) of our resident economic index closer to its corresponding 1990 mean among EZ tracts. Means of city level variables included in the regression model match exactly. Column five provides cluster robust wild bootstrapped p-values indicating that we cannot reject the null hypothesis that the mean pre-treatment tract characteristics are identical after reweighting. The only serious pre-treatment discrepancy is in 1990 city population, which is to be expected because EZs were awarded to many of the largest U.S. cities. We revisit the importance of this discrepancy in section VI. In Appendix Table A2 we document that second moments are also well balanced.

Reweightings yields similar improvements in pre-treatment trends. For instance, mean 1980-1990 changes among control tracts in the elements of the worker economic index (jobs, weekly wages of zone residents, and weekly wages of zone workers) all move closer to their corresponding 1980-1990 mean change among EZ tracts. Column ten of Table 3 shows that pre-treatment trends among control tracts are in general statistically indistinguishable from those among EZs after reweighting.

To illustrate these findings visually, Figure 1 shows the mean behavior of our six indices in the EZ and control tracts before and after reweighting across the three decades in our sample. After reweighting is applied to the pooled set of controls their history over the past two decades mirrors that of actual Empowerment Zones remarkably well. One can see evidence from these graphs of important post-treatment impacts of EZ designation on several dimensions of neighborhood quality including our indices of the economic opportunity of zone workers and zone residents and our

⁶⁸Specifically, for each control variable, the spatial moving average assigned to a tract, j , is the kernel weighted mean value of the control variable among a set of neighboring tracts $N(j)$, defined as those tracts (other than j itself) whose centroid falls within one mile of the centroid of tract j . The weight given to each tract in the set $N(j)$ is given by a truncated (at one mile) normal kernel with a standard deviation of 0.5 miles applied to the distance between the centroid of the neighboring tract and the centroid of tract j .

housing market index. Plots for some individual Census based variables are provided in Appendix Figure A2.

Figure 2 provides complementary evidence from the Longitudinal Business Database at annual frequencies. We see that, after reweighting, treated and control tracts exhibit similar patterns in economic activity prior to the start of the program in 1994. Notably, there is no evidence of an Ashenfelter (1978) style dip prior to program enactment. By 1997, LBD based measures of employment and the number of establishments begin to rise and continue to diverge. This timing is consistent with administrative data documenting a delay of several years in the usage of program benefits by firms.

V. Results

We turn now to our baseline differences-in-differences estimates of the impact of EZ designation. To deal with the hierarchical nature of our data we report standard errors clustered at the city level. As noted earlier, with only six treated clusters, these standard errors may give a misleading impression when used for testing in conjunction with the usual critical values based upon a normal approximation. To circumvent this problem, we use wild bootstrapped p-values to test the null hypothesis of no treatment effect as suggested by Cameron, Gelbach, and Miller (2008).⁶⁹ Stars in the Tables, indicating significance levels, are based on these p-values. Unsurprisingly, the bootstrapped p-values tend to be substantially larger than would be obtained with the usual normal approximation. Because the empirical bootstrap distributions of our test statistics differ substantially across estimators and outcomes, our analytical standard errors and p-values occasionally move in opposite directions across specifications.

Table 4 presents estimates of the impact of EZ designation on economic activity in EZ neighborhoods as measured in the LBD. As mentioned earlier, the LBD estimates compare EZ and control tracts over the interval 1992-2000 because an Economic Census was conducted in 1992. Column 1 reports simple differences-in-differences estimates which yield large (12.2%) positive effects on the number of jobs, modest insignificant increases in the number of establishments, and small insignificant decreases in average earnings per worker. Column 2 shows that after adjusting the differences-in-differences estimates for covariate imbalance via OLS the estimated impact on jobs jumps to nearly 18%, while the impact on establishments achieves statistical significance. Column 3 gives the results of our regression based reweighting estimator which yields even larger jobs impacts. We also detect in these specifications larger increases in the number of establishments. The general tendency for covariate adjustment to increase the point estimates suggests that EZs may have been awarded to economically declining neighborhoods.

⁶⁹See Supplemental Appendix B for details.

The second panel of Table 4 computes impacts on firms located in the zone in 1992. This attenuates the estimated job impacts suggesting that some of the overall employment impact is due to firm births. The negative impacts on the number of establishments in this restricted sample indicate that designation may have also increased firm death rates.⁷⁰ The bottom two panels of the table break impacts down by 1992 establishment size. Though the estimates are quite noisy, we find that employment increased only at establishments that were already large in the 1992 economic census. These findings are consistent with the survey evidence in Hebert et al. (2001) that large firms were more likely to take advantage of the tax credits and suggest an important role for this feature of the program. We also see some evidence that EZ designation is associated with employment reductions and elevated death hazards among small firms, though these estimates are not statistically significant.

Table 5 provides estimates of the number of jobs created based upon the Journey to Work component of the Decennial Census. The estimated impacts lie in the range 12 – 19%, which is reassuringly similar to the range of estimates obtained from the LBD. By crossing Census questions on place of work with place of residence we can determine who occupied any jobs that were created. The second panel of Table 5 reports the results of this exercise. Though all specifications find that the largest employment increases in the zone occurred among zone residents, the magnitude and precision of the results vary with the specification used. Parametric reweighting estimation yields an estimated impact of approximately 18% that is borderline significant. Impacts on the employment of non-resident commuters are insignificant but are estimated to be substantial, suggesting the wage credits may not be the only source of increased labor demand in the zones.

Table 6 provides estimates of the impact of EZ designation on the log weekly wages of individuals broken down by place of residence and place of work. To remove the influence of changes in neighborhood composition over time we also report results where the wages have been regression adjusted for individual characteristics at the micro-level via the procedure described in Section IV. We label the impact on the difference between adjusted and unadjusted wages a third “composition” effect, providing the change in wages that would be expected due solely to changes in the composition of the workforce.

Though all of our point estimates suggest modest wage increases for zone residents, we lack the power to reject the null hypothesis of no effect except in our OLS specification. Adjusting for individual characteristics has little effect other than to slightly increase precision. No detectable wage effects are present for zone workers as a whole. However because only roughly 10% of zone workers are zone residents, it is important to further disaggregate these estimates.

The second panel of Table 6 provides wage impacts broken down jointly by place of residence

⁷⁰The net impacts in the first panel suggest the effect on births is larger than the corresponding effect on deaths.

and place of work. Here we find large (8%–13%) wage increases among zone residents who work in the zone, with covariate adjustments leading to larger point estimates. Accounting for composition leads these estimates to rise slightly, typically by less than a percentage point. We also find in some specifications that the wages of resident commuters increased which may reflect spillovers in the demand for labor across zone boundaries. Non-resident commuters exhibit no statistically perceptible wage increase suggesting, in conjunction with the jobs increases for commuters, that the elasticity of supply of commuter labor to the zone is very large. As pointed out in our welfare analysis however, our confidence intervals include economically substantial wage effects, which would suggest a smaller supply elasticity of commuter labor to the zone.

The third panel of Table 6 reports estimated impacts on annual earnings broken down by place of residence and place of work. The qualitative pattern of results is similar to that found for weekly wages though the point estimates are somewhat larger because of small responses in annual weeks worked.⁷¹

Table 7 examines the impact of EZ designation on the housing market. As in Table 6, we use the Census microdata to construct adjusted estimates that hold tract dwelling characteristics constant over time. Owner occupied housing values exhibit dramatic increases of nearly a third across all specifications and samples. Adjusting for building characteristics has little effect on these impacts. Rental rates on the other hand exhibit no perceptible increase in any specification.

This large discrepancy between rental rates and housing values is, at first glance, troublesome. We suspect these findings reflect the fact that Census measures of owner occupied housing values and rents are self-reported. If housing markets in such neighborhoods are relatively illiquid, residents may overestimate the extent to which EZ designation has changed the value of their residence. Rents on the other hand are easy to assess as they are usually paid monthly. Moreover, many units in such neighborhoods may be rent controlled which could (at least temporarily) limit upward pressure on measured rents.⁷²

To examine these conjectures in more detail, we compute housing value and rental rate impacts on households who report living in a different house five years ago. Since homeowners in this category purchased their dwelling recently they are more likely to have an accurate sense of its market value. Although our estimates are imprecise, we find that the housing value impacts in this subpopulation are substantially smaller than our earlier estimates as might be expected if long time residents are overconfident about their neighborhood's prospects. We also find that impacts on

⁷¹In unreported results, we also investigated impacts on household earned income and household public assistance income. We found no effect on household public assistance income and a positive effect on household earned income similar to the effect on the annual earnings of residents.

⁷²We thank an anonymous referee for this suggestion.

rental rates are somewhat larger (though still insignificant) in this sample. We take this as suggestive evidence that, over longer horizons, rental rates may in fact rise.

Table 8 documents that neither total tract population nor the number of zone households seem to have been substantially affected by zone designation. Population registers a modest increase in the parametric reweighting specification, but this change is not statistically significant, and coupled with the negligible impacts on the number of households, suggests at most a slight increase in average household size. We also fail to find an appreciable effect on the fraction of housing units that are vacant.

Finally, if rents or other local prices had increased substantially one would expect outmigration rates to rise as lower skilled groups are priced out of the neighborhood. Yet Table 8 provides no evidence of an impact on the fraction of households living in the same house as five years ago. We do, however, find a small increase in the fraction of college graduates in these neighborhoods which suggests that when prior residents do leave, they may be replaced by a somewhat different mix of new arrivals, even if the total flow of new arrivals is essentially constant.

Overall, these findings suggest that EZ designation created jobs in zone neighborhoods, that both zone and nonzone residents obtained employment in these neighborhoods that would not have otherwise been available, and that earnings increased substantially for local workers. While housing prices rose, we find little evidence of important increases in the local cost of living for prior residents. We also fail to find significant increases in population though the composition of that population may have shifted to some extent. These results suggest that while commuting patterns may be relatively sensitive to changes in incentives, the residential choices of workers are (over the horizon studied) quite rigid, presumably because zone neighborhoods are poor substitutes for less distressed areas. The evidence also suggests an important role for both the wage credit and block grant features of the EZ program which appear to have disproportionately raised employment at large firms, raised wages among local workers, and still raised the employment of nonresident commuters albeit by less than local residents.

VI. Robustness

If unmeasured factors correlated with the future performance of neighborhoods influenced the process by which zones were awarded our estimates will be biased. To address such concerns, we now perform two tests of the assumptions underlying our research design.

Our first test is to create a series of “placebo” zones in treated cities and compare their performance over the 1990s to that of control tracts using our differences-in-differences estimators. A

finding of nonzero treatment effects in this sample would suggest that our analysis is confounded by city specific shocks.

To construct the placebo zones we estimated a pooled propensity score model for tracts in treated cities (see Appendix III for details) and then performed nearest neighbor propensity score matching without replacement in each city. We restrict the set of potential placebo tracts to obey the minimum poverty and unemployment eligibility criteria in 1990 along with our usual restrictions on tract population and the number of households. We also discard tracts located within a mile of actual EZs as they might experience spillovers. This yields a set of placebo zones of nearly the same size and with approximately the same census characteristics as each real EZ.⁷³

Table 9 shows the results of applying our differences-in-differences estimators to our sample of placebo tracts. After reweighting, none of the outcomes register statistically significant differences across placebo and control zones. Moreover, no systematic pattern is apparent from the placebo point estimates as a whole.

As a second check on our research design we convert the outcome variables to scaled within city ranks.⁷⁴ If our results are merely picking up city specific shocks then the rank of an average EZ tract in its city wide distribution of mean tract rental rates, for example, should not change over the 1990s relative to the rank of a similar rejected tract in its city-wide distribution. We scale our ranks by the number of tracts in each city so that the transformed outcomes can be thought of as percentiles which are comparable across cities of different absolute size.⁷⁵

Columns 5 through 7 of Table 9 show the results of applying the three differences-in-differences estimators to the transformed outcomes. The point estimates represent the average impact of EZ designation on the percentile rank of EZ neighborhoods. For example, Column 5 indicates that EZ designation led EZ neighborhoods to rise, on average, 2.7 percentiles in the within city distribution of jobs per tract. The results are in agreement with the findings of Tables 4-7 which we take as evidence that our prior results are unlikely to have been generated by spurious correlation with city wide trends.

Finally, Appendix Table A3 provides impact estimates in three alternative estimation samples. The first sample relies entirely upon rejected round I applicants for controls and hence discards later

⁷³The number of placebo tracts is somewhat less than the number of EZ tracts because some cities did not have enough tracts that met the eligibility criteria.

⁷⁴In a previous version of this paper we experimented with a reweighted difference-in-differences-in-differences (DDD) estimator that sought to find within city controls for both actual and rejected EZ tracts. This estimator performed quite poorly severely failing a number of robustness tests. This poor performance was caused by difficulties in finding suitable control tracts in rejected cities. We believe the following percentile rank approach to be a much more transparent and robust approach to making within city comparisons.

⁷⁵That is, for any outcome Y_{tzc} in tract t of zone z in city c , we form a new outcome $\tilde{Y}_{tzc} = \text{rank}_c(Y_{tzc})/N_c$ where rank_c is the track rank (the lowest value receives rank 1, the highest rank N_c) of Y_{tzc} in the city wide distribution of the variable in that year and N_c is the number of tracts in the relevant city.

round zones. The second sample drops New York city which may have been subject to different shocks during the sample period. The third sample adds the two SEZs (Cleveland and Los Angeles) to the sample. Much the same pattern of results is present in each sample with the rejected sample finding somewhat larger impacts and the SEZ sample yielding somewhat greater precision.

VII. Welfare Analysis

Our empirical analysis suggests that EZ designation generated important changes in local price levels and behavior. The model developed in Section II provides a framework for assessing the welfare consequences of these changes. We begin by considering the incidence of EZ designation on program stakeholders. Derivations analogous to those in (30) reveal that the total impact of the program on workers may be written:

$$\begin{aligned}
dV &= \sum_j \sum_k \sum_s N_{jks} [w_{jks} d \ln w_{jks} - r_j d \ln r_j + A_j d \ln A_j] \\
&\approx d \ln w^{local} \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} \sum_s N_{jks} w_{jks} + d \ln w^{commute} \sum_{j \in \mathcal{N}_0} \sum_{k \in \mathcal{N}_1} \sum_s N_{jks} w_{jks} \quad (39) \\
&+ d \ln A^{EZ} \sum_{j \in \mathcal{N}_1} N_j \cdot A_j - d \ln r^{NEZ} \sum_{j \in \mathcal{N}_0} N_j \cdot r_j - d \ln r^{EZ} \sum_{j \in \mathcal{N}_1} N_j \cdot r_j
\end{aligned}$$

where $d \ln w^{local}$ is the average impact on the wages of zone resident workers, $d \ln w^{commute}$ is the corresponding impact on non-resident commuters, $d \ln A^{EZ}$ is the average increase in zone amenities, $d \ln r^{NEZ}$ is the average impact on rental rates of housing outside of the zone, and $d \ln r^{EZ}$ is the average impact on rental rates of housing inside the zone.

Hence, to first order, the program's benefits may be measured as: a) the total earnings increase for zone resident workers, b) the earnings increase for non-resident commuters, c) the value of any improvements in local amenities, and d) the value of any rent reductions that occur outside the zone due to population decreases. These benefits to workers are offset by any increases in the cost of living in the zone which may be measured in terms of the total zone rental cost. Our estimates suggest little effect on population or rents inside the zone so we assume for simplicity that zone amenities and rents outside the zone were both unaffected by designation ($d \ln A^{EZ} = d \ln r^{NEZ} \approx 0$). Note that these assumptions provide a lower bound estimate of the benefits of EZ designation since we expect that amenity levels were positively impacted by the program, if only slightly.⁷⁶ Also noteworthy is that this accounting of benefits assumes perfect competition and hence ignores any economic rents that might accrue to business owners which will again lead us to understate the true social benefits of the EZ program.

⁷⁶For example Hebert et al. (2001) document 14 brownfield cleanup programs, 37 neighborhood beautification projects, and 23 parks and playgrounds built or rehabilitated as part of the EZ program. In unreported results we also found some evidence of small reductions in rates of violent crime in EZ cities.

Table 10 provides calculations converting our treatment effect estimates from Section V into effects on totals corresponding to the terms in (39). We rely on the results of our OLS specifications which tend to be most precise. Our “baseline” scenario takes our point estimates at face value even when statistically insignificant. To convey the uncertainty in our estimates we report 90% confidence intervals for the relevant impacts and also report a “pessimistic” scenario where impacts take on their least favorable values in these intervals.

Approximately 38,000 zone residents worked in EZs in 2000 with a payroll of roughly \$800M. Our estimate of the program’s impact on the wages of local residents is roughly 13% which translates into a \$109M increase in annual earnings for zone residents who work in the zone.⁷⁷ This figure is above the \$55M in wage credits disbursed in 2000 but the lower limit of our 90% confidence interval for this impact amounts to only \$38M in increased wages. It is in fact possible for the wages of zone residents to rise by more than the total amount of credits if the block grants were productive. Though imprecise, our point estimates of the impact of the program on the wages of nonresident zone workers (and the corresponding impacts on employment of nonresident commuters) suggest that such productivity effects may have indeed been present. We found a statistically insignificant 0.5% increase in the wages of nonresident EZ workers in response to designation but cannot rule out more substantial effects. However in our “pessimistic” scenario we simply set this impact equal to zero. We also failed to find significant increases in the wages of the roughly 141,000 zone residents who in 2000 lived in the zone but worked elsewhere. Our OLS point estimate of a 3.3% increase in this group’s weekly wages would yield roughly \$118M in additional annual earnings but, in our pessimistic scenario, we set this impact to zero as well.

Potentially offsetting the estimated increases in the earnings of local workers is the possibility of small increases in housing rents. Approximately 190,000 EZ households rented their dwellings in 2000 with total annual rental payments of \$900M. Our estimates of the impact of designation on rents are small and statistically insignificant. But the upper limit of a 90% confidence interval includes impacts as large as 7.3%. Thus, a pessimistic interpretation of the rent impacts would amount to an aggregate transfer from renters to landlords of \$67M per year. To verify that for the subpopulation of local workers the positive effects of the wage increases outweigh the negative effects of any rent increases we compute confidence intervals for the estimated impact on $q \equiv \ln w^{local} - s \ln r^{EZ}$, where $s \equiv \frac{r^{EZ}}{w^{local}}$ is the budget share of rental housing. We set s to 0.25, which is approximately the ratio of total rents to total earnings among local zone workers in the 1990 microdata. Because $dq = \frac{dw^{local} - dr^{EZ}}{w^{local}}$, the reweighted difference in difference impact on q provides an approximation to the percentage increase in disposable income of local workers. A 90% wild bootstrapped confidence interval for dq is provided in Table 10 and shown to have a lower

⁷⁷Our results are in log points. We compute impacts relative to 2000 levels for expositional ease. Similar results obtain if we take 1990 levels as the base.

bound of 3.4%. Thus, we conclude that, at least for local workers, the earnings increases associated with the program outweigh any increases in cost of living.

Finally, an additional 46,000 EZ households own their homes which were in aggregate worth \$4.8B in 2000. Our estimates suggest EZs boosted housing values by approximately 28%, which amounts to approximately \$1.35B in additional wealth. Our scepticism of these results leads us to also consider an alternative scenario where the housing value impacts are set to the lower limit of their confidence interval, which is below even the increase reported by new residents, whom we believe have more accurate information regarding their housing prices. This pessimistic scenario still yields a \$500M windfall to owner occupiers in the zone.

In sum, the point estimates in our baseline scenario imply that total worker earnings rose by roughly \$296M per year while rents rose by only \$5.5M per year and housing wealth rose for owner occupiers by roughly \$1.35B. Under our pessimistic scenario, aggregate earnings rose by only \$36M, rents rose by \$67M, and housing wealth rose by \$500M. Even under this worst case interpretation, we still find that earnings rose more for local workers than did rents. But nonworking households (or households working outside the zone) may have suffered cost of living increases making them strictly worse off.

We turn now to an analysis of the program's deadweight loss. We start with the tax credits, whose efficiency consequences depend critically upon the number of zone jobs created for zone workers in the covered sector. Unfortunately, we cannot directly identify which jobs are in the covered sector as the Census lacks information about employer characteristics and the LBD does not report worker residence. Our estimates from Table 5 indicate that EZs generated a roughly 15% increase in the number of zone jobs for zone residents. Since many local jobs are in the uncovered sector this figure is likely to provide a substantial underestimate of the impact on covered local employment ($d \ln N_{jk1}$), with the degree of understatement depending upon the relative size of the covered and uncovered sectors.

It is important then to supplement our analysis with auxiliary sources of information. Recall that \$55M in wage credits was disbursed to EZ firms in 2000. The maximum allowable credit per worker is \$3,000. In most cases the full credit will be claimed, but to be conservative, let us suppose that \$2,500 was claimed on the average worker. This yields 22,000 workers on whom the credit was claimed – roughly sixty percent of the local workforce. Therefore, if all of the jobs created were in the covered sector and there were no negative impact on uncovered employment we would estimate that employment expanded by roughly $\frac{15\%}{60\%} = 25\%$ in the covered sector. We use this as our baseline estimate as we suspect that employment may actually have increased in the uncovered sector as well in response to the block grants.

However, our model indicates that if the block grants were ineffective employment at firms in the uncovered sector may actually fall in response to the credit. Therefore we also consider an inflated estimate of the covered sector employment response obtained from survey data.⁷⁸ A 1999 General Accounting Office survey found that among firms making use of the wage tax credit, a third indicated that the credits were “very important” or “extremely important” for the hiring decision.⁷⁹ Hence, a more pessimistic estimate may be obtained by assuming that one third of the credits claimed resulted in jobs that would not otherwise have occurred (i.e. $d \ln N_{jk1} = \frac{1}{3}$).

Using our baseline estimate we can compute the jobs semi-elasticity as $\psi^{base} = \frac{1/4}{.2} = \frac{5}{4}$.⁸⁰ Plugging this number into (34) yields an estimated deadweight loss associated with the employment tax credit of $\frac{1}{2} \times \frac{5}{4} \times 0.2 \times \$55M = \$6.9M$ or roughly 13% of the flow cost of the subsidy.⁸¹ A corresponding calculation using the pessimistic value of $\psi^{pess} = \frac{1/3}{.2} = \frac{5}{3}$ results in a deadweight loss of roughly 18%. We consider this figure a substantial overestimate both because it presumes substantial flows from the uncovered to the covered sector and because the zone wage credit should offset pre-existing payroll taxes and hence, to some extent, actually reduce the amount of distortion in hiring decisions. Of course, these estimates must be inflated to take into account the marginal cost of funds. An upper bound estimate of this parameter is provided by Feldstein (1999) who obtains a deadweight cost of thirty cents of every dollar raised. This yields an upper bound composite deadweight loss estimate of approximately forty eight percent of the subsidy.

As noted in Section II, the block grants accompanying EZ designation may yield either a deadweight loss or a net welfare gain depending upon how effectively they were spent. We have already assumed that EZs had no effect on amenity levels, so we set $\lambda_a = 0$ in (35). Roughly $C = \$400M$ worth of federal block grants was invested in zone neighborhoods over the sample period. A worst case estimate then is that all $\$400M$ worth of block grants was wasted on unproductive activities, a hypothesis we cannot reject. Although we failed to detect statistically significant impacts on the wages of nonresident commuters, we caution that our point estimates do not rule out the possibility that the block grants were cost effective. To illustrate the sensitivity of such a calculation, note that the zone workforce $\left(\sum_{k \in \mathcal{N}_1} N_{.k} \right)$ consisted of approximately 400,000 workers in 2000 with approximately $\$15B$ in annual earnings. Even a 0.5% effect on productivity of the sort suggested by our OLS point estimates would yield $0.005 \times \$15B = \$75M$ in additional earnings per year.

⁷⁸Our LBD-based estimates of the employment impacts on small firms also suggest that EZs may have reduced employment at small firms likely to be in the uncovered sector.

⁷⁹See Table III.1 of General Accounting Office (1999).

⁸⁰This figure is substantially smaller than the intra-metropolitan job elasticity estimates surveyed by Bartik (1991). A potential explanation for this discrepancy is that these tax credits are tied to residence in distressed neighborhoods which the bulk of workers find relatively undesirable.

⁸¹We have made use here of the fact that $\tau \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1} w_{jk1}$ is the size of the *aggregate* subsidy when firms are able to claim a credit of 20% on the wage bill of every covered worker.

Assuming a social discount rate of ten percent yields an annuitized value for this earnings stream of \$750M which is well above the \$400M cost of the block grants over the period of study. Isolating the effectiveness of local block grant spending is a priority for future research.

VIII. Conclusion

Our comparison of EZ neighborhoods to rejected and future tracts revealed important impacts of EZ designation on local price levels and behavior. Designation seems to have resulted in substantial increases in zone employment along with increases in the wages of zone residents working in the zone. These changes in the zone labor market appear not to have been accompanied by dramatic changes in the local cost of living. Population and housing rents remained roughly constant, though evidence on the rental rates of new arrivals to the neighborhood suggests that rents may eventually rise. Though we find very large increases in the price of owner occupied housing, we suspect the magnitude of these results is to some extent a reflection of the manner in which housing value data are collected in the Census. However, these results may also foretell future increases in the local cost of living.

