Essays in Labor Economics

HARVARD UNIVERSITY
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Essays in Labor Economics

A dissertation presented by

Will Sinclair Dobbie

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Essays in Labor Economics

ABSTRACT

This dissertation consists of three essays in the area of Labor Economics.

The first essay uses 500,000 bankruptcy filings matched to administrative tax records to estimate the impact of Chapter 13 bankruptcy protection on subsequent earnings and mortality. Exploiting the random assignment of bankruptcy filings to judges, we find that Chapter 13 protection increases annual earnings by $5,012, increases employment by 3.5 percentage points, and decreases five-year mortality by 1.9 percentage points. We conclude by using our reduced form estimates to calibrate a general equilibrium model of the credit market, finding that the benefits of consumer bankruptcy are an order of magnitude larger than previously estimated.

The second essay attempts to better understand why market failures may exist in subprime credit markets. In this essay, my coauthors and I exploit sharp discontinuities in loan eligibility to test for moral hazard and adverse selection in the payday loan market. Both regression discontinuity and regression kink approaches suggest that payday borrowers are less likely to default when offered a larger loan. Conversely, there is economically and statistically significant adverse selection into larger payday loans when loan eligibility is held constant. Our results are therefore consistent with the view that adverse selection alone can lead to credit constraints in equilibrium, and that future policy should focus on resolving these types of selection problems in order to increase credit supply among the poor.

The third essay asks whether high quality schools are enough to significantly reduce social disparities using survey data from the Promise Academy charter school. Six years after the random admissions lottery, youth offered admission to the Promise Academy middle school score 0.283 standard deviations higher on a nationally-normed math achievement test and are 14.1 percentage points more likely to enroll in college. Admitted females are 12.1 percentage points less likely to be pregnant in their teens, and males are 4.3 percentage points less likely to be incarcerated. We find little impact of the Promise Academy on self-reported health. We conclude with speculative evidence that high-performing schools may be sufficient to significantly improve human capital and reduce risky behaviors among the poor.

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1. INTRODUCTION

This dissertation consists of three papers relating to the field of Labor Economics. The first two papers examine the subprime credit market, first through an examination of the consumer bankruptcy system, and second through the measurement of information asymmetries in payday lending. The third paper estimates the impact of a high-quality charter school on non-test score outcomes such as teen pregnancy, teen incarceration, and college enrollment. The common thread throughout this work is a focus on the causes and consequences of poverty in America using the classic tools of the Labor Economics literature. My work is also characterized by the use of new datasets compiled specifically for the question at hand, and an emphasis on policy relevant questions.

The first paper, coauthored with Jae Song at the Social Security Administration, uses 500,000 bankruptcy filings matched to administrative tax records to estimate the impact of Chapter 13 bankruptcy protection on subsequent earnings and mortality. Consumer bankruptcy is one the largest social insurance programs in the United States, but little is known about its impact on debtors. In this paper, we exploit the fact that most U.S. bankruptcy courts use a blind rotation system to assign cases to judges, effectively randomizing filers to judges within each court. Moreover, while there are uniform criteria by which a judge may dismiss a bankruptcy filing, there is significant variation in the interpretation of these criteria across judges. As a result, otherwise identical filers are assigned to judges with substantially different rates of granting bankruptcy protection. Exploiting this random assignment of bankruptcy filings to judges, we find that Chapter 13 protection increases annual earnings by $5,012, increases employment by 3.5 percentage points, and decreases five-year mortality by 1.9 percentage points. We conclude by using our reduced form estimates to calibrate a general equilibrium model of the credit market, finding that the benefits of bankruptcy are nearly 20 times larger than previously estimated. The results from this paper provide new evidence that bankruptcy is likely to benefit disadvantaged households, and that recent attempts to restrict bankruptcy filing may have important adverse consequences.

The second paper, coauthored with Paige Marta Skiba at Vanderbilt University, explores the empirical relevance of information asymmetries in a subprime consumer lending market. Theory has long emphasized the importance of private information in explaining credit-market failures, yet there is little evidence of which asymmetries are most important. In this paper, we exploit discontinuities in loan eligibility to test for moral hazard and adverse selection in the payday-loan market. Regression-discontinuity and regression-kink approaches suggest that payday borrowers are *less* likely to default on larger loans. A $50 larger payday loan leads to a 17 to 33 percent drop in the probability of default. Conversely, there is economically and statistically significant adverse selection into larger payday loans when loan eligibility is held constant. Payday borrowers who choose a $50 larger loan are 16 to 47 percent more likely to default. Given the emphasis placed on moral hazard by policymakers and within the theoretical literature, the results of this paper are somewhat surprising. These results also suggest that more emphasis should be placed on screening strategies or information sharing in the effort to expand credit access among disadvantaged households.

The third paper, coauthored with Roland Fryer at Harvard University, estimates the effects of a high-performing charter school on human capital, risky behaviors, and health outcomes using survey data from the Promise Academy in the Harlem Children's Zone. We find that, six years after the random admissions lottery, youth offered admission to the Promise Academy middle school score 0.283 standard deviations higher on a nationally-normed math achievement test and are 14.1 percentage points more likely to enroll in college. Admitted females are 12.1 percentage points less likely to be pregnant in their teens, and males are 4.3 percentage points less likely to be incarcerated. We find little impact of the Promise Academy on self-reported health. We also find that the cross-sectional correlation between test scores and adult outcomes may understate the true impact of a high quality school, suggesting that high quality schools change more than cognitive ability, and that the return on investment for high-performing charter schools could be much larger than that implied by the short-run test score increases. Taken as a whole, our results are consistent with those that argue that high-performing charter schools are effective at implementing educational "best-practices" – frequent teacher feedback, data-driven instruction, an extended school day and year, and a relentless focus on achievement – which develop basic skills that lead to both gains on short-run state test scores and longer-term non-tested measures.

2. DEBT RELIEF AND DEBTOR OUTCOMES: MEASURING THE EFFECTS OF CONSUMER BANKRUPTCY PROTECTION

With Jae Song, Social Security Administration

> "The Bankruptcy Act is...of public as well as private interest, in that it gives to the honest but unfortunate debtor...a new opportunity in life and a clear field for future effort, unhampered by the pressure and discouragement of pre-existing debt."
>
> - U.S. Supreme Court, *Local Loan Co.* v. *Hunt*, 292 U.S. 234 (1934)

2.1. Introduction

In 2010, 1.5 million Americans filed for over $450 billion in debt relief through the bankruptcy system.[1] American households now receive more resources through bankruptcy than from Temporary Assistance for Needy Families and state unemployment insurance programs combined (Lefgren, McIntyre, and Miller 2010), with nearly one in ten American households having filed for bankruptcy as of the late 1990s (Stavins 2000). The U.S. bankruptcy system is also considered more flexible than is typical, allowing debtors to choose between Chapter 7, which provides debt relief and garnishment protection in exchange for a debtor's non-exempt assets, and Chapter 13, which adds asset protection in exchange for the partial repayment of debt.

Despite providing billions in debt relief each year, it is not clear how bankruptcy protection impacts debtors. In theory, bankruptcy protection increases a debtor's incentive to work and insures against any sharp drops in consumption that may have important long-term consequences, such as becoming sick through lack of medical care or losing a home through foreclosure. Yet, in practice, households work about the same number of hours (Han and Li 2007), accumulate less wealth (Han and Li 2011), and have worse credit (Cohen-Cole, Duygan-Bump and Montoriol-Garriga 2009) after receiving bankruptcy protection, leading some to conclude that the benefits

[1] Non-business Chapter 7 and Chapter 13 filing statistics are available at
http://www.uscourts.gov/uscourts/Statistics/BankruptcyStatistics/BAPCPA/2010/Table1A.pdf
http://www.uscourts.gov/uscourts/Statistics/BankruptcyStatistics/BAPCPA/2010/Table1D.pdf

of debt relief have been overstated (Porter and Thorne 2006). The lack of demonstrable benefits, combined with a rapid increase in the number of filings, led Congress to pass the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act, making it more difficult and expensive to file for bankruptcy.

Empirically estimating the impact of bankruptcy protection on debtors has been complicated by two important issues. First, there is little information on the long-term outcomes of most bankruptcy filers. Bankruptcy filers are not tracked in a systematic way after filing, and datasets such as the PSID and NLSY only include a few hundred bankrupt households. Second, selection and endogeneity problems bias most comparisons. Bankruptcy filers are likely to have had worse outcomes even before filing, biasing cross-sectional estimates, and the most proximate causes of bankruptcy also impact later outcomes, biasing within-individual comparisons.

In this paper, we use a new dataset linking 500,000 bankruptcy filings with administrative tax records from the Social Security Administration (SSA) to estimate the causal effect of Chapter 13 bankruptcy protection on subsequent earnings and mortality. Our empirical strategy exploits the fact that most U.S. bankruptcy courts use a blind rotation system to assign cases to judges, effectively randomizing filers to judges within each court. Moreover, while there are uniform criteria by which a judge may dismiss a bankruptcy filing, there is significant variation in the interpretation of these criteria across judges (Sullivan, Warren, and Westbrook 1994, Norberg and Compo 2007, Chang and Schoar 2008). As a result, otherwise identical filers are assigned to judges with substantially different rates of granting bankruptcy protection.[2]

Using these differences in judge discharge rates as an instrumental variable for bankruptcy protection, we are able to identify the ex-post impact of Chapter 13 on the marginal recipient – filers whose bankruptcy decision is altered by the assigned judge due to disagreement on whether or not they should receive debt relief. The identified parameter holds fixed any ex-ante impacts of

[2]We are unable to estimate the impact of Chapter 7 bankruptcy protection using judge assignment, as there is relatively little variation in the treatment of Chapter 7 cases. In Appendix B, we use an event study design to show that filers granted protection under Chapter 7 earn $1,048 more each year, are 6.3 percentage points more likely to be employed, and are 1.45 percentage points less likely to be deceased after five years, compared to filers dismissed under Chapter 7.

bankruptcy, such as over-borrowing, moral hazard in the workplace (White 2011), entrepreneurial risk-taking (Fan and White 2003, Armour and Cummings 2008), or the crowding out of formal insurance (Mahoney 2010). Our empirical strategy is therefore similar to Kling (2006), who uses the random assignment of criminal cases to judges to estimate the ex-post impact of sentence length on earnings, and subsequent research estimating the ex-post effects of foster care (Doyle 2007, 2008), corporate bankruptcy (Chang and Schoar 2008), temporary-help employment (Autor and Houseman 2010), and Disability Insurance (French and Song 2011, Maestas, Mullen, and Strand forthcoming).

In our empirical analysis, we find compelling evidence that Chapter 13 bankruptcy benefits debtors. Over the first five post-filing years, Chapter 13 protection increases the marginal recipient's annual earnings by $5,012, a 21.7 percent increase from the pre-filing mean. Employment increases by 3.5 percentage points, a 4.2 percent increase from the pre-filing mean. Chapter 13 protection also decreases five-year mortality by 1.9 percentage points, a 47.5 percent decrease from the dismissed filer mean. The effects on mortality are larger for older filers, with an estimated impact on five-year mortality of 17.6 percentage points for filers 60 and older, compared to 2.8 percentage points for filers between 40 and 60 and 0.4 percentage points for filers between 25 and 40. All of the reported results are robust to a wide variety of specifications, and remain large and precisely estimated up to ten years after filing.

Next, we explore two possible mechanisms through which bankruptcy protection may impact debtors. First, we exploit within- and across-state variation in wage garnishment to assess the importance of the Chapter 13 provision protecting wages from garnishment. We find that the impact of Chapter 13 is sharply increasing in the marginal garnishment rate, with an implied earnings elasticity with respect to garnishment of 2.756. This is consistent with the idea that bankruptcy protection increases the incentive to work by lowering the effective marginal tax rate. Second, we use information from firm EINs to estimate the impact of Chapter 13 on mobility. We find that marginal recipient of Chapter 13 is 19.6 percentage points more likely to work in his or her pre-filing job, 20.0 percentage points more likely to work in the same industry, and 16.7 percentage points more likely to work in the same state. The impacts on stability and mortality are both larger in states with fewer restrictions on home foreclosure, suggesting that the home saving provisions of the bankruptcy code are particularly important in explaining our results. These results are consistent with Chapter 13 significantly reducing the likelihood of economic instability, such as that caused by moving to avoid creditors or after a home foreclosure.

We conclude by using our reduced form estimates to calibrate a general equilibrium model of the credit market. The evaluation of consumer bankruptcy laws has typically involved an assessment of two second-order effects. Specifically, these models assume that bankruptcy provides partial insurance against uncertainty, but at the cost of higher borrowing costs that makes life-cycle smoothing more difficult (e.g. Athreya 2002, Li and Sarte 2006, Livshits, MacGee, and Tertilt 2007, Chatterjee and Gordon forthcoming). However, the existing literature has largely ignored the first-order effect of bankruptcy protection on earnings that we estimate in this paper. Using our reduced form estimates to calibrate a stylized extension of a standard model of the credit market, we find that the benefits of bankruptcy are nearly 20 times larger than previously estimated.

The remainder of the paper is structured as follows. Section 2 provides a brief overview of consumer bankruptcy law in the United States. Section 3 presents a stylized model to formalize our research design. Section 4 describes our data and provides summary statistics. Section 5 presents our earnings and employment results. Section 6 presents our mortality results. Section 7 discusses our results in light of a stylized general equilibrium model, and Section 8 concludes. There are three Web Appendices. Web Appendix A provides additional results. Web Appendix B presents event study estimates of the impact of Chapter 7 and Chapter 13. Web Appendix C details the quantitative model.

2.2. Consumer Bankruptcy in the U.S.

2.2.1. Overview

Bankruptcy is the legal process to resolve unpaid debts. In the United States, individual debtors are allowed to choose between Chapter 7 and Chapter 13 bankruptcy protection.

Under Chapter 7, debtors forfeit all non-exempt assets in exchange for a discharge of all eligible debts and protection from wage garnishment. Nearly all unsecured debts are eligible for discharge under Chapter 7, including credit card debt, installment loans, medical debt, unpaid rent and utility bills, tort judgments, and business debt. Student loans, child support obligations, and debts incurred by fraud cannot be discharged under Chapter 7, and secured debts such as mortgages, home equity loans, and automobile loans can only be discharged if debtors give up the collateral.

Under Chapter 13, filers propose a three- to five-year plan to repay part of their unsecured debt in exchange for a discharge of any remaining eligible debts, wage garnishment protection, and the retention of any non-exempt assets included in the repayment plan. Chapter 13 also allows debtors to retain any assets pledged as collateral if the collateral amount is repaid in the plan. In practice, Chapter 13 is most often used as a home saving procedure, with 71 percent of filers including mortgage arrears in their repayment plans (White and Zhu 2010), and approximately the same number reporting that avoiding foreclosure is their most important goal in bankruptcy (Porter 2011). In comparison, 41 percent of Chapter 13 filers include car loans in their repayment plans, 38 percent include priority debt, and 0.5 percent include student loans (White and Zhu 2010).[3]

Under either Chapter, a randomly assigned bankruptcy judge decides any and all matters connected to a case, including whether or not to dismiss a filing.[4] The most common reason a filing is dismissed is that it constitutes a "substantial abuse" of the bankruptcy process, which is typically interpreted as meaning that a debtor is able to repay his or her debts without bankruptcy protection. Other commonly cited reasons for dismissal include a repayment plan being infeasible given a debtor's income constraints, a repayment plan paying too little to creditors, or a filing missing

[3]One additional difference between Chapter 7 and Chapter 13 is that dismissed Chapter 7 filers are not allowed to refile under any Chapter for at least six years, while dismissed Chapter 13 filers are allowed to refile for bankruptcy after only 180 days. While almost no dismissed Chapter 13 filers refile under Chapter 13, approximately 20 percent refile under Chapter 7. Dismissed Chapter 13 filers who refile under Chapter 7 tend to be those with fewer assets and higher debts.

[4]Procedurally, U.S. bankruptcy courts typically use a random number generator or blind rotation system to assign filings within a court. For example, Rule 1073-1 of the bankruptcy code of the Minnesota Assignment of Cases states "[e]ach case shall be assigned to a judge by random allocation as determined by order of the judges. Unless otherwise ordered, the judge assigned to the case shall thereafter hear all matters and preside at all times in the case. All adversary proceedings arising in or related to the case shall be assigned to the same judge."

important information (Hynes 2004).

Creditors have a number of options to collect unpaid debts after a filing is dismissed, such as sending letters or making telephone calls, visiting the debtor at home or work, or seizing assets through the courts (Dawsey, Hynes, and Ausubel 2009). Creditors may also collect unpaid debts by garnishing a portion of the debtor's wages. Federal law typically restricts the weekly total of most garnishments to be disposable earnings minus 30 times the federal minimum wage, with a cap at 25 percent of disposable earnings.[5] Debtors can make these collection efforts more difficult by ignoring collection letters and telephone calls, changing their telephone number, or moving without leaving a forwarding address. Debtors can also leave the formal banking system to hide their assets from seizure, or change jobs to force creditors to reinstate a garnishment order.

2.2.2. Potential Benefits of Bankruptcy Protection

There are at least two theories for why debtors might benefit from bankruptcy protection. First, bankruptcy protection increases the incentive to work by protecting future wages from garnishment that can total 25 percent of a debtor's disposable earnings, and up to 100 percent of a debtor's marginal earnings. Indeed, in *Local Loan Co.* v. *Hunt* (1934), the Supreme Court argued that increasing the incentive to work is "[o]ne of the primary purposes of the Bankruptcy Act," as "[f]rom the viewpoint of the wage earner, there is little difference between not earning at all and earning wholly for a creditor."

The second reason debtors may benefit from bankruptcy protection is a reduction in economic instability. Bankruptcy protection discharges most debts, allows debtors to repay mortgage arrears, and puts a hold on almost all debt collection efforts. As a result, debtors are less likely to be forced out of their home, either through foreclosure or eviction, and will have less incentive to strategically move across state lines or change jobs to avoid creditors. Bankruptcy protection may also reduce instability by helping debtors to avoid any sharp drops in consumption that have important long-

[5]Federal law allows garnishments of up to 50 percent of a debtor's disposable earnings for payment related to child support or alimony if the worker is supporting another spouse or child, and up to 60 percent if the worker is not. An additional five percent may be garnished for court order payments more than 12 weeks in arrears.

term consequences, such as becoming sick through lack of medical care or losing one's car through repossession. Finally, bankruptcy protection may reduce the psychic and time costs associated with excessive debt, both of which may impact a debtor's health or ability to stay employed.

There are also many reasons to believe that bankruptcy protection will have little impact on debtors. It is plausible that labor supply is highly inelastic for poor households, or that debt relief will reduce the incentive to work through the increase in income. It is also possible that debtors are able to avoid most debt collection efforts at a relatively low cost, or that creditors do not garnish a significant enough fraction of the marginal recipient's wages to impact employment decisions. Bankruptcy filers may also be in distress due to important underlying issues, such as low human capital or poor health, that the bankruptcy system is unable to remedy.

2.3. Model and Research Design

In this section, we develop a stylized bankruptcy and labor supply model to formalize our estimation strategy and identification assumptions. We simplify the model by assuming a single debt relief program and predetermined debt. Our model is therefore unable to shed light on any ex-ante impacts of bankruptcy or the interplay between Chapter 7 and Chapter 13.

Setup. Individuals are endowed with identical debts D, and an idiosyncratic disutility of work θ that captures differences in ability across individuals. We assume that $\theta \sim [0,\ \bar{\theta}]$, and that θ is known by the individual but only partially observable to the bankruptcy court.

In the first period of the model, individuals choose whether or not to file for bankruptcy protection at cost F that captures any psychic or monetary costs of filing. Individuals pay F whether bankruptcy protection is granted or not, and whether they choose to work or not. Individuals receive a full discharge of debt if bankruptcy protection is granted, but must repay their debts if it is not. Conditional on filing, the probability of receiving bankruptcy protection is equal to $p(\theta)$. We assume that $p(\theta)$ is increasing in θ to capture the idea that bankruptcy judges dismiss filings from individuals who are able to repay their debts outside the bankruptcy system (e.g. filings that are a "substantial abuse" due to the filer's high ability).

In the second period, individuals receive or do not receive bankruptcy protection. Individuals then choose whether to work at wage W, or to not work and receive C in social welfare. If individuals leave the labor market, they cannot be made to repay their debts. Dropping out of the labor market is therefore a different type of debt relief. We also assume that wage earnings pay a lump sum tax τ that finances both debt relief and social welfare payments.

An individual's utility is equal to earnings from social welfare C or wages W minus disutility of work θ, debt D, filing costs F, and a lump sum tax τ. We assume that $W - \bar{\theta} - \tau - F \geq C$ so that all individuals prefer to work if they are given debt relief. There are therefore three utility levels to consider: $U_W(\theta) = W - \theta - D - \tau$ for workers not receiving bankruptcy protection, $U_{WB}(\theta) = W - \theta - \tau$ for workers receiving bankruptcy protection, and $U_N(\theta) = C$ for individuals who are not working and not receiving bankruptcy protection.

Given the utility functions of workers and non-workers, we can analyze which individuals prefer working to not working if they do not receive bankruptcy protection, and, taking this decision as given, which individuals prefer filing for bankruptcy to not filing.

Proposition 1: There exists a cutoff θ_W that equates the utility of working without debt relief with the utility of not working:

$$U_W(\theta_W) = U_N$$

which implies $\theta_W = C - W + \tau - D$. Thus, individuals with $\theta \leq C - W + \tau - D$ will work even if their bankruptcy filing is dismissed, while individuals with $\theta > C - W + \tau - D$ will not work if they do not receive debt relief. This result is the well-known debt overhang problem, where debt distorts the ex-post decision of whether to exert costly effort or not (e.g. Krugman 1988).

Proposition 2: There exists a cutoff θ_F that equates the expected utility of filing for bankruptcy protection with not filing:

$$p(\theta_F)U_{WB}(\theta_F) - (1 - p(\theta_F))U_W(\theta_F) - F = U_W(\theta_F)$$

Since the utility of working with no debt is strictly greater than the utility of working with debt, the threshold for filing for bankruptcy protection is lower than the threshold for work.[6] Thus we have $\theta_F < \theta_W$, and all individuals on the margin of filing will work even if their filing is dismissed. These individuals compare the expected benefits of filing $p(\theta)D$ with the fixed filing costs F, holding fixed the work decision. Individuals will therefore file if $p(\theta)D \geq F$, and not file if $p(\theta)D < F$.

Figure 1 illustrates individuals' labor supply and filing choices, with $P(\theta)$ defined as the fraction of the population with disutility of labor θ who would be granted bankruptcy protection had they filed. Very productive individuals ($\theta < \theta_F$) work and never file for bankruptcy protection, as the expected benefit is too low to justify the fixed filing costs ($p(\theta)D < F$). Individuals who are slightly less productive file for debt relief, but will work regardless of the filing outcome as $U_W - F \geq U_N - F$. In contrast, individuals with $\theta > \theta_W$ will work only if they receive debt relief.

We now turn to the bankruptcy judge and the optimal allocation of debt relief. We assume that each bankruptcy judge j observes a noisy but unbiased signal of the disutility of labor $\hat{\theta}_i^j = \theta_i + \eta_{ij}$, where η_{ij} is assumed to be i.i.d. within and across judges. The problem for the judge is to choose an allocation of debt relief that maximizes the sum of all individual utilities, subject to an economy wide resource constraint equating government revenues and expenditures. Given the above setup, this is equivalent to choosing a cutoff value of observed ability $\hat{\theta}_B$, such that all filers with $\hat{\theta} \geq \hat{\theta}_B$ receive bankruptcy protection and all filers with $\hat{\theta} < \hat{\theta}_B$ do not.

Proposition 3: There exists a cutoff $\hat{\theta}_B$ that maximizes the sum of all individual utilities:

$$\begin{aligned}\int_0^{\theta_F(\hat{\theta}_B)} U_W \, dF(\theta) \quad &+ \quad \int_{\theta_F(\hat{\theta}_B)}^{\theta_W} [p(\theta(\hat{\theta}_B))(U_{WB} - F) + (1 - p(\theta(\hat{\theta}_B)))(U_W - F)] \, dF(\theta) \\ &+ \quad \int_{\theta_W}^{\bar{\theta}} [p(\theta(\hat{\theta}_B))(U_{WB} - F) + (1 - p(\theta(\hat{\theta}_B)))(U_N - F)] \, dF(\theta)\end{aligned}$$

[6]To simplify the model, we assume that all individuals with $\theta > \theta_F$ file for bankruptcy protection. This condition holds if $p(\theta)$ is concave and $p(\bar{\theta})(W - C - \bar{\theta} - tau) \geq F$. These conditions ensure that $p(\bar{\theta})$ is sufficiently high and that the disutility of work $\bar{\theta}$ is sufficiently low that no one prefers to drop out of the labor force immediately over filing for debt relief.

subject to a budget constraint that equates tax revenue from τ with expenditures on debt relief D and welfare payments C:

$$\int_0^{\theta_F(\hat{\theta}_B)} \tau \, dF(\theta) + \int_{\theta_F(\hat{\theta}_B)}^{\bar{\theta}} (1 - p(\theta(\hat{\theta}_B))) \, \tau \, dF(\theta) \; = \int_{\theta_F(\hat{\theta}_B)}^{\bar{\theta}} p(\theta(\hat{\theta}_B)) \, D \, dF(\theta) + \int_{\theta_W}^{\bar{\theta}} (1 - p(\theta(\hat{\theta}_B))) \, C \, dF(\theta)$$

Intuitively, the choice of cutoff $\hat{\theta}_B$ equates two opposing forces. On one hand, a more lenient cutoff reduces social welfare payments and increases tax revenues by inducing individuals with $\theta > \theta_W$ to work. On the other hand, a more lenient decision rule increases debt relief payments to individuals with $\theta \leq \theta_W$ who would have worked without debt relief.

This tradeoff is particularly clear when the bankruptcy judge observes true ability θ. When θ is fully observable, the sum of individual utilities is maximized by only providing debt relief to individuals who would otherwise drop out of the labor market and default on their repayment obligations. That is, by setting $\theta_B = \theta_W$ so that $p(\theta) = 0$ when $\theta < \theta_W$, and $p(\theta) = 1$ when $\theta \geq \theta_W$. Thus the judge provides debt relief to incentivize individuals with a high disutility of work to stay in the labor market and contribute to the tax base, without giving debt relief to individuals who would have worked anyway.