The conclusion of our welfare analysis is that the EZ program appears to have successfully transferred income to a small spatially concentrated labor force with modest deadweight losses aside from the usual cost of raising the funds for the subsidy itself. We caution however that our study provides only a short run evaluation of the EZ program. Administrative data indicate that participation in the EZ tax credit program increased only gradually over time and, as evidenced by our annual analysis of LBD data, it took many years for some economic outcomes to respond. The responses of firms, population, and prices may well differ substantially over longer periods of time, if EZ subsidies in fact persist over such horizons. If however, these subsidies eventually lapse as originally intended, an important question will be whether they have lasting effects, a subject studied in a different context by Kline and Moretti (2011).

Finally, we emphasize that many of our empirical results are imprecise and should not necessarily be expected to generalize to later round and future zones. Additional zones targeting less heavily distressed communities may yield larger distortions as such communities may be closer substitutes with surrounding areas. Moreover, later round zones utilize different combinations of benefits. While we find it plausible that the mix of large block grants and wage credits accompanying EZs would yield different results than their smaller state level predecessors, more work is necessary to disentangle the effectiveness of various combinations of spatial subsidies.

TABLE 2.1: 1990 CHARACTERISTICS OF
FIRST ROUND EMPOWERMENT ZONES (EZ)

City	Total Population	Population Rank	Population in EZ	Poverty Rate in EZ	Unemp. Rate in EZ	EZ Area (sq. miles)	Number of Census Tracts
Atlanta	395,337	37	43,792	58	20	8.1	20
Baltimore	736,014	13	72,725	42	16	7.1	23
Chicago	2,783,484	3	200,182	49	28	14.3	81
Detroit	1,027,974	7	106,273	47	28	19.5	42
New York	7,320,621	1	204,625	42	18	6.3	51
Philadelphia/Camden	1,594,339	5	52,440	50	23	4.3	17

Source: 1990 Decennial Census and HUD

TABLE 2.2: TOTAL SPENDING

	SSBG	Outside Money	Total
Total (in million \$):	386	2,848	3,234
Expenditure by category (in million dollars):			
Access to Capital	83	1,483	1,566
Business Assistance	56	482	538
Workforce Development	48	49	97
Social Improvement	76	163	240
Public Safety	18	255	272
Physical Development	14	82	97
Housing	71	326	397
Capacity Improvement	20	7	27
Average annual expenditure (in \$):			
Access to Capital per firm			20,881
Business Assistance per firm			7,172
Workforce Development per unemployed			261
Social Improvement per housing unit			138
Public Safety per person			56
Physical Development per poor person			44
Housing per housing unit			229
Capacity Improvement per EZ			891,295

Source: Appendix F of Hebert et al. (2001)

TABLE 2.3: PRE-TREATMENT SAMPLE MEANS

	Levels in 1990/1992 [†]					Changes 1980-1990 / 1987-1992 [†]				
	EZ's	Rejected/ Future Zones	Rejected/ Future Zones Reweighted	p-value of difference between [1] and [2]	p-value of difference between [1] and [3]	EZ's	Rejected/ Future Zones	Rejected/ Future Zones Reweighted	p-value of difference between [6] and [7]	p-value of difference between [6] and [8]
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
<i>Census tracts characteristics</i>										
Economic Index (Residents)	0.000	0.581	0.083	0.004	0.586	0.000	-0.170	0.028	0.324	0.689
Employment Rate	0.366	0.438	0.372	0.003	0.643	0.009	-0.013	0.001	0.098	0.066
Unemployment Rate	0.241	0.182	0.229	0.066	0.489	0.048	0.042	0.045	0.693	0.680
Poverty Rate	0.480	0.424	0.471	0.077	0.699	0.042	0.061	0.025	0.298	0.122
Economic Index (Workers -JTW)	0.000	-0.142	-0.232	0.539	0.125	0.000	-0.011	-0.047	0.973	0.828
Log (Jobs) -JTW	6.577	6.966	6.682	0.100	0.361	-0.199	-0.124	-0.181	0.448	0.857
Log (Weekly Wage of Zone Workers) - JTW	5.963	5.893	5.928	0.014	0.139	0.531	0.560	0.555	0.072	0.104
Log (Weekly Wage of Zone Residents)	5.555	5.456	5.444	0.139	0.038	0.535	0.470	0.481	0.489	0.384
Economic Index (Workers -LBD) [‡]	0.000	0.310	0.081	0.194	0.527	0.000	-0.129	-0.128	0.138	0.148
Log (Jobs) -LBD [‡]	5.774	6.340	5.992	0.165	0.281	-0.089	-0.093	-0.102	0.916	0.672
Log (Establishments) - LBD [‡]	3.106	3.559	3.204	0.006	0.287	-0.076	-0.081	-0.079	0.800	0.918
Log (Average Earnings per Worker) -LBD [‡]	2.968	2.951	2.954	0.669	0.812	0.243	0.174	0.177	0.038	0.133
Demographic Index	0.000	0.582	-0.070	0.001	0.430	0.000	-0.169	-0.101	0.067	0.126
% Households Female-headed	0.567	0.516	0.576	0.008	0.308	0.062	0.066	0.071	0.344	0.094
% College	0.067	0.077	0.059	0.372	0.186	0.026	0.014	0.016	0.021	0.068
% High school dropouts	0.316	0.275	0.313	0.004	0.748	0.025	0.019	0.029	0.297	0.576
% Black	0.739	0.610	0.757	0.015	0.362	0.025	0.035	0.015	0.476	0.127
% Hispanic	0.180	0.163	0.171	0.832	0.496	0.018	0.023	0.020	0.659	0.805
Population Index	0.000	-0.125	0.067	0.672	0.707	0.000	0.298	-0.037	0.188	0.719
Log (Population)	7.773	7.887	7.832	0.365	0.491	-0.209	-0.117	-0.206	0.300	0.937
Log (Households)	6.923	6.996	6.923	0.593	0.997	-0.175	-0.110	-0.184	0.123	0.746
% Same House as Five Years Ago	0.573	0.509	0.579	0.051	0.745	-0.022	-0.028	-0.029	0.647	0.689
Housing Index	0.000	0.175	-0.134	0.559	0.691	0.000	-0.006	0.163	0.979	0.311
Log (Rent)	5.350	5.370	5.295	0.831	0.565	0.600	0.608	0.622	0.883	0.527
Log (Housing Value)	10.490	10.566	10.310	0.750	0.448	0.653	0.600	0.668	0.697	0.800
% Houses that are Vacant	0.166	0.143	0.146	0.371	0.415	0.037	0.037	0.020	0.997	0.581
<i>City characteristics</i>										
Total crime / population x 100	0.099	0.105	0.099	0.710	1.000	0.009	0.013	0.014	0.568	0.413
Avg. across tracts % black	0.478	0.343	0.478	0.045	1.000	0.060	0.052	0.066	0.773	0.410
% Workers in Manufacturing	0.156	0.156	0.156	0.986	1.000	-0.070	-0.061	-0.070	0.255	0.968
% Workers in City Government	0.065	0.045	0.065	0.389	1.000	0.022	-0.003	-0.002	0.245	0.470
Log (City Population)	14.533	13.056	13.757	0.001	0.000	-0.064	-0.014	-0.064	0.200	1.000
Observations (number of census tracts)	234	1429	1429	1663	1663	234	1429	1429	1663	1663

Note. Sources: Variables marked as JTW are based on the Journey-to-Work component of the Decennial Census. Variables marked as LBD come from the Longitudinal Business Database. All other tract level covariates come from the Census. City covariates are from the County/City Databook. Indices are linear combination of the covariates listed below them, see Section IV for details. Columns: Column [1] reports sample means for census tracts inside EZs. Column [2] shows means for control tracts in rejected or future treated areas (listed in Table A1). Column [3] reports means for control tracts after parametric reweighting (see Section III for details.) Column [4] presents wild bootstrap p-values for a test of the null hypothesis that the mean in column [1] equals the mean in column [2]. Similarly, column [5] reports p-values for the equality of means in columns [1] and [3]. Column [6] shows the mean change between 1980 and 1990 in EZs, columns [7] and [8] report the change and the reweighted change in control areas. Columns [9] and [10] show the bootstrap p-values of the difference between columns [7] and [6] and between columns [8] and [6], respectively. † For LBD variables columns [1], [2] and [3] show the levels in 1992 and columns [6], [7] and [8] present the change between 1987 and 1992.

TABLE 2.4: WAGE AND JOBS IMPACTS
(Longitudinal Business Database -LBD-)

	Naïve	OLS	PW	Obs.
	[1]	[2]	[3]	[4]
<hr/>				
All firms				
Log (Jobs)	0.122 [0.048]*	0.179 [0.051]***	0.213 [0.072]***	1651
Log (Establishments)	0.028 [0.027]	0.041 [0.017]**	0.057 [0.036]*	1651
Log (Average Earnings per Worker)	-0.018 [0.013]	-0.002 [0.017]	0.001 [0.018]	1651
<hr/>				
All firms present in 1992				
Log (Jobs)	0.042 [0.044]	0.107 [0.053]	0.143 [0.068]*	1650
Log (Establishments)	-0.057 [0.033]	-0.022 [0.027]	-0.013 [0.035]	1650
Log (Average Earnings per Worker)	-0.022 [0.020]	-0.007 [0.020]	0.003 [0.027]	1650
<hr/>				
<5 Employees				
Log (Jobs)	-0.155 [0.108]	-0.048 [0.086]	-0.035 [0.115]	1577
Log (Establishments)	-0.093 [0.074]	-0.064 [0.059]	-0.059 [0.082]	1577
Log (Average Earnings per Worker)	-0.026 [0.025]	0.011 [0.027]	0.009 [0.032]	1577
<hr/>				
>5 Employees				
Log (Jobs)	0.065 [0.070]	0.119 [0.060]	0.150 [0.092]	1635
Log (Establishments)	0.007 [0.021]	0.030 [0.019]	0.043 [0.031]	1635
Log (Average Earnings per Worker)	-0.023 [0.023]	-0.016 [0.021]	-0.004 [0.026]	1635

Note: Each entry gives the 1992-2000 differences-in-differences (DD) estimate of EZ designation on the outcome presented in each row. Column [1] reports DD estimates without controls; [2] reports DD estimates controlling for lagged city and tract level characteristics; [3] reports parametric reweighting DD estimates. See Section IV for list of covariates. Column [4] shows the number of observations used in the estimation of the treatment effect for each outcome. Asymptotic standard errors are shown in square brackets and are clustered by city (63 clusters.) Stars reflect significance level obtained by a clustered wild bootstrap-t procedure described in Appendix I. Legend: * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

TABLE 2.5: EMPLOYMENT IMPACTS
(Census, Journey-to-Work -JTW-)

	Naïve [1]	OLS [2]	PW [3]	Obs. [4]
Log (Jobs)	0.187 [0.062]	0.145 [0.061]*	0.122 [0.085]	1656
<i>By place of residence and place of work</i>				
Log (Zone Jobs Held by Zone Residents)	0.166 [0.088]	0.150 [0.072]	0.176 [0.103]*	1653
Log (Zone Jobs Held by Non-Residents)	0.161 [0.050]*	0.097 [0.059]	0.064 [0.073]	1656
Log (Non-Zone Jobs Held by Zone Residents)	0.033 [0.060]	0.084 [0.062]	0.123 [0.061]	1654

Note: Each entry gives the 1990-2000 differences-in-differences (DD) estimate of EZ designation on the outcome presented in each row. Column [1] reports DD estimates without controls; [2] reports DD estimates controlling for lagged city and tract level characteristics; [3] reports parametric reweighting DD estimates. See Section IV for list of covariates. Column [4] shows the number of observations used in the estimation of the treatment effect for each outcome. Asymptotic standard errors are shown in square brackets and are clustered by city (63 clusters.) Stars reflect significance level obtained by a clustered wild bootstrap-t procedure described in Appendix I. Legend: * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

TABLE 2.6: WAGE IMPACTS
(Census, Journey-to-Work -JTW-)

	Unadjusted			Composition-adjusted			Composition Effect			Obs. [10]
	Naïve [1]	OLS [2]	PW [3]	Naïve [4]	OLS [5]	PW [6]	Naïve [7]	OLS [8]	PW [9]	
<i>Panel A: Weekly Wages</i>										
Log (Weekly Wage Income of Zone Residents)	0.037 [0.035]	0.047 [0.021]	0.040 [0.037]	0.026 [0.032]	0.053 [0.015]**	0.050 [0.033]	0.010 [0.006]	-0.006 [0.010]	-0.010 [0.008]	1653
Log (Weekly Wage Income of Zone Workers)	-0.010 [0.026]	0.011 [0.030]	0.003 [0.031]	0.001 [0.024]	0.017 [0.026]	0.010 [0.029]	-0.011 [0.009]	-0.006 [0.012]	-0.007 [0.011]	1652
<i>Panel B: Weekly Wages by place of residence and place of work</i>										
Log (Weekly Wage Income of Zone Residents Working in Zone)	0.078 [0.045]	0.127 [0.041]**	0.112 [0.055]*	0.088 [0.046]	0.133 [0.051]**	0.121 [0.051]**	-0.010 [0.024]	-0.006 [0.022]	-0.010 [0.026]	1646
Log (Weekly Wage Income of Non-Residents Working in Zone)	-0.014 [0.029]	-0.015 [0.033]	-0.010 [0.035]	0.006 [0.023]	0.005 [0.027]	0.006 [0.030]	-0.019 [0.013]	-0.020 [0.015]	-0.017 [0.014]	1644
Log (Weekly Wage Income of Zone Residents Working Outside Zone)	0.023 [0.028]	0.043 [0.034]	0.047 [0.031]*	0.006 [0.025]	0.036 [0.024]	0.045 [0.027]*	0.018 [0.010]*	0.007 [0.016]	0.002 [0.012]	1641
<i>Panel C: Annual Wage Income by place of residence and place of work</i>										
Log (Annual Wage Income of Zone Residents Working in Zone)	0.181 [0.062]**	0.244 [0.075]**	0.219 [0.074]**	0.108 [0.074]	0.184 [0.085]*	0.166 [0.078]	0.073 [0.042]	0.060 [0.043]	0.053 [0.043]	1646
Log (Annual Wage Income of Non-Residents Working in Zone)	-0.023 [0.040]	-0.022 [0.038]	-0.012 [0.043]	-0.002 [0.031]	0.000 [0.026]	0.005 [0.035]	-0.021 [0.017]	-0.022 [0.020]	-0.017 [0.018]	1644
Log (Annual Wage Income of Zone Residents Working Outside Zone)	0.020 [0.038]	0.040 [0.052]	0.038 [0.043]	-0.005 [0.030]	0.031 [0.036]	0.035 [0.035]	0.024 [0.013]*	0.009 [0.021]	0.003 [0.017]	1641

Note: Each entry gives the 1990-2000 differences-in-differences (DD) estimate of EZ designation on the outcome presented in each row. Columns [4]-[6] adjust the outcomes for demographic changes at the micro-level (see Section IV). Columns [7]-[9] report the results due to changes in demographic composition. Columns labeled "Naïve" report DD estimates without controls. Columns labeled "OLS" report the DD estimates controlling for lagged city and tract level characteristics. Columns labeled "PW" report parametric reweighting DD estimates. Asymptotic standard errors are shown in square brackets and are clustered by city (63 clusters.) Stars reflect significance level obtained by a clustered wild bootstrap-t procedure described in Appendix I. Legend: * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

TABLE 2.7: HOUSING IMPACTS

	Unadjusted			Composition-adjusted			Composition Effect			Obs. [10]
	Naïve [1]	OLS [2]	PW [3]	Naïve [4]	OLS [5]	PW [6]	Naïve [7]	OLS [8]	PW [9]	
Log (Rent)	0.023 [0.032]	0.019 [0.030]	0.029 [0.032]	0.014 [0.028]	0.006 [0.026]	0.018 [0.027]	0.009 [0.011]	0.013 [0.008]	0.011 [0.011]	1653
Log (Rent of New Residents)	0.055 [0.045]	0.038 [0.037]	0.055 [0.045]	0.044 [0.040]	0.028 [0.033]	0.046 [0.039]	0.011 [0.008]	0.010 [0.008]	0.009 [0.009]	1647
Log (House Value)	0.370 [0.129]*	0.281 [0.065]**	0.311 [0.142]	0.371 [0.125]*	0.281 [0.064]**	0.317 [0.138]*	0.000 [0.011]	0.000 [0.009]	-0.006 [0.013]	1581
Log (House Value of New Residents)	0.208 [0.145]	0.143 [0.104]	0.142 [0.163]	0.246 [0.131]	0.164 [0.098]	0.171 [0.151]	-0.045 [0.034]	-0.030 [0.024]	-0.038 [0.037]	1540

Note: Each entry gives the 1990-2000 differences-in-differences (DD) estimate of EZ designation on the outcome presented in each row. Columns [4]-[6] adjust the outcomes for demographic changes at the micro-level (see Section IV). Columns [7]-[9] report the results due to changes in demographic composition. Columns labeled "Naïve" report DD estimates without controls. Columns labeled "OLS" report the DD estimates controlling for lagged city and tract level characteristics. Columns labeled "PW" report parametric reweighting DD estimates. Asymptotic standard errors are shown in square brackets and are clustered by city (63 clusters.) Stars reflect significance level obtained by a clustered wild bootstrap-t procedure described in Appendix I. Legend: * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

TABLE 2.8: POPULATION AND MOBILITY IMPACTS

	Naïve [1]	OLS [2]	PW [3]	Obs. [4]
Log (Households)	-0.007 [0.071]	-0.003 [0.036]	0.020 [0.073]	1653
Log (Population)	-0.014 [0.055]	0.028 [0.035]	0.060 [0.059]	1656
% Same House as Five Yrs Ago	-0.004 [0.008]	-0.001 [0.012]	-0.006 [0.011]	1656
% Houses that are Vacant	0.016 [0.013]	-0.007 [0.009]	-0.010 [0.013]	1653
% Black	-0.018 [0.015]	-0.011 [0.009]	-0.015 [0.015]	1656
% College	0.015 [0.006]	0.020 [0.006]***	0.021 [0.007]***	1656

Note: Each entry gives the 1990-2000 differences-in-differences (DD) estimate of EZ designation on the outcome presented in each row. Column [1] reports DD estimates without controls; [2] reports DD estimates controlling for lagged city and tract level characteristics; [3] reports parametric reweighting DD estimates. See Section IV for list of covariates. Column [4] shows the number of observations used in the estimation of the treatment effect for each outcome. Asymptotic standard errors are shown in square brackets and are clustered by city (63 clusters.) Stars reflect significance level obtained by a clustered wild bootstrap-t procedure described in Appendix I. Legend: * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

TABLE 2.9: ROBUSTNESS CHECKS

	Placebo				Percentile			
	Naïve [1]	OLS [2]	PW [3]	Obs. [4]	Naïve [5]	OLS [6]	PW [7]	Obs. [8]
Log (Jobs) -LBD	-0.085 [0.095]	-0.024 [0.093]	-0.026 [0.105]	1574	0.027 [0.007]**	0.022 [0.009]*	0.023 [0.010]**	1651
Log (Establishments)	0.021 [0.035]	0.029 [0.023]	0.042 [0.040]	1574	0.014 [0.007]	0.002 [0.006]	0.000 [0.007]	1651
Log (Average Earnings per Worker)	-0.028 [0.018]	0.013 [0.023]	0.017 [0.026]	1574	0.006 [0.011]	-0.010 [0.011]	-0.009 [0.014]	1651
Log(Jobs) -JTW	0.089 [0.059]	0.065 [0.049]	0.032 [0.074]	1575	0.054 [0.010]**	0.036 [0.015]*	0.029 [0.014]**	1656
Log (Weekly Wage of Zone Residents)	0.004 [0.020]	0.008 [0.012]	0.017 [0.023]	1575	0.032 [0.016]	0.022 [0.015]	0.015 [0.019]	1653
Log (Weekly Wage of Zone Workers)	-0.023 [0.018]	-0.013 [0.032]	-0.009 [0.025]	1571	0.016 [0.018]	0.024 [0.028]	0.020 [0.027]	1652
Log (Rent)	0.003 [0.041]	-0.001 [0.029]	0.007 [0.040]	1575	0.019 [0.006]*	0.015 [0.009]	0.022 [0.008]*	1653
Log (Housing Value)	0.186 [0.070]	0.060 [0.047]	0.083 [0.089]	1529	0.110 [0.049]**	0.089 [0.029]**	0.104 [0.049]*	1581
Log (Households)	0.085 [0.061]	0.017 [0.038]	0.030 [0.064]	1575	-0.013 [0.022]	-0.020 [0.013]	-0.009 [0.023]	1653
Log(Population)	0.027 [0.057]	0.014 [0.039]	0.016 [0.058]	1578	-0.002 [0.012]	-0.005 [0.009]	0.005 [0.013]	1656
% Same House as Five Yrs Ago	0.019 [0.010]	0.011 [0.008]	0.009 [0.013]	1578	0.028 [0.030]	0.018 [0.032]	0.008 [0.033]	1656
% Houses that are Vacant	0.019 [0.019]	-0.005 [0.011]	-0.002 [0.018]	1575	-0.006 [0.029]	-0.042 [0.016]	-0.034 [0.029]	1653

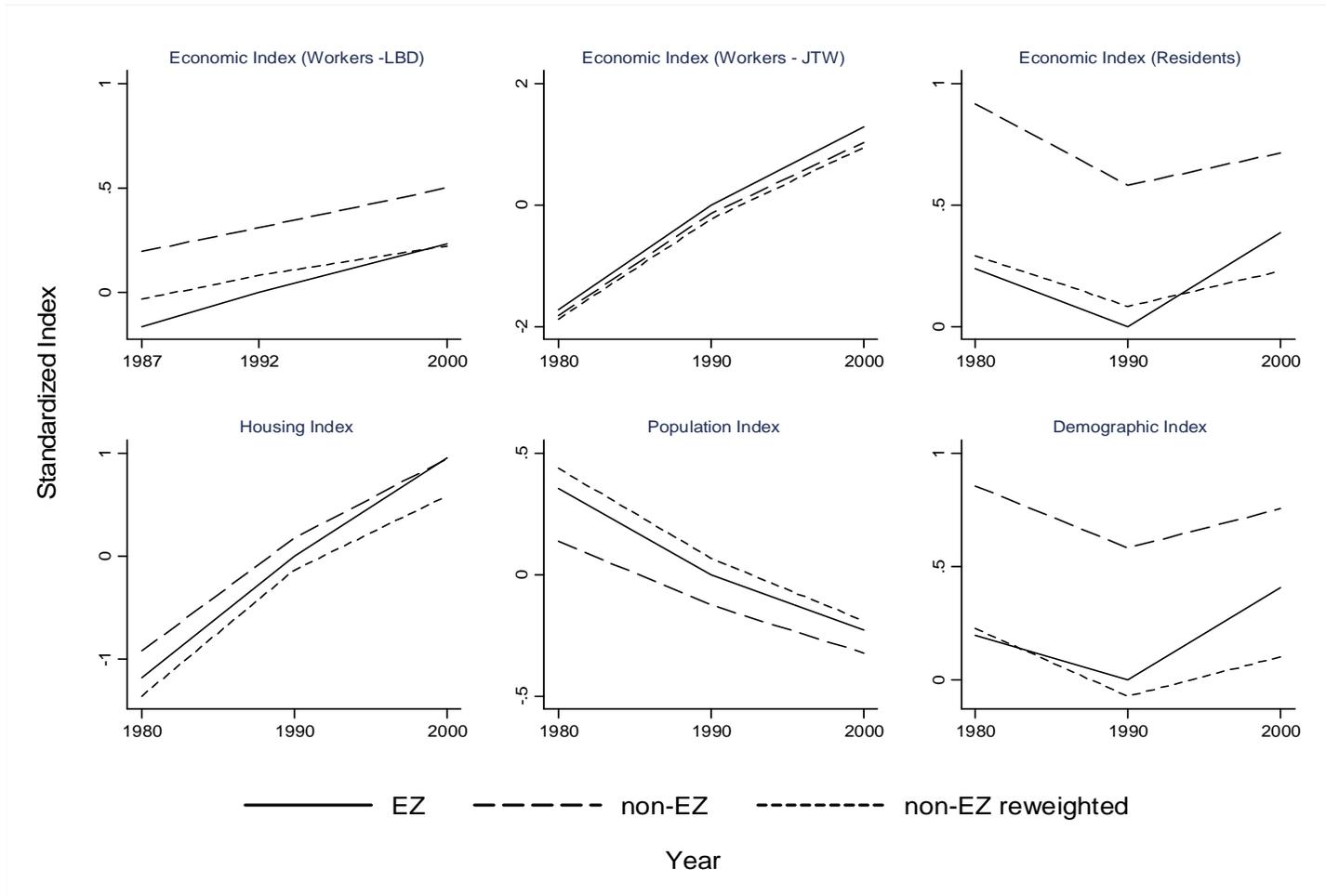
Note: Sources: Variables marked as JTW are based on the Journey-to-Work component of the Decennial Census. Variables marked as LBD come from the Longitudinal Business Database. All other tract level covariates come from the Census. Timing: Variables labeled as LBD are analyzed over the period 1992-2000, all other outcomes are analyzed over the period 1990-2000. Columns: Columns [1]-[3] give differences-in-differences (DD) estimates on a sample of untreated placebo tracts chosen by nearest neighbor matching (See Appendix III). Columns [5]-[7] give DD impacts on percentile ranks of outcomes (see Section VI). Columns [4] and [5] present the number of observations used to estimate each outcome. Estimators: Columns labeled "Naïve" report a DD estimate without controls. Columns labeled "OLS" report DD estimate controlling for lagged city and tract level characteristics. Columns labeled "PW" report parametric reweighting DD estimates. Asymptotic standard errors are shown in square brackets and are clustered by city (63 clusters). Stars reflect significance level obtained by a clustered wild bootstrap-t procedure described in Appendix I. Legend: * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