Estimation. Our objective is to estimate the causal impact of Chapter 13 bankruptcy protection on filer outcomes Y:

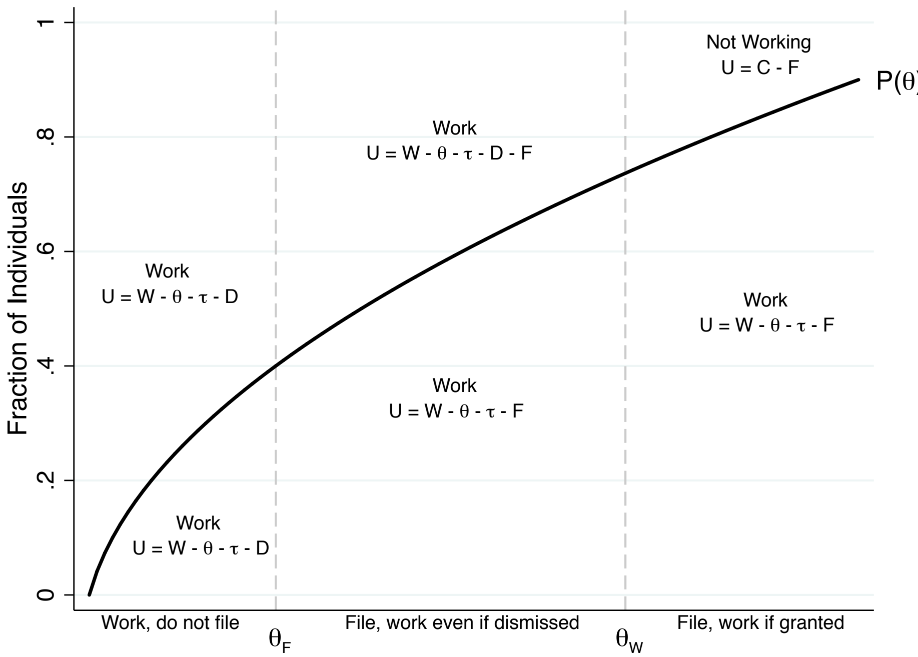
$$\gamma = E[Y|B = 1] - E[Y|B = 0]$$

where γ is the causal effect of interest conditional on filing, and B is an indicator for having received bankruptcy protection. In the context of our model, this treatment effect is equal to the structural parameter:

$$\gamma = \delta \cdot \frac{\int_{\theta_W}^{\bar{\theta}} dF(\theta)}{\int_{\theta_F}^{\bar{\theta}} dF(\theta)}$$

where δ is the change in labor market outcomes for individuals whose labor supply decisions are affected by bankruptcy protection (e.g. those with $\theta > \theta_W$), $\int_{\theta_W}^{\bar{\theta}} dF(\theta)$ is the proportion of affected filers, and $\int_{\theta_F}^{\bar{\theta}} dF(\theta)$ is the proportion of both affected and unaffected filers. The effect of

Figure 2.1
Labor Supply and Bankruptcy Protection



Notes: This figure summarizes the theoretical model.

bankruptcy protection is therefore increasing in the impact on responders, and the fraction of responders in the filing population.

The problem for inference is that OLS estimates of γ may be biased if bankruptcy protection is correlated with the unobservable determinants of later outcomes: $E[\theta_i|Bankruptcy_i] \neq 0$. It is plausible, for example, that bankruptcy filers had worse outcomes even before filing, biasing cross-sectional estimates with non-filers. Cross-sectonal estimates using dismissed filers as a control group are also likely to be biased, as the bankruptcy judge may be more likely to grant bankruptcy protection to those who are unable to repay their debts outside of bankruptcy protection, while within individual comparisons may be biased by the fact that the most proximate causes of bankruptcy, such as job loss or unexpected medical emergencies, also impact later outcomes.

The key insight of our approach is that this bias can be overcome if the distribution of unobservable characteristics is the same among filers assigned to more and less lenient judges. To see this, recall that judges grant bankruptcy protection to filers who cannot repay their debts outside of bankruptcy, that is, those filers whose disutility of work is above θ_B:

$$\hat{\theta}_{ij} > \hat{\theta}_B$$

where $\hat{\theta}_{ij}$ is composed of an observable component X_i and a partially unobservable component υ_i that is estimated by the bankruptcy judge as $\hat{\upsilon}_{ij}$. To illustrate how variation in judge leniency can identify equation (2.3.), we relax the assumption that $\hat{\upsilon}_{ij}$ is unbiased, and allow each judge's estimate of unobservable ability to be a function of individual i's true ability to repay, and characteristics of the judge assigned to the case, such as previous experience or personal biases:

$$\hat{\upsilon}_{ij} = \upsilon_i + \sigma_j + \eta_{ij}$$

where σ_j is the systematic component of judge j's decision-making that leads her to consistently over- or under-estimate a filer's disutility of labor, and η_{ij} is noise in the decision-making process that is i.i.d. within and across judges.[7] This implies that bankruptcy protection is granted if:

$$X_i + \upsilon_i + \sigma_j + \eta_{ij} > \hat{\theta}_B \tag{1}$$

which implies that judge j's probability of granting bankruptcy conditional on filer i's observable characteristics X_i is:

$$P_j(X_i) = 1 - F_{\upsilon+\eta}(\hat{\theta}_B - X_i - \sigma_j)$$

where $F_{\upsilon+\eta}$ is the cumulative distribution of unobservable ability to repay υ plus the idiosyncratic noise η. Thus, judge leniency σ_j is correlated with the probability of receiving bankruptcy protection, but uncorrelated with unobservable filer characteristics such as υ_i due to the random assignment of filings to judges. This setup implies that any relationship between future outcomes and judge assignment is due to the causal impact of bankruptcy protection.

Formally, we estimate the causal impact of receiving bankruptcy protection through a two-stage least squares regression of equation (2.3.) with judge leniency as an instrumental variable for bankruptcy protection. The second stage equation is:

$$y_{it} = \alpha + \alpha_{ct} + \beta X_i + \gamma Bankruptcy_i + \varepsilon_{it}$$

where i denotes individuals, t is the year of observation, γ is the causal impact of bankruptcy protection defined above, α_{ct} are office by month-of-filing fixed effects, X_i includes race, gender, a quadratic in age, baseline employment, and baseline earnings, and ε_{it} is noise. The first stage equation associated with equation (2.3.) is:

$$Bankruptcy_i = \alpha + \alpha_{ct} + \beta X_i + \pi \sigma_j + \varepsilon_i \tag{2}$$

where π represents the impact of judge leniency on the probability of receiving bankruptcy protec-

[7]The decision problem can also be expressed as one in which estimates of θ are unbiased, but judges use different cutoff values $\hat{\theta}_B^j$ due to pro-creditor or pro-debtor preferences. In this scenario, judge j grants a filing if $X_i + \hat{\upsilon}_i > \hat{\theta}_B + \sigma_j$, where σ_j represents judge specific differences in the optimal cutoff.

tion.

Estimating the first stage regression from equation (2) utilizing an exhaustive set of judge fixed effects as instruments yields a consistent two-stage least squares estimate of γ as the number of filers $i \to \infty$, but is potentially biased in finite samples because each filer's observation is included in the estimation of his own judge's effect on bankruptcy protection and future outcomes. There are several potential solutions to this own-observation issue. Jackknife IV, for example, eliminates the bias by omitting a filer's own observation when forming the instrument (Angrist, Imbens, and Krueger 1999). Split-sample two-stage IV addresses the own-observation issue by randomly splitting the sample into two groups, using judge tendencies in one part of the sample as an instrument for bankruptcy protection in the other part of the sample (Angrist and Krueger 1995). Limited information maximum likelihood (LIML) eliminates the own-observation bias by collapsing the parameter space and using maximum likelihood to obtain a consistent estimate of the effect of bankruptcy protection.

We address the own-observation problem by omitting a filer's own observation when calculating each judge's leniency relative to the court he serves in.[8] Formally, we define judge leniency Z_{icjt} as the leave-one-out fraction of filings granted by judge j in year t, minus the leave-one-out fraction granted in his court c in year t:

$$Z_{icjt} = \frac{1}{n_{cjt} - 1} \left(\sum_{k=1}^{n_{cjt}} (B_k) - B_i \right) - \frac{1}{n_{ct} - 1} \left(\sum_{k=1}^{n_{ct}} (B_k) - B_i \right) \tag{3}$$

where i again denotes individuals, c denotes courts, j is the assigned judge, t is the year of observation, B_i is an indicator for receiving bankruptcy protection, n_{cjt} is the number of cases seen by a judge in year t, and n_{ct} is the number of cases seen by a court in year t. This leave-one-out procedure, which is essentially a reduced-form version of the jackknife IV regression, purges the mechanical correlation between a filer's own outcomes and our measure of judge leniency.

Consistent with past research (Sullivan, Warren, and Westbrook 1994, Norberg and Compo

[8]Estimates using Jackknife IV and LIML are available Web Appendix Table 1. The results are nearly identical to those using Z_{icjt} as an instrument.

2007), we find considerable variation in the treatment of Chapter 13 cases within a court. The filer level standard deviation of Z_{ijct} is 0.025 for Chapter 13 filers in our sample. In other words, moving from the 5th percentile to the 95th percentile in the filer level distribution of judge leniency is associated with a 10.1 percentage point increase in the probability of receiving Chapter 13 protection, a 22 percent increase. There is also significant persistence in judge behavior over time, suggesting that this variation represents systematic differences in judge behavior. Web Appendix Figure 1 plots judge discharge rate against the discharge rate in the previous year, with each point representing a separate year by judge observation. Discharge rates are highly correlated across time, with an OLS regression relating judge discharge rate to the discharge rate in the previous year yielding a coefficient of 0.821.

In contrast to Chapter 13, there is almost no variation in the treatment of Chapter 7 cases across judges within an office, likely because almost all Chapter 7 filings are granted in our sample period. The standard deviation of Z_{ijct} for Chapter 7 filers is only 0.003 in our data, making it difficult to measure the impact of bankruptcy protection for these filers using our instrumental variables strategy. In Web Appendix B, we use an event study design to estimate the impact of Chapter 7, finding it has a modest positive impact on debtors.

Using our reduced form measure of judge leniency Z_{ijct} as an instrumental variable, the identified two-stage least squares parameter from equation (2.3.) measures the causal impact of Chapter 13 protection for the marginal recipient. Our estimates therefore measure the local average treatment effect for filers whose bankruptcy outcome is altered by judge assignment due to disagreement on whether they should receive bankruptcy protection. In the context of our empirical model, γ is identified for filers whose estimated disutility of work $\hat{\theta}$ is sufficiently close enough to the cutoff of receiving bankruptcy protection $\hat{\theta}_B$ that a high or low draw of σ_j will impact the probability of receiving bankruptcy protection.

The conditions necessary to interpret our two-stage least squares estimates as local average treatment effects are: (1) that judge assignment is associated with bankruptcy protection, (2) that judge assignment only impacts debtor outcomes through the probability of receiving bankruptcy protection, and (3) that the impact of judge assignment is monotonic across filers.

The first assumption is empirically testable. Web Appendix Table 2 presents first stage results of the impact of judge leniency on the probability of a filer being granted Chapter 13 bankruptcy protection. The sample includes first time filers between 1992 and 2005 in the 33 courts that randomly assign Chapter 13 filings to judges. The median court in our sample has two judges, with the largest court having eight judges. All specifications control for office by month-of-filing fixed effects, with column 2 adding controls for gender, race, age, and average baseline earnings. Standard errors are clustered at the court level.

Our results from Web Appendix Table 2 show that there is a large and precisely estimated relationship between judge leniency and the probability of receiving bankruptcy protection. While measurement error attenuates the coefficient on Z_{ijct} away from one, our reduced form measure of judge leniency is one of the strongest predictors of whether a filer receives Chapter 13 protection. With no controls, a one percentage point increase in Z_{ijct} increases the probability that a debtor receives bankruptcy protection by 0.700 percentage points. Controlling for gender, race, age, and baseline earnings, a one percentage point increase in Z_{ijct} increases the probability that a debtor receives bankruptcy protection by 0.676 percentage points. Thus, moving from the 5th percentile to the 95th percentile in the filer level distribution of judge leniency increases the likelihood of receiving Chapter 13 bankruptcy protection by to 6.83 to 7.07 percentage points. To put these magnitudes in perspective, our results from Web Appendix Table 2 show that male filers are 1.6 percentage points less likely to receive bankruptcy protection, black filers are 10.0 percentage points less likely to receive bankruptcy protection, and filers with baseline earnings that are \$10,000 lower are about 0.03 percentage points less likely to receive bankruptcy protection.

Our second identifying assumption is that judge assignment only impacts debtor outcomes through the probability of receiving bankruptcy protection. This assumption would be violated if judge leniency is correlated with unobservable determinants of future outcomes: $E[\sigma_j \upsilon_i] \neq 0$. Table 1 presents a series of randomization checks to partially assess the validity of this exclusion restriction. Column 2 reports results from an OLS regression of judge leniency on a filer's age,

Table 2.1
Test of Randomization

	Control Mean	Judge Leniency		F-test p-value
	(1)	(2)	(3)	(4)
Age	36.682	0.000009*	0.000009	[0.166]
	(16.663)	(0.000005)	(0.000015)	
Male	0.570	0.000142	−0.000279	[0.781]
	(0.494)	(0.000093)	(0.000510)	
White	0.462	0.000325*	−0.000075	[0.304]
	(0.498)	(0.000150)	(0.000427)	
Earnings	20.019	−0.000002	−0.000003	[0.232]
	(18.503)	(0.000027)	(0.000031)	
Employment	0.791	0.000655	0.009107	[0.315]
	(0.352)	(0.000399)	(0.008243)	
Self Earnings	0.649	0.000011	0.000010	[0.389]
	(3.188)	(0.000015)	(0.000015)	
401k	0.247	0.000020	0.000024	[0.674]
	(0.746)	(0.000071)	(0.000067)	
Self Emp.	0.065	−0.000141	−0.000112	[0.120]
	(0.187)	(0.000280)	(0.000272)	
Disability Ins.	0.042	0.000342	0.000294	[0.263]
	(0.194)	(0.000259)	(0.000254)	
Job Tenure	2.822	0.000034*	0.000034*	[0.062]
	(3.034)	(0.000020)	(0.000020)	
Firm Wage	18.896	−0.000011	0.000011	[0.532]
	(19.932)	(0.000016)	(0.000013)	
Pred. Earnings	20.230		0.000375	[0.189]
	(13.542)		(0.000413)	
Pred. Emp.	0.444		−0.038486	[0.131]
	(0.147)		(0.037885)	
Pred. Mortality	0.031		−0.013043	[0.214]
	(0.055)		(0.012249)	
Joint F-Test		[0.549]	[0.504]	
Observations	496880	496880	496880	

Notes: This table reports reduced form results testing the random assignment of filings to judges. The sample consists of all first time Chapter 13 filers between 1992 and 2005 in the 33 courts that randomly assign filings to judges. Column 1 reports the control mean and standard deviation for each variable. Columns 2 - 3 each report estimates from an OLS regression of judge leniency on the variables listed in the row and office by month-of-filing fixed effects. Standard errors are clustered at the court level. Judge leniency is the leave-one-out mean rate of granting bankruptcy protection for the assigned judge minus the leave-one-out mean rate of granting bankruptcy protection for the court in the same filing year. The p-value reported at bottom of columns 2 - 3 is for a F-test of the joint significance of the variables listed in the rows. Each row of column 4 reports a p-value from a separate OLS regression of the pre-determined variable listed in the corresponding row on judge and office by month-of-filing fixed effects. The p-value is for a F-test of the joint significance of the judge fixed effects. Predicted earnings, employment, and mortality are formed using the other variables listed in the rows. All monetary values are expressed in real 2000 dollars divided by 1,000. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

gender, race, baseline earnings, baseline self-employment earnings, baseline employment, baseline self-employment, baseline 401k contributions, baseline DI receipt, baseline job tenure, and average wages at the baseline employer. We control for office by month-of-filing fixed effects, and cluster standard errors at the court level. Age, race, and job tenure are related to judge leniency at the ten percent level, though the magnitudes are not economically significant. None of our other baseline variables appear systematically related to judge leniency, and a joint F-test of the hypothesis that all differences in background characteristics and baseline measures in column 1 are zero has a p-value of 0.549.

Columns 3 adds controls for predicted earnings, employment, and mortality. We predict each outcome over the first five post-filing years using gender, race, a quartic in age, baseline employment, and baseline earnings. There are no significant relationships in this pooled specification, and a joint F-test of the hypothesis that all differences in background characteristics and baseline measures in column 6 are zero has a p-value of 0.504.

Column 4 presents results from our final test of random assignment. We regress each demographic characteristic and baseline measure on an exhaustive set of judge fixed effects. Each regression controls for office by month-of-filing fixed effects, and clusters standard errors at the office by month-of-filing level. We report the p-value from a joint F-test of significance on the judge effects, which provides an omnibus test for the null hypothesis that filer covariates do not differ significantly among filers assigned to different judges within a office by month combination. None of the joint F-tests in column 4 suggest that there is systematic non-random assignment of filings to judges.

Our second identifying assumption would also be violated if judge assignment impacts future outcomes through channels other than bankruptcy protection, and these channels were correlated with judge leniency: $E[\sigma_j(\gamma_i - \bar{\gamma})] \neq 0$. If judge assignment does impact future outcomes through other channels, our measure of judge leniency would be correlated with the error term of our two-stage least squares model and the resulting local average treatment effect would incorporate any additional impacts associated with judge assignment. This restriction that judges only systematically affect debtor outcomes through bankruptcy is fundamentally untestable, and our estimates should be interpreted with this potential caveat in mind. With that said, we view this assumption as reasonable in our setting, as bankruptcy judges typically interact with debtors only at the confirmation hearing, with little communication before or after the hearing. Thus, it seems unlikely that judges would confer significant benefits to debtors other than through their bankruptcy ruling.

Our third identifying assumption is that judge assignment has a monotonic impact on filers. The monotonicity assumption implies that being assigned to a more (less) lenient judge does not result in an decrease (increase) in the likelihood of receiving bankruptcy protection. This monotonicity assumption would be invalid if judges treat some filings in a more lenient manner and others in a more strict manner. If the monotonicity assumption is violated, our estimates from equation (2.3.) are still a weighted average of marginal treatment effects, but the weights would be outside the unit interval (Angrist, Imbens, and Ruben 1996, Heckman and Vytacil 2005). To partially test the monotonicity assumption, Web Appendix Figure 2 plots judge leniency measures that are calculated separately for each judge by gender, race, baseline income, and age. We also report the coefficient and standard error from an OLS regression relating the separately calculated judge measures. Consistent with our monotonicity assumption, judges exhibit remarkably similar tendencies across observably different filers. Regressing the judge leniency for male filers on those for female filers yields a point estimate of 0.822. For white and non-white filers, the point estimate is 0.885, for high and low baseline earnings 0.998, and for filers older and younger than 40, the coefficient is 1.038. None of the results suggest that the monotonicity assumption is invalid in our setting.

2.4. Data

To estimate the impact of bankruptcy protection on debtors, we merge information from individual bankruptcy filings and administrative tax records from the Social Security Administration (SSA).

Bankruptcy records are available from 1992 to 2009 for the 72 federal bankruptcy courts that allow full electronic access to their dockets. These data represent approximately three quarters of all bankruptcy filings during this period.[9] Each record in our bankruptcy data contains information on the Chapter filed, filing date, court, office, outcome, and the judge and trustee the filing was assigned to. The data also contain information on each debtors' name, address, and social security number, whether the filing includes any assets, and whether the filing fee was paid immediately or in installments.

Our empirical strategy requires that a court randomly assign filings to judges within an office. This appears reasonable in our setting, as U.S. bankruptcy courts typically use a random assignment or blind rotation system to assign filings. There are two typical reasons that filings are not randomly assigned in our data. First, some offices assign every case from a county to a single judge. Second, cases that are still on a retiring judge's docket are reassigned on the day of retirement. This is problematic both because these cases will not be included in the estimated effect of the retiring judge, and because these cases are far more likely to be successful, as only cases where the repayment plan is in progress remain on a judge's docket.

To ensure the random assignment of filings to judges in our sample, we drop filings originating from counties that send all filings to a single judge, and drop office by year bins where a retiring judge's cases were reassigned and there is no documentation as to the original judge. We also drop courts that assign all Chapter 13 filings to a single judge, as there is no variation in judge behavior for us to exploit. Finally, we restrict the data to first time filers between 1992 and 2005 to ensure that we have five or more years of post-filing outcomes for all debtors, and that all filings occurred before the 2005 Bankruptcy Reform Act came into effect. These restrictions leave us with 506,420 Chapter 13 filings in 33 bankruptcy courts. This data represents just over 30 percent of the available Chapter 13 filings. Web Appendix Table 3 provides additional details on the courts and years in our sample.

To explore the impact of bankruptcy protection on subsequent outcomes, we match the bankruptcy records to administrative tax records from the SSA using last name and the last four digits of the

[9]Our bankruptcy data are drawn from Public Access to Court Electronic Records (PACER) records provided by Tal Gross, Matthew Notowidigdo, and Jialan Wang. Additional details on the PACER data and its coverage are available in Gross, Notowidigdo, and Wang (2012).

debtor's social security number. We were able to successfully match 97.4 percent of the bankruptcy data, with almost all of the unmatched records being the result of an individual sharing a name and last four digits of the social security number with another individual in the SSA data. The SSA data are remarkably complete and include nearly every individual in the United States. Information on earnings and employment comes from annual W-2s. Individuals with no W-2 in any particular year are assumed to have had no earnings in that year. We measure non-earnings outcomes using data from three sources. Information on annual 401k contributions, job location, and firm characteristics also comes from annual W-2s. Information on Disability Insurance (DI) and Supplemental Security Income (SSI) receipt comes from the Master Beneficiary Record. Information on mortality comes from the Death Master File that is compiled by the SSA and covers deaths occurring anywhere in the United States. All dollar amounts are in terms of year 2000 dollars.

Table 2 presents summary statistics for a five percent random draw of all first time filers between 1992 and 2005. Consistent with previous research on bankruptcy filers, 98.3 percent of Chapter 7 filers in sample are granted bankruptcy protection, compared to 47.9 percent of Chapter 13 filers. Fifty-nine percent of Chapter 7 filers are male, 74.2 percent are white, and 13.3 percent are black. For Chapter 13, 61.3 percent of filers are male, 55.3 percent are white, and 34.1 percent are black.

The typical bankruptcy filer earns far less than the average American worker. In the five years before filing, 80.6 percent of Chapter 7 filers are employed on average, with average annual earnings of just $21,064. Eighty percent of Chapter 13 filers are employed, earning $22,460 annually in the five years before filing. Over the same five year time period, 4.8 percent of Chapter 7 filers receive DI, and 9.7 percent receive SSI. Just over four percent of Chapter 13 filers receive DI, and 7.9 percent receive SSI. Consistent with the low individual earnings we observe, average wages at a typical filer's employer are just over $20,000 for both Chapter 7 and Chapter 13 filers.

Table 2 also presents summary statistics for filers in the 33 courts that randomly assign filings to judges. This analysis sample is very similar to the full sample of filers. Forty-five percent of Chapter 13 filers in our sample are granted bankruptcy protection, 2.9 percent less than the full

Table 2.2
Summary Statistics

	Full Sample		Analysis Sample
	Chapter 7	Chapter 13	Chapter 13
Characteristics	(1)	(2)	(3)
Bankruptcy	0.983	0.479	0.455
Age	40.399	40.892	38.112
Male	0.588	0.613	0.591
White	0.742	0.553	0.522
Black	0.133	0.341	0.366
Outcomes 5 years before filing			
Earnings	21.064	22.460	23.005
Employment	0.806	0.802	0.815
Self Emp.	0.060	0.062	0.060
Wages	20.559	21.856	22.433
Self Earnings	0.505	0.604	0.572
401k	0.262	0.293	0.319
Disability Insurance	0.048	0.043	0.041
Sup. Security Income	0.097	0.079	0.088
Job Tenure	3.425	3.746	3.660
Firm Wages	22.413	23.006	23.055
Observations	367103	83552	496880

Notes: This table reports summary statistics. The full sample consists of a 5% random draw from first time filers between 1992 and 2005 in 72 bankruptcy courts. The analysis sample consists of all first time filers between 1992 and 2005 in the 33 courts that randomly assign filings to judges. Bankruptcy is an indicator for being granted bankruptcy protection and receiving a discharge of debt. Earnings and employment outcomes come from 1978 - 2010 W-2s, DI and SSI receipt from the Master Beneficiary File, and mortality from the Death Master File. We restrict non-mortality outcomes to be for individuals who are alive. SSI outcomes are restricted to individuals older than 65 years old. All monetary values are expressed in real 2000 dollars divided by 1,000. Employed is an indicator for non-zero wage earnings. Self employment is an indicator for non-zero self employment earnings, including negative earnings. Firm wages are averaged over all employees listing the same EIN in the same calendar year.

sample. Fifty-nine percent of filers in our sample are male, 2.3 percent less than the full sample, and 52.2 percent are white compared, 3.1 percent less. In the five years before filing, average annual earnings were $23,005 for filers in our analysis sample, slightly more than in the full sample.[10]

2.5. Chapter 13 Bankruptcy Protection and Labor Supply

2.5.1. Results

As a benchmark for evaluating the causal effects described below, we begin with a descriptive analysis of granted and dismissed filers. Web Appendix Figure 3 plots pre- and post-filing earnings for a five-percent random sample of first time Chapter 13 filers between 1992 and 2005 in our full sample of bankruptcy courts. We also calculate expected earnings using a filer's gender, race, a quadratic in age, a quadratic in tenure, industry fixed effects, and earnings in the previous five years.

Filers granted Chapter 13 bankruptcy protection earn $1,500 to $2,000 more than dismissed filers even before filing. Earnings for both groups fall two to three years before filing, with a larger dip for dismissed filers.[11] The post-filing earnings of dismissed filers dip further, falling more than $4,000 below the expected trajectory five years after filing. In contrast to the large and permanent decline in earnings experienced by dismissed filers, individuals granted bankruptcy protection appear to have no long-term earnings losses. Taken together, our descriptive results from Web Appendix Figure 3 suggest that many Chapter 13 filers experience an adverse earnings shock before filing, but that bankruptcy protection may help to mitigate the long-term consequences of

[10] Web Appendix Table 4 presents summary statistics separately by census region. Filers in the south are less likely to be white and more likely to be black compared to filers in the east and midwest. Filers in the west are less likely to be white or black, likely because there are more Hispanic filers. Filers in the west are also the least likely to be granted either Chapter 7 or Chapter 13 bankruptcy protection, and have relatively higher earnings than filers in other regions. There is also significant variation in Chapter 13 discharge rate within regions. The Birmingham office, for example, discharges only 31.0 percent of Chapter 13 filings, while the Columbia office in the District of South Carolina discharges 74.9 percent of Chapter 13 filings. See Web Appendix Table 3 for additional details.

[11] The fall in pre-filing earnings is likely related to the "Ashenfelter dip" - the fact that individuals with negative earnings shocks are more likely to enroll in job training programs - discussed by Ashenfelter (1978), Ashenfelter and Card (1985), and Heckman and Hotz (1989), among many others. In our context, the selection of individuals with negative earnings shocks into bankruptcy filing will lead OLS estimates with a non-filing control group to overstate the true gains of bankruptcy if there is mean reversion in earnings, and to understate the impact of bankruptcy if shocks have consequences that increase over time. We return to this point when discussing the Chapter 7 estimates.

those earnings shocks.

Figures 2A and 2B present two-stage least squares results measuring the causal impact of Chapter 13 bankruptcy protection on earnings and employment using judge leniency as an instrumental variable for bankruptcy protection. The sample consists of first time filers between 1992 and 2005 in the 33 courts that randomly assign filings to judges. We use our reduced form measure of judge leniency Z_{ijct} as an instrumental variable for bankruptcy protection, and control for gender, race, a quartic in age, baseline employment, baseline earnings, and office by month-of-filing fixed effects. Standard errors are clustered at the court level. Table 3 presents results pooling outcomes across the first five post-filing years, and Web Appendix Table 1 presents results using Jackknife IV and LIML.

Figure 2A shows that Chapter 13 bankruptcy protection has a large and precisely estimated impact on earnings. In the year of filing, debtors granted Chapter 13 protection due to a more lenient judge earn \$3,375 more than dismissed filers. In the first full year after filing, these marginal recipients of Chapter 13 earn \$5,528 more than dismissed filers. Pooling outcomes across the first five full post-filing years, the longest period available for all filers, Chapter 13 protection increases the marginal recipient's annual earnings by \$5,012, a 21.7 percent increase from baseline earnings. In a sample of individuals filing between 1992 and 2000, Chapter 13 protection increases the marginal recipient's annual earnings by \$5,089 in the sixth through tenth post-filing years, suggesting that the impact of bankruptcy protection is persistent even after the end of the Chapter 13 repayment plan (see Web Appendix Table 5). The estimated effects are nearly identical using judge fixed effects as an instrument for bankruptcy protection, or when using Jackknife IV or LIML instead of two-stage least squares.[12]

[12]One additional concern is that individuals will be more likely to work in the informal labor market after being dismissed from the bankruptcy system. To partially test this hypothesis, we estimate the impact of Chapter 13 separately by baseline industry. We hypothesize that it is easier on the margin to increase informal earnings in industries such as construction and agriculture as compared to retail trade or health care. In results available upon request, we find no systematic differences in the impact of Chapter 13 across industries that may be more or less likely to use informal workers, though we cannot rule out modest differences.

Figure 2.2
Chapter 13 Bankruptcy and Labor Supply and Mortality

Figure 2.2A: Earnings

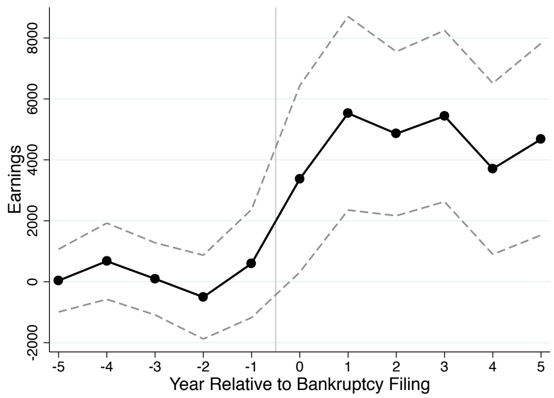


Figure 2.2B: Employment

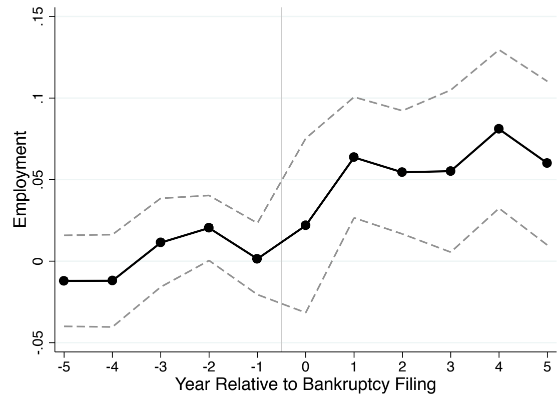
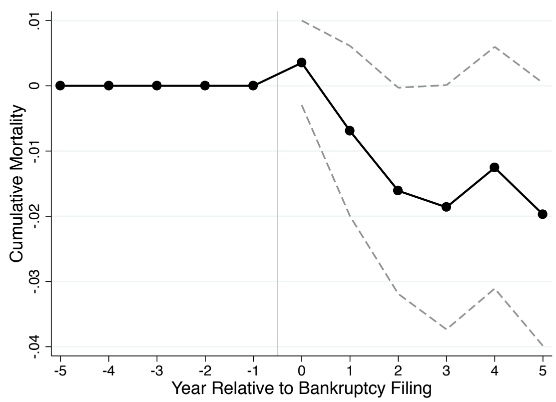


Figure 2.2C: Mortality



Notes: These figures plot two-stage least squares results of the impact of Chapter 13 bankruptcy protection on earnings, employment, and cumulative mortality. The sample includes all first time filings between 1992 and 2005 in courts that randomly assign cases to judges. We instrument for bankruptcy protection using judge leniency and control for gender, race, a quartic in age, baseline employment, baseline earnings, and office by month-of-filing fixed effects. The dashed lines are 95 percent confidence intervals from standard errors clustered at the court level. Year 0 indicates the year a debtor files for bankruptcy protection. Earnings are winsorized at the top and bottom one percent. Employment is an indicator for non-zero wage earnings on the W-2. All monetary values are expressed in real 2000 dollars. Mortality is an indicator for being deceased in the indicated year using information from the Death Master File.

Table 2.3
Chapter 13 Bankruptcy and Labor Supply and Mortality

	CM	2SLS Results	
Panel A: Labor Supply	(1)	(2)	(3)
Earnings	17.362	6.943^{***}	5.012^{***}
	(1.6381)	(1.635)	(1.339)
Employment	0.432	0.063^{***}	0.035^{***}
	(0.218)	(0.017)	(0.013)
Panel B: Mortality			
5-year Mortality	0.040	-0.024^{***}	-0.019^{**}
	(0.197)	(0.009)	(0.009)
Controls	–	No	Yes
Observations	226080	496880	496880

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection on earnings and employment averaged over the first five post-filing years. The sample consists of all first time filers between 1992 and 2005 in the 33 courts that randomly assign filings to judges. Column 1 reports the mean and standard deviation for dismissed filers. Columns 2 - 3 instrument for bankruptcy protection using the reduced form measure of judge leniency described in the text. All specifications control for office by month-of-filing fixed effects, and cluster standard errors at the court level. Column 3 also includes controls for gender, race, age, and the five year average of baseline employment and baseline earnings. All monetary values are expressed in real 2000 dollars divided by 1,000. Earnings information comes from the W-2. Employed is an indicator for non-zero wage earnings on the W-2. Self employment is an indicator for non-zero self employment earnings on the W-2. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

As an additional check of our identification strategy, Figure 2A also plots estimates for five pre-filing years. Consistent with our identifying assumptions discussed above, there is no systematic relationship between bankruptcy protection and earnings in the pre-filing years, with the estimated coefficients small and not statistically different from zero.

Figure 2B shows that there is also a large and precisely estimated effect of Chapter 13 bankruptcy protection on employment. While there is little relationship between bankruptcy protection and employment in the pre-filing period, average employment is 3.5 percentage points higher in the post-filing period for debtors with bankruptcy protection, a 4.2 percent increase from mean employment in the baseline period. The probability of being employed is also higher in the sixth through tenth post-filing years, but the point estimates are very imprecisely estimated.

Web Appendix Table 6 presents results for a number of other outcomes available in our SSA data. Bankruptcy protection increases self-employment by 2.9 percentage points, but does not have an economically or statistically significant impact on self-employment earnings. Among eligible filers, bankruptcy protection decreases the receipt of SSI by 12.2 percentage points, an 89 percent decrease. Bankruptcy protection is also associated with a decrease in the receipt of DI and an increase in 401k contributions, though neither point estimate is statistically significant.

Web Appendix Table 7 presents two-stage least squares results interacted with filer gender, race, age, and baseline earnings for outcomes over the first five post-filing years. Chapter 13 increases annual earnings by $6,968 for filers with above median incomes, compared to only $2,968 for filers with below median earnings. The impact of Chapter 13 protection on employment is also 3.2 percentage points higher for filers with above median baseline earnings. Chapter 13 increases the earnings of filers older than 60 by only a statistically insignificant $2,580, likely because these filers have already left the labor market. In contrast, Chapter 13 increases the annual earnings of filers who are between 25 to 40 years old by $5,759, and the annual earnings of filers who are between 40 and 60 years old by $6,507. Bankruptcy protection also increases annual earnings more for filers who are female and non-white, but the differences are not statistically significant.

To investigate heterogeneous treatment effects across unobservable debtor characteristics, we also estimate marginal treatment effects (MTE) (Heckman and Vytlacil 2005). In our setting, the MTE estimates describe how the outcomes for debtors on the margin of bankruptcy protection change as we move from more strict to more lenient judges, essentially a Wald estimator for a small change in the probability of receiving bankruptcy protection. MTE estimates therefore shed light on the types of filers who benefit most from bankruptcy protection, which may be important for policy. To calculate the MTE function, we predict the probability of bankruptcy protection using a quadratic in our reduced form measure of judge leniency Z_{ijct}. We then regress each outcome on a quadratic in the predicted probability of receiving bankruptcy protection. Finally, we evaluate the estimate of the first derivative of this relationship between each post-filing outcome and the quadratic in predicted probability at each percentile of predicted probability.

Web Appendix Figure 4 reports the MTE estimates for earnings and employment over the first five post-filing years. The MTE function for earnings is increasing in the predicted probability of bankruptcy protection. The upward slope in the earnings MTE suggests that filers on the margin of bankruptcy who are assigned to the most lenient judges experience the largest increases in earnings when granted bankruptcy protection. These are likely filers with unobservable characteristics that make them the least likely to be granted bankruptcy in the first place, since the margin for relatively lenient judges should entail relatively less deserving filers compared to judges who dismiss most filings. This implies that the impact of bankruptcy on earnings is somewhat higher for less deserving debtors. On the other hand, the MTE function for employment outcomes is essentially flat in judge leniency, suggesting that the employment effects do not differ systematically across unobservable characteristics.

2.5.2. Potential Channels

Why are there such large benefits of receiving bankruptcy protection? In this section, we explore two potentially relevant explanations: (1) protection from wage garnishment, and (2) protection against episodes of economic instability.

One explanation for our results is that Chapter 13 increases the incentive to work by protecting future wages from garnishment that can total 25 percent of a debtor's disposable earnings, and up to 100 percent of a debtor's marginal earnings. Table 4 partially tests this hypothesis by estimating the impact of Chapter 13 separately by predicted garnishment before filing. Columns 1 and 2 of Table 4 present results for filers in the four states that prohibit wage garnishment - Florida, Pennsylvania, South Carolina, and Texas - and filers in states that allow at least some wage garnishment. The impact of Chapter 13 on annual earnings over the first five post-filing years is $5,112 in states that allow garnishment, compared to $2,533 in the four states that prohibit garnishment. The pattern of results is reverse for employment, however, with larger impacts in the four states that prohibit garnishment, though the difference is not statistically significant.

Columns 3 through 5 present results for filers who are likely to be subject to different marginal garnishment rates. Within each state, potential garnishment is a non-linear function of earnings. In states that follow the Federal guidelines, for example, creditors are allowed to garnish each additional dollar of disposable earnings between 30 and 40 times the minimum wage, and 25 cents of every additional dollar after that point. There is also across state variation in garnishment from the twelve states that have higher exemptions on garnishment, the ten states that have lower caps on the total garnishment amount, and the four states that prohibit garnishment altogether. For each filer, we estimate disposable earnings using pre-tax earnings in the five most recent pre-filing years and the NBER TAXSIM Federal and state income tax calculator. We then apply the binding Federal or state garnishment law to calculate the marginal rate of garnishment a filer would face in the pre-filing period.

The effect of Chapter 13 protection is small and imprecisely estimated for filers unlikely to face wage garnishment. The impact on annual earnings is $1,611, while the impact on employment is 0.5 percentage points. Neither estimate is statistically significant at conventional levels. In contrast, there is a large and precisely estimated impact of Chapter 13 on filers subject to either 25 or 100 percent marginal garnishment rates. The impact on annual earnings is $7,522 for filers subject to marginal garnishment of 25 percent, and $3,415 for those subject to 100 percent. The impact on employment is 5.2 and 5.0 percentage points, respectively, for filers subject to a marginal

Table 2.4
Results by Wage Garnishment Regulations

	Garnishment in State?		Marginal Garnishment Rate		
	No	Yes	0%	25%	100%
Panel A: Labor Supply	(1)	(2)	(3)	(4)	(5)
Earnings	2.533^{*}	5.112^{***}	1.611	7.522^{***}	3.415^{*}
	(1.477)	(1.362)	(1.211)	(1.711)	(1.936)
Employment	0.066	0.034^{***}	0.005	0.052^{***}	0.050^{**}
	(0.053)	(0.013)	(0.022)	(0.017)	(0.025)
Log Earnings	0.593	0.463^{***}	-0.186	0.793^{***}	0.996^{***}
	(0.431)	(0.145)	(0.347)	(0.172)	(0.291)
Panel B: Mortality					
5-year Mortality	-0.033	-0.019^{*}	-0.005	-0.025^{**}	-0.040^{**}
	(0.058)	(0.010)	(0.027)	(0.012)	(0.018)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	64743	432137	234925	218080	43875

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection interacted with binding garnishment regulations. The sample consists of all first time filers between 1992 and 2005 in the 33 courts that randomly assign filings to judges. Columns 1 - 2 interact Chapter 13 with indicators for living in a state that does not allow any wage garnishment and living in a state that does allow wage garnishment. Columns 3 - 5 interact Chapter 13 with indicators for garnishment bracket implied by the filer's state of residence and baseline income. All specifications control for gender, race, age, and the five year average of baseline employment baseline earnings, and office by month-of-filing fixed effects, and cluster standard errors at the court level. All monetary values are expressed in real 2000 dollars divided by 1,000. Observations refer to the number of bankruptcy filers in the indicated group. The number of observations in each regression is the sum of both groups. See text for additional details. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

garnishment rate of 25 and 100 percent. The final row of Table 4 presents results for log earnings, showing that Chapter 13 increases annual log earnings by 0.793 for filers subject to a garnishment rate of 25 percent, and 0.996 for filers subject to a garnishment rate of 100 percent. The difference in the earnings and log earnings result is driven by differences in mean earnings between the two groups. The 25 percent rate applies to individuals who earn above a certain threshold, typically 40 times the minimum wage each week. In contrast, the 100 percent rate applies only to individuals earning less than that threshold.

Taking the estimates from Table 4 at face value, we can calculate the implied earnings elasticity with respect to garnishment for the 25 percent bracket. The earnings elasticity with respect to garnishment is equal to the log change in taxable earnings divided by the log change in the net tax rate. The log change in taxable earnings comes directly from Table 4. We assume that the state and Federal earnings tax rate is 20 percent, implying that the net tax and garnishment rate is 40 percent. This implies an elasticity of 2.756 for the 25 percent bracket. Note that we are unable to calculate the elasticity for the 100 percent bracket, as $log(0)$ is undefined. If we assume that the marginal garnishment rate is 99 percent, the implied elasticity is 0.227 for the 100 percent bracket.

A second explanation for the estimated effects is that bankruptcy protection reduces both the likelihood and consequences of economic instability. Bankruptcy protection discharges most debts, allows debtors to repay mortgage arrears, and puts a hold on almost all debt collection efforts. These features of Chapter 13 increase economic stability by allowing debtors to avoid eviction or home foreclosure, reducing the incentive to strategically move across state lines or change jobs to avoid creditors, and preventing sharp drops in consumption that may have important long-term consequences, such as becoming sick through lack of medical care. We partially test the empirical relevance of this channel by examining the impact of Chapter 13 on employment and geographic mobility, and by exploiting variation in state foreclosure laws.

Table 5 presents estimates of the impact of Chapter 13 protection on the probability of working in the same baseline job, industry, county, and state. We also present results for job tenure and average firm wages. The sample is restricted to filers with at least one year of employment in both

Table 2.5
Chapter 13 Bankruptcy and Mobility

	CM	2SLS Results	
	(1)	(2)	(3)
Same Job	0.241	0.219^{***}	0.196^{***}
	(0.364)	(0.035)	(0.037)
Same Industry	0.328	0.221^{***}	0.200^{***}
	(0.386)	(0.036)	(0.034)
Same County	0.351	0.211^{***}	0.194^{***}
	(0.391)	(0.036)	(0.035)
Same State	0.444	0.182^{***}	0.167^{***}
	(0.401)	(0.036)	(0.035)
Controls	–	No	Yes
Observations	195367	426567	426567

Notes: This table reports two-stage least squares results of the impact of Chapter 13 bankruptcy protection on mobility averaged over the first five post-filing years. The sample consists of all first time filers between 1992 and 2005 in the 33 courts that randomly assign filings to judges who are employed for at least one post-filing year. Column 1 reports the mean and standard deviation for dismissed filers. Columns 2 - 3 instrument for bankruptcy protection using the reduced form measure of judge leniency described in the text. All specifications control for office by month-of-filing fixed effects, and cluster standard errors at the court level. Columns 3 and 5 also include controls for gender, race, age, and the five year average of baseline employment and baseline earnings. All monetary values are expressed in real 2000 dollars divided by 1,000. Mobility and firm information comes from the firm EIN from annual W-2s. Each dependent variable is equal to one if the individual is in the same job, industry, county, or state as in the pre-filing period. See text for additional details. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

the pre- and post-filing period. Consistent with the economic instability hypothesis, bankruptcy protection increases the probability of working in the same (2-digit NAICS) industry by 20.0 percentage points, with the mean of 32.8 percent among dismissed filers. Bankruptcy also increases the probability that a filer stays at his or her baseline job by 19.6 percentage points, nearly double the dismissed filer mean of 24.1 percent. There is also a large impact of bankruptcy protection on geographic mobility, with bankruptcy protection increasing the probability of working in his or her baseline county by 19.4 percentage points, and his or her baseline state by 16.7 percentage points. Job tenure and average firm wages are also higher, suggesting that this increase in economic stability is beneficial.[13,14]

2.6. Chapter 13 Bankruptcy Protection and Mortality

2.6.1. Results

Figure 2C presents two-stage least squares results measuring the impact of Chapter 13 bankruptcy protection on mortality. Following our labor supply results from Figures 2A and 2B, the sample includes first time filers between 1992 and 2005 in the 33 courts that randomly assign filings to judges. We use our reduced form measure of judge leniency Z_{ijct} as an instrumental variable for bankruptcy protection, and control for gender, race, a quartic in age, baseline employment, baseline earnings, and office by month-of-filing fixed effects. Standard errors are clustered at the court level. The dependent variable for each regression is an indicator for being deceased in or

[13]Web Appendix Table 9 presents mobility results interacted with garnishment regulations. The impact of Chapter 13 protection on working in the same industry, county, and state are larger in states that allow garnishment, though the differences are not statistically significant. The estimated impacts on working in the same job, same industry, and same county are also increasing in the marginal garnishment bracket, though working in the same state is not. The estimated effects also remain large and precisely estimated for filers in the no garnishment bracket. Taken together, we interpret the results from Web Appendix Table 9 as suggesting that garnishment protections can only partially explain the impact of bankruptcy protection on economic stability.

[14]Web Appendix Tables 10 and 11 report results of the impact of Chapter 13 protection interacted with whether or not a state requires judicial approval during the foreclosure process. In states without a judicial requirement, lenders have the right to sell the house after providing a notice of sale to the borrower, resulting in significantly higher rates of foreclosure (Mian, Sufi, Trebbi 2011). We find that the impact of Chapter 13 on job stability is modestly larger in states that do not require a judicial foreclosure process, with mixed results on labor supply. We interpret these results as suggesting that protection from home foreclosures is of only modest importance in explaining our estimated effects on economic instability. On the other hand, it is possible that judicial foreclosure laws only impact the likelihood of filing for Chapter 13, not the impact of Chapter 13 once the foreclosure process is approved.

before the specified year. Thus, our estimates represent the impact of Chapter 13 protection on cumulative mortality.

Chapter 13 bankruptcy protection significantly lowers mortality in the first five years after filing. Chapter 13 protection decreases one-year mortality for the marginal recipient by statistically insignificant 0.6 percentage points, and two-year mortality by 1.6 percentage points. Five-year mortality, the longest time period available for our entire sample, is 1.9 percentage points lower, a 47.5 percent decrease from the control mean of 4.0 percentage points. In a sample of individuals filing between 1992 and 2000, Chapter 13 protection decreases ten-year mortality by a statistically insignificant 5.5 percentage points.

Panel B of Web Appendix Table 7 reports results separately by gender, race, age, and baseline earnings. The effect of Chapter 13 protection on mortality is larger for filers who are female, white, and who have above median baseline earnings. The most striking difference is by age, however. Chapter 13 protection decreases five-year mortality by 17.6 percentage points for filers 60 and older at the time of filing, despite little to no impact on earnings for these filers. In comparison, Chapter 13 decreases five-year mortality by a statistically insignificant 0.4 percentage points for filers between 25 and 40, and a marginally significant 2.8 percentage points for filers between 40 and 60. This pattern of results is consistent with a change in earnings playing little to no role in explaining the mortality results.

Web Appendix Figure 4C reports the MTE estimates for five-year mortality. The MTE function for earnings is essentially flat in the predicted probability of bankruptcy protection. This implies that the mortality effects do not differ systematically across unobservable characteristics.

To put the magnitude of these estimates in context, it is helpful to consider the effects of job loss, which is the most commonly cited reason for needing bankruptcy protection, on mortality. In a sample of Pennsylvania workers, Sullivan and von Wachter (2009) find that job displacement increases short-run mortality by 50 to 100 percent, and long-run mortality by 10 to 15 percent. In the specification closest to ours, they find that job displacement increases five-year mortality by 1.2 percentage points, albeit for a population with lower underlying mortality risk. One interpretation of our estimates is therefore that bankruptcy protection can, at least in part, offset the increased mortality risk from financial distress caused by events such as job loss.

2.6.2. Potential Channels

Bankruptcy protection may impact mortality through the change in earnings, or through any number of non-earnings channels, such as a reduction in stress or increased access to health care.

To estimate the extent to which the labor supply impacts estimated above can explain the impact of bankruptcy protection on mortality, we follow Sullivan and von Wachter (2009) and compare our two-stage least squares results to the effect implied by the cross-sectional correlation between mortality and both earnings and employment. Our two-stage least squares results suggest that bankruptcy protection increases annual earnings by $5,012, and employment by 3.5 percentage points. The estimated correlation between five-year mortality and $1,000 in average annual earnings is -0.00019, and the correlation with average employment is -0.00996. Thus, the change in labor supply can explain $(0.035*0.00996+5.102*0.00019)*100 = 0.13$ percentage point decrease in five-year mortality, or about 6.8 percent of the reduced form effect of 1.9 percentage points. This suggests that about 93 percent of the estimated effect of Chapter 13 on five-year mortality is driven by non-earnings channels. This result is also consistent with our subsample results, which suggest that the mortality estimates are driven by filers over 60 who are likely to have exited the labor market, and hence whose earnings are relatively unaffected by bankruptcy protection.[15,16]

There are several potentially relevant channels not related to earnings that we cannot examine with our current data. For example, bankruptcy protection may decrease an individual's stress

[15] Panel B of Table 4 reports results of the impact of Chapter 13 protection on five-year mortality interacted with garnishment regulations. Chapter 13 protection only decreases mortality by a statistically significant 0.5 percentage points for filers not subject to wage garnishment, compared to 2.5 and 4.0 percentage points, respectively, for filers subject to 25 percent and 100 percent marginal garnishment rates. Given the weak relationship between earnings and mortality in our sample, the relationship between garnishment and the impact on mortality is likely to be driven by the increased stress associated with debt collection, rather than the garnishment itself.

[16] Panel B of Web Appendix Table 10 reports results of the impact of Chapter 13 protection on five-year mortality interacted with whether or not a state requires a judicial foreclosure process. The impact of Chapter 13 on mortality is 1.8 percentage points higher in states that do not require a judicial foreclosure process compared to states that do. Taken together with the garnishment results, we interpret these as being consistent with the idea that the mortality decrease is driven by a reduction in economic instability and disruption.

by reducing contact with creditors and allowing greater control over his or her financial future. Consistent with this theory, 84 percent of debtors report being under extreme stress before filing for bankruptcy, compared to only 35 percent after filing (Porter 2011). Dismissed filers may also lose their health insurance or have changed family environments that could impact health. Unfortunately, it is not possible to link information on morbidity, health insurance, or family status to our data. The precise mechanisms for our estimated mortality effect therefore remain unclear, and likely include a combination of these factors.

2.7. Discussion

The results we have presented have potentially important implications for the modeling of the consumer bankruptcy system. The evaluation of consumer bankruptcy laws has typically involved an assessment of two second-order effects, as bankruptcy provides partial insurance against uncertainty, but at the cost of higher borrowing costs that makes life-cycle smoothing more difficult (e.g. Athreya 2002, Li and Sarte 2006, Livshits, MacGee, and Tertilt 2007, Chatterjee and Gordon forthcoming). The typical bankruptcy model does not account for the first-order importance of the relationship between bankruptcy and earnings estimated in this paper, however. As a result, previous research is likely to have dramatically understated the potential benefits of debt relief.

To see this, it is helpful to consider our earnings result in light of a stylized extension of the heterogeneous agent life cycle model of Livshits, MacGee, and Tertilt (2007). In the model, households borrow from a perfectly competitive financial market to smooth consumption from expected and unexpected changes in earnings and expenses. Bankruptcy allows households to avoid very low consumption after a particularly severe shock, but increases borrowing costs in all periods due to a higher risk of default. When the model is calibrated to match the credit market, the model suggests that the benefits from this increased smoothing across states through bankruptcy just outweigh the distortion of the credit market. Bankruptcy leads to tighter borrowing constraints early in the life-cycle, reducing a household's ability to smooth expected changes in earnings. On the other hand, bankruptcy helps households smooth income when hit by a particularly bad shock, reducing consumption variance later in life. Additional details on the setup and calibration of the baseline scenario are available in Web Appendix C.

We extend the Livshits, MacGee, and Tertilt (2007) model by assuming that default outside of the bankruptcy system lowers household productivity. This assumption is meant to capture in a transparent way the earnings loss observed among dismissed filers in our data. Holding fixed the other parameters, bankruptcy is over ten times more beneficial if default is assumed to lower household productivity by 10 percent, and nearly 20 times more beneficial when default is assumed to lower household productivity by 25 percent. To put these magnitudes in perspective, bankruptcy is estimated to be six times more beneficial when the frequency of expense shocks is doubled, and nearly 40 times more beneficial when the size of expense shocks is doubled (Livshits, MacGee, and Tertilt 2007).

The results of this stylized exercise suggest that individual debt relief is much more likely to be welfare-improving than previously realized. This conclusion differs substantially from a number of prominent models, such as Athreya (2002) and Chatterjee and Gordon (forthcoming), that abstract away from the effects of bankruptcy on earnings. Even models that suggest debt relief is welfare-improving, such as Livshits, MacGee, and Tertilt (2007), likely understate the benefits of the consumer bankruptcy system.

2.8. Conclusion

This paper presents evidence that Chapter 13 bankruptcy protection has a significant long-term impact on debtors. Over the first five post-filing years, Chapter 13 protection increases the marginal recipient's annual earnings by $5,012, a 21.7 percent increase, and employment by 3.5 percentage points, a 4.2 percent increase. Chapter 13 protection also decreases five-year mortality by 1.9 percentage points, a 47.5 percent decrease. The effects are increasing in the amount that creditors can garnish outside of the bankruptcy system, suggesting that garnishment protections can explain at least some of the estimated effects. There is also evidence that bankruptcy protection reduces the likelihood of economic instability, increasing the probability that a filer stays in the same job and geographic area.

Our estimates provide new evidence on the ex-post benefits of debt relief. These results are particularly important in light of the on-going debate surrounding the use of debt relief and mortgage modification to stimulate the economy. Work by Mulligan (2008), Hall (2011), and Eggertsson and Krugman (forthcoming) suggests that household borrowing constraints can help explain the severity of the recession, while Mian and Sufi (2012) show that differences in the incidence of debt overhang can help explain regional differences in unemployment. Our work also suggests that restrictions on bankruptcy filing, such as those introduced by the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act, may have important adverse consequences for the economy.

The main limitation of our analysis is that we are not able to estimate the impact of bankruptcy laws on either borrowing costs or pre-filing behavior. There may also be important impacts of bankruptcy protection on outcomes such as home ownership or credit access that we are unable to measure with our data. Finally, our analysis has focused on Chapter 13 bankruptcy, which makes up about 30 percent of all bankruptcy filings. This paper should therefore be viewed as a first step towards characterizing the impact of consumer bankruptcy protection on debtors.

3. INFORMATION ASYMMETRIES IN CONSUMER CREDIT MARKETS: EVIDENCE FROM PAYDAY LENDING

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Theory has long emphasized the importance of private information in explaining credit-market failures. Information asymmetries and the resulting credit constraints have been used to explain anomalous behavior in consumption, borrowing, and labor supply. Motivated in part by this research, policymakers and lenders have experimented with various interventions to circumvent such problems. Yet, the success of these strategies depends on which information asymmetries are empirically relevant. Credit scoring and information coordination can help mitigate selection problems, while incentive problems are better addressed by improved collection or repayment schemes.