TABLE 2.10: WELFARE ANALYSIS

	Total workers/ people/households	Total annual payroll/ rents/housing value (in billion \$)	OLS Impact on wages/ rents/housing values	Increase in annual payroll /rents/ housing value (in million \$)	
				Baseline Scenario [1]	Pessimistic Scenario [2]
<i>Panel A: Total impact of the program</i>					
Zone Residents Working in Zone	38,331	0.8	0.133	108.5	37.5
Zone Residents Working Outside Zone	140,708	3.3	0.036	117.5	0.0
Non-Residents Working in Zone	365,918	14.0	0.005	69.9	0.0
House Renters in the Zone	189,982	0.9	0.006	5.5	66.9
House Owners in the Zone	46,161	4.8	0.281	1350.4	499.8
<i>Panel B: Average impact of the program</i>					
	OLS Impact	Confidence Interval			
Log (Weekly Wage of Zone Residents Working in Zone) [†]	0.133	[0.046; 0.248]			
Log (Weekly Wage of Non-Residents Working in Zone) [†]	0.005	[-0.055; 0.076]			
Log (Weekly Wage of Zone Residents Working Outside Zone) [†]	0.036	[-0.011; 0.100]			
Log (Rent) [†]	0.006	[-0.054; 0.073]			
Log (Housing Value) [†]	0.281	[0.104; 0.426]			
Log(Weekly Wage of Zone Residents Working in Zone) - 0.25 Log(Rent) [†]	0.128	[0.034; 0.253]			
Log (Zone Jobs Held by Zone Residents)	0.150	[-0.003; 0.326]			

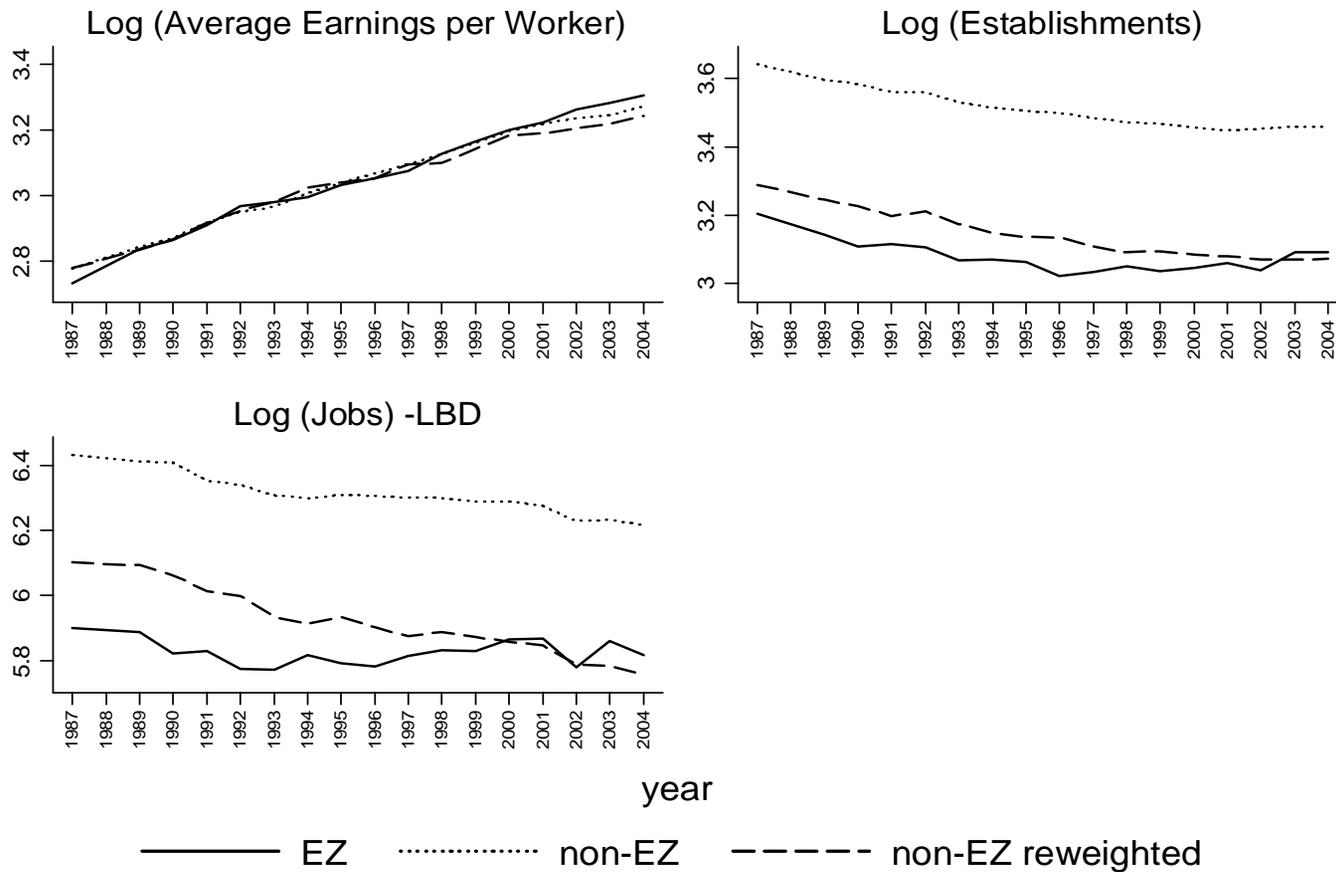
Note: See Section VII for details. Price variables in Panel B have been adjusted via a procedure described in Section IV. Confidence intervals were constructed by inverting a Wild bootstrap t-test. † denotes outcomes that have been adjusted for demographic or, in the case of rents and housing values, quality changes at the micro-level (see Section IV). "Baseline Scenario" uses OLS point estimates in computing impacts. "Pessimistic Scenario" uses lower limit of 90% confidence intervals for impacts on earnings of zone residents working in zone and housing values and upper limit of confidence interval for rent impacts.

FIGURE 2.1: MEANS BY YEAR AND TREATMENT STATUS



Notes: Figure depicts means of the listed variables in EZ tracts and controls. Reweighted lines correspond to weighted means using implicit propensity score weights described in Section III and Appendix I. Indices are linear combinations of thematically similar tract characteristics. See Section IV for details. Components of Economic Index (Residents): employment rate, unemployment rate, and poverty rate; Economic Index (Workers – JTW): $\log(\text{Jobs, JTW})$, $\log(\text{Hourly Wage of Zone Workers – JTW})$, and $\log(\text{Hourly Wage of Zone Residents – JTW})$; Demographic Index: % HHs Female Headed, % College, % Dropout, % Black, and % Hispanic; Population Index: $\log(\text{Population})$, $\log(\text{Households})$, % Same House as Five Years Ago; Housing Index: $\log(\text{Rent})$, $\log(\text{Housing Value})$, % Houses that are Vacant.

FIGURE 2.2: JOBS, WAGES AND ESTABLISHMENTS (LBD)



Notes: Figure depicts means of the listed LBD variables in EZ tracts and controls. Reweighted lines correspond to weighted means using implicit propensity score weights described in Section III and Appendix I.

TABLE 2.A1: TREATMENT BY CITY

City	Sample	EZ-1	Appli- cation	Round I	Round II	Round III	City	Sample	EZ-1	Appli- cation	Round I	Round II	Round III
Akron, OH (Summit)	x		x	EC-1			Louisville, KY	x		x	EC-1		
Albany, GA (Dougherty)			x	EC-1			Lowell, MA	x					RC
Albuquerque, NM (Bernalillo)	x		x	EC-1			Manchester, NH			x	EC-1		
Anniston, AL			x				Memphis, TN	x		x			RC
Atlanta, GA	x	x	x			RC	Miami, FL	x		x	EC-1	EZ-2	
Austin, TX	x		x				Milwaukee, WI	x		x			RC
Baltimore, MD	x	x	x				Minneapolis, MN	x		x	EC-1	EZ-2	
Bellmead, TX			x	EC-1			Mobile, AL	x		x			RC
Benton Harbor, MI			x				Monroe, LA			x			RC
Boston, MA			x	EEC-1	EZ-2		Muskegon, MI			x	EC-1		
Bridgeport, CT	x		x	EC-1			Nashville, TN (Davidson)			x	EC-1		
Buffalo, NY / Lackawanna, NY	x					RC	New Haven, CT			x	EC-1	EZ-2	
Camden, NJ						RC	New Orleans, LA			x			RC
Charleston, SC	x					RC	New York, NY	x	x	x			
Charleston, WV			x				Newark, NJ	x					RC
Charlotte, NC (Mecklenburg)	x		x	EC-1			Niagara Falls, NY						RC
Chattanooga, TN	x					RC	Norfolk, VA	x		x	EC-1	EZ-2	
Chester, PA			x				Oakland, CA	x		x	EEC-1		
Chicago, IL	x	x	x			RC	Ogden, UT (Weber)			x	EC-1		
Cincinnati, OH	x				EZ-2		Oklahoma City, OK	x		x	EC-1		EZ-3
Cleveland, OH			x	SEZ-1			Omaha, NE (Douglas)	x		x	EC-1		
Columbia, SC	x				EZ-2		Orange, TX			x			
Columbus, OH	x				EZ-2		Peoria, IL	x		x			
Corpus Christi, TX	x					RC	Philadelphia, PA	x	x	x			RC
Cumberland, NJ					EZ-2		Phoenix, AZ	x		x	EC-1		
Dallas, TX	x		x	EC-1			Pine Bluff, AR			x			
Denver, CO	x		x	EC-1			Pittsburgh, PA	x		x	EC-1		
Des Moines, IA (Polk)			x	EC-1			Port Arthur, TX			x			
Detroit, MI	x	x	x			RC	Portland, OR	x		x	EC-1		
East Chicago, IN	x		x		EZ-2		Portsmouth, VA	x		x	EC-1	EZ-2	
East St Louis, IL	x		x	EC-1	EZ-2		Providence, RI	x		x	EC-1		
El Paso, TX	x		x	EC-1	EZ-2		Richmond, VA	x		x			
Evans, CO			x			RC	Rochester, NY	x		x			RC
Fairbanks, AK			x				Sacramento, CA			x			
Flint, MI	x		x			RC	San Antonio, TX	x		x	EC-1		EZ-3
Fort Lauderdale, FL			x				San Diego, CA	x		x			RC
Fort Worth, TX			x				San Francisco, CA						RC
Fresno, CA	x		x			EZ-3	Santa Ana, CA					EZ-2	
Gary, IN	x		x		EZ-2		Savannah, GA	x		x			
Greeley, CO			x			RC	Schenectady, NY						RC
Hamilton, OH						RC	Shreveport, LA			x			
Hammond, IN	x		x		EZ-2		Sioux City, IA			x			
Harrisburg, PA (Dauphin)			x	EC-1			Springfield, MA (Hampden)	x		x	EC-1		
Hartford, CT	x						St. Louis, MO	x		x	EC-1	EZ-2	
Houston, TX	x		x	EEC-1			St. Paul, MN (Ramsey)	x		x	EC-1		
Huntington, WV					EZ-2		Steubenville, OH			x			
Indianapolis, IN (Marion)	x		x	EC-1			Sumter, SC	x				EZ-2	
Ironton, OH					EZ-2		Syracuse, NY	x					EZ-3
Jackson, MI (Hinds)	x		x	EC-1			Tacoma, WA			x			RC
Jacksonville, FL	x		x			EZ-3	Tampa, FL	x		x	EC-1		
Kansas city, KS	x		x	EEC-1			Tucson, AZ	x		x			EZ-3
Kansas city, MO	x		x	EEC-1			Waco, TX			x	EC-1		
Knoxville, TN	x		x		EZ-2		Washington, DC			x	EC-1		EnZ
Lake Charles, LA			x				Whitehall, AR			x			
Las Vegas, NV (Clark)			x	EC-1			Wilmington, DE (New Castle)			x	EC-1		
Lawrence, MA						RC	Yakima, WA						RC
Little Rock, AR (Pulaski)	x		x	EC-1		EZ-3	Yonkers, NY						EZ-3
Los Angeles, CA	x		x	SEZ-1		RC	Youngstown, OH			x			

Note: Sample refers to the estimation sample. EZ-1 refers to cities in the treated group (Empowerment Zones in Round I in 1994). Application refers to cities that applied to get an EZ-1. SEZ-1 refers to cities that received a Supplemental Empowerment Zone (Round I, 1996). EC-1 refers to Enterprise Community awarded in Round I (1994), EEC-1 refers to Enhanced Enterprise Community awarded in Round I (1994), EZ-2 refers to Empowerment Zone awarded in Round II (2000), RC refers to Renewal Community awarded in Round III (2002), EZ-3 refers to Empowerment Zone awarded in Round III (2002) and EnZ refers to the Enterprise Zone awarded in Round III (2002)

TABLE 2.A2: SECOND MOMENTS IN 1990 TREATMENT AND CONTROLS

	EZ's	Rejected/ Future Zones	Rejected/ Future Zones Reweighted	p-value of difference between [1] and [2]	p-value of difference between [1] and [3]
	[1]	[2]	[3]	[4]	[5]
<i>Census tracts characteristics</i>					
Economic Index (Residents)	0.996	1.014	0.632	0.985	0.507
Employment Rate	0.146	0.203	0.147	0.000	0.873
Unemployment Rate	0.071	0.039	0.059	0.139	0.498
Poverty Rate	0.251	0.197	0.243	0.110	0.742
Economic Index (Workers -JTW)	0.996	0.750	0.733	0.258	0.158
Log (Jobs) -JTW	44.715	50.049	45.626	0.103	0.564
Log (Hourly Wage of Zone Workers) - JTW	35.673	34.814	35.214	0.013	0.104
Log (Hourly Wage of Zone Residents)	30.926	29.815	29.678	0.127	0.037
Economic Index (Workers -LBD)†	0.996	1.112	0.951	0.282	0.671
Log (Jobs) -LBD†	35.718	42.529	37.940	0.157	0.364
Log (Establishments) - LBD†	10.584	13.841	11.075	0.002	0.471
Log (Average Earnings per Worker) -LBD†	8.965	8.844	8.870	0.613	0.775
Demographic Index	0.996	1.596	0.967	0.008	0.913
% Households Female-headed	0.340	0.281	0.345	0.004	0.747
% College	0.008	0.012	0.009	0.187	0.611
% High school dropouts	0.107	0.081	0.105	0.003	0.762
% Black	0.653	0.500	0.689	0.068	0.605
% Hispanic	0.114	0.096	0.113	0.683	0.933
Population Index	0.996	0.879	1.175	0.869	0.547
Log (Population)	60.797	62.539	61.571	0.416	0.591
Log (Households)	48.288	49.229	48.244	0.627	0.969
% Same House as Five Yrs Ago	0.339	0.276	0.348	0.059	0.682
Housing Index	0.995	0.993	0.791	0.994	0.496
Log (Rent)	28.759	28.983	28.146	0.819	0.573
Log (Housing Value)	110.667	112.040	106.581	0.787	0.458
% Houses that are Vacant	0.039	0.027	0.028	0.273	0.286
<i>City characteristics</i>					
Total crime / population x 100	0.011	0.012	0.011	0.737	1.000
Avg. across tracts % black	0.258	0.154	0.258	0.176	1.000
% Workers in Manufacturing	0.026	0.027	0.026	0.861	1.000
% Workers in City Government	0.005	0.003	0.005	0.487	1.000
Log (City Population)	212.022	171.201	188.673	0.000	0.000
Observations (number of census tracts)	234	1429	1429	1663	1663

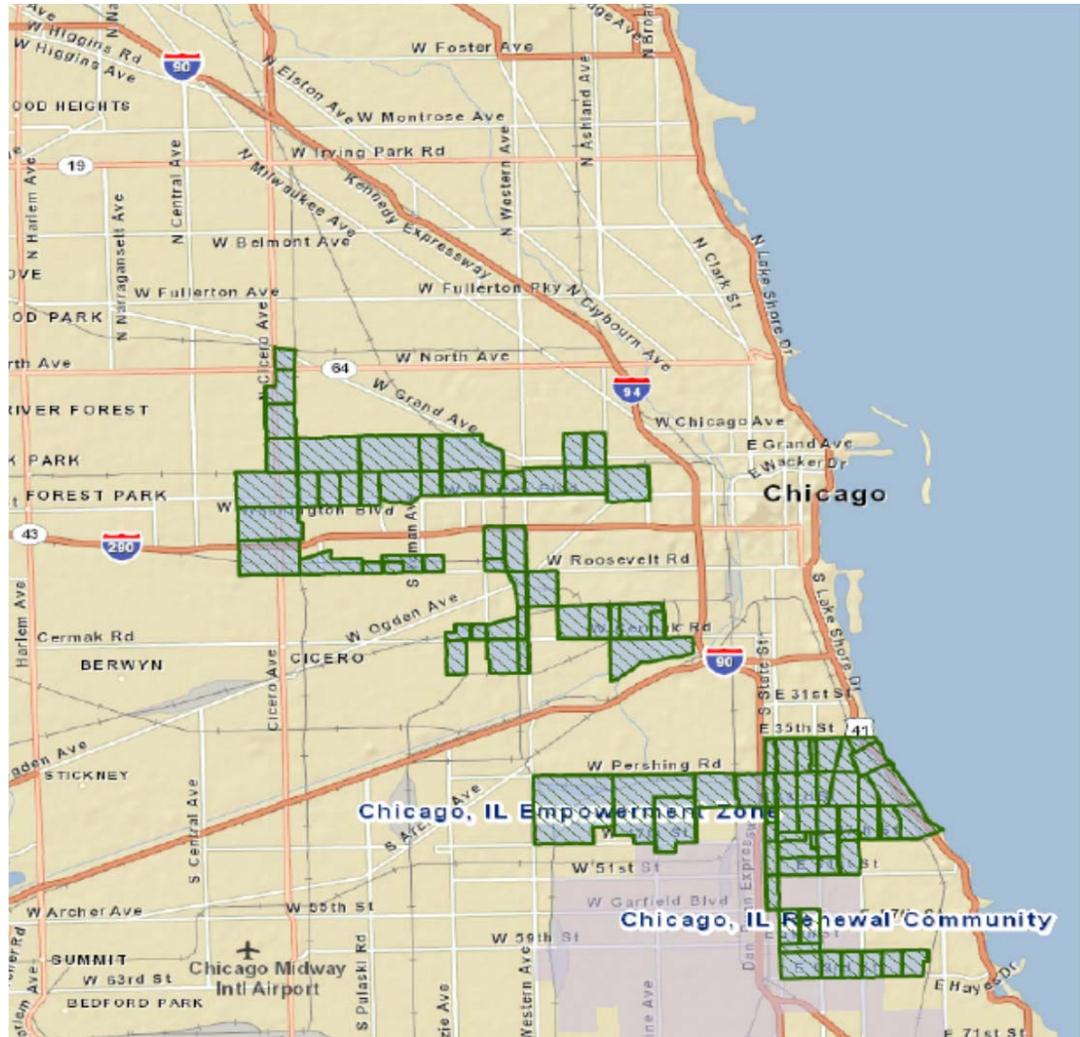
Note. Sources: Variables marked as JTW are based on the Journey-to-Work component of the Decennial Census. Variables marked as LBD come from the Longitudinal Business Database. All other tract level covariates come from the Census. City covariates are from the County/City Databook. Columns: Column [1] reports the uncentered second moment for census tracts inside EZs. Column [2] shows the uncentered second moment for control tracts in rejected or future treated areas (listed in Table A1). Column [3] reports the uncentered second moment for control tracts after parametric reweighting (see Section III for details.) Column [4] presents wild bootstrap p-values for a test of the null hypothesis that the uncentered second moment in column [1] equals the uncentered second moment in column [2]. Similarly, column [5] reports p-values for the equality of uncentered second moments in columns [1] and [3]. † For LBD variables columns [1], [2] and [3] show the uncentered second moment in 1987.

TABLE 2.A3: ROBUSTNESS CHECKS (SAMPLES)

Sample Model	Rejected				No New York				with SEZs			
	Naïve [1]	OLS [2]	PW [3]	Obs. [4]	Naïve [5]	OLS [6]	PW [7]	Obs. [8]	Naïve [9]	OLS [10]	PW [11]	Obs. [12]
Log (Jobs) -LBD	0.087 [0.055]	0.198 [0.068]**	0.219 [0.083]***	1100	0.074 [0.033]	0.142 [0.041]**	0.156 [0.061]**	1602	0.132 [0.039]***	0.198 [0.049]***	0.218 [0.062]***	1718
Log (Establishments)	0.006 [0.031]	0.048 [0.037]	0.060 [0.043]	1100	0.004 [0.029]	0.034 [0.015]*	0.048 [0.035]	1602	0.055 [0.025]	0.069 [0.020]***	0.075 [0.034]***	1718
Log (Average Earnings per Worker)	-0.030 [0.013]*	0.027 [0.033]	0.056 [0.036]	1100	-0.026 [0.013]*	-0.002 [0.019]	0.004 [0.016]	1602	-0.033 [0.015]**	0.000 [0.016]	0.003 [0.020]	1718
Ln(Jobs) -JTW	0.204 [0.071]*	0.238 [0.084]**	0.223 [0.109]**	1107	0.187 [0.080]	0.159 [0.068]*	0.164 [0.096]**	1605	0.187 [0.050]**	0.150 [0.051]**	0.131 [0.069]**	1724
Log (Zone Jobs Held by Zone Residents)	0.165 [0.098]	0.232 [0.098]*	0.327 [0.123]**	1105	0.115 [0.100]	0.116 [0.073]	0.142 [0.115]	1603	0.131 [0.074]	0.150 [0.057]**	0.180 [0.089]**	1720
Log (Zone Jobs Held by Non-Residents)	0.182 [0.058]*	0.189 [0.084]*	0.162 [0.100]*	1106	0.154 [0.062]	0.106 [0.066]	0.100 [0.081]	1605	0.174 [0.042]**	0.116 [0.054]*	0.085 [0.061]	1724
Log (Non-Zone Jobs Held by Zone Residents)	-0.001 [0.058]	0.164 [0.071]	0.239 [0.081]**	1104	0.001 [0.072]	0.063 [0.070]	0.087 [0.069]	1605	0.045 [0.049]	0.093 [0.052]	0.134 [0.050]*	1722
Log (Weekly Wage Income of Zone Residents) [†]	0.008 [0.031]	0.046 [0.026]	0.035 [0.039]	1104	0.052 [0.022]	0.063 [0.013]**	0.068 [0.023]***	1604	0.011 [0.028]	0.052 [0.014]**	0.051 [0.029]*	1721
Log (Weekly Wage Income of Zone Workers) [†]	-0.002 [0.025]	0.062 [0.017]***	0.043 [0.030]**	1105	0.017 [0.023]	0.019 [0.027]	0.015 [0.026]	1601	-0.010 [0.020]	0.020 [0.022]	0.017 [0.026]	1720
Log (Weekly Wage Income of Zone Residents Working in Zone) [†]	0.082 [0.046]	0.173 [0.068]**	0.141 [0.070]*	1099	0.092 [0.056]	0.129 [0.055]**	0.127 [0.059]*	1597	0.072 [0.039]*	0.128 [0.043]***	0.122 [0.046]**	1714
Log (Weekly Wage Income of Non-Residents Working in Zone) [†]	0.001 [0.025]	0.049 [0.018]**	0.047 [0.033]*	1100	0.019 [0.023]	0.005 [0.027]	0.006 [0.029]	1593	-0.011 [0.021]	0.005 [0.025]	0.010 [0.028]	1711
Log (Weekly Wage Income of Zone Residents Working Outside Zone) [†]	-0.018 [0.026]	0.008 [0.028]	0.018 [0.038]	1093	0.017 [0.027]	0.028 [0.025]	0.031 [0.029]	1592	-0.004 [0.023]	0.049 [0.022]*	0.058 [0.025]**	1707
Log (Rent) [†]	-0.011 [0.029]	0.004 [0.037]	0.043 [0.038]	1104	0.014 [0.033]	-0.006 [0.028]	0.002 [0.032]	1604	-0.003 [0.037]	0.022 [0.026]	0.032 [0.041]	1721
Log (Housing Value) [†]	0.390 [0.137]*	0.346 [0.083]***	0.383 [0.148]**	1050	0.438 [0.120]	0.285 [0.073]*	0.325 [0.131]*	1546	0.324 [0.110]**	0.301 [0.052]***	0.335 [0.126]**	1648
Log (Households)	0.017 [0.072]	0.057 [0.035]	0.089 [0.080]	1104	-0.076 [0.044]	-0.039 [0.033]	-0.046 [0.047]	1604	0.023 [0.061]	0.016 [0.032]	0.035 [0.063]	1721
Ln(Population)	-0.019 [0.058]	0.067 [0.054]	0.135 [0.070]*	1105	-0.074 [0.024]**	-0.010 [0.030]	-0.007 [0.029]	1607	0.009 [0.049]	0.040 [0.030]	0.069 [0.053]	1724
% Same House as Five Yrs Ago	-0.001 [0.008]	0.020 [0.013]	0.020 [0.013]	1105	-0.001 [0.010]	0.005 [0.014]	0.002 [0.011]	1607	-0.002 [0.009]	-0.002 [0.010]	-0.007 [0.011]	1724
% Vacant Houses	0.033 [0.014]**	0.002 [0.014]	-0.012 [0.017]	1104	0.012 [0.015]	-0.006 [0.010]	-0.008 [0.015]	1604	0.022 [0.012]*	-0.001 [0.009]	-0.004 [0.012]	1721
Log(Hourly Wage) - 0.25 Log(Rent) [†]	0.083 [0.046]	0.171 [0.068]**	0.128 [0.071]	1095	0.085 [0.058]	0.126 [0.058]**	0.122 [0.060]*	1590	0.070 [0.038]*	0.119 [0.046]***	0.111 [0.045]**	1707