This paper provides new evidence on the empirical relevance of asymmetric information using administrative data from the payday-lending market. Payday loans are short-term loans of \$100 to \$500. Loan fees average \$15 to \$20 per \$100 of principal, implying an annual percentage rate (APR) of over 400 percent. Despite these high interest rates, payday lenders have more storefronts in the United States than McDonald's and Starbucks combined, with nearly 19 million households receiving a payday loan in 2010 (Skiba and Tobacman 2011). The payday-loan market is also extremely high risk, with more than 19 percent of initial loans in our sample ending in default.

Payday borrowers are particularly vulnerable to market failures due to their low incomes and poor credit histories. Two-thirds of payday borrowers report not having applied for credit at least once in the past five years due to the anticipation of rejection, and nearly three-quarters report having been turned down by a lender or not given as much credit as applied for in the last five years (Elliehausen and Lawrence 2001, IoData 2002). Payday loans also have the unique feature that delinquencies are not reported to traditional credit-rating agencies, and default comes with few penalties outside of calls from debt collection agencies. Theory suggests that asymmetric-information problems are exacerbated by precisely these kinds of commitment problems (Athreya, Tam and Young 2009, Chatterjee et al. 2007, Livshits, MacGee, and Tertilt 2010, White 2007, White 2009).

We identify the impact of moral hazard in the payday-loan market using two separate empirical models. The first exploits discontinuities in the relationship between borrower pay and loan eligibility to estimate a regression-discontinuity design. Many payday lenders offer loans in $50 increments up to but not exceeding half of an individual's biweekly pay. As a result, there are loan-eligibility cutoffs around which very similar borrowers are offered different size loans. These institutional features allow us to attribute any discontinuous relationship between loan outcomes and pay at the loan-eligibility cutoffs to the causal impact of loan size. Our second empirical model uses a discontinuous change in slope relating borrower pay to loan eligibility to estimate a regression-kink design. In this separate sample of states, payday lenders offer loans in continuous increments that are no larger than half of a borrower's biweekly pay, capping loans for all borrowers at a state-mandated limit of either $300 or $500. The fact that loan amounts are offered in continuous increments up to these caps implies that there is a discontinuous change in the slope relating loan eligibility and biweekly pay at each loan cap. We use this discontinuous change in the slope to provide a second set of moral-hazard estimates. As the correlation between default and loan size combines the selection and incentive effects of loan size, we can, under reasonable assumptions, obtain an estimate of adverse selection by subtracting our moral-hazard estimates from the cross-sectional coefficient relating loan size and borrower default.

We begin our empirical analysis by documenting credit constraints among payday borrowers. Using our regression-discontinuity strategy, we find that a $50 increase in payday credit leads to a $19.73 to $22.02 increase in average loan size. Thus, payday borrowers borrow 39 to 44 cents per additional dollar of credit. These estimates are larger than previous findings using data from different types of debtors, likely reflecting the fact that payday borrowers are particularly credit constrained. For example, the typical credit-card holder consumes 10 to 14 cents out of every additional dollar of credit (Gross and Souleles 2002), while the typical financially constrained household consumes 20 to 40 cents out of every additional dollar in tax-rebate amount (Johnson, Parker, and Souleles 2006).

Surprisingly, both our regression-discontinuity and regression-kink empirical strategies suggest that relaxing these credit constraints lowers the probability that a payday borrower defaults. A $50 increase in payday-loan size leads to a 4.4 to 6.4 percentage point decrease in the probability of default in our regression-discontinuity strategy sample, a 22 to 33 percent decrease. Using our regression-kink design, we find that a $50 increase in payday-loan size lowers the probability of default by 1.6 to 4.6 percentage points, a 17 to 23 percent decrease. The finding that larger loans lower the rate of default is surprising given the prominence of moral hazard in the theoretical literature and the empirical relevance of moral hazard in other consumer-lending markets (Adams, Einav, and Levin 2009).

Conversely, we find economically and statistically significant adverse selection into larger payday loans. In our OLS results, which combine both adverse selection and moral hazard, a $50 increase in loan size is associated with a 1.0 to 2.3 percentage point increase in the probability of default in our regression-discontinuity sample. Taken together with our estimates of moral hazard, this suggests that borrowers who *choose* a loan that is $50 larger are 5.4 to 8.7 percentage points more likely to default, a 28 to 44 percent increase. In our regression-kink sample, the OLS results suggest that borrowers who choose a $50 larger loan are 16 to 47 percent more likely to default. Our results are therefore consistent with the view that adverse selection alone can lead to credit constraints in equilibrium.

We conclude our analysis by examining two key threats to our interpretation of the regression-discontinuity and regression-kink estimates. The first threat is that individuals may opt out of borrowing if they are not eligible for a sufficiently large loan. Such selective borrowing could invalidate our regression-discontinuity design by creating discontinuous differences in borrower characteristics around the eligibility cutoffs. We evaluate this possibility by testing whether the density of borrowers is a continuous function of the loan-eligibility cutoffs and by examining the continuity of observable borrower characteristics at the cutoffs. The second threat to our identification strategy is that our empirical design is misspecified. To ensure that our estimates identify discontinuities that exist solely due to institutional factors, we replicate our empirical results in a set of states where loan size is not a discontinuous function of income.

Our work fits into an important empirical literature estimating moral hazard and adverse selection in credit markets in the United States (Ausubel 1991, Edelberg 2003, Edelberg 2004) and abroad (Klonner and Rai 2006, Karlan and Zinman 2009). Ausubel (1999), for example, uses randomized credit-card offers to show that a 1 percent increase in introductory interest rate increases the probability of delinquency by 1.2 percentage points and the probability of bankruptcy by 0.4 percentage points. Adams, Einav, and Levin (2009) exploit exogenous variation in price and minimum down payments to identify moral hazard and adverse selection in an automobile-loan market. Adams, Einav, and Levin (2009) estimate that for a given auto-loan borrower, a $1,000 increase in loan size increases the probability of default by 16 percent. Individuals who borrow an extra $1,000 for unobservable reasons have an 18 percent higher rate of default than those who do not. Also related is Melzer and Morgan (2010), who find adverse selection into bank overdraft services when payday lending is available.

This paper complements this literature in three ways. First, the characteristics of the borrowers make this a particularly important population for which to study credit dynamics. As previously discussed, payday borrowers are particularly vulnerable to market failures given their low incomes and poor credit histories. Payday borrowers apply for payday loans precisely when they have exhausted traditional credit options. In fact, 80 percent of payday-loan applicants have no available credit on credit cards when they apply for a payday loan (Bhutta, Skiba, and Tobacman 2012). Second, the institutional features of the payday-loan market allow for a particularly sharp research design. Adams, Einav, and Levin (2009), whose work is most closely related to ours, use price and down-payment variation across time, credit categories, and regions to identify the impact of moral hazard. Their empirical design therefore relies on having controlled for all sources of endogenous variation. In contrast, we focus on two transparent and well-identified sources of variation in payday-loan size to identify moral hazard. Third, we are the first to explore the role of information frictions in the payday-loan market, one of the largest and fastest growing sources of subprime credit in the United States. Since the emergence of payday lending in the mid-1990s, annual loan volume has grown from approximately $8 billion in 2000 to $44 billion in 2008. In comparison, the subprime automobile-loan market totaled approximately $50 billion in 2006 (J.D. Power and Associates 2007).

Our paper also adds to a large literature documenting consumer-credit constraints. The majority of this literature has inferred credit constraints from the excess sensitivity of consumption to expected changes in labor income (e.g., Hall and Mishkin 1982, Altonji and Siow 1987, Zeldes 1989, Runkle 1991, Stephens 2003, Stephens 2006, Stephens 2008) or tax rebates (e.g., Parker 1999, Souleles 1999, Johnson, Parker, and Souleles 2006). Card, Chetty, and Weber (2007) and Chetty (2008) also find excess sensitivity of job-search behavior to available liquidity, which they interpret as evidence of liquidity constraints.

Finally, our paper is related to a rapidly expanding literature examining the impact of payday credit. There is evidence that loan access may help borrowers smooth negative shocks (Morse 2011) and avoid financial distress (Morgan, Strain, and Seblani 2012). On the other hand, there is also evidence that loan access may erode job performance (Carrell and Zinman 2008), increase bankruptcy (Skiba and Tobacman 2011) and lead to increased difficulty paying mortgage, rent, and utility bills (Melzer 2011).

The remainder of the paper is structured as follows. Section 3.1. provides background on our institutional setting and describes our data. Section 3.2. reviews the theoretical framework that motivates our empirical analysis. Section 3.3. describes our empirical strategy. Section 3.4. presents our results. Section 3.5. discusses potential mechanisms through which larger payday loans lower the probability of default. Section 3.6. concludes. Web Appendix A provides additional results.

3.1. Data and Institutional Setting

Payday loans are small, short-term loans collateralized with a personal check. In a typical payday-loan transaction, individuals fill out loan applications and present their most recent pay stubs, checking-account statements, utility or phone bills, and a government-issued photo ID. Lenders use applicants' pay stubs to infer their next payday and designate that day as the loan's due date. The customer writes a check for the amount of the loan plus a finance charge that is typically \$15 to \$18 per \$100 borrowed.[17] The lender agrees to hold the check until the next payday, typically for about two weeks, at which time the customer redeems the check with cash or the lender deposits the check. A loan is in default if the check does not clear.

Payday-loan eligibility is typically a discontinuous function of net pay, with the precise eligibility rules varying across firms and states. In our data, loan-eligibility rules take two forms. In the first form, loans are offered in \$50 increments that are no larger than half of a borrower's biweekly pay. Thus, loan eligibility increases discontinuously by \$50 at each \$100 pay interval. Stores using this rule form our regression-discontinuity sample. A second set of stores offer loans in continuous increments that are no larger than half of a borrower's biweekly pay, capping loans for all borrowers at a state-mandated limit of either \$300 or \$500. The fact that loan amounts are offered in continuous increments implies that there are no discontinuous jumps in loan eligibility. Instead, there is a discontinuous change in the slope relating loan eligibility and biweekly pay at the loan-limit amount. Stores using this eligibility rule form our regression-kink sample.

Our specific data come from three large payday lenders. Lending information is available from January 2000 through July 2004 in 15 states for the first firm in our data (hereafter Firm A), from January 2008 through April 2010 in two states for the second firm in our data (hereafter Firm B), and from January 2008 through June 2011 in two states for the third firm in our data (hereafter Firm C).[18] We combine these data with records of repayment and default from each firm. This gives us information on borrower characteristics, loan terms, and the subsequent loan outcomes. Our data from Firm A include information on each borrower's income, home address, gender, race, age, checking-account balance, and subprime credit score. Our data from Firms B and C are more sparse, only including information on each borrower's income, home address, and age.

[17]While some lenders use credit scores to screen applicants, none of the firms in our sample use risk-based pricing and all borrowers pay the same finance charge. See Agarwal, Skiba, and Tobacman (2009) for more information on the subprime credit-scoring process.

[18]Our data spans periods both before and after the Great Recession. Our regression-discontinuity sample is too small to provide estimates by period. Our regression-kink estimates are nearly identical for both Firm A and Firm C, whose data span both time periods.

Our regression-discontinuity sample consists of all initial loans made in four states that offer loans in \$50 increments. This sample includes Firm A stores in Ohio and Tennessee and Firm B stores in Kansas and Missouri. We restrict our analysis to borrowers paid biweekly or semimonthly, who make up nearly 70 percent of all borrowers, to allow a more straightforward presentation of the regression-discontinuity results. Results are nearly identical including all borrowers. Finally, we restrict our regression-discontinuity analysis to borrowers earning within \$100 of a loan-eligibility cutoff, or borrowers who make between \$100 and \$500 in Tennessee, which limits loans at \$200, and between \$100 and \$1,100 in the other three states in our sample. These restrictions leave us with 2,350 observations from Firm A and 7,123 observations from Firm B.

Columns 1 and 2 of Table 1 present summary statistics for the two firms in our regression-discontinuity sample. Weighting the mean from each firm by the number of borrowers, the typical borrower borrows \$226.71 (including fees) in his first transaction and earns \$682.39 every two weeks. Nineteen and a half percent of borrowers default on their first loan, with the rate being more than ten percentage points higher for borrowers at Firm B. The higher rate of default may be due, at least in part, to these loans being made during the Great Recession. The more detailed data from Firm A show that 28.3 percent of borrowers are male and 77.8 percent are black, although these numbers vary widely across store locations. Just under 27 percent of payday borrowers in our regression-discontinuity sample own a home, 25.3 percent use direct deposit, and 2.4 percent have their wages garnished by a creditor.

Our regression-kink sample consists of all initial loans made in four states that offer loans in \$1 or \$10 increments. This sample includes Firm A stores in Alabama, Colorado, Florida, Georgia, Indiana, Louisiana, Missouri, North Carolina, Oklahoma, and Texas. The sample for Firm C includes stores in California and Oklahoma. Stores in California limit loans at \$300, while all other states limit loans at \$500. Following our regression-discontinuity sample, we restrict our regression-kink analysis to borrowers paid biweekly or semimonthly. We also drop borrowers making less than \$100 each biweekly pay period and those making more than \$1,000 than the amount necessary to qualify for the largest available payday loan.

Table 3.1
Summary Statistics

	RD Sample		RK Sample		All Borrowers		
	Firm A	Firm B	Firm A	Firm C	Firm A	Firm B	Firm C
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Loan Amount	190.936	238.614	285.866	223.918	283.933	257.738	228.426
Biweekly Pay	580.827	715.852	790.856	961.133	809.250	822.658	1229.444
Default	0.112	0.222	0.090	0.202	0.090	0.210	0.187
Ever Default	0.369	0.616	0.343	0.643	0.342	0.608	0.618
Age	36.508	35.710	35.482	35.808	35.619	36.609	37.084
Male	0.283	–	0.335	–	0.336	–	–
White	0.089	–	0.110	–	0.110	–	–
Black	0.777	–	0.496	–	0.509	–	–
Checkings	207.166	–	272.456	–	275.147	–	–
Credit Score	513.171	–	443.155	–	446.470	–	–
Home Owner	0.270	–	0.321	–	0.323	–	–
Direct Deposit	0.413	–	0.428	–	0.429	–	–
Garnishment Flag	0.025	–	0.027	–	0.027	–	–
Observations	2,350	7,123	91,790	38,235	96,679	8,607	50,092

Notes: This table reports summary statistics. The regression-discontinuity (RD) sample consists of first-time payday-loan borrowers living in states offering payday loans in $50 increments who are paid biweekly or semimonthly earning between $100 and $1100 every two weeks. The regression-kink (RK) sample consists of first-time payday-loan borrowers living in states offering payday loans in $1 or $10 increments who are paid biweekly or semimonthly earning more than $100 and within $1000 of a kink point. All borrowers are all payday borrowers paid biweekly or semimonthly. Firm A data are available for 2000 to 2004. Firm B data are available for 2008 to 2010. Firm C data are available for 2008 to 2012. Default is an indicator for bounced payment on the first loan. Ever default is an indicator for ever bouncing a payment. Checkings balance is reported via the most recent bank statement. Credit score is a subprime credit score calculated at the time of application by a third-party credit-scoring agency called Teletrack. Direct deposit is an indicator for having one's paycheck directly deposited into a checking account. Garnishment is an indicator for a creditor currently garnishing a portion of one's wages. See text for additional details on the sample and variable construction.

Thus, we include borrowers making between \$100 and \$1,600 in California and \$100 and \$2,000 in all other states in our regression-kink sample. These restrictions leave us with 91,806 observations from Firm A and 38,311 observations from Firm C.

Columns 3 and 4 of Table 1 present summary statistics for our regression-kink sample. Weighting the mean from each firm by the number of borrowers, the typical borrower in our regression-kink sample borrows \$267.64 in his first transaction, \$40.93 more than in our regression-discontinuity sample, and earns \$840.92 every two weeks, \$158.53 more. Borrowers in our regression-kink sample also default at a rate of 12.3 percent, more than seven percentage points less than the regression-discontinuity sample. Borrowers in the regression-kink sample are also less likely to be black, have lower credit scores, and are more likely to own a home than borrowers in the regression-discontinuity sample. The positive selection into our regression-kink sample is due to the sample including borrowers earning between \$100 and either \$1,600 or \$2,000 every two weeks, as opposed to our regression-discontinuity sample that only includes borrowers earning between \$100 and \$1,100. Moreover, our regression-kink sample includes more borrowers from Firm A, whose data is drawn from before the Great Recession when default rates were lower for all payday-lending firms.

3.2. Conceptual Framework

Models of asymmetric information predict that information frictions will produce a positive correlation between loan default and the size or price of that loan.[19] In the moral-hazard version of the model, individual borrowers are more likely to default on larger or more expensive loans. The underlying behavioral mechanisms consistent with these moral-hazard models span situations whereby individuals have a great deal of control over their default decisions (e.g. strategic default) to situations where individuals have relatively little control and default is due largely to unexpected shocks. For instance, payday borrowers may have less incentive to repay a larger loan even when

[19] Models of asymmetric information typically assume limited commitment by borrowers, or the idea that borrowers always have the option of personal bankruptcy. An emerging literature suggests that asymmetric-information issues are no longer relevant when limited commitment can be fully resolved (Athreya, Tam, and Young 2009, Chatterjee et al. 2007, Livshits, MacGee, and Tertilt 2010, White 2007, White 2009).

they have the ability to do so. This can happen if the penalties of default increase less quickly than the benefits of default. Borrowers will therefore be more likely to voluntarily default as the loan amount increases. This can lead to credit constraints in the payday-loan market because borrowers will not internalize the full increase in default costs that come with larger loan sizes, with lenders needing to cap loan sizes to prevent overborrowing. In this scenario, improved collection or repayment schemes can help relax credit constraints for all payday borrowers.

In models of adverse selection, borrowers at a high risk of default choose larger loans. Adverse selection may result from forward-looking borrowers anticipating the high likelihood of default and therefore choosing larger and more valuable loans. Conversely, payday borrowers that are more illiquid today and more in need of a larger loan may also be more likely to be illiquid later and have trouble with repayment. Adverse selection of either kind will lead to credit constraints in the payday-loan market whenever lenders cannot observe a borrower's risk type, as lenders will need to deny credit to both high- and low-risk types. In this scenario, credit scoring and information coordination can help mitigate selection problems and increase the supply of credit to low-risk borrowers.

It is impossible to identify the separate impact of each of these channels with our available data. Instead, the goal of our paper is to document the presence of liquidity constraints in payday lending and to assess the consequences of moral hazard and adverse selection in our setting. Our estimates will likely reflect a number of the mechanisms discussed above. In Section 3.5., we will explore which of these mechanisms is most plausible given the pattern of results.

3.3. Empirical Strategy

We estimate two empirical models to identify the impact of moral hazard in the payday-loan market. The first empirical model exploits discontinuities in the relationship between net pay and loan eligibility to estimate a regression-discontinuity design. The second empirical model uses loan limits to estimate a regression-kink design.[20]

[20] A third empirical strategy to estimate the impact of moral hazard exploits the fact that payday loans in Tennessee are capped at $200. As a result, there is a trend break in the relationship between net pay and maximum loan size in

Consider the following model of the causal relationship between default (D_i) and loan size (L_i):

$$D_i = \alpha + \gamma L_i + \varepsilon_i \tag{4}$$

The parameter of interest is γ, which measures the causal effect of loan size on default (e.g., moral hazard). The problem for inference is that if individuals select a loan size because of important unobserved determinants of later outcomes, such estimates may be biased. In particular, it is plausible that people who select larger loans have a different probability of default even if loan size was held constant: $E[\varepsilon_i|L_i] \neq 0$. Since L_i may be a function of default risk, this can lead to a bias in the direct estimation of γ using OLS.

The key intuition of our first strategy is that this bias can be overcome if the conditional distribution of unobserved determinants of default $E[\varepsilon_i|pay_i]$ trends smoothly through the loan-eligibility cutoffs used by payday lenders. In this scenario, the distribution of unobserved characteristics of individuals who just barely qualified for a larger loan is the same as the distribution among those who just barely did not qualify:

$$E[\varepsilon_i|pay_i = c_l + \Delta]_{\Delta\to 0^+} = E[\varepsilon_i|pay_i = c_l - \Delta]_{\Delta\to 0^+} \tag{5}$$

where pay_i is an individual's net pay and c_l is the eligibility cutoff for loan size l. Equation (5) therefore implies that the distribution of individuals to either side of the cutoff is as good as random with respect to unobserved determinants of default, ε_i. Since loan size is a discontinuous function of pay, whereas the distribution of unobservable determinants of default, ε_i, is by assumption continuous at the cutoffs, the coefficient γ is identified. Intuitively, any discontinuous relation between default and net pay at the cutoffs can be attributed to the causal impact of loan size under the identification assumption in Equation (5).

Tennessee. Specifically, we can use the interaction of an indicator variable for a borrower residing in Tennessee and being eligible for a $200 loan with net pay as an instrumental variable. The differences in state trends in loan amounts and default after the $200 cutoff identifies the impact of moral hazard. Web Appendix Table 1 reports these difference-in-difference results. The results are qualitatively similar to our preferred regression-discontinuity and regression-kink estimates.

Formally, let loan size L_i be a smooth function of an individual's pay with a discontinuous jump at each of nine loan-eligibility cutoffs c_l:

$$L_i = f(pay_i) + \sum_{l=100}^{500} \lambda_l \mathbb{1}\{pay_i \geq c_l\} + \eta_i \tag{6}$$

where λ_l measures the effect of loan eligibility on loan size at each of the nine cutoffs. λ_l can be interpreted as the marginal propensity to borrow estimated by Gross and Souleles (2002) and others at each eligibility cutoff. We can use Equation (6) as the first stage to estimate the average causal effect for individuals induced into a larger loan by earning an amount just above a cutoff. The two-stage least squares regression controls for the underlying relationship between pay and both default and loan size using $f(pay_i)$, and instruments for loan size using loan eligibility $\mathbb{1}\{pay_i \geq c_l\}$ at each cutoff l.

In practice, the functional form of $f(pay_i)$ is unknown. In our empirical analysis, we experiment with several functional forms to control for borrower pay, including a seventh-order polynomial, a linear spline, and a local linear regression. To address potential concerns about discreteness in pay, we cluster our standard errors by pay (Lee and Card 2008). We also control for month-, year-, and state-of-loan effects in all specifications. Adding controls for age, gender, race, baseline credit score, and baseline checking-account balance leaves the results essentially unchanged.

As with any regression-discontinuity approach, one threat to a causal interpretation of our estimates is that individuals may opt out of borrowing if they are not eligible for a large enough loan. Such selective borrowing could invalidate our empirical design by creating discontinuous differences in borrower characteristics around the eligibility cutoffs. In Section 5.4 we evaluate this possibility in two ways: (1) by testing whether the density of borrowers is a continuous function of loan-eligibility cutoffs, and (2) by examining the continuity of observable borrower characteristics around the cutoffs. Neither test points to the kind of selective borrowing that invalidates our empirical design.

A more general threat is the possibility that our regression-discontinuity design is misspecified.

To ensure that our estimates identify actual discontinuities in loan size and default that exist due to institutional factors, we replicate our empirical specifications in a set of states where loan size is not a discontinuous function of income. Consistent with our empirical design, we do not find a relationship between loan size and income or default and income around the loan-eligibility cutoffs in these states.

Finally, our regression-discontinuity approach assumes that loan eligibility impacts default only through loan size. This assumes, for example, that individuals do not strategically repay lenders who offer higher credit lines in order to protect future access to credit. If this assumption is violated, our reduced-form estimates represent the net impact of increasing an individual's credit limit more generally. Note that Adams, Einav, and Levin (2009) use the same assumption to identify the impact of moral hazard in the subprime auto-loan market.

To complement our regression-discontinuity strategy, our second statistical approach exploits loan limits in states that offer payday loans in relatively continuous amounts. In these states, payday lenders offer loans in continuous increments that are no larger than half of a borrower's biweekly pay up to a state-mandated limit of either \$300 or \$500. The fact that loan amounts are offered in continuous increments up to these caps implies that there is a discontinuous change in the slope relating loan eligibility and biweekly pay at each loan cap. We use this discontinuous change in the slope to provide a second set of moral-hazard estimates.

Formally, let loan size L_i be a smooth function of an individual's pay with a discontinuous change in the slope after the largest available loan in a state c_{max}:

$$L_i = pay_i + \pi \mathbb{1}\{pay_i \geq c_{max}\} \cdot pay_i + \eta_i \tag{7}$$

where π measures the effect of the loan limit on the relationship between earnings and loan size. Under a number of assumptions, including a monotonicity condition analogous to the standard instrumental-variables framework (Angrist, Imbens, and Rubin 1996), we can use Equation (7) as the first stage to provide a second set of moral-hazard estimates. The two-stage least squares regression controls for the underlying relationship between pay and both default and loan size using pay_i, and instruments for loan size using the change in slope at the loan cap $\mathbb{1}\{pay_i \geq c_{max}\}$. The identified two-stage least squares parameter is a weighted average of marginal effects, where the weights are proportional to the magnitude of the individual-specific kinks (see Card . (2012) for additional details).

There are two important assumptions necessary to interpret our regression-kink estimates as causal. Following our regression-discontinuity design, the conditional distribution of unobserved determinants of default $E[\varepsilon_i|pay_i]$ must trend smoothly through the loan caps used by payday lenders. In addition, the conditional distribution of unobserved determinants $E[\varepsilon_i|pay_i]$ must be continuously differentiable in pay. In practice, these assumptions imply that borrowers cannot precisely change their income, while allowing for other less extreme forms of endogeneity such as borrowers having imperfect control over their preborrowing earnings.

Similar to our regression-discontinuity approach, the identifying assumptions required by the regression-kink design generate strong predictions for the distribution of predetermined covariates around the loan caps. Following our robustness checks for our regression-discontinuity design, we test our regression-kink design in two ways: (1) by testing whether the density of borrowers is a continuous function of kink point, and (2) by examining the continuity of observable borrower characteristics at the kink point. There is no evidence that the number of borrowers changes at the kink point, with the results from Section 5.4 ruling out even modest selection in or out of the sample around the kink point. However, there are some small changes in the observable characteristics of borrowers around the kink points. Thus, our regression-kink estimates should be interpreted with these changes in mind.

A simple extension of our regression-discontinuity and regression-kink approach, first pioneered by Adams, Einav, and Levin (2009), allows us to estimate the magnitude of selection in our sample. Recall that a cross-sectional regression of default on loan size combines both selection and incentive effects. By subtracting our estimate of moral hazard from the cross-sectional coefficient on loan size, we obtain an estimate of selection. It is important to note that this approach assumes that our estimate of moral hazard is the relevant estimate for the full population. There are nine cutoffs in our sample and this assumption would be violated if borrowers right around these eligibility cutoffs have a different marginal return to credit than other borrowers.

3.4. Results

3.4.1. *The Impact of Loan Eligibility on Loan Amount*

Figures 1A - 1C present regression-discontinuity estimates of the impact of loan eligibility on loan amount. Each figure plots average loan amounts in $25 income bins for the first loans of borrowers with biweekly take-home pay between $100 and $1,100. Figure 1A plots fitted values from a regression of loan size on a seventh-order polynomial in net pay. That is, the fitted values for Figure 1A come from the following specification:

$$L_i = \alpha_0 + \sum_{l=100}^{500} \alpha_{1l} \mathbb{1}\{pay_i \geq c_l\} + \sum_{p=1}^{7} \beta_{1p} pay_i^p + \varepsilon_i \tag{8}$$

where α_{1l} is the effect of having a biweekly income above the cutoff for each loan size l.

Figure 1B plots fitted values from a linear spline specification:

$$L_i = \alpha_0 + \sum_{l=100}^{500} \big(\alpha_{1l} \mathbb{1}\{pay_i \geq c_l\} + \beta_{1l} \mathbb{1}\{pay_i \geq c_l\} \cdot (pay_i - c_l)\big) + \varepsilon_i \tag{9}$$

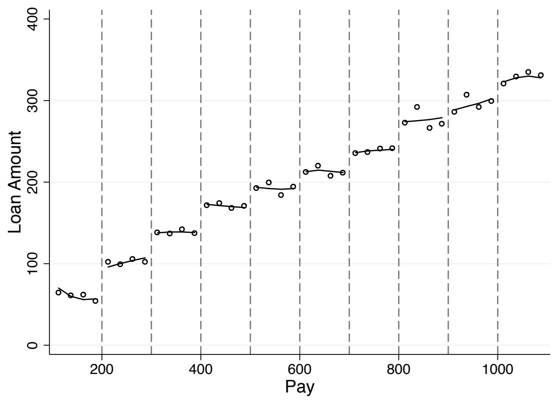
Figure 1C stacks data from each cutoff and controls for pay with a linear trend interacted with the loan-eligibility cutoff:

$$\widehat{L_i} = \alpha_0 + \alpha_1 \mathbb{1}\{pay_i \geq c\} + \beta_1 (pay_i - c) + \beta_2 \big(\mathbb{1}\{pay_i \geq c\} \cdot (pay_i - c)\big) + \varepsilon_i \tag{10}$$

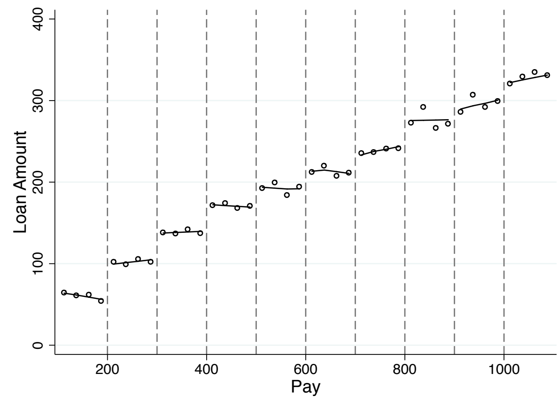
where α_1 is the impact of having an income above the loan-eligibility cutoff. To normalize the loan amounts across the nine cutoffs, Figure 1C plots residualized loan amounts $\widehat{L_i}$ from a regression of raw loan size on cutoff fixed effects. All three figures exclude borrowers from Tennessee earning more than $500.

Figure 3.1
Loan Eligibility and Loan Amount in the RD Sample

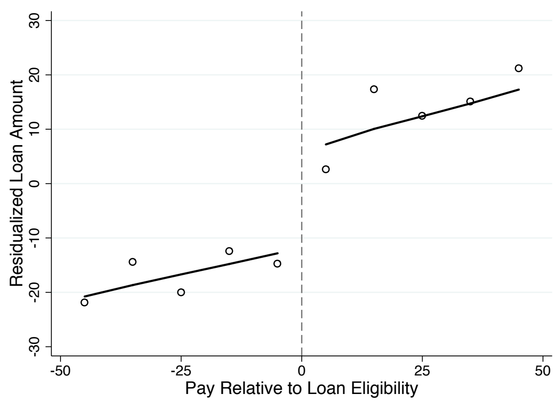
A. Polynomial



B. Linear Spline



C. Local Linear



Notes: These figures plot average loan size and biweekly pay for first-time payday borrowers in our regression-discontinuity sample. The sample consists of borrowers living in states offering payday loans in $50 increments who are paid biweekly or semimonthly between $100 and $1100. The smoothed line in Figure A controls for a seventh-order polynomial in net pay. Figure B controls for a linear spline in net pay. Figure C stacks data from each cutoff and controls for net pay using a linear regression and a linear regression interacted with the loan cutoff. See text for additional details.

Table 3.2
Regression-Discontinuity Estimates of the Effect of Loan Eligibility on Loan Amount

	Polynomial		Linear Spline		Local Linear	
	(1)	(2)	(3)	(4)	(5)	(6)
Loan Eligibility	22.021***	22.067***	21.906***	21.946***	19.633***	19.678***
	(2.887)	(2.902)	(2.911)	(2.930)	(4.365)	(2.523)
Age		0.026		0.025		0.020
		(0.083)		(0.083)		(0.083)
Black		−12.250***		−12.382***		−12.527***
		(4.613)		(4.610)		(4.606)
Male		−2.217		−2.045		−2.084
		(4.084)		(4.086)		(4.095)
Credit Score		−0.016*		−0.016*		−0.015*
		(0.009)		(0.009)		(0.009)
Checkings		0.007*		0.007*		0.007
		(0.004)		(0.004)		(0.004)
Home Owner		3.822		3.857		4.053
		(4.823)		(4.837)		(4.829)
Direct Deposit		0.869		0.814		0.885
		(3.416)		(3.412)		(3.402)
Garnishment Flag		7.809		8.111		8.804
		(14.244)		(14.237)		(14.141)
Observations	9,473	9,473	9,473	9,473	9,473	9,473

Notes: This table reports regression-discontinuity estimates of the impact of a \$50 increase in loan eligibility on loan amount. The sample consists of first-time payday-loan borrowers living in states offering payday loans in \$50 increments who are paid biweekly or semimonthly earning between \$100 and \$1100 every two weeks. Columns 1-2 control for a seventh-order polynomial in net pay. Columns 3-4 control for a linear spline in net pay. Columns 5-6 stack data from each cutoff and control for net pay using a linear regression interacted with the loan cutoff. The dependent variable is the dollar amount of the borrower's first loan. Loan eligibility indicates a \$50 increase in payday-loan eligibility. All regressions control for month-, year-, and state-of-loan effects. Columns 5 and also control for cutoff fixed effects. Standard errors are clustered by pay. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Loan eligibility is highly predictive of average loan size across all three specifications. While average loan amount is approximately constant between each two consecutive cutoffs, the typical loan increases approximately \$25 at each \$50 eligibility cutoff. It is also interesting to note that at lower cutoffs, borrowers take out loans that are near the maximum allowed level. The average loan size for borrowers earning just above the \$100 cutoff is at or just above \$100. In contrast, the typical debtor around higher cutoffs takes out loans that are significantly less than the maximum loan amount. The average loan size at the \$500 cutoff, for example, is just over \$300.

Table 2 presents formal estimates for the figures just described. The sample consists of first loans for borrowers with biweekly take-home pay between \$100 and \$1,100. Analogous to Figure 1A, columns 1 and 2 control for income using a seventh-order polynomial in net pay. Columns 3 and 4, corresponding to Figure 1B, control for income using a linear spline. Columns 5 and 6 present results that are analogous to Figure 1C, where we stack data from each cutoff and control for income using a linear trend and a linear trend interacted with earning above the loan-eligibility cutoff. The dependent variable is raw loan amount. All specifications control for month-, year-, and state-of-loan effects, with columns 5 and 6 adding controls for cutoff fixed effects. Columns 2, 4, and 6 also control for age, race, gender, credit score, checking-account balance, home ownership, direct-deposit status, and garnishment status. Observations from Firm B only control for age, the only demographic characteristic available. All specifications restrict the effect of each loan cutoff to have the same impact on loan size, and cluster standard errors at the pay level.

Consistent with the graphical evidence, loan eligibility is highly predictive of loan amount. Controlling for income using a seventh-order polynomial, borrowers with earnings just above a loan cutoff borrow \$22.02 more than borrowers with earnings just below a cutoff. Adding controls for age, race, gender, marital status, credit score, and checking-account balance leaves the results essentially unchanged. Controlling for income with a linear spline specification, the effect is \$21.91. Stacking data from each cutoff the effect is \$19.63.

Our regression-discontinuity estimates therefore imply that individuals in the payday market borrow 39 to 44 cents out of every additional dollar of available credit. Perhaps unsurprisingly, this suggests that payday borrowers are much more liquidity constrained than other individuals in the United States. For instance, Gross and Souleles (2002) find that a \$1 increase in a credit-card holder's limit raises card spending by 10 to 14 cents, and Johnson, Parker, and Souleles (2006) find that households immediately consumed 20 to 40 cents for every \$1 increase in their 2001 tax rebate.

Figure 2 plots average loan size and biweekly pay for first-time payday borrowers in our regression-kink sample. The sample consists of borrowers living in states offering payday loans in \$1 or \$10 increments who are paid biweekly or semimonthly. We restrict the sample to borrowers earning more than \$100, and less than the kink point plus \$1000. The smoothed line controls for pay interacted with being eligible for the maximum loan size in a state c_{max}. That is, the fitted values for Figure 2 come from the following local linear specification:

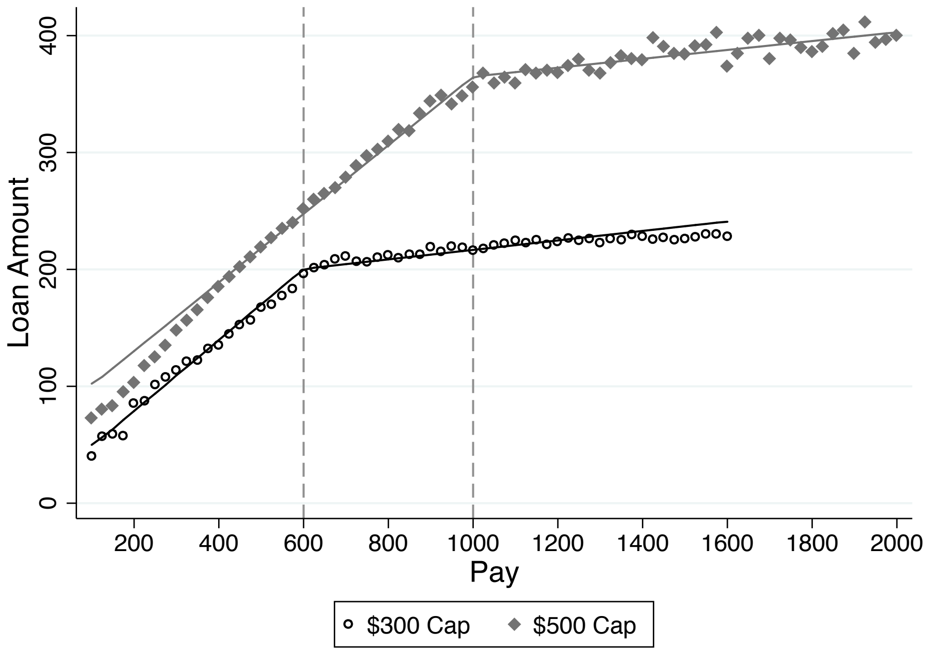
$$L_i = \alpha_0 + \alpha_1(pay_i - c_{max}) + \beta_1 \mathbb{1}\{pay_i \geq c_{max}\} \cdot (pay_i - c_{max}) + \varepsilon_i \tag{11}$$

estimated separately for borrowers in states with a \$300 and \$500 maximum loan size.

As expected given the loan-eligibility formula, Figure 2 shows very clear kinks in the empirical relationship between average loan size and biweekly earnings, with a sharp decrease in slope as earnings pass the loan-limit threshold. However, the relationship between loan amount and earnings before the kink is less than the 0.5 predicted by the loan-eligibility formula, again suggesting that not all borrowers take out the maximum loan available. Loan size is also increasing in earnings after the kink point, suggesting that there is a slight positive relationship between underlying loan demand and earnings.[21]

[21]Web Appendix Table 2 presents results estimating the association between borrower characteristics and loan choice in our regression-discontinuity and regression-kink samples. The dependent variable for each regression is an indicator for choosing the largest available loan. Thirty-three percent of borrowers in our regression-discontinuity sample choose the largest available loan, as do 28 percent of borrowers in the regression-kink sample. Web Appendix Table 2 shows that an additional hundred dollars of biweekly pay is associated with a 7.4 to 7.6 percentage point decrease in the probability of choosing the largest loan in the regression-discontinuity sample, and a 0.6 percentage point decrease in the regression-kink sample. In both samples, borrowers who are older, white, and male are more likely to choose a larger loan. Borrowers with higher credit scores and lower checking-account balances are also somewhat more likely to choose larger loans, though not all point estimates are statistically significant.

Figure 3.2
Loan Eligibility and Loan Amount in the RK Sample



Notes: These figures plots average loan size and biweekly pay for first-time payday borrowers in our regression-kink sample. The sample consists of borrowers living in states offering payday loans in $1 or $10 increments who are paid biweekly or semimonthly and paid more than $100 and within $1000 of a kink point. The smoothed line controls for pay interacted with being eligible for the maximum loan size in a state. See text for additional details.

Table 3.3
Regression-Kink Estimates of the
Effect of Loan Eligibility on Loan Amount

	$300 Cutoff		$500 Cutoff	
	(1)	(2)	(3)	(4)
Pay x Loan Cap	-0.257^{***}	-0.256^{***}	-0.251^{***}	-0.249^{***}
	(0.005)	(0.005)	(0.004)	(0.004)
Pay	0.294^{***}	0.291^{***}	0.286^{***}	0.283^{***}
	(0.005)	(0.005)	(0.002)	(0.002)
Age		0.423^{***}		0.013
		(0.030)		(0.037)
Black		–		0.483
				(1.185)
Male		–		-4.810^{***}
				(1.261)
Credit Score		–		-0.006^{*}
				(0.003)
Checkings		–		0.004^{***}
				(0.001)
Home Owner		–		11.669^{***}
				(1.416)
Direct Deposit		–		0.424
				(0.972)
Garnishment		–		-1.154
				(4.045)
Observations	33,259	33,259	96,766	96,766

Notes: This table reports regression-kink estimates of the impact of loan eligibility interacted with pay on loan amount. The sample consists of first-time payday-loan borrowers living in states offering payday loans in $1 or $10 increments who are paid biweekly or semimonthly earning more than $100 and within $1000 of a kink point. Loan amount is limited to half of net pay up to the loan limit. Columns 1-2 include states with a $300 loan limit. Columns 3-4 include states with a $500 loan limit. The dependent variable is the dollar amount of the borrower's first loan. Loan cap is an indicator for eligibility for the largest loan available in a state. All regressions control for month-, year-, and state-of-loan effects. Standard errors are clustered by pay. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 3 presents formal regression-kink estimates controlling for month-, year-, and state-of-loan effects. For borrowers in states capping loans at \$300, loan amount increases by 29.4 cents for each additional dollar of earnings before the kink point, compared to only 3.7 cents after the kink point. In \$500 cap states, loan amount increases by 28.6 cents for each additional dollar of earnings before the kink point, compared to only 3.5 cents after the kink point.

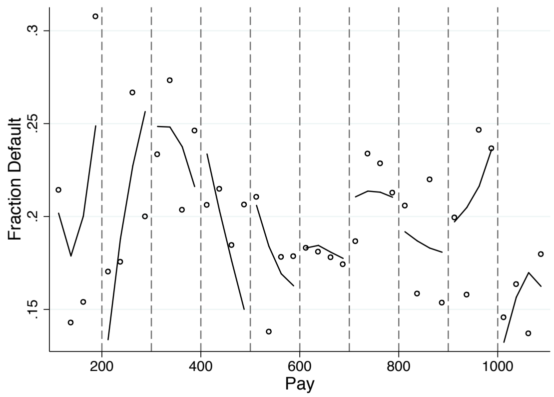
3.4.2. *Moral Hazard*

Figures 3A - C plot default and biweekly pay for payday borrowers in our regression-discontinuity sample. These figures represent the reduced-form impact of loan eligibility on default. Following the first-stage regression-discontinuity results, each figure plots average loan amounts in \$25 income bins for the first loans of borrowers with biweekly take-home pay between \$100 and \$1,100. Figure 3A plots fitted values controlling for income using a seventh-order polynomial. Figure 3B plots fitted values using a linear spline. Figure 3C plots residualized default rates after stacking data from each cutoff and controlling for income using a linear trend interacted with earning above the loan-eligibility cutoff. In sharp contrast to previous research, there is no evidence of moral hazard in our setting. In fact, default appears to be somewhat lower for borrowers with earnings just above loan cutoffs.

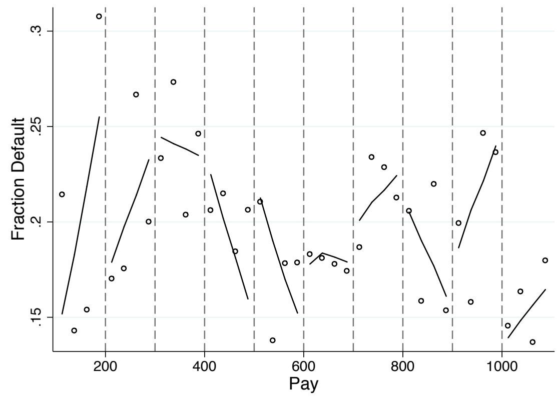
Table 4 presents formal two-stage least squares estimates of the causal impact of an additional dollar in loan amount on default. These two-stage least squares estimates pool information across all loan-eligibility cutoffs and are therefore more precise than the reduced-form results presented in Figure 3. All specifications instrument for loan amount using the maximum eligible loan, and control for month-, year-, and state-of-loan effects. Columns 5 and 6 control for cutoff fixed effects, with columns 2, 4, and 6 controlling for age, race, gender, credit score, checking-account balance, home ownership, direct-deposit status, and garnishment status. Observations from Firm B control for age, the only available demographic characteristic. All specifications restrict the effect of each loan cutoff to have the same impact on loan size and cluster standard errors at the pay level.

Figure 3.3
Loan Eligibility and Default in the RD Sample

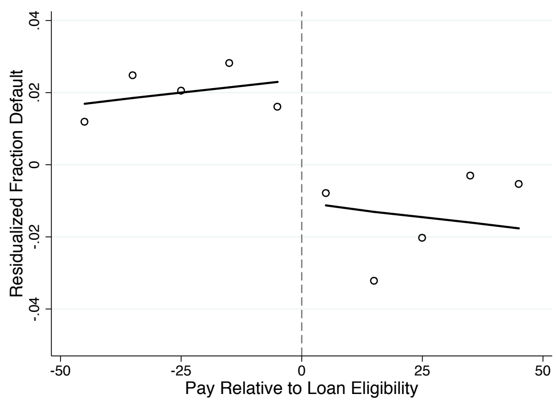
A. Polynomial



B. Linear Spline



C. Local Linear



Notes: These figures plot average default and biweekly pay for first-time payday borrowers in our regression-discontinuity sample. The sample consists of borrowers living in states offering payday loans in $50 increments who are paid biweekly or semimonthly between $100 and $1100. The smoothed line in Figure A controls for a seventh-order polynomial in net pay. Figure B controls for a linear spline in net pay. Figure C stacks data from each cutoff and controls for net pay using a linear regression and a linear regression interacted with the loan cutoff. See text for additional details.

Table 3.4
Regression-Discontinuity Estimates of the Effect of Loan Amount on Default

	Polynomial		Linear Spline		Local Linear	
	(1)	(2)	(3)	(4)	(5)	(6)
Loan Amount	−0.127**	−0.113**	−0.121**	−0.123**	−0.087*	−0.098**
	(0.053)	(0.050)	(0.052)	(0.052)	(0.050)	(0.051)
Age		−0.462***		−0.461***		−0.462***
		(0.035)		(0.035)		(0.035)
Black		−1.507		−1.628		−1.336
		(2.190)		(2.210)		(2.193)
Male		1.931		1.976		2.183
		(1.958)		(1.968)		(1.935)
Credit Score		−0.044***		−0.044***		−0.044***
		(0.004)		(0.004)		(0.004)
Checkings		0.001		0.001		0.000
		(0.002)		(0.002)		(0.002)
Home Owner		−1.406		−1.429		−1.525
		(2.044)		(2.064)		(2.027)
Direct Deposit		0.123		0.180		0.252
		(1.671)		(1.683)		(1.655)
Garnishment Flag		16.075**		16.542**		16.149**
		(8.094)		(8.151)		(8.205)
Observations	9,473	9,473	9,473	9,473	9,473	9,473

Notes: This table reports regression-discontinuity estimates of loan amount on default. The sample consists of first-time payday-loan borrowers living in states offering payday loans in $50 increments who are paid biweekly or semimonthly earning between $100 and $1100 every two weeks. Columns 1-2 control for a seventh-order polynomial in net pay. Columns 3-4 control for a linear spline in net pay. Columns 5-6 stack data from each cutoff and control for net pay using a linear regression interacted with the loan cutoff. The dependent variable is an indicator for bouncing a check on the first loan. All regressions instrument for loan amount using loan eligibility and control for month-, year-, and state-of-loan effects. Columns 5 and 6 also control for cutoff fixed effects. Standard errors are clustered by pay. Coefficients and standard errors are multiplied by 100.
*** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

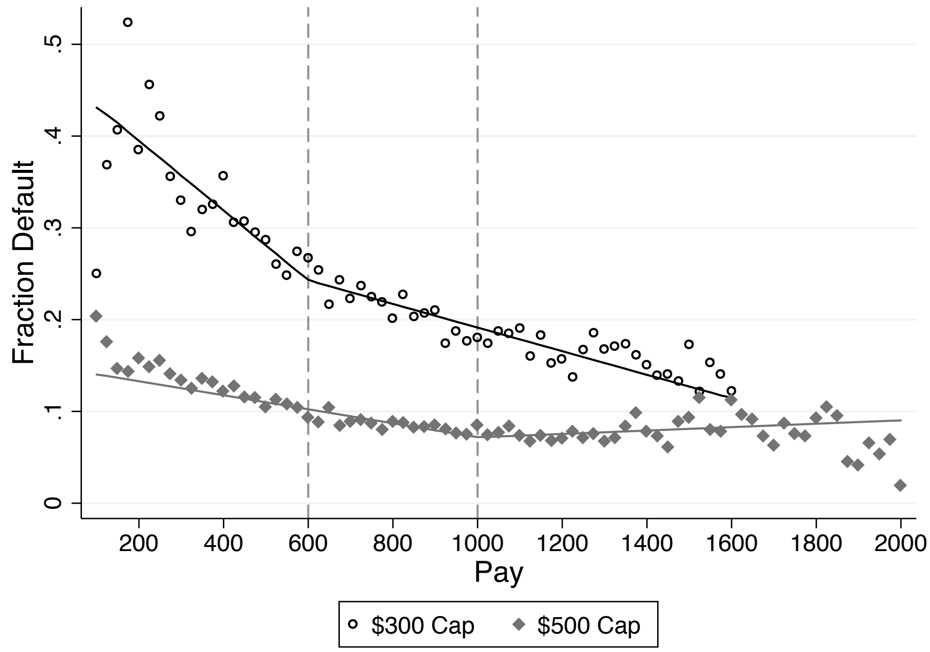
The dependent variable in each specification is an indicator variable equal to one if the debtor defaults on their payday loan. We multiply all estimates by 100 so that each coefficient can be interpreted as the percentage change in the probability of default.

Our regression-discontinuity results from Table 4 suggest that a larger loan decreases the probability that a payday borrower defaults on his first loan. Controlling for income using a seventh-order polynomial, a \$1 larger loan is associated with a 0.127 percentage point decrease in default. This implies that a \$50 larger loan (e.g. the typical increase in loan eligibility) is associated with a 6.35 percentage point decrease in default, a 32 percent decrease from the mean default rate of 19.47 percent. Controlling for income with a linear spline specification, a \$50 larger loan lowers default by 6.05 percentage points, a 31 percent decrease. Stacking data from each cutoff, the effect of a \$50 larger loan is 4.35 percentage points, a 22 percent drop in the probability of default in our regression-discontinuity sample.

Figure 4 plots default and biweekly pay for payday borrowers in our regression-kink sample. Following the first-stage results, there is a clear kink in the empirical relationship between default and biweekly earnings, with a sharp decrease in slope as earnings pass the loan-limit threshold. This pattern is consistent with larger loans decreasing the probability of default.

Table 5 presents formal two-stage least squares estimates of the causal impact of an additional dollar in loan amount on default using our regression-kink design. We instrument for loan amount using the interaction between pay and the kink point, and use a local linear control specification to control for pay. We also control for month-, year-, and state-of-loan effects, and multiply all estimates by 100. In our regression-kink specification, a \$1 larger loan is associated with a 0.09 percentage point decrease in the probability of default at the \$300 cutoff and a 0.3 percentage point decrease in the probability of default at the \$500 cutoff. This implies that a \$50 larger loan is associated with a 4.55 percentage point decrease in the probability of default at the \$300 cutoff, a 21.9 percent drop, and a 1.60 percentage point decrease in the probability of default at the \$500 cutoff, a 17.2 percent drop. It is worth noting the closeness of our regression-discontinuity and regression-kink estimates given the very different samples and identification strategies.

Figure 3.4
Loan Eligibility and Default in the RK Sample



Notes: These figures plots default and biweekly pay for first-time payday borrowers in our regression-kink sample. The sample consists of borrowers living in states offering payday loans in $1 or $10 increments who are paid biweekly or semimonthly and paid more than $100 and within $1000 of a kink point. The smoothed line controls for pay interacted with being eligible for the maximum loan size in a state. See text for additional details.

Table 3.5
Regression-Kink Estimates of the Effect of Loan Amount on Default

	\$300 Cutoff		\$500 Cutoff	
	(1)	(2)	(3)	(4)
Loan Amount	-0.091^{***}	-0.088^{***}	-0.032^{***}	-0.028^{***}
	(0.017)	(0.017)	(0.004)	(0.004)
Pay	-0.009^{***}	-0.007^{***}	0.001	0.003^{***}
	(0.002)	(0.001)	(0.001)	(0.001)
Age		-0.381^{***}		-0.238^{***}
		(0.023)		(0.009)
Black		–		3.111^{***}
				(0.276)
Male		–		1.789^{***}
				(0.294)
Credit Score		–		-0.019^{***}
				(0.001)
Checkings		–		-0.001^{***}
				(0.000)
Home Owner		–		-0.956^{***}
				(0.355)
Direct Deposit		–		-2.733^{***}
				(0.273)
Garnishment		–		0.302
				(1.125)
Observations	33,259	33,259	96,766	96,766

Notes: This table reports regression-kink estimates of loan amount on default. The sample consists of first-time payday-loan borrowers living in states offering payday loans in \$1 or \$10 increments who are paid biweekly or semimonthly earning more than \$100 and within \$1000 of a kink point. Columns 1-2 include states with a \$300 loan limit. Columns 3-4 include states with a \$500 loan limit. The dependent variable is an indicator for default on the first loan. All regressions instrument for loan amount using an indicator for eligibility for the largest loan available in a state and control for month-, year-, and state-of-loan effects. Coefficients and robust standard errors are multiplied by 100. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 6 and Table 7 report regression-discontinuity and regression-kink estimates interacted with borrower age, gender, race, baseline home ownership, baseline credit score, and baseline checking-account balance. We focus on our regression-kink results, where the larger sample size allows for increased precision. We follow our earlier specifications by controlling for a local linear trend in pay and month-, year-, and state-of-loan effects. We instrument for loan size using the triple interaction of pay with the loan kink point and the relevant borrower characteristic. We dichotomize all borrower characteristics by splitting the sample at the median. Finally, we restrict our attention to the $500 kink point, as we do not have information on borrower characteristics for borrowers in the $300 kink point states.

The effect of loan size on default is larger for borrowers who are younger and who are male. A $50 increase in loan size decreases the probability that a borrower under 40 defaults by 2.2 percentage points, compared to only 0.6 percentage points for borrowers over 40. A $50 increase in loan size also decreases the probability that a male borrower under 40 defaults by 1.65 percentage points, compared to only 0.05 percentage points for female borrowers. However, both younger borrowers and male borrowers are more likely to default in general, implying that the relative effect of loan size on default is comparable between the different groups.

More striking is the lack of difference between borrowers with high and low baseline credit scores and high and low baseline checking-account balances. In both cases, the interaction term is economically small and not statistically significant. This suggests that the impact of loan size on repayment behavior is similar across high- and low-risk individuals. This pattern of results is also consistent with the regression-kink estimates from Table 5 showing similar impacts at the $300 and $500 kink points, despite large differences in the type of borrowers on those margins.