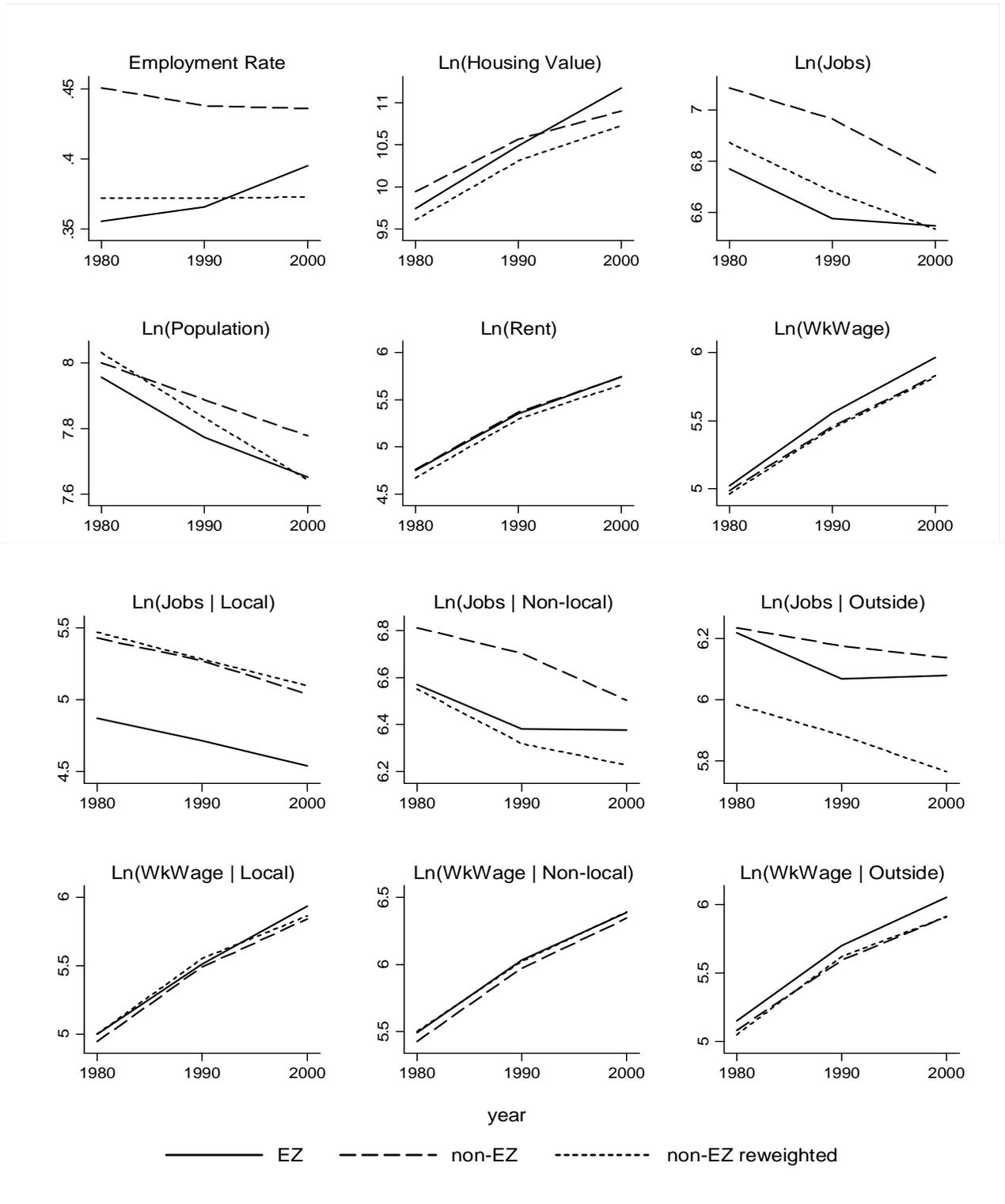
Note. Sources: Variables marked as JTW are based on the Journey-to-Work component of the Decennial Census. Variables marked as LBD come from the Longitudinal Business Database. All other tract level covariates come from the Census. "Adjusted" outcomes controls for demographic changes at the micro-level (see Section IV). Timing: Variables labeled as LBD are analyzed over the period 1992-2000, all other outcomes are analyzed over the period 1990-2000. Samples: Columns [1]-[3] give differences-in-differences (DD) estimates on a sample that includes as controls only the original tracts that were rejected by HUD in the application process. Columns [5]-[7] show DD estimates on a sample that includes the baseline sample but discards New York's census tracts. Columns [9]-[11] show DD estimates on a sample that includes the baseline sample and the two Supplemental EZs as treated (Los Angeles and Cleveland). Columns [4], [8] and [12] show the number of observations used in the estimation for each outcome in each sample. Estimators: Columns labeled "Naïve" report a DD estimate without controls. Columns labeled "OLS" report the OLS DD estimate controlling for lagged city and tract level characteristics. Columns labeled "PW" report parametric reweighting DD estimates. Asymptotic standard errors are shown in square brackets and are clustered by city. Stars reflect significance level obtained by a clustered wild bootstrap procedure described in Appendix I. Legend: * significant at 10% level; ** significant at 5% level; *** significant at 1% level. † denotes outcomes that have been adjusted for demographic or, in the case of rents and housing values, quality changes at the micro-level (see Section IV)

FIGURE 2.A1: CHICAGO EMPOWERMENT ZONE



Source: U.S. Department of Housing and Urban Development

FIGURE 2.A2: MEANS BY YEAR AND TREATMENT STATUS



Note: Table 3 presents wild bootstrap p-values for a test of the null hypothesis that the levels and trends are the same for the first six variables. For the last six variables, the wild bootstrapped p-values of the reweighted difference between 1980 and 1990 are as follows: Log (Jobs | Local): 0.508 ; Log (Jobs | Non-local): 0.817 ; Log (Jobs | Outside): 0.783 ; Log (Wage | Local): 0.650 ; Log (Wage | Non-local): 0.622 ; Log (Wage | Outside): 0.650.

Appendix I: Methods

A. Computation of PW Estimator

We run a pooled tract-level regression of the form

$$\Delta Y_{tzc} = \mu^1 T_z + (1 - T_z) \times X'_{n(t)} \alpha^x + (1 - T_z) \times P'_c \alpha^p + e_{tzc}$$

where $X_{n(t)}$ is assumed to include a constant. Note that because this regression is fully interacted, $\hat{\mu}^1$ will evaluate to the mean of ΔY_{tzc} among the EZ tracts. Let $Z_t = [X_{n(t)}, P_c]$ and $\hat{\alpha} = [\hat{\alpha}^x, \hat{\alpha}^p]'$. The counterfactual mean estimate for treated observations may be computed as

$$\begin{aligned} \hat{\mu}^0 &= \frac{1}{N_1} \sum_t T_t Z'_t \hat{\alpha} \\ &= \frac{1}{N_1} \sum_t T_t Z'_t \left[\left(\sum_l (1 - T_l) Z_l Z'_l \right)^{-1} \sum_m (1 - T_m) Z_m \Delta Y_{mzc} \right] \\ &= \sum_m (1 - T_m) \omega_m \Delta Y_{mzc} \end{aligned}$$

where the $\omega_m = \frac{1}{N_1} \sum_t T_t Z'_t \left(\sum_l (1 - T_l) Z_l Z'_l \right)^{-1} Z_m$ are weights obeying $\sum_m (1 - T_m) \omega_m = 1$. It is straightforward to verify that for any covariate $Q_t \in Z_t$, $\sum_t (1 - T_t) \omega_t Q_t = \frac{1}{N_1} \sum_t T_t Q_t$. Hence the regression weights yield reweighted covariate means among the controls numerically equivalent to the corresponding covariate means in the treatment group. See Kline (2011) for the interpretation of this procedure as a propensity score reweighting estimator. We use these weights in computing the reweighted control means reported in Figures 1 and Table 3. Tract level covariate means are not perfectly balanced in Table 3 because we condition on distance weighted averages of covariates rather than tract level variables themselves.

The treatment effect estimator in (38) may be written $\widehat{ATT} = \hat{\mu}^1 - \hat{\mu}^0$, which is the quantity reported in our PW impact estimates. An analytical variance estimate may be computed as

$$\widehat{Var}(\widehat{ATT}) = \widehat{V}_1 + \left(\frac{1}{N_1} \sum_t T_t Z'_t \right) \widehat{V}_0 \left(\frac{1}{N_1} \sum_t T_t Z'_t \right)'$$

where \widehat{V}_0 is the standard OLS cluster robust estimator of the covariance matrix of the estimated parameters $(\hat{\alpha}^x, \hat{\alpha}^p)$ and \widehat{V}_1 is the corresponding variance estimate of $\hat{\mu}^1$. We use this analytical variance estimate to construct an asymptotic pivot for use in our wild bootstrap procedure.

B. Wild Bootstrap Inference

As suggested by Cameron, Gelbach, and Miller (2008) we conduct inference using a cluster robust percentile-t wild bootstrap procedure with Rademacher weights. We impose the null hypothesis that the coefficient on the EZ dummy is zero when computing our residuals for resampling. Bootstrap p-values are computed by assessing the fraction of bootstrap test statistics greater in absolute value than the sample test statistic. All bootstrap tests use 1,999 repetitions.

The confidence intervals in section VII are constructed via test inversion. That is, we conduct a grid search over null hypothetical values for the treatment effect in question, compute the corresponding restricted residuals, and the Wild bootstrapped p-value. Our 90% confidence intervals correspond to the set of points with estimated p-values above 0.1.

Appendix II: Data

A. Missingness/Weighting

We exclude observations with missing and allocated values when constructing several of the tract-level variables included in the analysis. In most of these cases, we correct for the potential introduction of non-random selection by weighting nonmissing observations by the inverse of an estimate of the probability of the observation's inclusion.

A first set of missingness weights (applied to Decennial Census data) equals the inverse of the probability of an individual having a valid (non-missing and non-allocated) place of work variable conditional on observable traits and on the individual being employed. We estimate that conditional probability with a linear probability model that includes main effects and all two-way interactions of age (under 20, 20-39, 40-64, and 65+), sex, race (black, white, and other), and education (dropout, high school grad, some college, and bachelors) and includes main effects for class of worker, wage decile (where missing wages are treated as an eleventh decile), and tract of residence. The model is estimated separately by county, year, and EZ assignment status according to tract of residence. Predicted values were winsorized to lie in the interval $[0.025, 1]$. These weights are applied when computing tract aggregates of quantities defined by individuals' places of work. Those aggregates include numbers of jobs and total earnings for tract workers residing in the zone, for tract workers residing outside of the zone, and for tract residents working outside of the zone.

A second set of missingness weights (applied to Decennial Census data) equals the inverse of the probability that an individual has a valid (non-missing and non-allocated) place of work variable conditional on observable traits and on the individual being employed and having a non-allocated wage. We again estimate that conditional probability with a linear probability model that includes main effects and all two-way interactions of age, sex, race, and education and includes main effects for class of worker, wage decile, and tract of residence. The model is estimated separately by county, year, and EZ assignment status in the tract of residence. Predicted values are again winsorized to lie in the interval $[0.025, 1]$. These weights are applied when computing mean wages by individuals' places of work. These variables include mean log wages of tract workers residing in the zone, mean log wages of tract workers residing outside of the zone, and mean log wages of tract residents employed outside of the zone.

A third set of weights (applied to LBD data) equals the inverse of the probability that an establishment received a valid geocode during our geocoding algorithm conditional on observable establishment traits. Because the set of potential covariates was much smaller in this case the probabilities were estimated using parametric logit models. The explanatory variables in these models were dummies for establishment age (full vector of indicators for each possible age), establishment size (defined by total employment categories; 0-99, 100-249, 250-499, 500-999, and 1000+), and 1-digit industry categories. Separate missingness models were estimated for single establishment firms and establishments belonging to multi-establishment firms within each county-year combination. These weights were applied in construction of all LBD based variables.

For some tract-years, we did not observe any tract workers in particular place of residence / place of work cells. For example some tracts lack any workers who reside in the zone containing the tract (local workers). To deal with this problem we replaced the change in the log of the number of local workers with the gross change divided by the average number of local workers in the two periods as suggested by Davis, Haltiwanger, and Schuh (1993). This measure varies between -2 and 2 and is well defined for tracts that have at least one local worker in either 1990 or 2000. For most tracts this measure yields values very close to the change in logs. Equivalent replacements were made for the change in the log of the number of nonlocal workers and the change in the log of the number of tract residents working outside the zone. Again, the approximation to the log change is very good in cases where the log change is well defined.

For tracts with no local workers sampled we stochastically impute the mean log wage of such workers. We first regress the mean log wage of local workers on a large set of contemporaneous tract level covariates and averages of those covariates over the three decades in tracts for which the mean log wage of local workers is well defined.⁸² A separate regression is run for each Decennial Census year by EZ treatment status. We then impute a mean log wage/earnings of local workers for tracts missing that variable by assigning the sum of the linear prediction from this regression and a draw from a normal distribution with mean zero and standard deviation equal to the root mean squared error from the regression. Similar point estimates (and inferences) are obtained from the imputed and non-imputed variables.

B. Geocoding Algorithm

Our analysis of business data from the SSEL and LBD required that each establishment be coded to a 1990 census tract. While a census tract variable appears on the SSEL files for 1992 and later, the values are very often missing. Instead of using the existing tract variable, we implemented an algorithm to assign establishments to census tracts based on their raw street addresses. Our algorithm consisted of three steps. First we attempted to code each address in each cross-section of the SSEL to a 2000 Census block⁸³. For this step, we used the SAS/GIS batch geocoding module (invoked by the “%GCBATCH” macro). Second, using the longitudinal links provided by the LBD, we filled in establishment-years with missing geocodes with the codes assigned to the same establishment in neighboring years. Third, we assigned each establishment a 1990 census tract based on its assigned 2000 Census block.

⁸²The covariates included in this regression are: log wage of tract residents, log wage of tract workers, average over three decades of log wage of tract residents, average over three decades of log wage of tract workers, kernel weighted average across neighboring tracts of log wage of zone residents working in tract, kernel weighted average across neighboring tracts of log wage of non-zone commuters working in tract, kernel weighted average across neighboring tracts of log wage of tract residents working outside zone, averages across three decades of the three kernel weighted average variables, fraction of tract residents with a commute less than 25 minutes, fraction of tract residents who are black, fraction of tract residents who are Hispanic, fraction of tract residents who are high school dropouts, fraction of tract residents with college attendance, fraction of tract residents greater than 65 years old, fraction of tract residents less than 18 years old, fraction of tract residents who are employed, fraction of tract residents below the poverty line, log of tract population, log of tract area, log of the number of households living in the tract, an indicator for whether the tract was in the central business district in 1990, the distance to the central business district, and a vector of state-city fixed effects.

⁸³We tested our geocoding algorithm using both 1990 TIGER/Line data and 2000 TIGER/Line data. An advantage of using the 1990 TIGER/Line files is that all coded establishments receive a 1990 Census block code, a unit within which treatment status does not vary (EZs were awarded to collections of 1990 census tracts, which nest 1990 census blocks). We found however that the rate at which we successfully assigned geocodes was higher by several percentage points using 2000 TIGER/Line files than when using 1990 TIGER/Line files. While the mapping from 2000 census blocks to 1990 census tracts is not one-to-one, less than 0.5 percent of 2000 Census blocks overlap multiple 1990 census blocks in the counties containing an EZ or control zone. We decided that the benefit of the higher successful geocoding rate outweighed the cost of slight mis-measurement of treatment assignment

The SSEL provides at least one street address field for each establishment in each annual cross-section. For single establishment firms, a mailing address is nearly always provided, and a physical address is sometimes provided. SSEL documentation suggests that the physical address field should be non-missing in each case in which a single establishment firm's physical address and mailing address differ. For establishments belonging to multi-establishment firms, only a physical address is provided.

As the first step of our geocoding process, we applied the following algorithm to all SSEL physical and mailing addresses of establishments located in counties containing an EZ or a control zone. Note that for single establishment firms, we attempted to code two addresses when two addresses were provided.

1. Import 2000 TIGER/Line data into SAS/GIS spatial data sets.
2. Geocode SSEL address data using the SAS/GIS batch geocoder.
3. Set aside all observations that received a geocode in step 2. Proceed using only observations that have not yet received a geocode.
4. If all items on the following list have been reached, go to step 6. Otherwise, proceed and perform the first task on the following list that has not yet been performed.
 - (a) Remove all punctuation marks.
 - (b) Replace ordinal words with their numeric equivalents (e.g. third becomes 3rd).
 - (c) Remove gaps between two groups of numbers appearing at the beginning of address strings (e.g. "123 45 Elm St" becomes "12345 Elm St").
 - (d) Remove official U.S. Postal Service secondary address identifiers and all characters that follow them (e.g. "123 Elm St Suite 1" becomes "123 Elm St").
 - (e) Abbreviate all official US Postal Service primary address identifiers with their official abbreviations (e.g. "123 Elm Street" becomes "123 Elm St").
 - (f) Remove spaces between adjacent letters commonly used to identify cardinal directions (e.g. "123 S W Elm St" becomes "123 SW Elm St").
5. Return to step 2.
6. Stop.

In cases in which a physical address was successfully geocoded, we assigned the establishment the geocode associated with that address. In cases in which we were unable to assign a geocode to a physical address (usually because none was provided), we assigned the establishment the geocode associated with its mailing address.

In the second step of our geocoding process, we exploited the longitudinal links provided by the LBD to impute missing geocodes for establishments that were successfully coded in some, but not all, of the years in which they appeared in the SSEL. If an establishment's first observation to receive a successful geocode occurred in year t , we assigned the year t geocode to any observations for years prior to t . Similarly, if an establishment's last observation to receive a successful geocode occurred in year t , we assigned the year t

geocode to any observations for years later than t . When an observation on the “interior” of an establishment’s panel failed to receive a geocode, the observation was assigned the geocode of the nearest successfully geocoded observation. When an interior observation of this sort was equally close to two successfully geocoded observations, we chose between the geocodes of those two observations randomly, giving each a 0.5 probability of being selected.

In the final step of our geocoding process, we assigned each successfully coded establishment-year a 1990 Census tract based on the 2000 Census block assigned in the first two steps. To do this, we constructed a many to many crosswalk file relating 2000 Census blocks to 1990 Census tracts. We began by downloading the Census provided Census Block Relationship File relating 1990 Census tabulation blocks to 2000 Census Tabulation blocks. The Census Block Relationship File has one observation for each 1990 Census tabulation block and 2000 Census tabulation block pair with a non-empty intersection. We created a 1990 Census tract variable from the provided 1990 Census block variable and dropped any duplicate observations of 1990 Census tract and 2000 Census block. We then merged this file by 2000 Census block to the list of geocoded addresses. In cases in which a 2000 Census block mapped to N 1990 Census tracts, we duplicated the firm’s observation N times, assigned one observation to each potential 1990 Census tract, and assigned weight $1/N$ to each of those observations in any subsequent analysis.

Appendix III: Construction of Placebo Zones

To construct placebo zones we performed nearest neighbor matching without replacement on a propensity score estimated on all tracts in the six cities receiving Round I EZs. To ensure a broad enough donor pool of placebo tracts, we define city broadly to include other municipalities in the same counties as the city itself. The propensity score was estimated on the pooled sample using a logit of assignment status on a large number of 1980 and 1990 covariates. The 1990 covariates include a vector of city indicators interacted with the fraction of households below the poverty line, a vector of city indicators interacted with the fraction unemployed, a vector of city indicators interacted with the log of tract population, a vector of city indicators interacted with the log of the number of jobs in the tract, the fraction black, the fraction Hispanic, the fraction who were high school dropouts, the fraction older than 65 years in age, the fraction less than 18 years old, the fraction of structures that were vacant, the fraction of households headed by a female, a tract building age index, and dummy indicators for tract poverty share below 25 percent and for tract poverty share below 35 percent. A similar list of 1980 covariates was used including the fraction black, the fraction Hispanic, the fraction who were high school dropouts, the fraction older than 65 years old, the fraction less than 18 years old, the fraction of structures that are vacant, and the fraction of households headed by a female.

Finally, we included 1980 to 1990 changes in the following variables: the fraction of employed workers with commute times less than 25 minutes, the log of the number of tract households, the mean log of tract rent, the mean log of tract housing value, the log of tract population, the log of tract jobs, the percent of tract workers with a college degree, the mean log wages of tract workers, and the mean log wages of tract residents. We also included 1987 to 1992 changes in the log of average tract wages (LBD) and the log of tract employment (LBD).

Supplemental Appendix

A. Model Extension with Two Types of Workers

Let a fixed proportion π_S of the agents be skilled and more productive than their unskilled counterparts who constitute the remaining $\pi_U = 1 - \pi_S$ of the population. Write the utility of individual i of skill group $g \in \{S, U\}$ living in community $j \in \mathcal{N}$ and working in community $k \in \{\emptyset, \mathcal{N}\}$ and sector $s \in \{1, 2\}$ as:

$$\begin{aligned} u_{ijk}^g &= w_{jks}^g - r_j - \kappa_{jk} + A_j + \varepsilon_{ijk}^g \\ &= v_{jks}^g + \varepsilon_{ijk}^g \end{aligned}$$

where w_{jks}^g is the wage a worker of skill group g from neighborhood j receives when working in sector s of neighborhood k . Define a set of indicator variables $\{D_{ijk}^g\}$ equal to one if and only if $\max_{j'k's'} \{u_{ij'k's'}^g\} = u_{ijk}^g$ for worker i and denote the measure of agents of skill group g in each residential/work location by $N_{jks}^g = P\left(D_{ijk}^g = 1 \mid \left\{v_{j'k's'}^g\right\}\right)$.

Suppose that skilled and unskilled workers are perfect substitutes in production so that firm output may be written $B_k(qS_{ks} + U_{ks})f(\chi_{ks})$ where the S_{ks} and U_{ks} refer to total skilled and unskilled labor inputs respectively, $\chi_{ks} = \frac{K_{ks}}{B_k(qS_{ks} + U_{ks})}$ is the capital to effective labor ratio, and q is the relative efficiency of skilled labor. Now wages will obey

$$\begin{aligned} B_k [f(\chi_{ks}) - \chi_{ks}f'(\chi_{ks})] &= w_{jks}^U (1 - \tau\delta_{jks}) \\ w_{jks}^S &= qw_{jks}^U \\ f'(\chi_{ks}) &= \rho \end{aligned}$$

where w_{jks}^U is the wage for unskilled workers and w_{jks}^S the wage for skilled workers. Note that

$$\frac{d \ln w_{jks}^U}{d \ln B_k} = \frac{d \ln w_{jks}^S}{d \ln B_k} = 1$$

so that productivity increases may still be detected by examining impacts on the wages of commuters. However, productivity effects may also shift the skill composition of local workers and commuters which could lead us to over or understate these effects. For this reason we adjust our wage impacts in the paper for observable skill characteristics.

Our final modification is that with two skill groups, clearing in the housing market requires:

$$H_j = \sum_g \pi_g \sum_k \sum_s N_{jks}^g$$

With these features in place the social welfare function may be written:

$$W = \sum_g \pi_g V^g + \sum_j \left[r_j H_j - \int_0^{H_j} G_j^{-1}(x) dx \right]$$

It is straightforward then to verify that for some community m :

$$\begin{aligned}\frac{d}{dB_m}W\Big|_{\tau=0} &= \sum_g \pi_g \sum_j \sum_k \sum_s N_{jks}^g \frac{dw_{jks}^g}{dB_m} \\ &= [\pi_U N_{.m}^U + q\pi_S N_{.m}^S] R(\rho) \\ \frac{d}{dA_m}W\Big|_{\tau=0} &= N_m.\end{aligned}$$

where $N_{.m}^g = \sum_j \sum_s N_{jms}^g$ and $N_m. = \sum_g \pi_g \sum_k \sum_s N_{mks}^g$. Furthermore we may write the deadweight losses attributable to taxes as:

$$\begin{aligned}DWL_\tau &= \sum_g \pi_g \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1}^g w_{jk1}^g \int_0^{\tau} t \frac{d \ln N_{jk1}^g}{dt} dt \\ &\approx \frac{1}{2} \psi d\tau^2 \sum_g \pi_g \sum_{j \in \mathcal{N}_1} \sum_{k \in \mathcal{N}_1} N_{jk1}^g w_{jk1}^g\end{aligned}$$

where in the second line we have assumed a constant semi-elasticity of local employment $\psi = \frac{d \ln N_{jk1}^g}{d\tau}$. This formula is effectively the same as that in (34), relying on the total covered wage bill and the elasticity ψ . Were the elasticity to vary by type we would simply need to compute the deadweight loss separately within skill group and average across groups using the marginal frequencies π_g . Finally, we may write the deadweight attributable to the block grants as:

$$\begin{aligned}DWL_G &\approx C \left[1 - \lambda_a \sum_{j \in \mathcal{N}_1} \frac{dW}{d \ln A_j} \Big|_{\tau=0} - \lambda_b \sum_{k \in \mathcal{N}_1} \frac{dW}{d \ln B_k} \Big|_{\tau=0} \right] \\ &= C \left[1 - \lambda_a \sum_{j \in \mathcal{N}_1} A_j N_j. - \lambda_b \sum_g \pi_g \sum_j \sum_{k \in \mathcal{N}_1} \sum_s N_{jks}^g w_{jks}^g \right]\end{aligned}$$

As before, the deadweight loss computation relies on the parameters λ_a and λ_b . Heterogeneity provides no essential complication to the exercise since, with knowledge of these parameters, one only needs to know the total wage bill and population inside of the zone to compute DWL_G .

B. Monte Carlo Experiments

We simulated hierarchical datasets of 64 zones with a random number of tracts N_z within each zone. The number of tracts per zone was generated according to $N_z = 10 + \tilde{\eta}_z$ where $\tilde{\eta}_z$ is a Negative Binomial distributed random variable with the first two moments matching the ones observed in the data (i.e. a mean 21 and a standard deviation of 16 tracts). Hence, each simulated sample was expected to yield approximately 1,344 census tracts with no zone containing less than 10 tracts in any draw.

Outcomes were generated according to the model:

$$Y_{tz} = \beta_z T_z + \alpha_t^x X_{tz} + \alpha_z^p P_z + \xi_z + e_{tz}$$

where T_z is an EZ assignment dummy, X_{tz} is a tract level regressor, P_z is a zone level regressor, ξ_z a random

zone effect, and e_{tz} an idiosyncratic tract level error. We assume throughout that:

$$\begin{bmatrix} X_{tz} \\ P_z \\ e_{tz} \end{bmatrix} \sim N(0, I_3)$$

To build in some correlation between the covariates and EZ designation, and to reflect the fact that treated zones tend to be larger, we model the EZ assignment mechanism as:

$$\begin{aligned} T_z &= I(\text{rank}(T_z^*) \geq 6) \\ T_z^* &= \bar{X}_z + P_z + 0.008 \times N_z + u_z \\ u_z &\sim N(0, 1) \end{aligned} \tag{40}$$

where $\bar{X}_z = \frac{1}{N_z} \sum_{t \in z} X_{tz}$ and the $\text{rank}(\cdot)$ function ranks the T_z^* in descending order. Note that this assignment process imposes that exactly six zones will be treated. Hence, each simulation sample will face the inference challenges present in our data.

The nature of the coefficients $(\beta_z, \alpha_t^x, \alpha_z^p)$ and the random effect ξ_z vary across our Monte Carlo designs as described in the following table. We have two sets of results. In the first set, which we label symmetric, ξ_z follows a normal distribution. In a second set of results, which we label asymmetric, ξ_z follows a χ^2 distribution.

Data Generating Processes

	Symmetric	Asymmetric
	$\xi_z \sim N(0, 1)$	$\xi_z \sim \chi^2(4)$
1. Baseline	$\beta_z = 0, \alpha_t^x = \alpha_z^p = 1$	$\beta_z = 0, \alpha_t^x = \alpha_z^p = 1$
2. Random Coefficient on X_{tz}	Same as 1) but, $\alpha_t^x \sim N(1, 1)$	Same as 1) but, $(\alpha_t^x + 3) \sim \chi^2(4)$
3. Random Coefficient on P_z	Same as 1) but, $\alpha_z^p \sim N(1, 1)$	Same as 1) but, $(\alpha_z^p + 3) \sim \chi^2(4)$
4. Random Coefficient on T_z	Same as 1) but, $\beta_z \sim N(0, 1)$	Same as 1) but, $(\beta_z + 4) \sim \chi^2(4)$
5. All deviations from baseline	(2) + (3) + (4)	(2) + (3) + (4)

Note that the null of zero average treatment effect among the treated is satisfied in each simulation design. Specification 1) corresponds to the relatively benign case where our regression model is properly specified and the errors are homoscedastic. Specification 2) allows for heteroscedasticity with respect to the tract level regressor, while specification 3) allows some heteroscedasticity in the zone level regressor. Specification 4) allows heteroscedasticity with respect to the treatment, or alternatively, a heterogeneous but mean zero treatment effect. Specification 5) combines all of these complications so that heteroscedasticity exists with respect to all of the regressors.

For each Monte Carlo design we compute three sets of tests of the true null that EZ designation had no average effect on treated tracts. The first (analytical) uses our analytical cluster-robust standard error to construct a test statistic $\hat{t} = \left| \frac{\hat{\beta}}{\hat{\sigma}} \right|$ where and rejects when $\hat{t} > 1.96$. The second (wild bootstrap-se) uses a clustered wild bootstrap procedure to construct a bootstrap standard error σ^* and rejects when $\left| \frac{\hat{\beta}}{\sigma^*} \right| > 1.96$. The third approach (wild bootstrap-t) estimates the wild bootstrap distribution $F_t^*(\cdot)$ of the test statistic $\hat{t} = \left| \frac{\hat{\beta}}{\hat{\sigma}} \right|$ and rejects when $\hat{t} > F_t^{*-1}(0.95)$ – where $F_t^{*-1}(0.95)$ denotes the 95th percentile of the bootstrap distribution of t statistics. Both the bootstrap-se and bootstrap-t procedures simulate the bootstrap distribution imposing the null that $\beta = 0$ as recommended by Cameron, Gelbach, and Miller (2008). The false rejection rates for these three tests in each of the five simulation designs are given in the table below.

False Rejection Rates in Monte Carlo Simulations

Tract-level models						
	Analytical s.e. OLS	Analytical s.e. PW	Wild BS-s.e. OLS	Wild BS-s.e. PW	Wild BS-t OLS	Wild BS-t PW
Symmetric						
Baseline	0.126	0.074	0.039	0.111	0.054	0.053
Random Coefficient on X_{tz}	0.125	0.075	0.036	0.113	0.056	0.051
Random Coefficient on P_z	0.124	0.077	0.041	0.110	0.055	0.048
Random Coefficient on T_z	0.123	0.073	0.041	0.110	0.055	0.053
All	0.124	0.080	0.042	0.110	0.059	0.051
Asymmetric						
Baseline	0.123	0.106	0.037	0.138	0.055	0.056
Random Coefficient on X_{tz}	0.121	0.109	0.041	0.136	0.047	0.049
Random Coefficient on P_z	0.123	0.111	0.039	0.139	0.054	0.052
Random Coefficient on T_z	0.132	0.111	0.039	0.142	0.053	0.056
All	0.125	0.111	0.038	0.128	0.051	0.051

Standard error based methods tend to overreject in both designs save for in the case of OLS where the wild bootstrapped standard errors perform well. However the wild bootstrapped-t procedure yields extremely accurate inferences for both the OLS and PW estimators across all designs.

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Chapter 3

Do Basketball Scoring Patterns Reflect Widespread Illegal Point Shaving?⁸⁴

Measuring corruption is inherently difficult because law-breakers cover their tracks. For that reason, empirical studies in forensic economics typically develop indirect tests for the presence of corruption. These tests look for behavior that is a rational response to incentives that only those who engage in the particular corrupt behavior face. The validity of these indirect tests depends critically on the assumption that similar patterns do not occur if agents only respond to the incentives generated by the institutions that govern non-corrupt behavior.

Research designs in forensic economics vary to the extent that they are informed by formal economic theory. In a survey of the field, Zitzewitz (2011) proposes a “taxonomy” of forensic economic research designs that ranges from the entirely atheoretical, in which corrupt behavior is measured directly, to the formally theoretical, in which corrupt behavior is inferred from particular violations of price theory or the efficient-market hypothesis. A common intermediate approach is to posit a statistical model of non-corrupt behavior and to measure the extent to which observed behaviors deviate from that model in a manner that is consistent with corrupt incentives. The soundness of a research design of this variety depends on the plausibility of the assumed statistical model and the extent to which the study’s findings are robust to deviations from the assumed statistical model.

In this paper, I adopt a structural modeling approach to reassess the findings of a prominent recent study in which the research design relies upon a non-economic statistical model of behavior under the no-corruption null hypothesis. Wolfers (2006) considers the prevalence of illegal gambling-related point shaving in National Collegiate Athletic Association (NCAA) basketball games. The research design adopted by Wolfers assumes that the distribution of winning margins is symmetric under the null hypothesis of no point shaving. Wolfers demonstrates that point shaving introduces a right skewness in the distribution of winning margins for favored teams. The identifying assumption of symmetry in the absence of point shaving is quite plausible on its face. During a game, basketball teams take alternating turns (or possessions) attempting to score points. The Central Limit Theorem

⁸⁴I am grateful to John Bound, Charlie Brown, Robert Gillezeau, Sam Gregory, Dmitry Lubensky, Brian McCall, Mike McWilliams, Todd Pugatch, Colin Raymond, Lones Smith, and Justin Wolfers for helpful comments. All remaining errors are my own.

suggests that winning margins should be symmetric if each team's points per possession are approximately i.i.d. across possessions. Citing a particular aspect of right skewness in winning margins for games in which one team is strongly favored, Wolfers concludes that point shaving is rampant in those games.

The structural modeling approach adopted in this study provides a more formal benchmark of the patterns that one should expect under the no-point-shaving null hypothesis. I first propose a model of the environment in which teams search for scoring opportunities. I demonstrate that the model's parameters can be estimated from play during the first halves of games. With the estimated model, I simulate play during the second halves of games in which teams optimally adjust their strategies in response to the state of the game (current-score differential and time remaining). I find that the simulated scoring patterns exhibit the same skewness patterns that Wolfers (2006) attributes to point shaving, which suggests that the skewness-based test greatly exaggerates the prevalence of point shaving.

Point shaving occurs when a player exerts less than full effort so that his team will not "cover" the point spread.⁸⁵ The point shaving player and any accomplices profit by placing a bet on the player's opponent. The basis of Wolfers' (2006) test is the observation that a point shaving player on a favored team has two objectives. The player profits if his team's winning margin does not exceed the point spread, but also the player presumably prefers that his team does not lose outright. The player's objective function is stepwise in his team's winning margin, with the player best off if his team's winning margin falls between zero and the point spread. Wolfers shows that the optimal response to these incentives introduces a right-skewness into the distribution of the favorite's winning margins. Point shaving causes the winning margin to fall below the median winning margin in some games in which the winning margin otherwise would have fallen above the median⁸⁶. Wolfers tests for the presence of point shaving by comparing the fraction of winning margins that fall in regions of equal width below the point spread and above the point spread. Under the null, the proportions should be equal, and the difference between the fraction of winning margins that fall in the two regions identifies the fraction of games in which point shaving occurs.

This skewness-based test for point shaving finds evidence of rampant point shaving in the two highest point-spread deciles. Wolfers (2006) finds that when the point spread is above 12⁸⁷, the favorite wins the game but by less than the point spread in 46.2 percent of games, and the favorite wins by more than the point spread but by less than twice the point spread in 40.7 percent of games. Under the assumption that winning margins are symmetrically distributed in the absence of point shaving, these results suggest that the intent to shave points is present in about six percent of games when the point spread is above 12. Wolfers' study garnered significant attention in the popular media⁸⁸, reflecting public surprise that corruption might be so pervasive in amateur athletics. While

⁸⁵A point-spread bet allows a gambler to wager that a favored team's winning margin will exceed a given number, the *point spread*, or bet that the winning margin will not exceed the point spread. A typical arrangement is for the bettor to risk \$11 to win \$10 for a point spread bet on either the favorite or the underdog.

⁸⁶Because the bettor pays the same price for a point spread bet on either team and the bettor's stake is larger than the sports book's stake, the sports book profits on average if the point spread efficiently forecasts the median of the distribution of winning margins (Wolfers and Zitzewitz, 2006; Levitt, 2004).

⁸⁷The analysis is conducted separately for each decile of the point spread distribution. The suspicious pattern is present in the highest two point spread deciles, games with a point spread above 12.

⁸⁸Bernhardt and Heston (2008) cite a group of media outlets in which Wolfers' (2006) study was featured. These

point-shaving scandals have been uncovered with some regularity dating as far back as the early 1950s, the public's perception seems to be that such scandals are fairly isolated incidents.

Before moving to the richer structural model, I consider a simpler two-stage model of dynamic competition that illustrates how strategic adjustments might generate an asymmetric distribution of winning margins. Even if the stochastic component to scoring in each stage is symmetric, the winning margin is not in general symmetric, because the competitors' strategic adjustments introduce dependence between the first-stage and second-stage relative scores. The direction of skewness in the winning margin is related to whether, during the second stage, the first-stage laggard tends to gain on the leader or fall further behind. In the simple model, which of these occurs in equilibrium depends on whether the opportunity cost in terms of foregone expected points is larger for a very high-variance strategy or for a very low variance strategy. Similarly, in the richer model that includes many alternating possessions of varying duration, which of these occurs in equilibrium depends on whether the opportunity cost is larger for a team that stalls or for a team that hurries. In both models, the competitor that faces a lower marginal opportunity cost to its desired strategic adjustment will make larger adjustments.

In the structural model, teams take turns as the offensive side searching for scoring opportunities. The offensive side faces a sequence of arriving of shot opportunities which vary stochastically in quality. As in actual NCAA basketball games, the offensive side has 35 seconds to attempt a shot before the opponent is automatically awarded the ball. Like a worker in a job-search model, the searching team compares each arriving opportunity with the value of continued search. Because of the fixed horizon, the optimal strategy within a possession is a declining reservation policy where initially only the most advantageous opportunities are accepted and less advantageous opportunities become acceptable as time goes by. The optimal reservation policy depends on the current relative score and the time remaining in the game. Especially near the end of the game, a trailing team prefers to hurry by taking short possessions, and the leading team prefers to stall by taking long possessions. The parameters of the search process determine the opportunity cost of stalling and the opportunity cost of hurrying.

I estimate the model's parameters using play-by-play data from the first halves of games, and I find that for the estimated parameter values stalling is less costly than hurrying. As a result, the leading team makes larger strategic adjustments than the trailing team, and the score difference tends to shrink (or grow more slowly) on average compared to what would occur if each team chose the strategy that maximized its expected points per possession. These optimal adjustments result in a right-skewed distribution of winning margins in games in which one team is a large favorite. As a false experiment, I apply the skewness-based test for point shaving to simulated data. I find "evidence" of point shaving in the two highest point-spread categories, and the implied prevalence of point shaving is statistically indistinguishable the Wolfers (2006) estimate.

It should be noted that Wolfers' (2006) article acknowledges the limitations of the skewness-based test relative to a test more formally grounded in theory, and characterizes the resulting estimates as "prima facie" evidence of widespread point shaving. The structural approach that Wolfers' article outlines as a possible extension differs from the approach in this study. Wolfers suggests that

include the "New York Times, Chicago Tribune, USA Today, Sports Illustrated and Barrons, as well as National Public Radio and CNBC TV."

a structural model of the point shaver's behavior might allow for a more accurate inference about the prevalence of point shaving based on the observed deviation from symmetry in the empirical distribution. This study adopts a structural approach to more accurately characterize the distribution one might expect under the no-point-shaving null.

Studies in economics that treat sports as a research laboratory are sometimes criticized as being unlikely to generalize to more typical economic settings. However, for forensic economics, studies involving sports have provided particularly compelling case studies. Participants' willingness to engage in illegal behavior in the highly monitored environment of sports competitions suggests that corruption likely plays a more prominent role in less well-monitored settings (Duggan and Levitt, 2002; Wolfers, 2006; Price and Wolfers, 2010; Parsons, Sulaeman, Yates, and Hamermesh, 2011). The high-quality data and well-defined rules and institutions that are perhaps unique to sports also provide an excellent opportunity to assess the robustness of forensic economic methodology. This study's findings suggest that forensic economic studies should take great care to assess the robustness of their methods to unmodelled assumptions about even seemingly innocuous institutional features. Further, the findings suggests that structural modeling can yield improved predictions of behavior under the no-corruption null hypothesis even in settings in which off-the-shelf price theory and efficient market theory do not yield immediate predictions.

The conclusions of this paper conform with those of Bernhardt and Heston (2008). Bernhardt and Heston find that the patterns attributed to point shaving by Wolfers (2006) are present in subsets of basketball games in which gambling related malfeasance is less likely on prior grounds. The authors conclude that even in the absence of point shaving, asymmetries exist in the distribution of the final-score differentials among games in which one team is a large favorite⁸⁹. Because the approach of this paper and the purely empirical approach of Bernhardt and Heston (2008) are vulnerable to different criticisms, I consider my study a complement to their work. Their reduced-form empirical approach is vulnerable to criticism if the comparison sample is not free of the possibility of point shaving. The structural approach is vulnerable if the suggested model fails to accurately represent important aspects of the innocent environment.

The remainder of the paper is organized as follows: Section I develops a simple illustrative model, Section II develops a richer model of the basketball scoring environment, Section III derives an approximate solution to the full model, Section IV describes estimation, Section V describes the data, Section VI presents point estimates of the model's structural parameters and examines the model's fit, Section VII presents the results of simulations calibrated with estimated parameters, and Section VIII concludes.

I. A Stylized Model of Dynamic Competition

In a dynamic competition, participants observe the outcomes that occur early in the competition and can select their subsequent strategies based on these observations. To understand how the incentives provided by a dynamic competition might generate an asymmetric distribution of winning margins, I first consider a simple stylized model. Two competitors A and B accumulate points during a two

⁸⁹Bernhardt and Heston (2008) suggest that the goal of maximizing the probability of winning could induce an asymmetric final-score distribution, but stop short of suggesting a theoretical model.

stage game. Let X_1 represent the difference between A 's and B 's points during stage 1, and let X_2 represent the difference between A 's and B 's points during stage 2. Following stage 2, A receives a payoff of one if $X_1 + X_2 \geq 0$ and zero otherwise, and B receives a payoff of one if $X_1 + X_2 < 0$ and zero otherwise.

A random component influences the scoring process. Before each stage, A and B each select an action that influences the mean and the variance of scoring during that stage. Before stage 1, A selects $\sigma_{A1} \in [0, 1]$ and B selects $\sigma_{B1} \in [0, 1]$. Stage 1 then occurs, and both competitors observe the realized value of X_1 . Then before stage 2, A selects $\sigma_{A2} \in [0, 1]$ and B selects $\sigma_{B2} \in [0, 1]$. The quantities X_1 and X_2 are given by,

$$X_1 = \mu(\sigma_{A1}) - \mu(\sigma_{B1}) + (\sigma_{A1} + \sigma_{B1})Z_1 + \Delta \quad (41)$$

$$X_2 = \mu(\sigma_{A2}) - \mu(\sigma_{B2}) + (\sigma_{A2} + \sigma_{B2})Z_2 + \Delta \quad (42)$$

where $\mu(\cdot)$ is a twice differentiable real-valued function that describes the relationship between a player's chosen action and the mean of scoring, Z_1 and Z_2 are standard normal random variables that are independent from one another, and $\Delta \geq 0$ is a constant that allows for the possibility that A is stronger than B . I assume that $\mu(\cdot)$ is bounded, strictly concave, and reaches an interior maximum on $[0, 1]$. The choice of a very high variance (σ near one) or a very low variance (σ near zero) involves a lower expected value of scoring. To ensure an interior solution, I assume that the $\mu'(\sigma)$ is unbounded, going to $-\infty$ as σ approaches 1 and going to ∞ as σ approaches 0 (An example of a function satisfying these assumptions is a downward-facing semi-circle).

Because each competitor maximizes a continuous function on a compact set, the minimax theorem ensures that a solution to this zero-sum game exists, and inspection of the players' best reply-functions finds that the solution is unique. The optimal stage 2 actions $(\sigma_{A2}^*, \sigma_{B2}^*)$ satisfy the first order condition,

$$\mu'(\sigma_{A2}^*) = \frac{X_1 + \mu(\sigma_{A2}^*) - \mu(\sigma_{B2}^*) + \Delta}{\sigma_{A2}^* + \sigma_{B2}^*} = -\mu'(\sigma_{B2}^*) \quad (43)$$

Figure 1 plots A 's iso-payoff curves along with the choice set $\mu(\cdot)$ to illustrate the reasoning behind this result. If $X_1 + \Delta = 0$, A and B each have an equal chance of winning heading into stage 2, and each chooses the strategy that maximizes its expected points. If $X_1 + \Delta > 0$, A 's chance of winning is greater than $1/2$, and A selects a relatively low-variance action in order to reduce the chances of a come from behind win for B . If $X_1 + \Delta < 0$, A 's chance of winning is less than $1/2$, and A selects a relatively high-variance action in order to increase its chances of a come from behind win.

In stage 1, A and B each have a unique optimal strategy. Define $V(X_1)$ to be A 's expected payoff entering stage 2 if the relative score after stage 1 is X_1 . The optimal stage 1 actions $(\sigma_{A1}^*, \sigma_{B1}^*)$ are the solution to,

$$\max_{\sigma_{A1}} \min_{\sigma_{B1}} \int_{-\infty}^{\infty} V\left(\mu(\sigma_{A1}) - \mu(\sigma_{B1}) + \Delta + (\sigma_{A1} + \sigma_{B1})z\right) d\Phi(z) \quad (44)$$

where $\Phi()$ is the standard normal CDF.

Now consider how the players' optimal strategies influence the shape of the distribution of winning margins. One might expect for the winning margin X to be symmetrically, because $X = X_1 + X_2$ is the sum of two normally distributed random variables. That is not the case in general, because the players' choices introduce dependence between X_1 and X_2 . In two particular cases, the distribution of X is symmetric. The first case relies on the symmetry of the game when A and B are evenly matched.

Proposition 1: The winning margin is symmetric when A and B are evenly matched: Assume that $\Delta = 0$, which means that the two teams are evenly matched. Then the distribution of winning margins is symmetric.⁹⁰

Symmetry also holds when A and B are not evenly matched if the menu function $\mu()$ is symmetric. In that case A 's and B 's strategic adjustments, based on the realized value of X_1 , have exactly offsetting impacts on the mean and the variance of X_2 .

Proposition 2: The winning margin is symmetric (normally distributed) when increasing variance and decreasing variance are equally "costly": Assume that $\mu()$ is symmetric, that is, that $\mu'(.5) = 0$ and that $\mu(.5 + c) = \mu(.5 - c)$ for any $c \in [0, .5]$. Then, $E(X_1) = E(X_2|X_1) = \Delta$, $\sigma_{A1} + \sigma_{B1} = \sigma_{A2} + \sigma_{B2} = 1$, and $X \sim N(2\Delta, 2)$.⁹¹

When $\mu()$ is symmetric, $E(X_2|X_1)$ is constant. In general, though, $E(X_2|X_1)$ is not constant, and dependence between X_1 and X_2 can generate skewness in the sum of X_1 and X_2 . Proposition 3 provides conditions under which $E(X_2|X_1)$ monotonically increases or decreases in X_1 .

Proposition 3: The monotonicity of $E(X_2|X_1)$ if strategic adjustment of the variance is more difficult in one direction than the other: Let σ^* be the action that maximizes $\mu()$. Assume that for any $\sigma' < \sigma^*$ and $\sigma'' > \sigma^*$ with $u'(\sigma') = -u'(\sigma)$ that $|u''(\sigma')| < |u''(\sigma'')|$ (a player who prefers to increase variance faces a steeper marginal cost than a player who prefers to reduce variance). Then $E(X_2|X_1)$ monotonically decreases in X_1 . Conversely, assume that for any $\sigma' < \sigma^*$ and $\sigma'' > \sigma^*$ with $u'(\sigma') = -u'(\sigma)$ that $|u''(\sigma')| > |u''(\sigma'')|$ (a player who prefers to reduce variance faces a steeper marginal cost than a player who prefers to increase variance). Then $E(X_2|X_1)$ monotonically increases in X_1 .⁹²

Proposition 3 is not sufficient to sign the skewness of the winning margin distribution. A statistical result for comparing the skewness of two distributions (Zwet, 1964) suggests that it is the curvature of the function $E(X_2|X_1)$ that dictates the skewness of X .

Proposition 4: A sufficient condition for determining the direction of skewness in winning margins: Assume that $E(X_2|X_1)$ is convex in X_1 . Then the winning margin is right skewed. Conversely, assume that $E(X_2|X_1)$ is concave in X_1 . Then the winning margin is left skewed.⁹³

⁹⁰This result follows directly from the symmetry of the game. Because the solution is unique, switching the names of A and B cannot change the distribution of winning margins.

⁹¹Proof provided in Appendix I.

⁹²Proof provided in Appendix I.

⁹³Proof provided in Appendix I.