Table 3.6
Regression-Discontinuity Subsample Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Loan Amount	−0.120**	−0.527	0.032	−0.008	0.007	0.095	0.274
	(0.051)	(0.450)	(0.101)	(0.073)	(0.100)	(0.149)	(0.668)
Loan Amount x Over 40		−0.039					
		(0.042)					
Loan Amount x High Credit Sc.			0.020				
			(0.028)				
Loan Amount x High Checking				−0.021			
				(0.025)			
Loan Amount x Male					0.011		
					(0.037)		
Loan Amount x Black						−0.006	
						(0.037)	
Loan Amount x Home Owner							−0.006
							(0.062)
Observations	9,473	9,443	2,165	2,274	1,316	1,316	1,160

Notes: This table reports regression-discontinuity estimates interacted with borrower characteristics. The sample consists of first-time payday-loan borrowers living in states offering payday loans in $50 increments who are paid biweekly or semimonthly earning between $100 and $1100 every two weeks who report information on the relevant characteristic. The dependent variable is an indicator for bouncing a check on the first loan. All regressions instrument for loan amount using loan eligibility interacted with the relevant characteristic and control for a seventh-order polynomial in net pay, the borrower characteristic, and month-, year-, and state-of-loan effects. Standard errors are clustered by pay. Coefficients and standard errors are multiplied by 100. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 3.7
Regression-Kink Subsample Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Loan Amount	−0.032***	−0.044***	−0.029***	−0.034***	−0.010	−0.014**	−0.053***
	(0.004)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.013)
Loan Amount x Over 40		0.032***					
		(0.008)					
Loan Amount x High Credit Sc.			−0.001				
			(0.008)				
Loan Amount x High Checking				0.012			
				(0.008)			
Loan Amount x Male					−0.023*		
					(0.012)		
Loan Amount x Black						−0.012	
						(0.012)	
Loan Amount x Home Owner							0.020
							(0.016)
Observations	96,766	96,631	91,261	89,844	40,878	40,878	34,133

Notes: This table reports regression-kink estimates interacted with borrower characteristics. The sample consists of first-time payday-loan borrowers living in states offering payday loans in $1 or $10 increments who are paid biweekly or semimonthly earning more than $100 and within $1000 of a kink point who report information on the relevant characteristic. The dependent variable is an indicator for bouncing a check on the first loan. All regressions instrument for loan amount using an indicator for eligibility for the largest loan available in a state interacted with characteristic listed in the left-most column, and control for pay interacted with the listed characteristic, the listed characteristic, and month-, year-, and state-of-loan effects. Coefficients and robust standard errors are multiplied by 100. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

3.4.3. *Adverse Selection*

Table 8 presents OLS estimates relating default to loan size. Recall that these cross-sectional estimates combine the causal impact of loan size with the selection of borrowers into different size loans. Under our identifying assumptions discussed in Section 4, the magnitude of adverse selection is the coefficient from our OLS regressions minus the impact of moral hazard implied by Tables 4 and 5.

Following our earlier results, the dependent variable is an indicator variable equal to one if a loan ends in default. All specifications control for month-, year-, and state-of-loan effects. We report robust standard errors in parentheses and multiply all coefficients and standard errors by 100. Columns 1 and 5 present our baseline results using data from both firms in our sample and no controls other than month-, year-, and state-of-loan fixed effects. Columns 2 and 6 add controls for net pay, columns 3 and 7 add controls for age, race gender, marriage, credit score, and checking-account balance, and columns 4 and 8 add controls for the maximum loan a borrower is eligible for. Observations from Firms B and C only control for age and the maximum loan available, as other demographic controls are not available.

Consistent with the view that information frictions lead to credit constraints in equilibrium, there is a positive association between loan size and the probability of default. Scaling the estimates to be equivalent to our two-stage least squares results, a \$50 increase in loan size is associated with a 1.0 percentage point increase in the probability of default in our regression-discontinuity sample, and a 0.4 percentage point increase in the probability of default in our regression-kink sample. Controlling only for biweekly pay, a \$50 increase in loan size is associated with a 2.3 percentage point increase in the probability of default in our regression-discontinuity sample, and a 1.3 percentage point increase in our regression-kink sample. Controlling for borrower characteristics and loan eligibility yields similar results to those that control for pay only.

Table 3.8
OLS Estimates of the Effect of Loan Amount on Default

	RD Sample				RK Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Loan Amount	0.020***	0.046***	0.045***	0.047***	0.008***	0.026***	0.027***	0.029***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)
Biweekly Pay		−0.023***	−0.019***	0.009		−0.012***	−0.009***	−0.007***
		(0.002)	(0.002)	(0.008)		(0.000)	(0.000)	(0.000)
Age			−0.466***	−0.465***			−0.303***	−0.303***
			(0.033)	(0.033)			(0.008)	(0.008)
Black			0.472	0.408			3.151***	3.161***
			(1.997)	(1.997)			(0.266)	(0.266)
Male			2.337	2.440			2.432***	2.511***
			(1.878)	(1.880)			(0.285)	(0.285)
Credit Score			−0.041***	−0.042***			−0.018***	−0.018***
			(0.004)	(0.004)			(0.001)	(0.001)
Checkings			−0.001	−0.001			−0.001***	−0.001***
			(0.002)	(0.002)			(0.000)	(0.000)
Home Owner			−2.011	−2.053			−1.276***	−1.277***
			(1.905)	(1.907)			(0.340)	(0.340)
Direct Deposit			0.082	−0.175			−2.377***	−2.221***
			(1.576)	(1.575)			(0.258)	(0.258)
Garnishment			15.522*	15.410*			0.155	0.119
			(8.191)	(8.261)			(1.076)	(1.076)
Loan Eligibility				−2.943***				−0.525***
				(0.873)				(0.081)
R^2	0.028	0.039	0.066	0.067	0.037	0.048	0.066	0.067
Observations	9,473	9,473	9,473	9,473	130,025	130,025	130,025	130,025

Notes: This table reports OLS estimates of the cross-sectional correlation between loan amount and default. The regression-discontinuity (RD) sample consists of first-time payday-loan borrowers living in states offering payday loans in $50 increments who are paid biweekly or semimonthly earning between $100 and $1100 every two weeks. The regression-kink (RK) sample consists of first-time payday-loan borrowers living in states offering payday loans in $1 or $10 increments who are paid biweekly or semimonthly earning more than $100 and within $1000 of a kink point. All regressions control for month-, year-, and state-of-loan effects. Coefficients and robust standard errors are multiplied by 100. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Taken together with our moral-hazard estimates discussed above, our results from Table 8 imply that borrowers who *select* a \$50 larger loan are 5.4 to 8.65 percentage points more likely to default on their first payday loan in our regression-discontinuity sample, and 2.00 to 5.85 percentage points more likely to default in our regression-kink sample. These represent a 28 to 44 percent increase in the probability of default in our discontinuity sample, and a 16 to 47 percent increase in our regression-kink sample. The precision of both our two-stage least squares and OLS estimates result in our adverse-selection estimates also being highly statistically significant, with p-values of less than 0.001 across all specifications.

3.4.4. *Specification Checks*

This section presents results from a series of specification checks for our regression-discontinuity and regression-kink estimates. First, we test the assumption that individuals do not selectively borrow based on loan eligibility. Second, we replicate our results in states without the discontinuity as a more general falsification test.

Our first set of specification checks examines the assumption that individuals eligible for larger loans are not more or less likely to borrow. Such selective borrowing could invalidate our empirical design by creating discontinuous differences in borrower characteristics around the eligibility cutoffs. Although the continuity assumption cannot be fully tested, its validity can be evaluated by testing whether the observable characteristics of borrowers trend smoothly through the cutoffs and by testing the density of borrowers around the cutoffs.

Web Appendix Figure 1A plots observable borrower characteristics and biweekly pay for borrowers in our regression-discontinuity sample. Following our earlier results, we also plot predicted lines controlling for a seventh-order polynomial in pay, a linear spline in pay, and a local linear line stacking data from each eligibility cutoff. There is little evidence of the type of systematic selection that would bias our results. Borrower characteristics appear to trend smoothly through each cutoff.

Web Appendix Table 3 presents formal results testing whether observable baseline characteristics trend smoothly through the loan-eligibility cutoffs. We regress each baseline characteristic on the maximum loan for which a borrower is eligible, controlling for income and month-, year-, and state-of-loan effects. Consistent with the results from Web Appendix Figure 1A, none of the point estimates are statistically significant in any of the three specifications we consider.

Web Appendix Figure 1B plots the number of borrowers and biweekly pay for our regression-discontinuity sample. The bottom row of Web Appendix Table 3 presents formal estimates testing whether the number of borrowers trends smoothly through the loan-eligibility cutoffs. Specifically, we regress the number of borrowers in each $10 bin on a seventh-order polynomial in pay, a linear spline in pay, and local linear in pay stacking data from each cutoff. Consistent with our identifying assumptions, none of these specifications suggest that the number of borrowers changes with loan eligibility. Results are identical across a range of specifications and choice of binwidth.

Web Appendix Figure 2A and Web Appendix Table 4 present results testing whether observable characteristics trend smoothly in our regression-kink sample. Following our earlier results, we also plot predicted lines controlling for pay interacted with the kink point. The results from Web Appendix Figure 2A suggest that the fraction of borrowers who are black trends down after the kink point. There are also changes in direct deposit and garnishment. Conversely, gender, credit score, checking-account balance, home ownership, and age all appear to trend smoothly through the kink point. Formal estimates available in Web Appendix Table 4 further suggest we cannot rule out economically small differences at the kink point for a number of characteristics. Thus, our regression-kink estimates should be interpreted with this caveat in mind.

Web Appendix Figure 2B plots the number of borrowers and biweekly pay for our regression-kink sample. We also plot a predicted line from a seventh-order polynomial interacted with the kink point, the polynomial order that has the lowest Akaike criterion. The bottom row of Web Appendix Table 4 presents formal estimates from the same specification. Following Card et al. (2012) we report the coefficient and standard error on the linear interaction term. There is no evidence that the number of borrowers changes at the kink point, with the results from Web Appendix Table 4 ruling out even modest selection in or out of the sample around the kink point.

We conclude this section by considering a more general falsification test of our regression-discontinuity design. To ensure that our estimates identify discontinuities in loan size and default that exist due to institutional rules determining loan eligibility, we replicate our main results in our regression-kink sample, where loan size is not a discontinuous function of income before the kink point. As in the rest of our results, we restrict this falsification sample to biweekly borrowers with take-home pay between \$100 and \$1,100. These restrictions leave us with a large sample of 101,026 borrowers.

The first-stage estimates from our falsification test are presented in Web Appendix Figure 3 with corresponding regression results in Web Appendix Table 5. There is no evidence of an economically or statistically significant relationship between income and loan size in our falsification sample of states where loan size is not institutionally set to be a discontinuous function of pay. Loan amount trends smoothly through each cutoff, with the first-stage point estimates ranging from 1.65 to 2.75, with none of the point estimates reaching statistical significance.

Reduced-form estimates from our falsification test are presented in Web Appendix Figure 4 and Web Appendix Table 6. Again, there is no evidence of an economically or statistically significant relationship between pay and default in the falsification sample. Default trends smoothly through each cutoff, with none of the two-stage least squares estimates suggesting a statistically significant relationship between loan size and default.

3.5. Discussion

This paper has presented evidence that larger payday-loan amounts decrease the probability of payday-loan default. This is a surprising result given the prominence of moral hazard in the theoretical literature and the empirical relevance of moral hazard in other consumer-lending markets (e.g., Adams, Einav, and Levin (2009)). There are at least five potential reasons why moral hazard is not empirically relevant in payday lending.

First, it is possible that borrowers repay larger loans to maintain a larger credit line in the future. In this scenario, the marginal benefit of a higher credit line tomorrow is larger than the marginal benefit of defaulting on a larger loan today. This scenario also assumes that it is prohibitively costly for borrowers to increase their credit line in other ways, such as increasing earnings to qualify for a larger loan or petitioning the lender for an exemption. Payday firms in our sample report that they offer these types of exemptions on second loans, suggesting that this mechanism is unlikely to play an important role in explaining our results.

Second, borrowers may fear more aggressive collection efforts if they default on a larger loan. If lenders are able to increase the cost of default sufficiently, the marginal cost of default may increase faster with loan size than the marginal benefit. Conversely, the payday firms in our sample have no official policy of pursuing larger loans more aggressively, and there is no evidence that payday lenders are more effective at collecting larger loans in our sample. However, we are unable to rule out differences in borrower beliefs regarding collection efforts.

Third, larger loans may increase the ability of borrowers to repay in the future. For example, if electricity or telephone service is shut off, the time and expense to restart service can exceed the payday-loan fees. A larger payday loan may also allow an individual to fix her car and stay employed, or pay rent or her mortgage and avoid eviction or foreclosure. Consistent with this mechanism, approximately one-half of payday borrowers report that they plan to use their loan for bills, emergencies, transportation expenses, food or to repay another debt (Bertrand and Morse 2011). In a separate sample, approximately one-half of payday borrowers report that they plan to use their loan to deal with an unexpected expense shock, while another fifth report that they plan to use their loan to deal with an unexpected income shock. Only one-third of payday borrowers plan to use their loan for a discretionary expense (Elliehausen and Lawrence 2001).

Fourth, it is possible that individuals who do not qualify for a large enough loan substitute toward even more costly forms of credit which makes it more difficult to repay. Many sources of short-term credit are more expensive than payday loans, including overdraft charges on a checking account, returned check fees, credit-card late fees, and automobile-title loans. Consistent with this explanation, Skiba and Tobacman (2011) find that rejected payday-loan applicants are more likely to take out a pawn loan. This is likely because 80 percent of payday applicants have precisely $0 in available credit-card liquidity at the time of application, with 90 percent having less than \$300 in liquidity when they apply (Bhutta, Skiba, and Tobacman 2012).

Finally, our results are consistent with a number of alternative models of decision-making.[22] For instance, if borrowers suffer from limited attention, they may be more likely to repay larger loans due to their increased salience. Forward-looking borrowers suffering from limited attention problems may also be more likely to set reminders or seek commitment devices to repay larger loans (O'Donoghue and Rabin 2001). It is also possible that payday borrowers discount smaller dollar amounts more than larger amounts (e.g. the magnitude effect discussed by Loewenstein and Prelec (1992)).

3.6. Conclusion

This paper exploits sharp discontinuities in loan eligibility to test for moral hazard and adverse selection in the payday-loan market, one of the largest sources of subprime credit in the United States. Both regression-discontinuity and regression-kink approaches suggest that payday-loan borrowers are *less* likely to default when offered a larger loan. A \$50 larger payday loan leads to a 17 to 33 percent drop in the probability of default on the first loan. Conversely, we find evidence of economically and statistically significant adverse selection into larger payday loans when loan eligibility is held constant. Payday borrowers who choose a \$50 larger loan are 16 to 44 percent more likely to default on the first loan.

Given the emphasis placed on moral hazard by policymakers and within the theoretical literature, our results are somewhat surprising. We hope that our findings spur new work estimating the impact of moral hazard in other settings and continue to explore new identification strategies as we have done here. Our work also highlights the significant adverse-selection problems facing firms in the payday-loan market. Improved screening strategies or information sharing may play an important role in alleviating these frictions.

With that said, the welfare effects of resolving information frictions in the payday-loan market

[22] Campbell et al. (2011) discuss behavioral anomalies in the payday-loan market. See DellaVigna (2009) and Rabin (1998) for a broader discussion of potential deviations from the neoclassical model of decision-making.

are still unknown, as we cannot say with certainty what is driving our effects. A better understanding of which model of decision-making best characterizes the behavior of credit constrained borrowers would go a long way toward addressing this issue. We view the parsing out of these various mechanisms, both theoretically and empirically, as an important area for future research.

4. ARE HIGH QUALITY SCHOOLS ENOUGH TO REDUCE SOCIAL DISPARITIES? EVIDENCE FROM THE HARLEM CHILDREN'S ZONE

With Roland Fryer, Harvard University

4.1. Introduction

The typical charter school is no more effective at increasing test scores than the typical traditional public school (Gleason et al. 2010). Yet, an emerging body of research using admissions lotteries suggests that high-performing charter schools can significantly increase the achievement of poor urban students. Students attending an over-subscribed Boston-area charter school score approximately 0.4 standard deviations (hereafter σ) higher per year in math and 0.2σ higher per year in reading (Abdulkadiroglu et al. 2011). Promise Academy students in the Harlem Children's Zone (HCZ) score 0.229σ higher per year in math and 0.047σ higher per year in reading (Dobbie and Fryer 2011a). The Knowledge is Power Program (KIPP) schools – America's largest network of charter schools – and the SEED urban boarding school in Washington D.C. experience similar test score gains (Angrist et al. 2010, Tuttle et al. 2010, Curto and Fryer forthcoming).

An important open question is whether these increases in student achievement translate into comparable gains on medium-term outcomes such as high school graduation, college enrollment, drug-use, teen pregnancy, or incarceration. Charter advocates argue that high-performing charter schools are effective at implementing educational "best-practices" – frequent teacher feedback, data-driven instruction, an extended school day and year, and a relentless focus on achievement – which develop basic skills that lead to both gains on short-run state test scores and longer-term non-tested measures (Carter 2000, Thernstrom and Thernstrom 2004, Whitman 2008).[23] Conversely, critics argue that high-performing charter schools increase test scores through intense test

[23]There is also evidence that students assigned to high test score value-add teachers are more likely to attend college, earn higher salaries as adults, and are less likely to become pregnant as teenagers (Chetty, Friedman, and Rockoff 2011). Additionally, attending a high-quality public school can reduce crime and increase college enrollment even when there is little impact on state test scores (Cullen, Jacob, and Levitt 2006, Deming 2011, Deming et al. 2011), perhaps due to the development of non-tested forms of intelligence or changes in social networks (Heckman and Rubenstein 2001, Heckman et al. 2006, Segal 2008, Whitman 2008, Chetty et al. 2011).

prep (Haladyna, Nolen, and Hass 1991, Haladyna 2006, Jacob 2005), a paternalistic environment (Whitman 2008), strategic resource allocation (Jacob 2005), or blatant cheating (Jacob and Levitt 2003), without instilling long-term or general knowledge in their students.

In this paper, we use data from the Promise Academy in the HCZ to provide a "proof of concept" that the best practices used by high-performing charter schools can impact medium-term outcomes. Like many other high-performing charters, the Promise Academy largely adheres to the five tenets of effective charter schools identified by Dobbie and Fryer (2011b). The school has an extended school day and year, emphasizes the recruitment and retention of high-quality teachers, uses extensive data-driven monitoring to track student progress and assign students to small group-tutoring sessions based on these data, and makes a concerted effort to change the culture of achievement (Dobbie and Fryer 2011b). Web Appendix Table 1 provides evidence that suggests the Promise Academy charter school is emblematic of other successful charter schools, but not an outlier.

Our identification strategy exploits the fact that the Promise Academy is required to select students by lottery when the number of applicants exceeds the number of available slots for admission. The treatment group is composed of youth who are lottery winners and the control group consists of youth who are lottery losers. This empirical strategy allows us to provide a set of causal estimates of the effects of the Promise Academy.

Outcomes for our analysis come from survey data collected from youth entered in the 2005 and 2006 Promise Academy sixth grade admissions lotteries. The survey included questions about educational achievement and attainment, risky behaviors, and health outcomes. We also administered the Woodcock-Johnson math and reading tests as an alternative measure of cognitive ability, and included questions on a number of potential mechanisms such as non-cognitive skills, social networks, risk aversion, and discount rates. We surveyed 407 out of 570 lottery entrants, a high response rate for survey studies on low-income urban youth (Cullen, Jacob, and Levitt 2006, Kling, Liebman, and Katz 2007, Rodriguez-Planas 2012). We augment this survey data with administrative data on high school course-taking from the New York City Department of Education

(NYCDOE) and college enrollment data from the National Student Clearinghouse (NSC).

We find that the Promise Academy increases a wide range of human capital measures. Six years after the admissions lottery, lottery winners outscore lottery losers by 0.283σ higher on the no-stakes Woodcock-Johnson math exam, and by 0.119σ on the Woodcock-Johnson reading exam. On New York City's high school Regents exams, designed to measure mastery in core subjects, lottery winners pass approximately one additional exam, score 0.359σ higher on exams taken by the majority of the sample, and are more than twice as likely to take and pass more advanced exams such as chemistry and geometry. Lottery winners are also 14.1 percentage points more likely to enroll in college compared to lottery losers, and 21.3 percentage points more likely to enroll in a four-year college, a 102 percent increase from the control mean. Lottery winners are also 7.2 percentage points less likely to enroll in a two-year college, likely due to the fact that these youth enroll in a four-year college instead. Combining our five primary human capital variables into a single index measure, we find that lottery winners increase their human capital by 0.277σ compared to lottery losers.

The Promise Academy's effect on risky behaviors is mixed and we find very little evidence of impacts on self-reported health. Female lottery winners are 12.1 percentage points less likely to report being pregnant during their teenage years, a 71 percent drop from the control mean of 17 percent among lottery losers. Male lottery winners are 4.3 percentage points less likely to be incarcerated, essentially a 100 percent drop. Students who win the lottery to attend the Promise Academy report similar drug and alcohol use and criminal behavior as students who lose the lottery. An index measure of risky behavior that combines all four variables is positive and marginally significant (p-value$= 0.06$). Finally, there is little impact of the Promise Academy on asthma, obesity, or mental health, though lottery winners are more likely to report eating nutritious foods.

We complement our main analysis with two robustness checks. First, we consider the extent to which differential sample attrition threatens our estimates by calculating Lee (2009) bounds and imputing outcomes for youth who did not respond to the survey. Lottery winners were 11.8

percentage points more likely to respond to our survey. If lottery losers who did not respond to the survey differ in some important way, this could invalidate our empirical design by creating unobserved differences between the treatment and control groups. Second, we account for multiple-hypothesis testing by calculating p-values with an algorithm that accounts for the Familywise Error Rate (Westfall and Young 1993, Kling, Liebman and Katz 2007, Anderson 2008). The most conservative bounding procedures reduce some individual effects to statistical insignificance, but our main findings are not significantly altered by these robustness checks.

We conclude with a more speculative discussion on the potential mechanisms driving our results. First, we investigate the empirical importance of the HCZ neighborhood programs and the Promise Academy school policies by separately estimating the effects on youth who are more or less likely to receive neighborhood benefits based on their home address. Consistent with Dobbie and Fryer (2011a), we find little evidence that the neighborhood programs drive our results. Second, we consider the extent to which changes in test scores might explain the impact of the Promise Academy on non-test score outcomes. Using the cross-sectional relationship between test scores and non-test score outcomes reported by Chetty, Friedman, and Rockoff (2011), we find that only a small portion of our estimated effects can be explained by the test score change. Third, we estimate the impact of the Promise Academy on a number of other possible mechanisms. We find little impact on non-cognitive skills, social networks, or discount rates, though lottery winners are more averse to risk than lottery losers.

Our analysis has three important caveats. First, we present evidence from only one New York City charter school, which could differ from other high-performing schools in important ways that limit our ability to generalize the results. As discussed earlier, the inputs and impacts of the Promise Academy are similar to other high-performing charter schools, and turn-around efforts that use the similar practices have yielded similar results on state test scores (Angrist et al. 2010, Tuttle et al. 2010, Abdulkadiroglu et al. 2011, Dobbie and Fryer 2011a, Fryer 2011b). We chose to obtain a higher response rate on a detailed face-to-face survey with lottery entrants from one school, as opposed to a lower rate with lottery entrants from multiple schools using online or other methods, in order to maximize the internal validity of our study. The cost of this face-to-face approach – roughly $2,150 per student, or $895,000 for the entire sample – necessitated the focus on a single school.[24]

Second, the survey respondents may not have truthfully answered our questions. In particular, it is plausible that Promise Academy students were directly or indirectly pressured to overstate the impact of the school. However, results using administrative outcomes are even larger than the survey results, suggesting this issue does not significantly impact our findings.

Third, our analysis is necessarily limited to various medium-term outcomes. Longer-term outcomes, such as college graduation, earnings, and mortality, are not a part of our analysis due to the age of the lottery entrants.

The remainder of the paper is structured as follows. Section 2 provides a brief overview of the Harlem Children's Zone. Section 3 describes the data collected for this paper and our lottery-based research design. Section 4 estimates the impact of the Promise Academy on human capital, risky behaviors, and health. Section 5 discusses potential mechanisms. Section 6 concludes. There are five Web Appendices. Web Appendix A presents additional analyses to supplement the results in the text. Web Appendix B is a data appendix that details our sample and variable construction. Web Appendix C details the tracking and outreach efforts used to contact lottery entrants. Web Appendix D includes the full survey instrument. Finally, Web Appendix E details the algorithm used to calculate p-values corrected for multiple hypothesis-testing.

[24]Interviewing a random subsample of lottery entrants from multiple schools proved to be infeasible, as there are not enough charter schools with a large enough alumni sample and binding admissions lotteries for a study with multiple high-performing schools in a single city. The additional cost of interviewing subjects in multiple cities would have forced a much smaller survey population.

Table 4.1
Characteristics of Charter Schools

	HCZ	NYC	
	Promise Academy	Above Median	All Middle Schools
Human Capital	(1)	(2)	(3)
Teacher Formal Feedback	3.00	4.21	2.84
Teacher Informal Feedback	12.50	14.08	8.39
Total Teacher Hours	45.00	57.08	54.68
Max Teacher Pay	11.00	9.08	8.55
Data Driven Instruction			
Number of Interim Assessments	9.00	3.90	2.83
Tracking Using Data	1.00	0.33	0.57
Parent Engagement			
Academic Feedback	13.50	12.67	10.25
Behavior Feedback	54.00	26.25	21.36
Regular Feedback	54.00	13.90	8.15
Tutoring			
High Quality Tutoring	0.00	0.17	0.07
Any Tutoring	1.00	0.83	0.79
Small Group Tutoring	0.00	0.20	0.18
Frequent Tutoring	1.00	0.60	0.45
Instructional Time			
+25% Increase in Time	1.00	0.83	0.64
Instructional Hours	7.50	8.25	8.04
Instructional Days	210.00	193.50	188.64
Culture			
High Expectations	0.00	0.83	0.50
School-wide Discipline	0.00	0.33	0.36
Traditional Inputs			
Small Classes	0.00	0.40	0.64
High Expenditures	1.00	0.75	0.67
High Teachers with MA	1.00	0.40	0.64
Low Teachers without Certification	0.00	0.20	0.45
Schools	1	5	13

Notes: This table reports results from a survey of 35 New York City charter schools administered by Dobbie and Fryer (2011b). Column (1) reports the mean of each variable for the Promise Academy Middle School. Column (2) includes all schools with entry in middle school grades (5th - 8th) whose average treatment effects on Math and ELA scores are above the median in the sample. Column (3) includes all Middle Schools in the sample with a tested grade in 2010-2011. See Dobbie and Fryer (2011b) for variable definitions and codings.

4.2. Harlem Children's Zone

The Harlem Children's Zone consists of over 20 neighborhood and school programs meant to address the myriad problems that children from low income families face – housing, schools, crime, asthma, and so forth – through a "conveyor belt" of services from birth to college. The approach is based on the assumption that one must improve both communities and schools to have a long-term impact on disadvantaged youth. Starting with a 24-block area in central Harlem, the Zone expanded to a 64-block area in 2004 and a 97-block area in 2007.

Neighborhood Programs

The HCZ neighborhood programs serve as broad investments in community development. These programs include early childhood programs, K-12 tutoring, after-school programs, a college success office, family programs, health programs, a foster-care prevention program, a tax assistance program, and so on. Consistent with Wilson's (1987) theory of non-linear neighborhood effects and cycles of poverty, HCZ's vision is to create a "tipping point" in the neighborhood so that children are surrounded by an enriching environment of college-oriented peers and supportive adults. HCZ neighborhood programs are available to anyone living near HCZ and serve more than 8,000 youth and 5,000 adults each year.