Because the function $\mu(\cdot)$ is bounded, $E(X_2|X_1)$ is also bounded and, therefore, may not be convex or concave over all values of X_1 . Nonetheless, propositions 3 and 4 together with the boundedness of $E(X_2|X_1)$ suggest circumstances under which one might expect to find skewness in the distribution of winning margins. Figures 2 and 3 illustrates this idea. When Δ is large, X_1 will typically be large. If $E(X_2|X_1)$ monotonically decreases in X_1 but is bounded, one might expect $E(X_2|X_1)$ to be convex over many large values of X_1 . In that case, one would expect the distribution of winning margins in games with large Δ to be right skewed. Similarly, if $E(X_2|X_1)$ monotonically increases in X_1 but is bounded, one might expect $E(X_2|X_1)$ to be concave over many large values of X_1 . In that case, one would expect the distribution of winning margins in games with large Δ to be left skewed.

Wolfers' (2006) statistical test attributes right skewness in the distribution of the favorite's winning margins to the influence of point shaving. If winning margins are right skewed in the absence of point shaving, then that test overstates the true prevalence of point shaving. On the other hand, if winning margins are left skewed in the absence of point shaving, then that test understates the true prevalence of point shaving.

The remainder of the paper considers a model of basketball teams' search for scoring opportunities in order to assess whether a skewed distribution of winning margins is consistent with optimal strategies. In basketball games, the two teams alternate turns as the offensive side, and the game ends after a fixed amount of time has passed. The total number of possessions that occurs has a significant influence on the variance of the score. During a given possession, a team can reduce the variance of the scoring that will occur during the remainder of the game by stalling; that is, taking a lot of time before attempting a shot. A team can increase the variance of the scoring that will occur during the remainder of the game by hurrying; that is, attempting a shot very quickly.

The reasoning behind this simple model suggests that the favored team's winning margin should be right skewed if increasing the variance of points has a lower opportunity cost than reducing the variance of points. In the context of the richer model that I consider, that condition is satisfied when stalling has a lower opportunity cost than hurrying. The estimated model finds that, indeed, the opportunity cost to stalling is lower than the opportunity cost of hurrying in NCAA basketball games. Further, the simulations that I conduct using the estimated model suggest that teams' optimal end-of-game adjustments generate skewness patterns that closely resemble those previously attributed to point shaving.

II. Full Model

In this section, I propose a more detailed model of the environment in which basketball teams compete. Teams alternate turns as the offensive side, and the team that is on offense faces a sequence of arriving shot opportunities that vary stochastically in quality. I refer to a single turn as the offensive side as a *possession*. The team that is searching for a scoring opportunity must compare the potential reward of each opportunity with the option value of continued search. As such, I model the game as a sequence of many alternating periods of finite horizon search.

In this model, the tradeoff between the expected value of points and the variance of points is endogenous. A unique reservation policy exists that maximizes a team's expected points per

possession. If the team chooses to hurry, by selecting a lower reservation policy, its expected points per possession will be lower. Also if it stalls by choosing a very high reservation policy, its expected points will be lower. The duration of a given possession influences the variance in the score in the remainder of the game through its influence on the total number of possessions that will occur. Hence, as in the simple model, an opportunity cost is associated with choosing a strategy that substantially raises or substantially reduces the variance of points.

In this more complex model, the team that is on defense is given the opportunity to intentionally foul the offensive team. I include this feature because the strategy is ubiquitous in the final minutes of actual NCAA basketball games, and the strategy influences the distribution of winning margins in a way that the simulation experiments I conduct find to be important.

In this more detailed model, two teams, A and B , accumulate points during a single competition with a typical duration of T seconds. In NCAA basketball games, $T = 2400$ seconds (40 minutes). If the score is not tied at time T , then the game ends. If the score is tied at time T , then play is repeatedly extended by an additional 300 seconds (five minutes) until a winner is determined. Two measures of time are relevant. Let $t \in [0, 1, 2, \dots, T]$ measure the time since the game began in discrete periods of one second each. At any t , let $s \geq 0$ describe the number of seconds since the current possession began. The variable s corresponds to the “shot clock” in NCAA basketball games.

One team at a time is on offense searching for a scoring opportunity. Let $o \in \{A, B\}$ indicate the team that is on offense. The team that is on offense faces a sequence of scoring opportunities. A scoring opportunity is characterized by a pair of variables, p and π . The variable $p \in [0, 1]$ describes the probability with which an opportunity will succeed if it is accepted. The variable $\pi \in \{1, 2, 3\}$ describes the number of points that will be awarded if the opportunity is accepted and succeeds. For standard opportunities, $\pi \in \{2, 3\}$, and the particular case when $\pi = 1$ is described below.

Each period, time t increases by one. The team on offense switches if one of three events occurs; the team on offense accepts a scoring opportunity at $t - 1$, the possession duration s reaches a maximum limit or 35 seconds at $t - 1$ (known as a *shot clock violation*), or an exogenous *turnover* occurs between period $t - 1$ and t . The arrival process for turnovers is described later. If the offensive team does not switch from one period to the next, s increases by one. Otherwise, s is reset to zero.

The variable $X = X_A - X_B$ measures the difference in the two teams’ accumulated point totals. When a team accepts a shot opportunity, the uncertainty is resolved and the offensive team receives either 0 or π points.

Scoring opportunities and turnovers arrive stochastically. I impose that no turnovers or shot opportunities arrive during the first five seconds of the possession to reflect the time required in actual games for the offensive team to move the ball from its defensive portion of the court to its offensive portion. Following this initial five-second span, one scoring opportunity arrives each second⁹⁴. The variables that describe a scoring opportunity are drawn from the conditional density

⁹⁴The assumption that a scoring opportunity arrives every second is not as restrictive as it might seem, because the arrival of a very poor scoring opportunity (one that succeeds with very low probability) is no different from a non-opportunity.

function $f(p, \pi|t, s, X, o)$. I impose the simplifying assumption that the distribution from which p and π are drawn depends only on the team that is offense but does not otherwise vary. That is $f(p, \pi|t, s, X, o) = f(p, \pi|o)$. I also assume that draws of (p, π) are independent across time. Finally, I assume that turnovers arrive with a constant probability for each team, and I let v_A and v_B denote the turnover probabilities when the team on offense is A or B . I refer to these assumptions jointly as a conditional independence assumption (CIA). These assumptions facilitate estimation of the model and allow me to conduct simulations of end-of-game play using parameters estimated from data on play early in games.

Both the offense and the defense face a choice during each one-second increment. If no turnover occurs, the team on defense has the option to intentionally foul the team on offense. The defense's choice of whether to foul or not occurs before realizations of the draw of (p, π) . When an intentional foul is committed, the offense is granted two one-point ($\pi = 1$) scoring opportunities during the same period known as free throws. Free throws succeed with known (to the teams) probabilities p_A^{ft} and p_B^{ft} . If no turnover occurs and the defense chooses not to intentionally foul, a shot opportunity (p, π) is drawn. The offense must choose whether to accept the current opportunity or to continue searching. I denote the choice spaces of the defense and offense with $A^D = \{0, 1\}$ and $A^O = \{0, 1\}$. For the team on defense, 1 represents the choice to intentionally foul. For the team on offense, 1 represents the choice to accept a scoring opportunity.

When the game ends, the teams receive payoffs U^A and U^B . The team with the higher score receives a payoff of one and the team with the lower score receives a payoff of zero. Teams' only objective is to win the game. Each team chooses its strategy to maximize its expected payoff. Denote the vector of state variables with $\omega = \{t, s, X, o, p, \pi\}$, and let $\Omega = \{\omega\}$ denote the state space. Define the value function $V^A(\omega) = E(U^A|\omega)$ to be A 's expected payoff from the state ω . Because the game is zero-sum, this value function also characterizes B 's expected payoff. The uniqueness of this expected payoff function follows directly from the uniqueness of the teams' reservation policies.

By CIA and the assumed process for the state transitions, state transitions are Markovian. That property allows the optimization to be expressed as the following dynamic programming problem.

$$V^A(\omega) = \max_{a \in A^A(\omega)} \left\{ \min_{b \in A^B(\omega)} \left\{ E[V^A(\omega')|\omega, a, b] \right\} \right\} \quad (45)$$

The sets $A^A(\omega)$ and $A^B(\omega)$ represent the teams' choice sets given the current state. A team's choice set is A^O if ω indicates that the team is on offense and is A^D otherwise.

III. Approximate Model Solution

The model developed in the previous section does not have an analytic solution. However, for any parameterization of the density function f , a numerical solution can be computed using backward induction. I compute the full numerical solution to the model when performing dynamic simulations in section VII. In this section, however, I develop an approximate solution method that describes the optimal policy within a single possession. I use this approximate solution as the basis for estimating the model's parameters.

Define the function EV to be A 's expected payoff in a state prior to the realization of the offensive team's shot opportunity,

$$EV([t, s, X, o]) = E_{(p', \pi')} \left[V \left([t, s, X, o, p', \pi'] \right) \right] \quad (46)$$

I now construct a linear approximation of EV within a single possession that begins in the state $\omega_0 = [t, s = 0, X, o]$. Let s^* denote the duration of the possession, and let x^* denote the change in X that occurs as a result of the possession. Then the state of the following the possession is $\omega^* = [t + s^*, s = 0, X + x^*, -o]$, where $-o$ indicates the opponent of team o . I next note that the expected payoff in the state ω^* can be approximated linearly using,

$$\begin{aligned} \widetilde{EV}^A(\omega^*) &\approx EV^A(\omega_0) + EV_X^A(\omega_0) x^* + EV_t^A(\omega_0) s^* \\ \widetilde{EV}^A(\omega^*) &\approx EV^A(\omega_0) + EV_X^A(\omega_0) \left(x^* + \phi(\omega_0^*) s^* \right) \end{aligned} \quad (47)$$

where $EV_X^A = EV([t, s, X+1, o]) - EV([t, s, X, o])$, $EV_t^A = EV([t+1, s, X, o]) - EV([t, s, X, o])$, and $\phi(\omega_0^*) = EV_t^A(\omega_0^*) / EV_X^A(\omega_0^*)$. The term $\phi(\omega_0^*)$ can be thought of as a marginal rate of substitution between time and points. The partial effect of one second passing on A 's expected payoff is the same as a $\phi(\omega_0^*)$ point change in the relative score.

Now note that because $EV^A(\omega^*)$ is a positive affine transformation of the expression $\left(x^* + \phi(\omega_0^*) s^* \right)$, the within-possession policy that maximizes EV^A is the same as the policy that maximizes $\left(x^* + \phi(\omega_0^*) s^* \right)$. Thus, up to a linear approximation of the value function, all strategically relevant information from the states t and X is embedded in $\phi(\omega_0^*)$.

The optimal policy for the team on offense is a reservation rule that I denote with $R(s; \phi)$. For each value of s within the possession, the reservation rule describes the point value $p\pi$ for which the offense is indifferent between accepting the opportunity and opting for continued search. Using the linear approximation developed above, it is straightforward to show that when A is on offense the optimal reservation rule is,

$$R^A(s; \phi) = E \left(x^* \mid s^* > s \right) + \phi E \left(s^* - s \mid s^* > s \right) \quad (48)$$

Given the fixed boundary at $s = 35$, the reservation value can be defined recursively. First, define the auxiliary function $z^A(s; \phi) = E \left[s^* \phi + x^* \mid s^* \geq s \right]$. I first construct an expression for z^A , and then use z^A to construct R^A .

$$z^A(s; \phi) = \begin{cases} 35 v_A \phi + (1 - v) E \max(p\pi, 0) & \text{if } s = 35 \\ s v_A \phi + (1 - v) E \max(p\pi, z^A(s + 1; \phi)) & \text{if } s < 35 \end{cases} \quad (49)$$

$$R^A(s; \phi) = \begin{cases} 0 & \text{if } s = 35 \\ (1 - v_A) [E \max(p\pi, z^A(s + 1; \phi)) - s \phi] & \text{if } s < 35 \end{cases} \quad (50)$$

Recall that v is the hazard of a turnover in each one-second period. The optimal strategy is a declining reservation policy. An analogous derivation yields the optimal policy for team B .

To illustrate how the optimal policy varies across game states, figure 4 plots the predicted reservation policies in three different game states holding the model parameters constant; one for which $\phi = 0$, one in which the $\phi < 0$ (offensive team trails late in game), and one in which $\phi > 0$ (offensive team leads late in game). The plots are constructed using the search parameters estimated for the favored team in games with a point spread between 0 and 4. When $\phi = 0$, the intermediate reservation policy is implemented. That is the point-maximizing policy. When $\phi < 0$ a lower reservation policy is chosen, which leads to shorter average possessions and fewer points per possession than when $\phi = 0$. When $\phi > 0$ a higher reservation policy is chosen, which leads to longer average possessions and fewer points per possession than when $\phi = 0$.

IV. Estimation

I estimate the parameters governing the search process separately for each of six categories defined by the size of the point spread. The first five point-spread categories are each four points wide, and the sixth category includes all games with a point spread above 20. I estimate one set of parameters describing the favorite's search process and another set of parameters describing the underdog's search process. Favorites and underdogs in the six point-spread categories make up twelve team types. This approach allows me to conduct separate simulation experiments by point-spread categories.

I estimate the model's parameters using a nested-fixed-point estimator, as in Rust (1987), that I apply to data that describing possessions from the first halves of games. Estimating the model's parameters from first-half possession data requires two approximating assumptions. I assume that during that first half of the game the team on defense does not intentionally foul.⁹⁵ I also impose that $\phi = 0$, an approximation that is reasonable because the importance of expected scoring swamps the importance of possession duration early in the game. In addition to the computational savings that result from the $\phi = 0$ simplification, restricting attention to first-half possessions is also appealing, because the approach ensures that the model's parameters are identified from choices that are entirely distinct from the second-half choices that the model is used to investigate.

⁹⁵A team on defense that is trailing near the end of the game may choose to intentionally foul its opponent in order to increase the number of possessions in the remainder of the game. In practice, the expected number of points scored by the offense when it is intentionally granted two free throws far exceeds the expected number of points scored on a typical possession. Two free throws result in an average of nearly 1.4 points, and an average possession results in less than one point. See Tables 3 and 4 for the relevant success rates across point-spread categories.

For estimation, I impose a parametric functional form for the joint density $f(\pi, p|o)$; the joint density from which shot opportunities come for a given offense. Because π is discrete, it is convenient to express the joint density as the product of a probability mass function and a conditional density; $f(\pi, p) = f_\pi(\pi) f(p|\pi)$. I treat $f_\pi(2)$, the probability that an opportunity is worth two points, as a parameter to be estimated. I next impose that $f(p|\pi = 2)$ and $f(p|\pi = 3)$ belong to the family of beta density functions, each described by two parameters to be estimated ($M_2, V_2, M_3,$ and V_3)⁹⁶. The final parameter governing the search process is v , the constant per-second turnover hazard. The full parameter vector describing a single team type's search process is given by $\theta = [f_{\pi=2}, M_2, V_2, M_3, V_3, v]$ ⁹⁷.

The nested-fixed-point algorithm requires an expression of the conditional probability of observed outcomes in terms of model parameters. In available data, I observe the duration of each possession and a record of which of the five mutually exclusive events caused the possession to end. Terminal events include turnovers and successful and unsuccessful two- or three-point shots. I observe the score differential X and time t when each possession begins, but I do not observe the sequence of state variables p and π .

Given a reservation rule $R^o(s; \phi = 0)$ defined as in equation (50), the conditional probabilities of each event for each value of the shot clock can be calculated by taking appropriate integrals over values of (p, π) with respect to the parameterized joint density function f . I let F represent the distribution function corresponding to density function f and S represent the survivor function corresponding to density function f . The conditional probabilities of the various events e are given by,

$$P(e|s) = \begin{cases} v & \text{for } e = \text{turnover} \\ (1 - v) f_\pi(2) S\left(R(s)/2 \mid \pi = 2\right) E_F\left[p \mid s, \pi = 2, 2p > R(s, 2)\right] & \text{for } e = \text{successful 2} \\ (1 - v) f_\pi(2) S\left(R(s)/2 \mid \pi = 2\right) E_F\left[1 - p \mid s, \pi = 2, 2p > R(s, 2)\right] & \text{for } e = \text{unsuccessful 2} \\ (1 - v) f_\pi(3) S\left(R(s)/3 \mid \pi = 3\right) E_F\left[p \mid s, \pi = 3, 3p > R(s, 3)\right] & \text{for } e = \text{successful 3} \\ (1 - v) f_\pi(3) S\left(R(s)/3 \mid \pi = 3\right) E_F\left[1 - p \mid s, \pi = 3, 3p > R(s, 3)\right] & \text{for } e = \text{unsuccessful 3} \\ (1 - v) \left[f_\pi(2) F\left(R(s)/2 \mid \pi = 2\right) + f_\pi(3) F\left(R(s, 3)/3 \mid \pi = 3\right) \right] & \text{for } e = \text{continued search} \end{cases} \quad (51)$$

⁹⁶The family of beta density functions is a convenient choice because the functional form is flexible, parsimonious, and has support confined to the interval $[0, 1]$. Because the random variables being drawn from the densities $f(p|\pi)$ are themselves probabilities (of particular shots succeeding), any chosen functional forms for $f(p | 2)$ and $f(p | 3)$ must have support confined to $[0, 1]$. A common parameterization of the beta density is given by $f_X(x) = \frac{x^{\alpha-1} (1-x)^{\beta-1}}{\int_0^1 x^{\alpha-1} (1-x)^{\beta-1} dx}$ for $x \in [0, 1]$. I use the re-parameterization $M = \frac{\alpha}{\alpha + \beta}$ and $V = \alpha + \beta$. M is the mean of the random variable and V is inversely related to the variable's dispersion.

⁹⁷For numerical stability in the estimation routines, I estimate continuous transformations of parameters that have unbounded support instead of directly estimating those parameters. The likelihood-maximizing parameter vector is invariant to these transformations, and the transformations ensure that an intermediate iteration of the hill-climbing algorithm does not step outside of a parameter's support.

I use these expressions for the conditional probabilities of discrete events to construct the likelihood function that is the basis for estimation.

I next construct a likelihood function. For each possession, I observe the time elapsed from the shot clock when the possession ended (s^*) and the event that caused the possession to end (e^*). Five of the six possible events listed in the piecewise definition of equation (51) are terminal events (turnovers and successful and unsuccessful two-point and three-point attempts), and, hence, are directly observed. All non-terminal events fall in the sixth category, continued search. The full sequence of events in any possession is a sequence of choices for continued search followed by a terminal event. By CIA, the probability of observing a possession described by the pair (s^*, e^*) is given by:

$$\Pr(s^*, e^*) = \Pr(e^* | s^*) \prod_{s=6}^{s^*-1} \Pr(\text{continued search} | s) \quad (52)$$

CIA further implies that the probability of observing a sample containing possessions $j = 1..N$ described by the pairs $\{(s_j^*, e_j^*)\}_{j=1}^N$ is given by:

$$\Pr\left(\{(s_j^*, e_j^*)\}_{j=1}^N\right) = \prod_{j=1}^N \Pr(s_j^*, e_j^*) \quad (53)$$

The right-hand side of equation (53) makes use of the expression defined in equation (52), and the right-hand side of equation (52) makes use of the expressions defined in equation (51). Finally I define the log-likelihood function,

$$l(\theta) = \ln \left(\Pr\left(\{(s_j^*, e_j^*)\}_{j=1}^N \mid \theta\right) \right), \quad (54)$$

where $\theta = [f_{\pi=2}, M_2, V_2, M_3, V_3, v]'$.

An inner loop of the NFXP algorithm computes the log-likelihood function at each candidate parameter vector using the numerical solution to the optimal reservation rule. An outer loop searches the parameter space for the likelihood maximizing parameter vector. To reduce the chances that a set of parameter estimates represent a local maximum to the likelihood but not a global maximum, I repeat the estimation routine from several different starting points in the parameter space⁹⁸.

V. Data

I construct the dataset used for estimation from two sources. The first data source is a compilation of detailed play-by-play records for a subset of the regular-season basketball games played between November 2003 and March 2008 downloaded from the website statsheet.com. Appendix 2 describes the process of constructing possession-level data from the raw event data, and the procedure for coding possessions that did not strictly fit in to one of the outcomes included in the model. The

⁹⁸My estimation routines converge to the same parameter vectors regardless of the initial guess.

second data source is a set of point spreads for regular-season games played during the same time period for which a point spread was available. These data come from the website covers.com. The final dataset contains 5,258 games that appear in both data sources.

Table 1 describes the distribution of games across seasons and across point spreads. More recent seasons are more heavily represented in the dataset, reflecting the increasing availability of detailed play-by-play game data. The lowest point-spread categories are most common, and a smaller fraction of games fall in each larger point-spread category.

Table 2 presents regression estimates of the home team's winning margin (negative if the home team loses) on the amount by which the home team is favored on the point spread (negative if the home team was an underdog). I estimate an OLS regression to fit a conditional mean and a minimum absolute deviation regression to fit a conditional median. The point spread appears to provide an excellent forecast of the final-score differential. Consistent with efficiency in the point-spread betting market, neither estimated constant is statistically different from zero, and neither estimated slope coefficient is statistically different from one.

Table 3 provides descriptive statistics at the possession level. I report these values separately for favorites and underdogs in each point-spread category. Because the estimation routine restricts attention to possessions from the first halves of games, I provide one set of descriptive statistics for all possessions and another restricted to first-half possessions. To accommodate estimation, I code all possessions meeting my sample-selection criteria to one of the outcomes that is explicitly modeled. As expected, possessions of favored teams end more frequently with made shots and less frequently with missed shots and turnovers than the possessions of underdogs, and the disparity between the outcomes of favorites and underdogs tends to grow larger in the higher point-spread categories.

Figure 5 illustrates the skewness patterns that are the focus of the study. Panels 5-a and 5-b plot kernel density estimates of the favored team's winning margin relative to the point spread. Panels 5-c and 5-d plot kernel density estimates of the difference between the favorite's lead at halftime and the predicted value of that quantity⁹⁹. Consistent with the findings of Wolfers (2006), the distribution of favorites' winning margins is approximately symmetric in games with a low point spread and is right skewed in games with a high point spread. Panels 5-c and 5-d demonstrate that any skewness in winning margins arises during the second half of games, as the distributions of halftime leads are nearly symmetric.

VI. Structural Parameter Estimates

To recover estimates of the model parameters, I apply the estimation routine described in Section IV to the data described in Section V. Table 4 provides estimates of the model's parameters by point-spread category.

Table 5 assesses the fit of the model to the first half data. I compare empirical average possession duration to the average duration predicted by the model, and I compare the fractions of possessions

⁹⁹The favorite's predicted halftime lead is estimated with a regression of that quantity on a constant and the point spread

ending in each of the five modeled terminal events to the fractions predicted by the model. The predicted moments closely resemble the empirical moments both within and across point-spread categories. For each combination of point-spread category and favorite/underdog, I construct tests of the null that empirical moments are equal to their predicted values. I compute two-sided t-tests to test the equality of empirical and predicted average possession durations, and use Pearson’s chi-squared tests for the equality of the empirical and predicted proportions of possessions that end with each of the five modeled terminal events. Not surprisingly, the match is imperfect, and given the large sample size I can reject the null of equality between empirical and predicted moments at the 1 percent level.

Figure 6 allows for a visual inspection of the model’s fit to the observed dynamics of the offensive possessions during the first halves of games. To reduce clutter, the figures restrict attention to one low point-spread category $[0, 4]$ and one high point-spread category $(16, 20]$. The figure presents the predicted reservation values and predicted points per attempted shot by possession duration, along with actual mean points per shot attempt over the course of the 35-second shot clock. Consistent with the model, all observed points per shot attempt fall above the predicted reservation value, and average points per attempt tend to fall as time passes and the reservation value falls.

To understand how the average drift in the relative score should vary across game states during the second half of games, figure 7 plots the average change in the favorite’s lead predicted to results from the play of one offensive possession for each team as a function of $\phi(\omega)$, the marginal rate of substitution between time and points for the favorite.¹⁰⁰ In all six point-spread categories the average drift in the favorite’s lead decreases with $\phi(\omega)$ and is convex in ϕ when ϕ is high. Given this drift pattern one would expect the distribution of the relative score to become right skewed in the higher point-spread categories, because the relative score commonly evolves through the high- ϕ portion of the state space when team A is significantly stronger than team B . The next section uses simulations to examine the magnitude of this induced skewness.

VII. Dynamic Simulation

Using the estimated model, I conduct a series of simulation experiments to assess the skewness patterns that result from teams’ optimally chosen strategies. I conduct the simulations separately for each point-spread category. I initialize each simulation by drawing a halftime score difference from a discretized normal distribution that matches the first two moments of the empirical distribution of favorite’s halftime leads. This approach guarantees that any skewness that is found in the simulated distributions is generated by the model’s predicted strategies. Table 6 reports the mean and standard deviation of the favorite’s halftime lead in each point-spread category. Then I use the model to simulate the distribution of the favorite’s winning margin.¹⁰¹ I then compute analogs of the two quantities required to construct the skewness based test for point shaving from the simulated distributions. For each point-spread category, I compute the fraction of the simulated favorite’s

¹⁰⁰For a given ϕ I compute each team type’s optimal reservation policy according to Equation 50, taking expectations with respect to each type’s estimated search parameters, and I calculate the average points per possession associated with these policies. The average scoring drift at a given ϕ for a particular point spread is the difference between the average points per possession for the favorite and the underdog. I construct the curves in figure 7 by repeating this procedure for a range of ϕ .

¹⁰¹See Appendix 3 for details on computing the simulated distributions.

winning margins that fall between zero and the median of the winning-margin distribution. I also compute the fraction of the simulated favorite's winning margins that fall between the median of the winning-margin distribution and twice the median.¹⁰²

I compute three separate versions of the simulations in order to isolate the impact of several model features. In the first set of simulations, I impose that each offense simply maximizes expected points per possession in all game states. This is not an optimal policy, but the exercise provides a reference to which more nearly optimal policies can be compared. In a second set of simulations, the offensive team optimally solves the model, but I do not allow the defense to foul intentionally. In a third set of simulations, both the offense and defense play optimal strategies.

Figure 8 plots the fraction of the favorite's winning margins falling between zero and the median winning margin (lower region) and between the median winning margin and twice the median (higher region) for each of the three simulation scenarios and for the empirical distribution. Within each panel, I plot these two quantities against the midpoint of the point spread-category from which it was computed.

Panel 8-a reports the results of the first set of simulations in which the offensive team always maximizes its expected points per possession. The simulations find that the distribution of winning margins is nearly symmetric, with about the same fraction of winning margins falling in the lower region as in the upper region. Panel 8-b reports the results of the second set of simulations in which the offense solves the full model and the defensive team never fouls. Under that scenario, the fraction of winning margins falling in the lower region exceeds the fraction of games falling in the higher region in all but the lowest point-spread category. This result suggests that optimal offensive strategies introduce right skewness into the favorite's winning margin, and that the degree of right skewness grows with the strength of the favorite.

Panel 8-c reports the results of the third set of simulations in which the offense and the defense both adopt the optimal strategies predicted by the model. The simulations find that the fraction of simulated winning margins falling in the lower and higher is almost identical to the fractions of winning margins falling in those regions in actual games. That finding is the key result of the study. The empirical patterns are graphed in panel 8-d. In games with a large favorite (the two highest point-spread categories), the fraction of games falling in the lower region exceeds the fraction falling in the higher region in simulations of the full model and in actual games. In games with a small favorite, similar fractions of games fall in the lower region and higher region in simulations of the full model and in actual games.

Applying the skewness test for point shaving to these simulated data, the conclusion is that the favorite intends to shave points in roughly 7 percent of games in the two highest point-spread categories. That predicted point-shaving prevalence is statistically indistinguishable from that computed from the empirical winning-margin distribution. Because panel 8-c follows from innocent optimizing play, the simulation exercise suggests that the patterns previously attributed to point shaving are actually indistinguishable from the patterns expected under a null hypothesis of no point shaving.

¹⁰²I use the median of the distribution instead of a particular point spread for two reasons. First, each point-spread category contains many individual point spreads, so the comparison to a single quantity is convenient. Second, the medians of the predicted distributions fall systematically below each category's mean point spread. Presumably this is attributable to an unmodelled dimension by which the strengths of favorites and underdogs differ, rebounding for instance. Using the median preserves the interpretation of a difference in the two proportions as a departure from symmetry.

VIII. Conclusion

This paper finds that the inference of widespread point shaving from skewness in the distribution of final score differentials is ill-founded. While the skewness-based test for point shaving relies on an assumption that winning margins are symmetric in the absence of point shaving, the model considered in this paper finds that teams adopt end-of-game strategies that do not in general lead to symmetric distributions. Calibrated with parameters estimated from first-half play-by-play data, the model of innocent dynamic competition predicts skewness patterns that are statistically indistinguishable from the empirical patterns. While we know from the historical record (Porter, 2002; Rosen 2001) that the true prevalence of point shaving is not zero, this finding suggests that the skewness-based test is likely to drastically overstate the true prevalence.

A possible extension to this study might formally model the behavior of a player or team engaged in point shaving. Wolfers (2006) proposes an exercise of that sort as a potential extension. Developing a credible model of a game in which one team is point shaving poses several obstacles that are not a problem for this study. A realistic model of point shaving requires a departure from the perfect-information framework. A more complex information structure would recognize that a team is probably never certain that its opponent is point shaving. The team that is point shaving must then consider the beliefs of its opponent regarding its objective, beliefs regarding those beliefs, and so on. An important second complication is the need for a point shaver to avoid detection. A simple but flawed model of point shaving might begin with the model used in this paper and replace the favorite's objective function with one providing a reward to not covering the point spread¹⁰³. In that model, the optimal strategy for a large favorite who is shaving points is to play normally until near the end of the game and then, if necessary, deploy the strategy that most rapidly reduces its lead¹⁰⁴. Using that strategy, the favorite would win with the same frequency as when not point shaving and would almost never cover the point spread. But in practice, a casual spectator would recognize that the corrupt team was not simply trying to win. Because point shaving is illegal, a realistic model of point shaving must include some penalty for strategies that are easily detected.

The findings of this study cast some doubt on forensic economic studies that rely on unmodelled assumptions about innocent behavior. In the case considered in this study, a theoretical model is sufficient to raise the possibility that the skewness-based test for point shaving leads to biased estimates. Theory alone is insufficient to predict the direction or magnitude of any bias, and therefore a calibration exercise proves informative. These findings suggest that the indirect inference techniques that are common in forensic economic studies can be sensitive to seemingly minor institutional features of the environment in which the behavior of interest takes place, and highlight the promise of structural estimation as a tool for validating forensic economic methodology.

¹⁰³For instance, the favorite might receive a payoff of one if its winning margin fell between zero and the point spread and receive a payoff of zero otherwise.

¹⁰⁴In the model considered in this paper, that strategy is to intentionally foul one's opponent when on defense and to attempt the first available shot that provides a low success probability when on offense

Table 3.1 Descriptive Statistics - Games

Category	(1) Percent
2003-2004 Season	12
2004-2005 Season	17
2005-2006 Season	21
2006-2007 Season	24
2007-2008 Season	26
Point Spread $\in [0, 4]$	29
Point Spread $\in (4, 8]$	27
Point Spread $\in (8, 12]$	18
Point Spread $\in (12, 16]$	13
Point Spread $\in (16, 20]$	7
Point Spread $\in (20, \infty]$	6
Observations	5,258

Note: The sample comprises play-by-play game data from statsheet.com merged to point-spread data from covers.com. A game is included in the sample if it appears in both data sources and the game's play-by-play data contains enough detail to perform the analysis conducted in this study (see Data Appendix 1 for details). Source: author's calculations.

Table 3.2 Regression Analysis of the Point Spread's Predictive Accuracy

	(1) OLS	(2) MAD
Amount by which home team is favored	1.01 (0.02)	1.00 (0.03)
Constant	0.09 (0.18)	0.00 (0.31)
Observations	5258	5258

Standard errors in parentheses.

Note: This table displays regression estimates of the conditional mean and median of the home team's winning margin (negative if the home team loses). Column (1) reports coefficient estimates for an OLS regression of the home team's winning margin on a constant and the amount by which the home team is favored (negative if the home team is the underdog). Column (2) reports coefficient estimates for minimum absolute deviation (median) regression of the home team's winning margin on the amount by which the home team is favored. Source: author's calculations using play-by-play data from statsheet.com merged to point-spread data from covers.com.

Table 3.3 Descriptive Statistics - Possessions

All Possessions	All	$PS \in [0, 4]$		$PS \in (4, 8]$		$PS \in (8, 12]$		$PS \in (12, 16]$		$PS \in (16, 20]$		$PS > 20$	
		F	U	F	U	F	U	F	U	F	U	F	U
Duration of Possession (sec)	16.11	16.32	16.26	16.27	16.17	16.09	16.23	15.78	16.18	15.28	16.19	14.51	16.14
Points	0.84	0.85	0.82	0.88	0.81	0.90	0.78	0.92	0.75	0.93	0.72	0.94	0.69
Made two point shot	0.30	0.31	0.30	0.31	0.29	0.33	0.28	0.33	0.27	0.33	0.26	0.34	0.24
Missed two point shot	0.25	0.25	0.25	0.24	0.26	0.23	0.26	0.23	0.27	0.24	0.27	0.22	0.28
Made three point shot	0.10	0.10	0.09	0.10	0.09	0.10	0.09	0.10	0.09	0.10	0.08	0.11	0.08
Missed three point shot	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.17	0.16	0.17	0.17	0.17
Turnover	0.18	0.18	0.18	0.17	0.19	0.17	0.20	0.17	0.20	0.17	0.22	0.17	0.23
Observations	641,447	93,547	93,760	87,139	88,005	57,293	58,325	39,879	41,005	21,121	21,762	19,384	20,227
First Half Possessions													
Duration of Possession (sec)	16.11	16.18	16.29	15.96	16.37	15.71	16.59	15.46	16.61	14.97	16.76	14.34	16.59
Points	0.80	0.82	0.79	0.83	0.77	0.87	0.75	0.90	0.72	0.90	0.69	0.93	0.65
Made two point shot	0.27	0.28	0.27	0.28	0.26	0.30	0.25	0.30	0.25	0.30	0.24	0.32	0.22
Missed two point shot	0.26	0.25	0.26	0.25	0.27	0.24	0.27	0.24	0.27	0.24	0.27	0.22	0.27
Made three point shot	0.10	0.10	0.10	0.10	0.09	0.11	0.09	0.11	0.09	0.11	0.08	0.11	0.08
Missed three point shot	0.17	0.17	0.17	0.18	0.17	0.18	0.17	0.18	0.17	0.17	0.17	0.18	0.17
Turnover	0.20	0.19	0.20	0.19	0.20	0.18	0.22	0.17	0.22	0.17	0.24	0.17	0.26
Observations	320,064	46,531	46,697	43,845	43,889	28,719	28,928	19,945	20,277	10,644	10,814	9,736	10,039

Standard errors in parentheses.

Note: The sample excludes possessions with a duration of five seconds or less and possessions that end with a defensive foul that does not result in free throws. Source: author's calculations using play-by-play data from statsheet.com merged to point-spread data from covers.com.

Table 3.4 Maximum Likelihood Estimates of Structural Parameters by Point-Spread Category

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	$PS \in [0, 4]$		$PS \in (4, 8]$		$PS \in (8, 12]$		$PS \in (12, 16]$		$PS \in (16, 20]$		$PS > 20$	
	F	U	F	U	F	U	F	U	F	U	F	U
Prob. shot opportunity is worth two points	0.533	0.536	0.603	0.563	0.568	0.520	0.607	0.551	0.797	0.515	0.823	0.489
	(0.036)	(0.031)	(0.041)	(0.035)	(0.062)	(0.037)	(0.069)	(0.044)	(0.049)	(0.058)	(0.043)	(0.062)
Mean of success prob. distribution - two pointers	0.139	0.133	0.134	0.127	0.157	0.118	0.163	0.112	0.144	0.102	0.151	0.121
	(0.008)	(0.007)	(0.008)	(0.007)	(0.015)	(0.008)	(0.016)	(0.008)	(0.012)	(0.011)	(0.012)	(0.0144)
Scale of success prob. distribution - two pointers	3.557	3.695	3.543	3.959	3.590	3.579	3.721	3.834	3.633	3.391	3.364	5.436
	(0.138)	(0.143)	(0.144)	(0.162)	(0.220)	(0.167)	(0.255)	(0.231)	(0.283)	(0.254)	(0.259)	(0.636)
Mean of success prob. distribution - three pointers	0.068	0.058	0.089	0.056	0.095	0.047	0.109	0.043	0.177	0.038	0.223	0.030
	(0.005)	(0.004)	(0.008)	(0.004)	(0.012)	(0.004)	(0.015)	(0.004)	(0.025)	(0.005)	(0.024)	(0.0034)
Scale of success prob. distribution - three pointers	6.215	5.109	7.250	5.233	7.178	4.879	7.652	4.280	11.450	4.637	18.284	2.662
	(0.371)	(0.265)	(0.525)	(0.297)	(0.714)	(0.331)	(0.888)	(0.339)	(2.450)	(0.547)	(4.772)	(0.211)
Turnover hazard	0.016	0.016	0.016	0.016	0.015	0.017	0.015	0.018	0.016	0.018	0.017	0.020
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	46,531	46,697	43,845	43,889	28,719	28,928	19,945	20,277	10,644	10,814	9,736	10,039
Free throw success probability	0.691	0.68	0.694	0.686	0.697	0.68	0.692	0.667	0.695	0.678	0.702	0.652
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)	(0.01)	(0.008)	(0.01)
Observations	10,294	9,913	11,187	9,709	8,084	6,516	5,813	4,324	3,426	2,352	3,475	2,102

Standard errors in parentheses.

Note: Estimation is by maximum likelihood using data from the first halves of games. Free throw success probabilities are estimated by the sample mean success rate for free throws. A nested fixed point algorithm is used to compute all remaining parameters. The parameters are estimated separately for the favorite and underdog within each of the six point-spread categories. Source: author's calculations using play-by-play data from statsheet.com merged to point-spread data from covers.com.

Table 3.5 Comparison of Empirical and Predicted Moments

	$PS \in [0, 4]$				$PS \in (4, 8]$				$PS \in (8, 12]$			
	F		U		F		U		F		U	
	\bar{m}	\hat{m}	\bar{m}	\hat{m}	\bar{m}	\hat{m}	\bar{m}	\hat{m}	\bar{m}	\hat{m}	\bar{m}	\hat{m}
Duration	16.18	15.85	16.29	15.94	15.96	15.69	16.37	16.00	15.71	15.48	16.59	16.16
Made 2	0.278	0.279	0.270	0.268	0.280	0.281	0.264	0.262	0.296	0.296	0.255	0.254
Missed 2	0.254	0.265	0.256	0.271	0.249	0.260	0.271	0.286	0.237	0.246	0.267	0.282
Made 3	0.0993	0.0969	0.0962	0.0974	0.102	0.100	0.0911	0.0915	0.109	0.107	0.0912	0.0899
Missed 3	0.175	0.188	0.175	0.185	0.182	0.193	0.171	0.182	0.179	0.190	0.170	0.185
Turnover	0.194	0.171	0.203	0.179	0.187	0.167	0.203	0.178	0.180	0.161	0.216	0.189
Observations	46,531		46,697		43,845		43,889		28,719		28,928	
t (equality of mean duration)	10.00		10.29		7.94		10.57		5.48		10.00	
p-value	0.000		0.000		0.000		0.000		0.000		0.000	
Pearson's χ^2 (equality of proportions)	209.95		215.66		154.82		218.56		93.21		170.50	
p-value	0.000		0.000		0.000		0.000		0.000		0.000	
	$PS \in (12, 16]$				$PS \in (16, 20]$				$PS > 20$			
	F		U		F		U		F		U	
	\bar{m}	\hat{m}	\bar{m}	\hat{m}	\bar{m}	\hat{m}	\bar{m}	\hat{m}	\bar{m}	\hat{m}	\bar{m}	\hat{m}
Duration	15.46	15.28	16.61	16.17	14.97	14.84	16.76	16.25	14.34	14.24	16.59	16.14
Made 2	0.303	0.303	0.248	0.245	0.305	0.308	0.242	0.239	0.317	0.319	0.218	0.207
Missed 2	0.236	0.244	0.274	0.292	0.239	0.246	0.273	0.292	0.223	0.228	0.274	0.300
Made 3	0.110	0.109	0.0867	0.0886	0.111	0.106	0.0800	0.0809	0.113	0.109	0.0845	0.0959
Missed 3	0.178	0.186	0.167	0.179	0.173	0.182	0.167	0.181	0.180	0.187	0.166	0.171
Turnover	0.173	0.158	0.225	0.196	0.172	0.158	0.238	0.207	0.168	0.157	0.257	0.226
Observations	19,945		20,277		10,644		10,814		9,736		10,039	
t (equality of mean duration)	3.60		8.63		1.52		7.18		1.52		6.16	
p-value	0.000		0.000		0.065		0.000		0.065		0.000	
Pearson's χ^2 (equality of proportions)	40.68		127.39		22.88		75.80		12.67		86.25	
p-value	0.000		0.000		0.000		0.000		0.013		0.000	

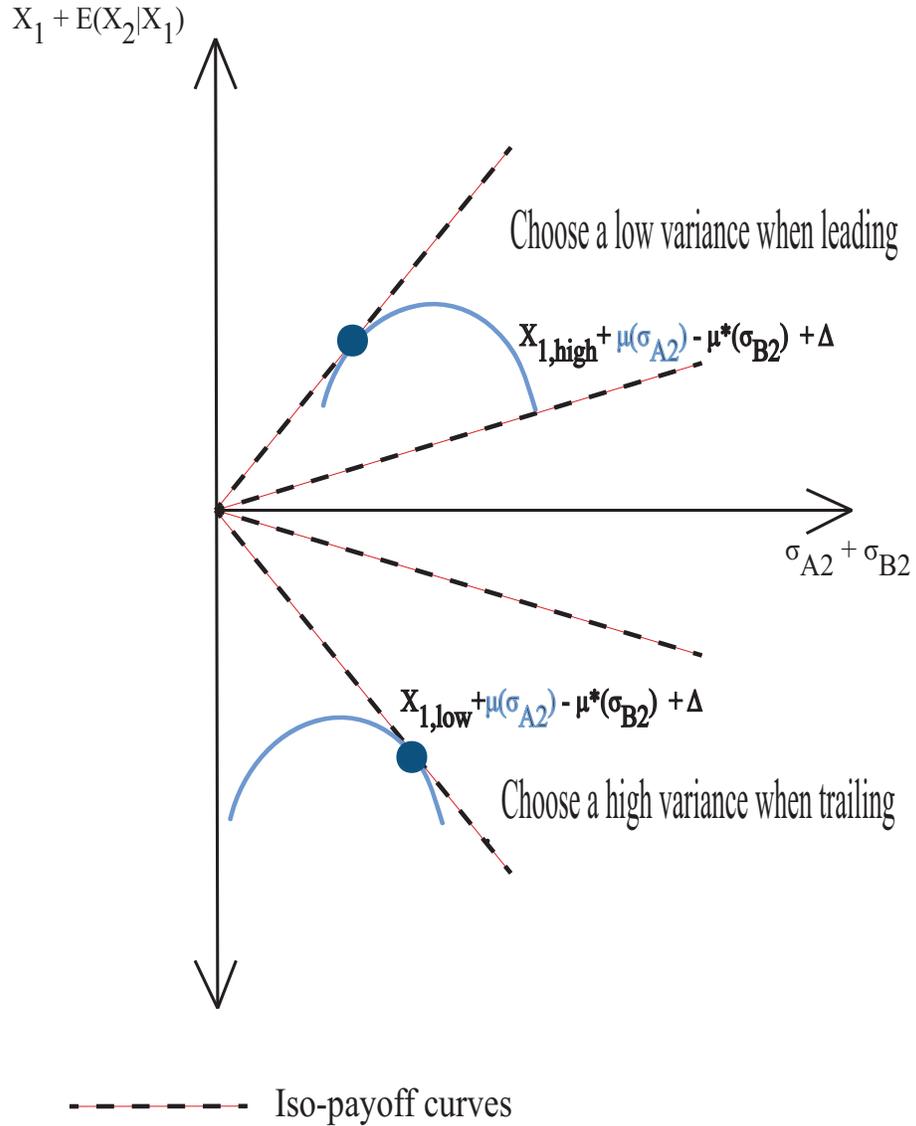
Note: Within each point-spread category, sample moments (\bar{m}) and predicted moments (\hat{m}) are provided separately for favorites and underdogs. Empirical moments are sample means. Predicted moments are the equivalent means predicted by the model when each team searches optimally given estimated structural parameters with the objective of maximizing expected points per possession. Two test statistics and corresponding p-values are provided for each combination of point-spread category and favorite/underdog. A two-sided t-test is performed to test the null that the empirical mean possession duration is equal to the predicted mean duration. A Pearson's chi-squared test is performed to test the null that the five empirical proportions are equal to the predicted proportions. Under the null, the Pearson's statistic is distributed chi-squared with $J - 1 = 4$ degrees of freedom (where J is the number of categories). Source: authors calculations using play-by-play data from statsheet.com merged to point-spread data from covers.com.

Table 3.6 Mean and Standard Deviation of Favorite's Halftime Lead by Point-Spread Category

	Mean	Standard Deviation
Point Spread $\in [0, 4]$	1.16	8.59
Point Spread $\in (4, 12]$	3.04	8.39
Point Spread $\in (8, 12]$	5.57	8.83
Point Spread $\in (12, 16]$	8.19	8.56
Point Spread $\in (16, 20]$	10.17	8.82
Point Spread > 20	13.82	9.40

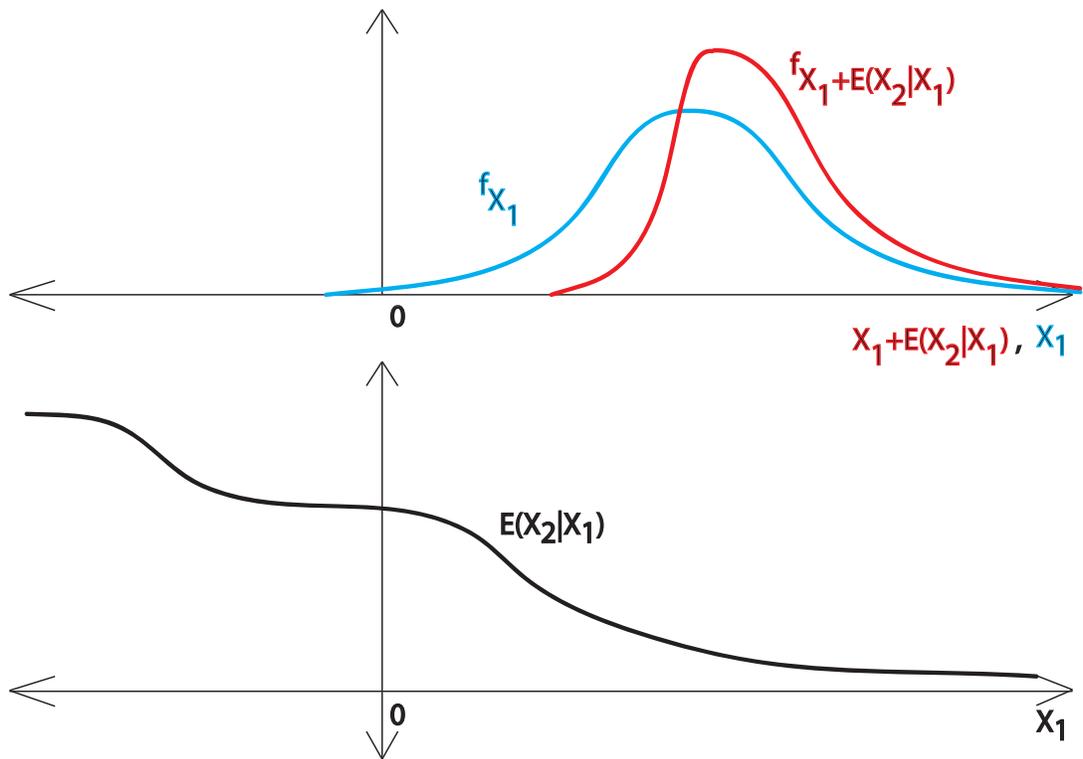
Note: The simulations described in this paper assume that half-time score differentials follow a (discretized) normal distribution with the empirical means and standard deviations contained in this table. The simulated final score distributions, then, reveal the extent to which optimal second-half play induces asymmetries. Source: author's calculations using play-by-play data from statsheet.com merged to point-spread data from covers.com.

Figure 3.1 The Optimal Choice of a Mean-Variance Pair During Stage 2 of the Stylized Model



Note: Taking B 's strategy as given, A faces a choice among combinations of the mean and the variance of the final score. Two example choice sets are provided, one corresponding to A trailing after stage 1 and the other corresponding to A leading after stage 1. Iso-expected payoff curves are represented by dashed lines. Because X_2 , the score during stage 2, is normally distributed, A 's probability of winning depends on the ratio of the expected value of the final score to the standard deviation of X_2 . As such, the iso-expected payoff curves are rays away from the origin.

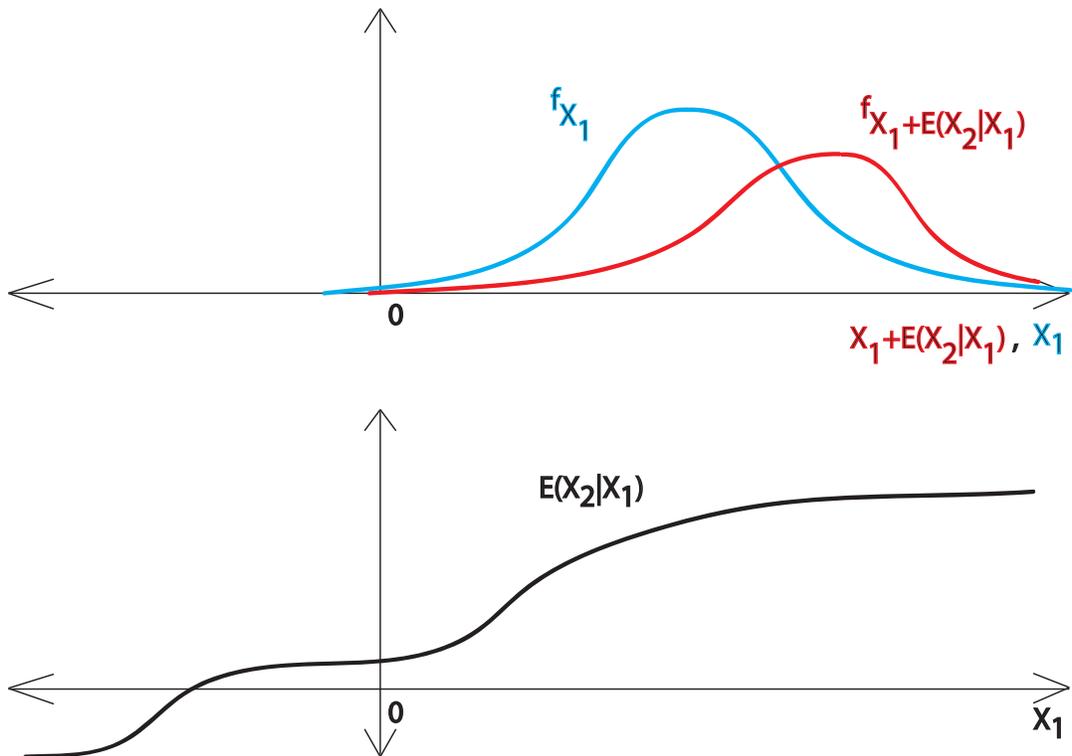
Figure 3.2 Winning Margins Will Be Right Skewed if the Expected Value of the Relative Score in Stage 2 is a Convex Function of the Relative Score from Stage 1.