School Programs

The Promise Academy largely adheres to the five correlates of effective schools identified by Dobbie and Fryer (2011b). The Promise Academy has an extended school day and year with coordinated after-school tutoring and additional classes on Saturdays for children who need remediation in mathematics and English Language Arts skills. Promise Academy middle schoolers spent 1,785 hours in school during the 2010-11 school-year, 46.1 percent more time than the typical New York City public school student and 11.8 percent more than the typical student in a high-performing New York City charter school (Dobbie and Fryer 2011b). The Promise Academy also emphasizes the recruitment and retention of high-quality teachers and uses measures of test score value-added to incentivize and evaluate current teachers. In the search for high-achieving teachers, the Promise Academy had high teacher turnover during the first three years of operation, with 48 percent of teachers not returning for the 2005-2006 school year, 32 percent leaving before 2006-2007, and 14 percent leaving before 2007-2008. The Promise Academy also uses extensive data-driven monitoring to track student progress and differentiate instruction, with students who have not met the required benchmarks receiving small-group tutoring. Like other "No Excuses" charters, the Promise Academy also makes a concerted effort to change the culture of achievement, stressing the importance of hard work in achieving success. It is assumed that every student will enroll in college, with the goal of establishing college attendance as the default option.[25]

4.3. Data and Research Design

4.3.1. Data and Summary Statistics

We merge information from lottery files at the Harlem Children's Zone, administrative records on student demographics and outcomes from the New York City Department of Education (NYCDOE), information on college enrollment from the National Student Clearinghouse (NSC), and survey data collected from Promise Academy lottery participants for the purposes of this study.

Survey Data

In order to investigate the impact of the Promise Academy on various medium-term outcomes, we conducted in-person interviews with youth who entered the 2005 and 2006 sixth grade admissions lotteries. Web Appendix B contains additional information on the coding of variables. Web Appendix C describes our tracking and survey administration, and Web Appendix D contains the full survey instrument and protocols used to administer the survey. This section summarizes the most relevant information from our Web Appendices.

[25]There are at least two potentially important differences between the Promise Academy and the typical high-performing New York charter school. First, the Promise Academy does not require parents or students to sign a behavioral contract, resulting in students that are more similar to the surrounding neighborhood than other charter schools. HCZ argues that only the most motivated and trusting parents are willing to sign even a non-binding contract. Second, Promise Academy students are exposed to a wide range of wrap-around services that are not available at most charter schools. The schools provide free medical and dental services, student incentives for achievement, nutritious cafeteria meals, parental engagement and supports (e.g. bus fare, meals), and so on.

Table 4.2
An Accounting of the Sample

	Pooled		2005 Lottery		2006 Lottery	
	Winner	Loser	Winner	Loser	Winner	Loser
	(1)	(2)	(3)	(4)	(5)	(6)
Lottery Entrants	189	410	96	237	93	173
Matched To NYC Data	181	390	90	223	91	167
Match Rate	0.958	0.951	0.938	0.941	0.978	0.965
Survey Pool	189	381	96	222	93	159
Survey Respondents	150	257	76	145	74	112
Survey Response Rate	0.794	0.676	0.792	0.653	0.796	0.709

Notes: This table describes the match rate for Promise Academy lottery entrants to New York City administrative data and response rates for the in-person survey. The first row tabulates all students who entered the Promise Academy Middle School lottery in the Spring of 2005 or 2006, excluding students who were automatically admitted due to sibling preferences. The second row tabulates students whom we are able to match to New York City administrative data using the matching algorithm described in the Web Appendix. The third row displays the percentage of students who are successfully matched. Our survey pool includes all lottery entrants except for the group of randomly selected lottery losers that were used to test and calibrate the survey instrument during the Fall of 2011, along with any records that were discovered to be mistaken matches and/or duplicates during the survey process. The fifth row tabulates all students who completed our survey, and the sixth reports the percentage of the survey pool who responded.

From January 2012 through July 2012, we attempted to contact 570 Promise Academy lottery entrants using letters, phone calls, and home visits.[26] Using information from NYCDOE administrative data, internet searches of current addresses, and publicly available address records, we were able to successfully contact 501 of these lottery entrants. Contacted youth were offered a financial incentive between $40 and $200 to participate in the study, with the amount increasing as the survey period progressed. Parents were also offered an additional cash incentive to review the consent form. Of the 501 lottery entrants we contacted, 407 agreed to participate in the study, 61 refused to participate in the study, and 33 were unable to participate due to distance, language barriers, health, incarceration, or another obstacle. We obtained a final response rate of 79.4 percent for lottery winners and 67.6 percent for lottery losers. Section 3.2 examines the differences between lottery winners and lottery losers who respond to our survey, finding no evidence of differential selection into our sample along observable characteristics or administrative outcomes.

The questionnaire, based largely on the comprehensive survey used to evaluate the Moving to Opportunity experiment (Kling, Liebman, and Katz 2007), took approximately 110 minutes to complete. The survey was designed to investigate three main outcomes: (1) human capital, (2) risky behaviors, and (3) health. We also asked about non-cognitive skills, peer networks, and economic preferences in order to assess potential underlying mechanisms.

Human capital is measured through the Woodcock-Johnson Broad Math and Reading tests, which is meant to augment the human capital measures available in the NYCDOE and NSC datasets.[27] The Woodcock-Johnson exams are designed to test general knowledge rather than the subject-specific skills emphasized on New York State tests. The assessments are designed

[26]There were 599 unique entrants in the 2005 and 2006 Promise Academy admissions lotteries. We randomly selected 30 lottery losers to test and calibrate the survey instrument, leaving 189 lottery winners and 381 lottery losers in the potential survey sample after a duplicate row was discovered in the pretest sample. Results are identical including the pre-test respondents.

[27]The Woodcock-Johnson Brief Battery that we use in our survey is an updated version of the Woodcock-Johnson Revised Battery administered as a part of the MTO evaluation. Accordingly, there is not perfect alignment between the sub-tests. We followed the advice of Woodcock-Johnson staff and administered the four sub-tests included in the MTO follow-up – Letter-Word Identification, Passage Comprehension, Applied Problems, and Calculation – in addition to the Math Fluency and Reading Fluency sections. Following Kling, Liebman, and Katz 2007, we omit the Writing sections to reduce the length of the survey. Treatment effects for each individual sub-test can be found in Web Appendix Table 3.

to be appropriate for all grades and ability levels and to have a high degree of internal reliability.[28] The Woodcock-Johnson Broad Math score is composed of Applied Problems, Calculation, and Math Fluency subscores. The Applied Problems section consists of word problems read aloud to youth. The Calculation section tests computation skills ranging from arithmetic to Calculus. The Math Fluency section requires youth to answer as many simple questions as possible in three minutes. The Broad Reading score consists of Letter-Word Identification, Passage Comprehension, and Reading Fluency subscores. The Letter-Word Identification section tests pronunciation of increasingly difficult words. The Passage Comprehension questions require youth to identify a word or phrase that completes a sample sentence. The Reading Fluency section, like the Math section, requires youth to answer as many simple questions as possible in three minutes. Web Appendix B contains additional details on the Woodcock-Johnson and the administration of the tests.

Risky behaviors are measured through a series of questions on pregnancy, controlled substance use, and crime. For pregnancy, we ask female youth if they have ever been pregnant, even if no child was born. In our sample, 14.6 percent of females have been pregnant at some point. We measure criminal behavior using an indicator for incarceration at the time of our survey. We also constructed an index based on youth' self-reported criminal behaviors, such as theft, destruction of property, fighting, or carrying a gun. The reported incidence of these behaviors is relatively low. Twenty-two percent of control youth report having ever been in a serious fight, and 14.1 percent report having stolen an item worth less than $50. Rates of all other criminal behaviors we measure are less than ten percent. To measure drug and alcohol use, we construct a summary index based on whether a youth reports that she has consumed alcohol in the last 30 days, smoked marijuana in the last 30 days, or used hard drugs within the past year.

We measure mental health using the K6 anxiety scale used in Kling, Liebman, and Katz (2007), standardized to have a mean of zero and standard deviation of one in the control group. Physical

[28] Sanbonmatsu et al. (2006) analyze test results in the Panel Study of Income Dynamics Child Development Supplement and find that the internal reliability of the test is strong for a population similar to ours, with scores for eight to seventeen year-old black students showing a correlation between 0.5 and 0.6 with the same test taken five years earlier. In our sample, the correlation between students' Woodcock-Johnson scores and their eighth grade state test scores is approximately 0.6 in both math and reading.

health is measured using an index based on indicators for self-reported poor health, having had an asthma attack in the past year, having a Body Mass Index (BMI) above the 95th percentile for the respondent's age and gender, and having reported chronic health problems. To investigate health risk factors, we ask about the number of times in the past week the youth has consumed foods such as fruits and vegetables, soft drinks, savory snacks, and fast food. We use these responses to create a nutrition index, reversing the sign on the unhealthy food variables. We also construct a health behavior index from questions about having a physical examination in the past year, the frequency of light exercise, the frequency of vigorous exercise, and having a dental exam in the past year. These measures of health-related behavior are important to the extent that many ailments are not easily detected among teenagers. For instance, while black adults are one and a half times more likely to develop hypertension and diabetes than white adults (Lopes and Port 1995), the rates of these diseases among black and white youth are roughly the same (Liese et al. 2006). However, many risk factors for both hypertension and diabetes, such as childhood obesity and youth dietary patterns, are more prevalent in black youth.

The remainder of the survey explores three potential exploratory theories that may explain any impacts of the Promise Academy. First, we explore the importance of non-cognitive skills by assessing self esteem, persistence, and locus of control. Second, we measure differences in peer networks by asking youth to how important it is for their friends to study, stay in school, and attending class regularly, in addition to whether their friends use drugs, drink alcohol, smoke cigarettes, steal, fight, and join gangs. Finally, we measure changes in discount rates and risk aversion, both common determinants of decision-making in economic models.

Administrative Data

We augment our in-person survey data with administrative data from the Harlem Children's Zone, NYCDOE, and NSC. The data from the Harlem Children's Zone consist of lottery files from the 2005 and 2006 middle school lotteries. To ensure that all youth in the lottery have an equal chance of being admitted to the Promise Academy, we drop entrants with a sibling that received a winning lottery number in a previous year, as these entrants are automatically admitted. Entrants with a sibling entered in a Promise Academy in the same year are included in our analysis, although we control for the fact that these entrants have a higher probability of admission due to potential admission through sibling preference. Results are identical dropping all siblings. When youth enter more than one lottery, we only include them in the first lottery cohort. A typical student's data include her name, birth date, parents' or guardians' names, home address, and lottery outcome. Following Dobbie and Fryer (2011a), we define lottery winners as youth who receive a winning lottery number or whose waitlist number was below the average highest number called across both years.

The NYCDOE data contain student-level administrative data on approximately 1.1 million students across the five boroughs of the NYC metropolitan area. The data include information on student race, gender, free and reduced-price lunch eligibility, behavior, attendance, matriculation for all students, state math and English Language Arts (ELA) test scores for students in grades three through eight, and Regents test scores for high school students. The data also include a student's first and last name, birth date, and address. We have complete NYCDOE data spanning the 2003-2004 to 2010-2011 school years, with test score and basic demographic data available from the 1999-2000 school year onwards.

The state math and ELA tests are high-stakes exams conducted every year for third through eighth grade students. All public school students, including those attending charters, are required to take the math and ELA tests unless they are medically excused or have a severe disability. We normalize test scores to have a mean of zero and a standard deviation of one for each grade, subject, and year across the entire New York City sample.

Regents Exams are statewide subject examinations required for high school graduation. In order to graduate, students must score 65 or higher on Global History and Geography, U.S. History and Government, Comprehensive English, at least one math exam, and at least one science exam. To receive Advanced Designation, students must pass all of exams required for graduation, along with two additional math exams and a second science exam. We create two measures to capture general performance on Regents. Our first measure is the total number of Regent exams passed.

The second is the average score on the Living Environment, Global History, and Integrated Algebra exams standardized to have a mean of zero and standard deviation of one in the entire New York City sample. These are the only three Regents exams taken by over 70 percent of both lottery winners and lottery losers. If youth are missing one or two of these exams, we impute the mean score in the lottery sample when calculating the average. Results are nearly identical dropping these youth or calculating the average score only on taken exams. Web Appendix Table 1 presents estimates on taking each exam, passing each exam, and exam score conditional on taking.

The HCZ data were matched to the New York City administrative data using name and date of birth. We were able to match 95.8 percent of lottery winners to the NYC data (N=189), and 95.1 percent of lottery losers (N=410). Our match rates and attrition are similar to previous work using charter lottery data (e.g. Hoxby and Muraka 2009, Angrist et al. 2010, Angrist et al. 2011, Curto and Fryer forthcoming, Dobbie and Fryer 2011b, Abdulkadiroglu et al. 2011). Additional information on the match rates and attrition for each lottery cohort are available in Table 2, with additional details on the match procedure available in Web Appendix B.

To explore the impact of HCZ attendance on college outcomes, we also match the lottery admissions records to information on college attendance from the National Student Clearinghouse (NSC), a non-profit organization that maintains enrollment information for nearly every college and university in the country. The NSC data contain information on enrollment and degrees granted for each college that a student attends. The Promise Academy lottery data were matched to the NSC database by NSC employees using each student's full name, date of birth, and high school graduation date. Youth who are not matched to the NSC database are assumed to have never enrolled in college, including one (unknown) student whose record was blocked by her school. NSC data is available for the entire 2005 lottery cohort.

Table 4.3
Summary Statistics

	All	HCZ	Lottery Sample			Survey Respondents		
	NYC	Area	Winners	Losers	Difference	Winners	Losers	Difference
Baseline Characteristics	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	0.489	0.490	0.453	0.541	−0.088*	0.456	0.564	−0.108*
White	0.140	0.018	0.011	0.010	0.001	0.000	0.012	−0.012*
Black	0.328	0.637	0.845	0.824	0.021	0.859	0.806	0.053
Hispanic	0.395	0.320	0.133	0.161	−0.028	0.134	0.174	−0.040
Free Lunch	0.842	0.878	0.821	0.811	0.010	0.846	0.809	0.037
5th Grade Sp. Ed.	0.096	0.150	0.052	0.055	−0.003	0.056	0.058	−0.002
5th Grade LEP	0.104	0.082	0.035	0.041	−0.006	0.035	0.046	−0.011
5th Grade Math	−0.012	−0.292	−0.248	−0.284	0.036	−0.258	−0.269	0.011
5th Grade ELA	0.005	−0.272	−0.242	−0.260	0.018	−0.274	−0.234	−0.040
Enrollment Outcomes								
Attended Promise Academy	0.001	0.040	0.630	0.066	0.564***	0.707	0.082	0.625***
Years in Promise Academy	0.004	0.172	2.767	0.227	2.540***	3.247	0.323	2.924***
Observations	154,988	1,311	189	410	599	150	257	407

Notes: This table describes summary statistics and balance tests for baseline observable data and post-lottery enrollment outcomes. Column (1) reports means for all New York City students enrolled in fifth grade during the Spring of 2005 or 2006. Column (2) reports means for New York City public school students living within 400 meters of the original 24-block area of HCZ, ranging from 116th to 123rd Streets, 5th Avenue to 8th Avenue. Columns (3) through (5) report means and differences for all students who enter the sixth grade lottery. Columns (6) through (8) report the same information for students who respond to our in-person survey. Differences control for lottery year effects, indicators for having a sibling enrolled in the same lottery, and a sibling-year interaction term. 5th Grade Math and 5th Grade ELA indicate students' scores on the New York State Math and English Language Arts tests taken in the pre-lottery year. These scores are standardized to have mean zero and standard deviation one for the entire New York City sample by year. LEP denotes students who received special assistance due to Limited English Proficiency. Free Lunch indicates students who meet federal guidelines to receive free or reduced price lunch. See the Data Appendix for full variable definitions. ***, **, and * indicate statistically significant differences between lottery winners and losers with 99%, 95%, and 90% confidence, respectively.

Columns 1 through 4 of Table 3 present summary statistics for baseline characteristics for our lottery sample and two comparison populations. We report separate sample means for all NYC students who were enrolled in 5th grade in the 2004-05 or 2005-06 school year, all such students who live in the HCZ neighborhood, lottery winners, and lottery losers. Eighty-four and a half percent of lottery entrants are black, compared to 32.8 percent of NYC fifth graders and 63.7 percent of neighborhood fifth graders. Promise Academy lottery entrants under-perform the City average on math and ELA tests by roughly a quarter of a standard deviation. Lottery entrants score marginally higher than their neighbors, but the difference is not statistically or economically significant. Taken together, these summary statistics suggest that the Promise Academy serves a disproportionately black population whose academic performance is similar to students in their geographic area.

4.3.2. Research Design

To estimate the causal impact of providing student, teacher, and parent incentives on outcomes, we estimate both Intent-To-Treat (ITT) effects and Local Average Treatment Effects (LATEs). The ITT estimates measure the causal effect of winning the Promise Academy admissions lottery by comparing the average outcomes of youth who 'won' the lottery to the average outcomes of youth who 'lost' the lottery:

$$outcome_i = \mu + \gamma X_i + \pi Z_i + \sum_j \nu_j Lottery_{ij} + \sum_j \phi_j Lottery_{ij} * 1(sibling_i) + \eta_i \qquad (12)$$

where Z_i is an indicator for winning an admissions lottery, and X_i includes controls for gender, race, 5th grade special education status, eligibility for free or reduced-price lunch, receipt of Limited English Proficiency (LEP) services, and a quadratic in two prior years of math and ELA test scores. $Lottery_{ij}$ is an indicator for entering the middle school lottery in year j, and $1(sibling_i)$ indicates whether student i had a sibling enter a Promise Academy lottery in the same year. Equation (12) identifies the impact of being offered a chance to attend the Promise Academy, π, where the lottery losers form the control group corresponding to the counterfactual state that would have occurred for youth in the treatment group if they had not been offered a spot in the charter school.

Under several assumptions (that the lottery outcomes are random, that winning the lottery does not prevent anyone from attending who would have otherwise enrolled, and that being selected affects outcomes through its effect on Promise Academy enrollment), we can also estimate the causal impact of attending the Promise Academy. This parameter, commonly known as the Local Average Treatment Effect, measures the average effect of attending the Promise Academy on youth who attend the school as a result of winning the admissions lottery (Angrist and Imbens 1994). The LATE parameter can be estimated through a two-stage least squares regression of student achievement on an indicator variable for having ever attended the Promise Academy (PA_i), using the lottery offer Z_i as an instrumental variable for the first-stage regression. The second-stage equations for the two-stage least squares estimates therefore take the form:

$$outcome_i = \mu + \gamma X_i + \pi PA_i + \sum_j \nu_j Lottery_{ij} + \sum_j \phi_j Lottery_{ij} * 1(sibling_i) + \eta_i \tag{13}$$

and the first stage equation is:

$$PA_i = \alpha + \delta X_i + \lambda Z_i + \sum_j \theta_j Lottery_{ij} + \sum_j \iota_j Lottery_{ij} * 1(sibling_i) + \kappa_i \tag{14}$$

where λ measures the impact of the lottery offer on the probability of attending the Promise Academy. There is a powerful first-stage effect of winning the lottery on Promise Academy enrollment. Table 3 shows that 63.2 percent of lottery winners attend the Promise Academy at some point, compared to 6.5 percent of lottery losers. The typical lottery winner attends the Promise Academy for 2.8 years, over 2.5 more years than the typical lottery loser.

One potential threat to a causal interpretation of our estimates is that the Promise Academy admissions offer is not random ($E[\eta_i|Z_i] \neq 0$). We evaluate this possibility in column 5 of Table 3 by examining observed differences between lottery winners and lottery losers in the NYCDOE data. Lottery winners are 8.8 percentage points less likely to be female in the NYC sample. There are no other statistically significant differences between lottery winners and lottery losers, and a joint F-test that all coefficients are equal to zero has a p-value of 0.789.

A second threat to our interpretation of the estimates is that lottery entrants may have selectively responded to our survey. In particular, one may be concerned that lottery winners were 11.8 percentage points more likely to respond (see Table 2). If lottery losers who did not respond to our survey differ in some important way, this could invalidate our empirical design by creating unobserved differences between the treatment and control groups. We investigate selection into the survey sample in three ways: (1) measuring observed differences between lottery winners and lottery losers in the survey sample, (2) correlating survey response with baseline characteristics for lottery winners and lottery losers, and (3) correlating survey response with observed administrative outcomes for lottery winners and lottery losers.

Column 8 of Table 4 reports the difference between lottery winners and lottery losers in our survey sample following our results from column 5. Lottery winners in the survey sample are 10.8 percentage points less likely to be female, and 1.2 percentage points less likely to be white. There are no other statistically significant differences between lottery winners and lottery losers in the survey sample, and a joint F-test that all differences are equal to zero has a p-value of 0.389. Taken together with our results from column 5 of Table 4, these results suggest that selection into the survey sample is similar for lottery winners and lottery losers.

Panel A of Table 4 explores selection into our survey sample further by reporting results from a series of regressions of an indicator for survey response on baseline characteristics. The sample is restricted to lottery entrants in the survey pool who we are able to match to the NYCDOE data. All regressions include cohort fixed effects, an indicator for having a sibling in the same lottery, and a sibling-by-cohort interaction. Column 1 reports regression results for the pooled sample of lottery entrants. The coefficients are all small and statistically insignificant, and a joint F-test of all of the listed variables are equal to zero has a p-value of 0.867. These results suggest that baseline characteristics are not systematically associated with survey response.

Table 4.4
Youth Characteristics and Survey Response

	All Lottery Entrants	Lottery Winners	Lottery Losers	Difference
Panel A. Characteristics	(1)	(2)	(3)	(4)
Female	0.057	−0.022	0.091*	−0.112
	(0.039)	(0.064)	(0.050)	(0.081)
Black	−0.005	0.096	−0.045	0.139
	(0.049)	(0.091)	(0.060)	(0.108)
Free Lunch	0.026	0.114	−0.029	0.141
	(0.051)	(0.088)	(0.064)	(0.108)
5th Grade Sp. Ed.	0.016	0.047	0.022	0.027
	(0.092)	(0.122)	(0.117)	(0.168)
5th Grade LEP	0.064	−0.365	0.141	−0.508*
	(0.120)	(0.263)	(0.124)	(0.288)
5th Grade Math	−0.013	0.015	−0.025	0.040
	(0.035)	(0.058)	(0.046)	(0.073)
5th Grade ELA	0.007	−0.051	0.043	−0.095
	(0.033)	(0.051)	(0.044)	(0.067)
Missing 5th Grade Math	−0.073	−0.383	−0.029	−0.352
	(0.160)	(0.273)	(0.190)	(0.330)
Missing 5th Grade ELA	0.023	0.529**	−0.093	0.624**
	(0.135)	(0.262)	(0.161)	(0.305)
Missing Demographics	0.004	−0.114	0.027	−0.143
	(0.139)	(0.187)	(0.173)	(0.253)
	541	181	360	541
Panel B. Observed Outcomes				
Eight Grade Math	0.049**	0.034	0.036	−0.002
	(0.023)	(0.035)	(0.032)	(0.047)
	452	157	295	452
Eight Grade ELA	−0.007	−0.038	0.007	−0.045
	(0.030)	(0.037)	(0.043)	(0.056)
	457	160	297	457
College Enrollment	0.207***	0.204***	0.189***	0.016
	(0.048)	(0.074)	(0.063)	(0.097)
	298	90	208	298
p-value from Joint F-test Panel A	0.867	0.241	0.667	0.163
p-value from Joint F-test Panel B	0.000	0.006	0.016	0.808

Notes: This table reports the results of OLS regressions of an indicator for survey response on baseline characteristics and observed outcomes. All regressions control for lottery-year indicators, indicators for having a sibling enrolled in the same lottery, and a sibling-year interaction term. Regressions in Panels B also control for the baseline demographic variables summarized in Table 3 and a quadratic of 4th and 5th grade math and ELA test scores. The final two rows report the p-value from a joint F-test of the null hypothesis that all coefficients in each Panel equal zero, estimated via seemingly unrelated regression in Panel B. Heteroskedasticity-robust standard errors are reported in parenthesis. The number of observations is reported below the standard errors in Panel B. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Columns 2 and 3 reports results of the same regression estimated separately for lottery winners and lottery losers, and Column 4 reports the difference between lottery winners and lottery losers. Lottery winners eligible for LEP at baseline are 50.8 (28.8) percentage points less likely to respond to the survey compared to lottery losers eligible for LEP, and lottery winners missing a 5th grade ELA score are 62.4 (30.5) percentage points more likely to respond than lottery losers missing an ELA score. There are no other significant differences between lottery winners and lottery losers, however, and a joint F-test of the individual differences yields a p-value of 0.163.[29]

Panel B of Table 4 reports results correlating survey response with administrative outcomes that are available for both respondents and non-respondents. By examining survey response along realized outcomes, we are able to determine whether survey response differs by changes in outcomes not predicted by baseline characteristics. The administrative outcomes available for this test include eighth grade math scores, eighth grade ELA scores, and college enrollment. In the pooled sample, a one σ increase in eighth grade math scores is associated with a 4.9 (2.3) percentage point increase in the probability of response, and college enrollment is associated with a 20.7 (4.8) percentage point increase in survey response. Columns 2 and 3 present results for lottery winners and lottery losers separately, and column 4 reports the difference between the two groups. There is nearly identical selection into the survey sample among lottery winners and lottery losers. None of the individual differences are statistically significant, and a joint F-test on the null that all three differences are equal to zero yields a p-value of 0.808. Thus, while there is positive selection into our survey sample based on realized outcomes, there is no evidence that lottery winners and lottery losers differentially select into the survey sample.

[29]In results available upon request, we correlate survey response with predicted outcomes using baseline variables. Consistent with the results from Table 3 and Panel A of Table 4, there are no significant predictors of survey response among lottery winners or lottery losers. There are also no statistically significant differences between the lottery winners and lottery losers, and a joint F-test on the null that all five differences are equal to zero yields a p-value of 0.987.

4.4. Analysis

4.4.1. Main Outcomes

We estimate the impact of the Promise Academy in the Harlem Children's Zone on human capital outcomes, risky behaviors, and physical and mental health.

A. Human Capital

Dobbie and Fryer (2011a) find that Promise Academy students gain 0.229σ in math and 0.047σ in ELA per year on the required state exams. To provide evidence on whether these state test score gains reflect increases in general knowledge and skills, as opposed to test-specific skills, we estimate the impact of the Promise Academy on a number of alternative measures of human capital.

Panel A of Table 5 presents ITT and LATE estimates of the impact of the Promise Academy on human capital outcomes. Woodcock-Johnson results include lottery entrants who responded to the survey and complete the indicated Woodcock-Johnson test. High school Regents results include lottery entrants who attend a NYC high school for at least one year, while college enrollment results include all 2005 lottery entrants. Each regression controls for the demographic variables listed in Table 3, lottery effects, sibling by lottery effects, and a quadratic in 4th and 5th grade math and ELA scores. We report standard errors that are robust to arbitrary heteroskedasticity throughout.

Lottery winners score 0.283σ (0.083) higher than lottery losers on the math portion of the Woodcock-Johnson test, and 0.119σ (0.083) higher on the reading portion. Youth who attend the Promise Academy due to a winning lottery draw score 0.439σ (0.121) higher in math and 0.185σ (0.123) higher in reading. Attending the Promise Academy has the largest impact on Math Calculation, with Promise Academy students scoring 0.595σ (0.127) higher than they otherwise would have. Promise Academy students also score 0.366σ (0.153) higher in Math Fluency, and 0.325σ (0.138) higher on Letter-Word Identification. The estimated impacts on the other sub-test results are not statistically significant.