(a) Choosing a very low variance has a greater opportunity cost than choosing a very high variance

Note: The top panel illustrates the expected effect of stage 2 scoring on the distribution of winning margins when $E(X_2|X_1)$ is convex over the most commonly realized values for X_1 (depicted in the bottom panel). The distribution of stage-1 score differences, X_1 , is symmetric, and stage-2 play introduces a right skewness.

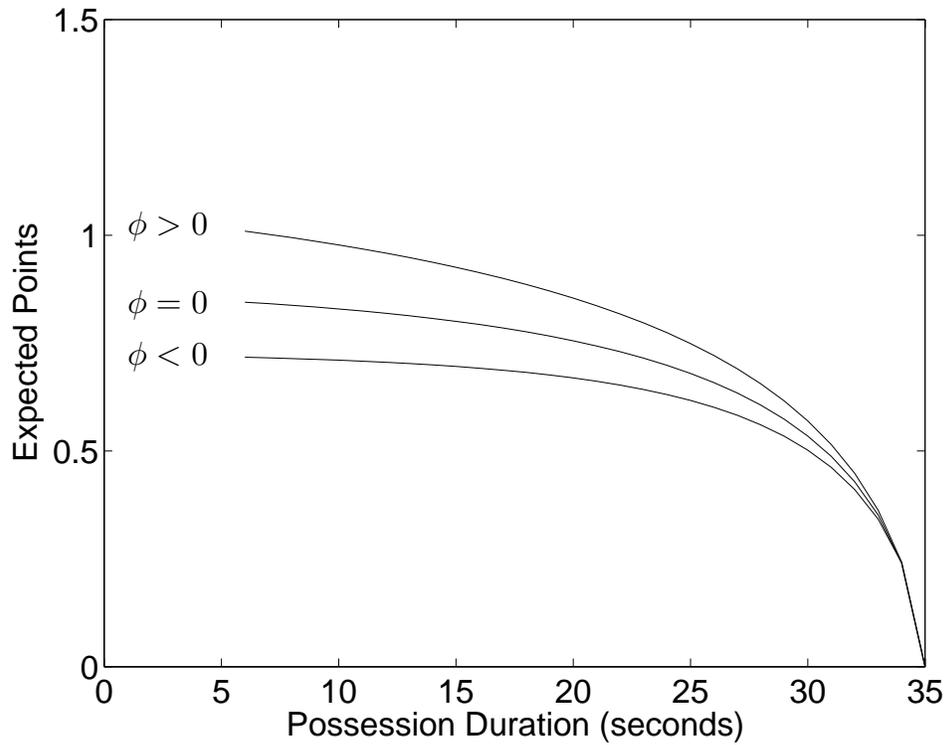
Figure 3.3 Winning Margins Will Be Left Skewed if the Expected Value of the Relative Score in Stage 2 is a Concave Function of the Relative Score from Stage 1.



(b) Choosing a very low variance has a greater opportunity cost than choosing a very high variance

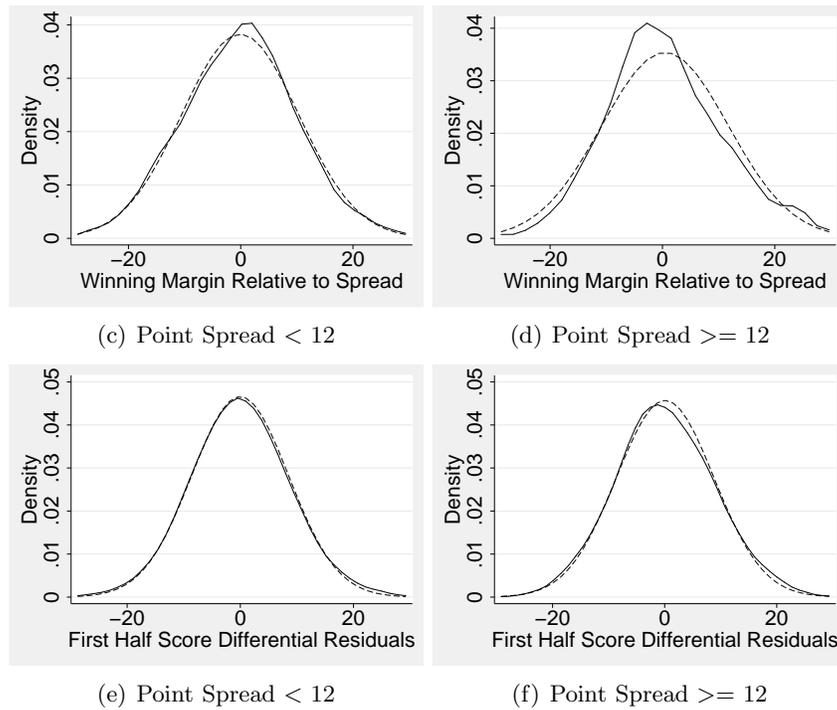
Note: The top panel illustrates the expected effect of stage 2 scoring on the distribution of winning margins when $E(X_2|X_1)$ is concave over the most commonly realized values for X_1 (depicted in the bottom panel). The distribution of stage-1 score differences, X_1 , is symmetric, and stage-2 play introduces a left skewness.

Figure 3.4 Predicted Reservation Policies Across Game States



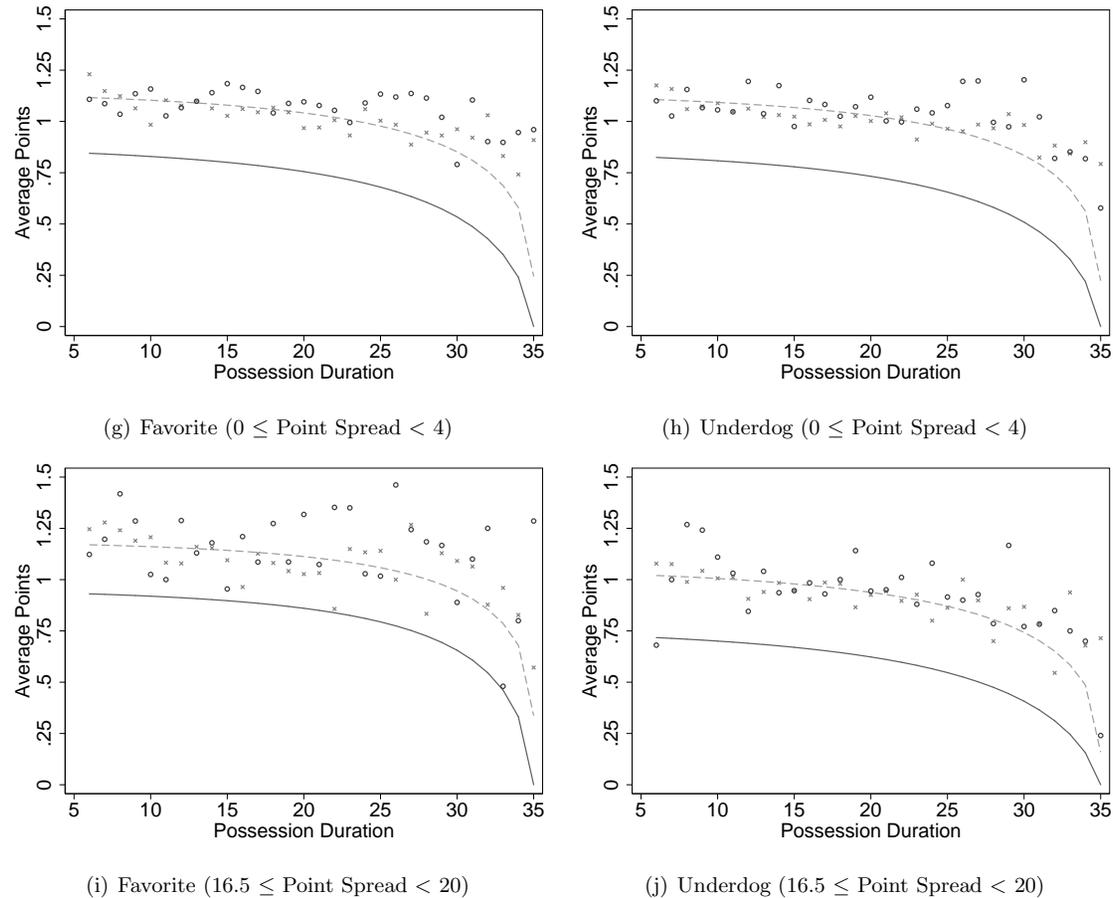
Note: Shot opportunities are characterized by a success probability and a point value. The graphed reservation values depict for a single team type (teams favored by 0 to 4 points) the expected success probability times point value above which it is optimal to accept the opportunity. The optimal policy depends on the game state. The top, middle, and bottom lines correspond to game states in which the marginal rate of substitution between time and points (ϕ) for the favorite is positive (0.15), zero, and negative (-0.15). When the partial effect of the passing time on a team's expected payoff is higher, the team adopts a higher reservation policy that results in longer average possessions. Source: author's calculations.

Figure 3.5 Game Outcomes Relative to Point Spread Prediction



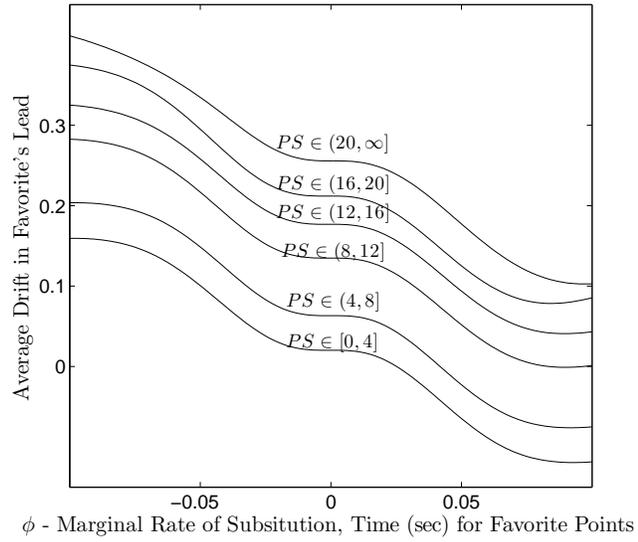
Note: Plots (a) and (b) provide kernel-density estimates of the difference between the favorite's winning margin and the point spread. Plots (c) and (d) provide kernel-density estimates of the difference between the favorite's halftime lead and the predicted value of that quantity from a linear regression of the halftime lead on a constant and the point spread. As a reference, each plot is overlaid with a normal density function with the same first two moments as the estimated density. Source: author's calculations using play-by-play data from statsheet.com merged to point-spread data from covers.com.

Figure 3.6 Predicted Reservation Values and Average Points by Time Elapsed from Shot Clock



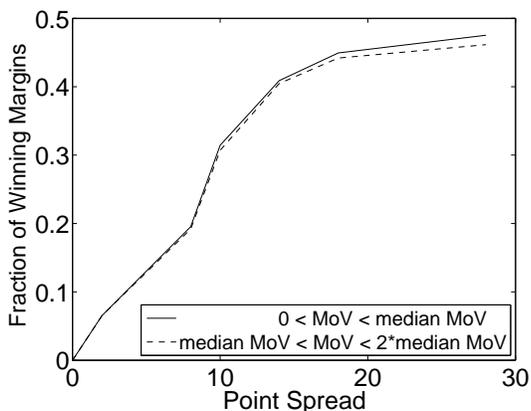
Note: Shot opportunities are characterized by a success probability and a point value. A reservation policy is an expected point value (success probability times point value) above which an optimizing offense is predicted to attempt an available shot. The solid line on each figure depicts the optimal reservation policy by possession duration consistent with the estimated structural parameters. The dashed line on each plot depicts the predicted average point value of attempted shots (shots with expected point values exceeding the reservation level). The scatter plot depicts the empirical average points per attempted shot against possession duration for first-half possessions included in the estimation sample. The points marked with x's depict two-point attempts and the points marked with o's depict three-point attempts. Source: author's calculations using play-by-play data from statsheet.com merged to point-spread data from covers.com.

Figure 3.7 Predicted Scoring Drift Across Game States

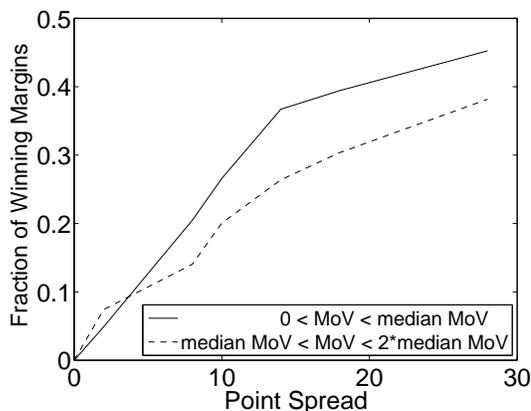


Note: The plotted curves depict the predicted average drift in the favorite's lead over a pair of possessions (one for the favorite one for the underdog) across states. Game states are characterized by ϕ , the marginal rate of substitution between time and the favorite's points. Source: author's calculations using estimated model parameters.

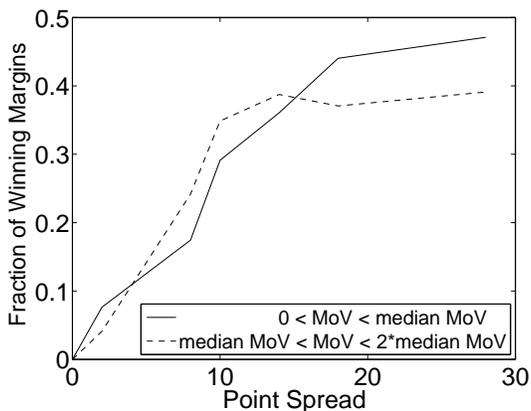
Figure 3.8 False Experiments - Skewness Based Test for Point Shaving Applied to Simulated Outcomes



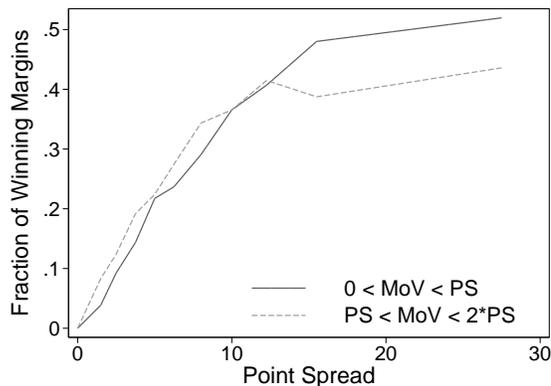
(k) Simulations Assuming Offense Maximizes Points in all Possessions



(l) Simulations Assuming Offense Plays Optimal End-of-Game Strategy



(m) Simulations Assuming Offense and Defense Play Optimal End-of-Game Strategy



(n) Empirical Pattern

Note: The solid line on each plot provides the predicted probability that the favorite's winning margin falls between zero and the median of the winning margin distribution. The dashed line on each plot provides the predicted probability that the favorite's winning margin falls between the median of the winning margin distribution and twice the median of the winning margin distribution. Source: (a-c) author's simulations calibrated with estimated model parameters, and (d) author's calculations using play-by-play data from statsheet.com merged to point-spread data from covers.com.

Appendix I

Proof of Proposition 2: Under the proposition's premise, it follows immediately from the first order condition in equation (43) that $(\sigma_{A2} + \sigma_{B2})$ does not vary with X_1 . The strategic adjustments of A and B exactly offset.

Consider how $E(X_2|X_1)$ varies with X_1 . Totally differentiating the first order condition in Equation (43) and rearranging terms finds,

$$\frac{\partial \sigma_{A2}}{\partial X_1} = \frac{1}{(\sigma_{A2} + \sigma_{B2})\mu''(\sigma_{A2})} \quad \text{and} \quad \frac{\partial \sigma_{B2}}{\partial X_1} = \frac{-1}{(\sigma_{A2} + \sigma_{B2})\mu''(\sigma_{B2})}$$

Using these expressions, one can then express,

$$\begin{aligned} \frac{\partial E(X_2|X_1)}{\partial X_1} &= \frac{\partial \mu(\sigma_{A2})}{\partial \sigma_{A2}} \frac{\partial \sigma_{A2}}{\partial X_1} - \frac{\partial \mu(\sigma_{B2})}{\partial \sigma_{B2}} \frac{\partial \sigma_{B2}}{\partial X_1} \\ &= \frac{\mu'(\sigma_{A2})}{(\sigma_{A2} + \sigma_{B2})\mu''(\sigma_{A2})} + \frac{\mu'(\sigma_{B2})}{(\sigma_{A2} + \sigma_{B2})\mu''(\sigma_{B2})} \\ &= \mu'(\sigma_{A2}) \left(\frac{1}{(\sigma_{A2} + \sigma_{B2})\mu''(\sigma_{A2})} - \frac{1}{(\sigma_{A2} + \sigma_{B2})\mu''(\sigma_{B2})} \right) \end{aligned}$$

Under the symmetry assumption, the bracketed term is equal to zero, and the proposition follows.

Proof of Proposition 3: Again, make use of the expression,

$$\frac{\partial E(X_2|X_1)}{\partial X_1} = \mu'(\sigma_{A2}) \left(\frac{1}{(\sigma_{A2} + \sigma_{B2})\mu''(\sigma_{A2})} - \frac{1}{(\sigma_{A2} + \sigma_{B2})\mu''(\sigma_{B2})} \right)$$

If for any $\sigma' < \sigma^*$ and $\sigma'' > \sigma^*$ with $u'(\sigma') = -u'(\sigma)$ that $|u''(\sigma')| < |u''(\sigma'')|$ (where σ^* is the action that maximizes $\mu(\cdot)$), then $\mu'(\sigma_{A2})$ will be opposite in sign from the term in brackets. Conversely if for any $\sigma' < \sigma^*$ and $\sigma'' > \sigma^*$ with $u'(\sigma') = -u'(\sigma)$ that $|u''(\sigma')| > |u''(\sigma'')|$, then $\mu'(\sigma_{A2})$ will be the same sign as the term in brackets. Therefore, the proposition holds.

Proof of Proposition 4: By Zwet (1964) a random variable with distribution function G is more right skewed than another random variable with distribution function H if $G^{-1}(H(x))$ is convex in x . Let H be the CDF of X_1 and let G be the CDF of $X_1 + E(X_2|X_1)t$. Then $G^{-1}(H(x)) = x + E(X_2|X_1 = x)$, and, because the skewness of X_1 is zero, the random variable $X_1 + E(X_2|X_1)$ is right skewed if $E(X_2|X_1 = x)$ is convex in x . By the same reasoning, the random variable $X_1 + E(X_2|X_1)$ is left skewed if $E(X_2|X_1 = x)$ is concave in x .

Appendix 2: Data

The data used in this study come from two sources. The first data source is a compilation of detailed play-by-play records for 8,102 regular season basketball games played between November, 2003 and March, 2008 downloaded from the website statsheet.com. The raw play-by-play data consists of a list of game events for each team (i.e. shot attempts, rebounds, and turnovers) and, for each event, the time remaining in the game when the event occurred. These 8,102 games exclude games for which the play-by-play record is insufficiently detailed to allow an accurate calculation of the time elapsed from the shot clock at the conclusion of each possession¹⁰⁵. The second data source is a set of gambling point spreads for 24,868 regular-season games played during the same time period. This data is publicly available from the website covers.com. Merging these two data sources by the two team names and the game's date shows that the intersection contains 5,258 games. Data from those 5,258 games are used to construct a data set for analysis.

From these raw data, I construct a data set containing one observation per possession. The set of information for each possession includes the team that was on offense, the time that elapsed from the shot clock

¹⁰⁵In particular, some games not selected for the sample do not contain a record of turnovers.

during the possession, and the event that caused the possession to end. The key piece of information that must be computed is the time elapsed from the shot clock. Beginning with the raw play-by-play records, I use the following steps.

1. Classify each play-by-play observation in to one of the categories; made two point shot, missed two point shot, made three point shot, missed three point shot, turnover, rebound, foul, free throw attempt, assist, blocked shot, substitution, or timeout using keyword searches of the record's raw text string.
2. Drop events that are not relevant to the model's estimation and do not impact the shot clock. These include assists, blocked shots, substitutions, and timeouts. The events that are not dropped retain all information relevant to estimation.
3. Flag events that reset the shot clock. These include made shots, fouls, turnovers, and rebounds.
4. For each event, compute the time elapsed from the shot clock when the event occurred as the difference between the game time at which the event occurred and the game time at which the shot clock was most recently reset¹⁰⁶.
5. Reshape the dataset from one containing one observation per event to one containing one observation per possession. For possessions including multiple events, maintain the record of the event sequence that occurred during the possession, and the possession's duration. The possession's duration is the time elapsed from the shot clock at the time of the possession's final event.
6. Code each possession to one of the model's terminal events. For possessions ending with a turnover, a made or missed two point shot, or a made or missed three point shot, this coding is straightforward. In other cases, use the following rules. Possessions in which a defensive foul was committed during a two point shot attempt are coded as successful two-point shots. Possessions in which a defensive foul was committed during a three-point shot attempt are coded as successful three-point shots. Possessions that end with an offensive foul are coded as turnovers. Shot clock violations are coded as turnovers occurring in the final second of the shot clock.
7. Drop possessions for which this procedure computes a possession duration exceeding the maximum possible duration (by the shot clock rule) of 35 seconds.¹⁰⁷
8. Exclude possessions from the analysis if they have duration of five seconds or less.
9. Exclude possessions from the analysis if they end with a defensive foul for which no free throws are awarded.

I create a second dataset containing one observation per observed free throw. Each observation in this dataset describes the point spread of the game in which the free throw occurred, the team that attempted the free throw, the game time at which the free throw occurred, and the result of the free throw.

Appendix 3: Simulations

I simulate the distribution of winning margins in three steps. First, I numerically solve the full model calibrated by the estimated structural parameters to obtain teams optimal policy functions. Second, I used the optimal policy function to compute a transition matrix describing the probability of achieving each possible winning margin conditional on each possible half-time score differential. Third, I apply these transition probabilities to a smoothed version of the empirical halftime distribution. The smoothed distribution is a discretized normal distribution with the same first and second moments as the empirical distribution. This

¹⁰⁶For possessions following a made shot by the opponent, I set the time at which the shot clock was reset to four seconds after the game time at which the shot was made. Following a made shot, the next shot clock does not begin counting until the team to be on offense carries the ball out of bounds and passes the ball in. This procedure typically takes several seconds.

¹⁰⁷This procedure coded 1.21 percent of observations to durations exceeding 35 seconds.

procedure ensures that any observed asymmetry in the simulated winning margin distribution comes from simulated second-half play.

I numerically compute the solution to the full value function $V : \Omega \rightarrow [0, 1]$, mapping each element of the full state space to a probability of victory using backward induction according to equation (45), and I obtain the corresponding optimal policy function $P : \Omega \rightarrow A^d \times A^o$. Backward induction is feasible if the payoff functions U^i provide a fixed boundary condition at the game's conclusion: time T . However, in NCAA basketball games, a five-minute overtime period is played whenever the game is tied at the end of play. To facilitate backward induction, I impose that each team receives a payoff of 0.5 when the game ends with a tie when I compute the numerical solution to the full model using backward induction for the entire second half, which contains 1200 seconds.. Then, in the simulations I allow five additional minutes (using teams' policies for the final five minutes) to be played when the score is tied at the end of the game and repeat until the game ends with a winner and a loser.

I handle several details of the simulation process as follows. I discretize the shot opportunity distributions, F^o , using ten points of support, so that I can use discrete dynamic programming methods. In states in which one team has a very large lead and therefore the winning team is not in doubt, $V = 0$ or 1 ¹⁰⁸, I imposed that the offensive team reverts to maximizing expected points per possession and the defensive team chooses not to foul¹⁰⁹. The model does not make a unique prediction in these states, because many policies yield the same expected payout when the winner is not in doubt.

¹⁰⁸The winning team is no longer in doubt in the context of the model when insufficient time remains for the trailing team to come from behind even given the most fortuitous possible string of events.

¹⁰⁹One could imagine other possible policies adopted by teams when the game's winner is no longer in doubt. These policies are consistent with rational play if, for very small $\epsilon > 0$, each team placed a weight of $(1 - \epsilon)$ on the discrete outcome of winning and a weight of ϵ on the margin of victory/defeat.

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