Table 4.5
The Impact of Attending the Promise Academy on Human Capital, Risky Behaviors, and Health

	CM	ITT	LATE
Panel A. Human Capital	(1)	(2)	(3)
Woodcock Johnson Math	0.000	0.283***	0.439***
	(1.000)	(0.083)	(0.121)
	243	386	386
Woodcock Johnson Reading	0.000	0.119	0.185
	(1.000)	(0.083)	(0.123)
	243	386	386
Regents Passed	2.385	1.026***	1.657***
	(2.414)	(0.230)	(0.336)
	327	482	482
Regents Test Scores	−0.045	0.359***	0.560***
	(0.672)	(0.064)	(0.090)
	285	423	423
College Enrollment	0.288	0.141**	0.242**
	(0.454)	(0.061)	(0.097)
	236	313	313
Human Capital Index	−0.047	0.277***	0.465***
	(0.830)	(0.068)	(0.105)
	391	552	552
Panel B. Risky Behaviors			
Ever Pregnant (Female)	0.170	−0.121***	−0.183***
	(0.377)	(0.046)	(0.067)
	141	205	205
Incarcerated (Male)	0.041	−0.043**	−0.075**
	(0.200)	(0.018)	(0.030)
	145	234	234
Drug/Alcohol Index	−0.001	−0.016	−0.025
	(0.692)	(0.067)	(0.103)
	256	406	406
Criminal Behavior Index	0.000	−0.004	−0.007
	(0.618)	(0.065)	(0.101)
	257	407	407
Risky Behavior Index	0.053	−0.135*	−0.223*
	(0.896)	(0.072)	(0.117)
	289	445	445

Table 4.5 (Continued)
The Impact of Attending the Promise Academy on Human Capital, Risky Behaviors, and Health

	CM	ITT	LATE
Panel C. Health	(1)	(2)	(3)
Nutrition Index	0.000	0.108*	0.173*
	(0.572)	(0.061)	(0.095)
	257	407	407
Mental Health	0.000	−0.034	−0.054
	(1.000)	(0.103)	(0.161)
	254	403	403
Physical Health Index	0.000	−0.050	−0.079
	(0.599)	(0.063)	(0.098)
	257	407	407
Health Behavior Index	−0.001	0.031	0.050
	(0.499)	(0.052)	(0.081)
	257	407	407
Health Index	0.000	0.032	0.051
	(0.533)	(0.057)	(0.087)
	257	407	407

Notes: This table reports estimates of the effect of attending the Promise Academy. Column (1) reports the mean and standard deviation of each variable for the control group. Column (2) reports ITT estimates of the impact of winning the admissions lottery. Column (3) reports LATE estimates of the impact of ever attending the Promise Academy using a winning lottery number as an instrument. All regressions control for the baseline demographic variables summarized in Table 3, a quadratic of 4th and 5th grade math and ELA test scores, lottery-year indicators, indicators having a sibling enrolled in the same lottery, and a sibling-year interaction term. The last row of each Panel is a summary index equal to the average of the standardized value of each of the preceding variables. Each standardized survey outcome is renormed using the mean and standard deviation of the control group. Administrative outcomes are renormed using the mean and standard deviation of the entire NYC sample. Web Appendix B contains additional details on each variable. Heteroskedasticity-robust standard errors are reported in parenthesis. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively. See text for additional details.

Lottery winners also take more New York State Regents exams and score higher on the exams that most students take.[30] Lottery winners pass 1.026 (0.230) more Regents exams than lottery losers, a 43 percent increase from the control mean of 2.385 exams. On the three core exams that over 70 percent of lottery winners and lottery losers take – Living Environment, Global History, and Integrated Algebra – lottery winners score 0.359σ (0.064) higher than lottery losers. The gains are largest in Integrated Algebra, where lottery winners score 0.550σ (0.092) higher (see Web Appendix Table 3). Lottery winners are also 20.0 (4.7) and and 10.5 (3.4) percentage points more likely to take the more advanced Geometry and Chemistry exams, and, conditional on taking these exams, score 0.561σ (0.124) and 0.624σ (0.303) higher.

Our final measure of human capital is college enrollment. Lottery winners are 14.1 (6.1) percentage points more likely to enroll in college the fall after their senior year, a 49.0 percent increase from the control mean of 28.8 percent. Attending the Promise Academy increases the probability of enrolling in college by 24.2 (9.7) percentage points, an 84 percent increase. In Table 6, we show that lottery winners are also 21.3 (5.9) percentage points more likely to attend a four-year college and 7.2 (2.3) percentage points less likely to attend a two-year college. These results are consistent with the Promise Academy inducing at least some students to enroll in a four-year college instead of a two-year school. Table 6 also shows that lottery entrants are 4.5 (3.8) percentage points more likely to enroll at a college where the average student has SAT scores of 1,000 points or higher (out of 1600), but the point estimate is not statistically significant.

We summarize the impact of the Promise Academy on human capital using an index measure that combines all five individual human capital measures. To construct the index, we standardize each individual measure to have a mean of zero and a standard deviation of one in the control group. We then take the average of each standardized z-score measure. The impact of winning the admissions lottery on this human capital index measure is 0.277σ (0.068), suggesting a large impact of the Promise Academy on non-test score skills.

[30] Selection into the Regents exams complicates the interpretation of these estimates. If, for example, the Promise Academy pushes weaker students to take harder Regents exams, then our results are likely to be too conservative. Consistent with this, Web Appendix Table 1 shows that lottery winners are at least as likely to take each exam except Comprehensive English, and are more likely to take and pass advanced subjects like Geometry, Physics, and Chemistry.

Table 4.6
The Impact of Attending the Promise Academy on College Quality

	CM	ITT	LATE
	(1)	(2)	(3)
College Enrollment	0.288	0.141^{**}	0.242^{**}
	(0.454)	(0.061)	(0.097)
	236	313	313
Two Year College	0.081	-0.072^{***}	-0.124^{***}
	(0.273)	(0.023)	(0.039)
	236	313	313
Four Year College	0.208	0.213^{***}	0.366^{***}
	(0.406)	(0.059)	(0.093)
	236	313	313
1000+ SAT College	0.085	0.045	0.078
	(0.279)	(0.038)	(0.062)
	236	313	313

Notes: This table reports estimates of the effect of attending the Promise Academy on college quality. Column (1) reports the mean of each variable for the control group. Column (2) reports ITT estimates of the impact of winning the admissions lottery. Column (3) reports LATE estimates of the impact of ever attending the Promise Academy using a winning lottery number as an instrument. The sample is restricted to 2005 lottery entrants. All regressions control for the baseline demographic variables summarized in Table 3, a quadratic of 4th and 5th grade math and ELA test scores, lottery-year indicators, indicators having a sibling enrolled in the same lottery, and a sibling-year interaction term. Colleges that we cannot match to SAT or ACT data are coded as zero. Heteroskedasticity-robust standard errors are reported in parenthesis. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

B. Risky Behaviors

Panel B of Table 5 presents estimates of the impact of the Promise Academy on teen pregnancy, incarceration, self-reported drug and alcohol use, and self-reported criminal behavior. Pregnancy results include all female survey respondents, while self-reported results include all survey respondents. The incarceration results include the 234 male lottery entrants whom we successfully contacted, regardless of whether or not they completed a survey. Following Panel A, each regression controls for the demographic variables listed in Table 3, lottery effects, sibling by lottery effects, and a quadratic in 4th and 5th grade math and ELA test scores. Standard errors have been adjusted to account for arbitrary heteroskedasticity.

Seventeen percent of female lottery losers report having been pregnant at some point. In comparison, 10.0 percent of minority women and 10.4 percent of low-income women in New York City schools give birth in their teens (Chetty, Friedman, and Rockoff 2011). Female lottery winners are 12.1 (4.6) percentage points less likely to report that they have ever been pregnant, a 71 percent reduction from the control mean.[31]

Four percent of male lottery losers were incarcerated during our sample period, compared to none of the male lottery winners. One female lottery loser and one female lottery winner were also incarcerated during our sample period.[32] In our ITT framework, male lottery winners are 4.3 (1.8) percentage points less likely to be incarcerated, essentially a one hundred percent decrease. To put this estimate in context, Deming (2011) finds being offered a spot at a student's first choice public school in Charlotte-Mecklenburg decreases the probability of spending at least 90 days in jail over the next five years by 10.7 percentage points for males in the highest risk quintile, a 81.1 percent drop. The effect decreases to 8.4 percentage points, or 53.8 percent, six years after the school choice lottery.

[31] We also asked survey respondents about various self-reported sexual habits which might explain the effect on pregnancy. As the results in Panel D of Table 12 show, there are no detectable differences in these behaviors. Promise Academy youth are equally likely to have had sex, and are about as likely to have used a condom or another form of contraception during their most recent sexual experience, though we are under-powered to detect modest differences.

[32] Using national samples of men born after 1965, Pettit and Western (2004) estimate that 2.06 percent of black men aged 15 - 19 and 6.06 percent of black men aged 20 - 24 have been incarcerated at least once. Equivalent figures for white men are 0.39 percent and 0.73 percent.

We find little evidence that the Promise Academy impacts self-reported drug and alcohol use or self-reported criminal behavior. Lottery winners are 0.016σ (0.111) less likely to report using drugs and alcohol, and 0.012σ (0.064) less likely to report criminal behavior, with neither estimate close to statistical significance. The results are similar if estimate effects for males and females separately. There are at least three possible explanations for the positive impact on administrative outcomes and no effect on self-reported outcomes. First, our self-reported measures are likely biased downwards due to the fact that incarcerated youth are unable to respond to our survey. Second, there may be underreporting of risky behavior that masks a true treatment effect. For instance, youth in the MTO follow-up study under report criminal behavior by 15 to 20 percent, with treated youth only slightly less likely to self-report crime (Kling, Ludwig, and Katz 2005). Finally, it is possible that criminal behaviors are the same, but that lottery winners are less likely to be caught.

Following our human capital results in Panel A, we summarize the impact of the Promise Academy on risky behavior using an index measure that combines all four individual measures. Lottery winners are 0.135σ (0.072) less likely to engage in risky behavior according to our index measure. The result is driven by the incarceration and pregnancy results, as there is relatively little variation across students in the self-reported measures.

C. Health

Panel C of Table 5 presents estimates of the impact of the Promise Academy on healthy eating, mental health, physical health, and an index of surveyed health behaviors. Each regression includes all survey respondents, and follows the same specification as Panels A and B.

Lottery winners are 0.108σ (0.061) more likely to report healthy eating habits, yet these habits do not appear to have translated into improvements on any other health outcomes. Lottery winners self-report physical health that is 0.050σ (0.063) lower, with no discernible effects on asthma attacks, obesity, or self-reported health. Lottery winners also report mental health that is 0.034σ (0.103) lower than lottery losers. Our summary index of both physical and mental health is 0.032σ (0.057) higher for lottery winners as compared to lottery losers.

4.4.2. Robustness Checks

In this section, we explore the robustness of our results to two potential threats to validity: (1) differential attrition from the survey sample, and (2) false positives due to multiple hypothesis-testing.

First, we consider the extent to which sample attrition threatens our estimates by calculating Lee (2009) bounds and imputing outcomes for youth who did not respond to the survey. Panel A of Table 7 presents these results for the administrative outcomes that are available for all lottery entrants. Column 1 presents standard ITT estimates using the full sample of lottery entrants as reported in column 2 of Table 5. Column 2 restricts the sample to lottery entrants in the survey sample to explore the extent of any attrition bias on these outcomes. The impact of being offered admission to the Promise Academy is similar in the full and survey samples across all of our administrative outcomes. Lottery winners score 0.446σ (0.077) higher in math in the full sample and 0.466σ (0.081) higher in the survey sample. ELA scores are 0.156σ (0.057) higher in the full sample and 0.132σ (0.055) higher in the survey sample. Conversely, the effect on the number of Regents exams passed is 0.241 higher in the survey sample, the effect on Regents scores is 0.022σ higher, and the impact on college enrollment is 1.4 percentage points higher. These results suggest that there is, at worst, modest upwards bias in the survey sample.

Column 3 of Panel A reports the Lee (2009) bound for each administrative outcome. Each bound is calculated by dropping the fraction of the highest-achieving lottery winners necessary to equalize the response rate among lottery winners and lottery losers. In this worst case scenario, there is still a statistically significant effect of the Promise Academy on three out of five of our administrative outcomes, with lottery winners scoring 0.183σ (0.067) higher on our human capital index compared to lottery losers. In each case, the Lee (2009) bounds are much lower than the true ITT estimates from column 1.

Table 4.7
Attrition and Bounding

	Admin ITT	Survey ITT	Lee Bound	p-value (2)=(3)	Imputed	p-value (2)=(5)
Panel A1. Human Capital (Admin.)	(1)	(2)	(3)	(4)	(5)	(6)
Eight Grade Math	0.446***	0.466***	0.260***		0.343***	
	0.077	(0.081)	(0.081)	0.072	(0.065)	0.236
	472	361	335		547	
Eight Grade ELA	0.156***	0.132**	0.006		0.111***	
	0.057	(0.055)	(0.052)	0.099	(0.041)	0.764
	477	365	337		547	
Regents Passed	1.026***	1.267***	0.675***		0.957***	
	0.230	(0.249)	(0.255)	0.097	(0.218)	0.348
	482	360	334		547	
Regents Test Scores	0.359***	0.381***	0.207***		0.262***	
	0.064	(0.069)	(0.068)	0.072	(0.057)	0.182
	423	325	298		547	
College Enrollment	0.141**	0.155**	0.042		0.140**	
	0.061	(0.072)	(0.074)	0.274	(0.063)	0.869
	313	220	206		299	
Human Capital Index	0.277***	0.331***	0.183***		0.274***	
	0.068	(0.070)	(0.067)	0.128	(0.059)	0.532
	552	403	382		547	
Panel A2. Human Capital (Survey)						
Woodcock Johnson Math		0.283***	0.124		0.197***	
	—	(0.083)	(0.082)	0.175	(0.072)	0.429
		386	364		547	
Woodcock Johnson Reading		0.119	−0.060		0.076	
	—	(0.083)	(0.080)	0.120	(0.071)	0.694
		386	364		547	
Panel B. Risky Behaviors						
Ever Pregnant (Female)		−0.121***	−0.118**		−0.091**	
	—	(0.046)	(0.047)	0.973	(0.036)	0.617
		205	202		272	
Incarcerated (Male)		−0.043**			−0.039**	
	—	(0.017)	—	—	(0.016)	0.880
		234			281	
Drug/Alcohol Index		−0.016	0.038		−0.008	
	—	(0.067)	(0.071)	0.579	(0.050)	0.926
		406	384		547	
Criminal Behavior Index		−0.004	0.033		−0.006	
	—	(0.065)	(0.069)	0.693	(0.049)	0.982
		407	386		547	
Risky Behavior Index		−0.043	0.036		−0.035	
	—	(0.062)	(0.066)	0.383	(0.047)	0.911
		407	386		547	

Table 4.7 (Continued)
Attrition and Bounding

	Admin ITT	Survey ITT	Lee Bound	p-value (2)=(3)	Imputed	p-value (2)=(5)
Panel C. Health	(1)	(2)	(3)	(4)	(5)	(6)
Nutrition Index		0.108^{*}	-0.019		0.076	
	—	(0.061)	(0.059)	0.134	(0.047)	0.681
		407	386		547	
Mental Health		-0.034	-0.223^{**}		-0.032	
	—	(0.103)	(0.103)	0.193	(0.077)	0.991
		403	382		547	
Physical Health Index		-0.050	-0.125^{*}		-0.033	
	—	(0.063)	(0.066)	0.407	(0.047)	0.837
		407	386		547	
Health Behavior Index		0.031	-0.073		0.028	
	—	(0.052)	(0.051)	0.156	(0.040)	0.961
		407	386		547	
Health Index		0.032	-0.079		0.024	
	—	(0.057)	(0.055)	0.160	(0.043)	0.908
		407	386		547	

Notes: This table reports ITT estimates accounting for survey attrition. Column (1) reports ITT estimates in the administrative sample not subject to attrition bias. Column (2) reports ITT estimates in the sample of survey respondents. Column (3) reports Lee (2009) bounds by dropping lottery winners with the best outcomes until there is an equal survey response rate between lottery winners and lottery losers. Column (4) reports the p-value from a test that the coefficients in Columns (2) and (3) are equal. Column (5) reports results imputing outcomes for all survey non-respondents using all baseline characteristics reported in Table 3 and the five administrative outcomes reported in Panel A. Column (6) reports the p-value from a test that the coefficients in Columns (2) and (5) are equal. All regressions follow the specification and sample restrictions from Table 5. Heteroskedasticity-robust standard errors are reported in parenthesis. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Column 5 presents results imputing outcomes for non-respondents. We impute outcomes for all non-survey respondents, including lottery winners, using the baseline characteristics listed in Table 3 and the administrative outcomes available for all lottery entrants. Our results in column 5 show that the imputation results are downwards biased relative to the true ITT estimates in column 1, suggesting that these results are also a conservative approach to dealing with attrition in our sample.

Panel B reports Lee (2009) bounds and imputation results for our survey outcomes. The only Lee (2009) bound that is statistically significant in our survey outcomes is pregnancy, though due to large standard errors we cannot rule out the bounds and the survey estimates being statistically identical. We are also unable to calculate a bound for incarceration, as there are no incarcerated males in the treatment group. Conversely, our imputation results in column 5 are nearly identical to our results reported in Table 5. None of the results lose statistical significance, and none are statistically different than the reported results from Table 5.

A second concern is that we are detecting false positives due to multiple hypothesis-testing. Table 8 presents results controlling for the Family-Wise Error Rate, using an algorithm similar to those described by Westfall and Young (1993), Kling, Liebman, and Katz (2007), and Anderson (2008). For a given family of k-hypothesis tests, the algorithm estimates the probability that the observed t-statistic is larger than the equivalently-ranked test statistic that would be generated by random chance. Web Appendix E provides a full description of how we implemented this procedure.

Table 8 confirms the robustness of our main findings. The p-value on the human capital index remains less than 0.001 after adjusting for multiple-hypothesis testing, while the p-value on the risky behavior index rises from 0.063 to 0.123. The more conservative Bonferroni correction, which controls the family-wise error rate under the assumption that all test statistics are independent, is calculated by multiplying the per-comparison p-values by the number of hypothesis tests. Thus, the Bonferroni corrected p-value on the human capital index is 0.0002, and the Bonferroni corrected p-value on the risky behavior index rises to 0.190.

Table 4.8
Main Estimates with Familywise-Error-Rate-Controlled p-values

	ITT Estimate	Uncorrected p-value	Corrected p-value
	(1)	(2)	(3)
Human Capital Index	0.277	0.000	0.000
	(0.068)		
Risky Behavior Index	−0.135	0.063	0.123
	(0.072)		
Health Index	0.032	0.573	0.573
	(0.057)		

Notes: This table reports ITT estimates correcting for multiple-hypothesis testing. Column (1) reports ITT estimates following the specification described in Table 5. Column (2) reports the unadjusted p-value. Column (3) reports the p-value correcting for the Familywise Error Rate for the three outcomes. Web Appendix E contains additional information on the algorithm used. Standard errors reported in parenthesis are robust to arbitrary heteroskedasticity. The number of observations is reported below the standard error.

4.5. Interpretation

4.5.1. Neighborhoods vs. Schools

In addition to the school investments typical of a high-performing charter school, Promise Academy students are exposed to a network of community services in the Harlem Children's Zone. The community programs may plausibly impact future outcomes by providing a more supportive out-of-school learning environment.

Following Dobbie and Fryer (2011a), we investigate the empirical importance of the HCZ neighborhood programs and the Promise Academy school investments by estimating effects separately for youth living within 400 meters of the original 24-block Harlem Children's Zone (inside HCZ), who are more likely to receive neighborhood benefits, and youth living more than 400 meters away (outside HCZ), who are less likely to receive neighborhood benefits.

Table 6 presents these ITT estimates for youth living in and outside HCZ. Consistent with Dobbie and Fryer (2011a), there are no statistically different effects by HCZ residence for any of our summary indices. Lottery winners living in HCZ have human capital scores that are 0.318σ (0.117) higher than lottery losers in the Zone, while lottery winners living outside the Zone have human capital scores that are 0.283σ (0.075) higher. Lottery winners in the Zone also are 0.127σ (0.103) less likely to engage in risky behaviors, and are 0.045σ (0.094) healthier than lottery losers in the Zone. In comparison, lottery winners out of the Zone are 0.135σ (0.077) less likely to engage in risky behaviors and 0.034σ (0.062) healthier than lottery losers out of the Zone.

Table 10 presents estimates for the individual index components for students living inside and outside HCZ. There is only one statistically significant difference between the in and outside of HCZ treatment estimates, with lottery winners inside of HCZ increasing their Regents test scores by 0.234σ more than lottery winners living outside of HCZ. None of the other 14 point estimates we consider are different, and many of the estimates larger for youth living outside of HCZ.

Table 4.9
The Impact of Attending the Promise Academy
Inside and Outside the Zone

	Inside Zone	Outside Zone	p-value
	(1)	(2)	(3)
Human Capital Index	0.318^{***}	0.283^{***}	
	(0.117)	(0.075)	0.784
	148	369	
Risky Behavior Index	−0.127	-0.135^{*}	
	(0.103)	(0.077)	0.932
	122	315	
Health Index	0.045	0.034	
	(0.094)	(0.062)	0.907
	112	287	

Notes: This table reports ITT estimates for youth with baseline addresses inside and outside of the Harlem Children's Zone. Column (1) presents ITT estimates for youth living within 400 meters of the original 24-block Zone. Column (2) presents ITT estimates for youth living outside 400 meters of the original 24-block Zone, and Column (3) reports a p-value of a test that the two coefficients are equal. All specifications and variable definitions are identical to those in Table 5. Heteroskedasticity-robust standard errors are reported in parenthesis. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Table 4.10
The Impact of Attending the Promise Academy
Inside and Outside the Zone

	Inside Zone	Outside Zone	p-value
Panel A. Human Capital	(1)	(2)	(3)
Woodcock Johnson Math	0.326**	0.273***	
	(0.131)	(0.091)	0.707
	108	270	
Woodcock Johnson Reading	0.173	0.114	
	(0.135)	(0.092)	0.683
	108	270	
Regents Passed	1.000**	1.034***	
	(0.427)	(0.251)	0.942
	134	322	
Regents Test Scores	0.529***	0.295***	
	(0.094)	(0.074)	0.029
	117	283	
College Enrollment	0.114	0.167**	
	(0.110)	(0.068)	0.666
	91	205	
Panel B. Risky Behaviors			
Ever Pregnant (Female)	−0.106	−0.128***	
	(0.086)	(0.048)	0.800
	65	136	
Incarcerated (Male)	−0.051**	−0.041**	
	(0.025)	(0.017)	0.553
	54	176	
Drug/Alcohol Index	−0.031	−0.009	
	(0.109)	(0.072)	0.850
	112	286	
Criminal Behavior Index	0.051	−0.024	
	(0.117)	(0.069)	0.535
	112	287	

Table 4.10 (Continued)
The Impact of Attending the Promise Academy
Inside and Outside the Zone

	Inside Zone	Outside Zone	p-value
Panel C. Health	(1)	(2)	(3)
Nutrition Index	0.176^{*}	0.085	
	(0.104)	(0.067)	0.413
	112	287	
Mental Health	-0.005	-0.038	
	(0.149)	(0.115)	0.835
	111	284	
Physical Health Index	-0.061	-0.039	
	(0.096)	(0.070)	0.832
	112	287	
Health Behavior Index	-0.011	0.050	
	(0.092)	(0.057)	0.538
	112	287	

Notes: This table reports ITT estimates for youth with baseline addresses inside and outside of the Harlem Children's Zone. Column (1) presents ITT estimates for youth living within 400 meters of the original 24-block Zone. Column (2) presents ITT estimates for youth living outside 400 meters of the original 24-block Zone, and Column (3) reports a p-value of a test that the two coefficients are equal. All specifications and variable definitions are identical to those in Table 5. Heteroskedasticity-robust standard errors are reported in parenthesis. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Table 4.11
Comparison of Promise Academy and MTO Effects

	Female		Male	
	MTO	HCZ	MTO	HCZ
Panel A. Woodcock Johnson	(1)	(2)	(3)	(4)
Woodcock Johnson Math	0.119	0.251^{*}	−0.095	0.310^{***}
	(0.095)	(0.137)	(0.097)	(0.108)
Woodcock Johnson Reading	0.093	0.162	−0.087	0.096
	(0.084)	(0.133)	(0.096)	(0.113)
Panel B. Health				
Self Reported Health Poor/Fair	−0.008	−0.019	0.033	0.030
	(0.029)	(0.045)	(0.019)	(0.034)
Had Asthma Attack in Last Year	0.002	0.075	0.016	−0.003
	(0.037)	(0.071)	(0.032)	(0.051)
BMI > 95th Percentile	−0.009	−0.037	0.026	0.018
	(0.034)	(0.060)	(0.037)	(0.061)
Mental Health	0.289^{*}	−0.088	−0.095	0.056
	(0.094)	(0.169)	(0.085)	(0.138)
Panel C. Risky Behaviors				
Drank Alcohol in Last 30 Days	−0.060	−0.062	0.063	0.017
	(0.037)	(0.067)	(0.033)	(0.071)
Smoked Marijuana in Last 30 Days	-0.065^{*}	−0.019	0.051	0.063
	(0.029)	(0.067)	(0.030)	(0.068)
Smoked Cigarette in Last 30 Days	−0.054	0.025	0.103^{*}	0.023
	(0.033)	(0.045)	(0.032)	(0.038)
Ever Been Pregnant or Caused Pregnancy	0.036	-0.121^{***}	0.032	0.031
	(0.040)	(0.046)	(0.035)	(0.054)

Notes: This table reports ITT estimates of the Promise Academy and the Moving to Opportunity experiment evaluated by Kling, Liebman, and Katz (2007). Columns (1) and (3) are drawn from Table G2 of Kling, Liebman, and Katz (2007). For all MTO estimates we report the effects from the full experimental treatment that included the neighborhood quality restriction (as opposed to the Section 8-only treatment). Column (2) and (4) follow the specifications described in Table 5. Heteroskedasticity-robust standard errors are reported in parenthesis. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

To provide further evidence on this issue, Table 11 compares the effects of the Promise Academy with that of the Moving to Opportunity (MTO) intervention that relocated individuals from high-poverty to lower-poverty neighborhoods while keeping the quality of schools roughly constant. We use estimates from Kling, Liebman, and Katz (2007), who evaluate the impact of MTO four to seven years after random assignment for youth who are 15 to 20 years old.[33] We also follow Kling, Liebman, and Katz (2007) and present ITT estimates separately by gender.

The comparison with the MTO estimates suggests little overlap in the effects of neighborhood quality compared to school quality. The Promise Academy significantly increases Woodcock-Johnson math scores for both males and females, while MTO has no impact, particularly for males. The Promise Academy also significantly decreases teen pregnancy, while MTO appears to have no effect. Conversely, MTO increases mental health by 0.289σ (0.094) for females, while there is no impact of the Promise Academy on this measure for either males or females.

4.5.2. Test Scores and Later-Life Outcomes

This section considers the extent to which changes in test scores might explain the impact of the Promise Academy on non-test score outcomes. Specifically, we compare the reduced form estimates of the impact of the Promise Academy on non-test score outcomes to the effects implied by the cross-sectional relationship between test scores and non-test score outcomes in Chetty, Friedman, and Rockoff (2011) and the control group.

Chetty, Friedman, and Rockoff (2011) find that a one σ increase in math or ELA achievement is associated with a 5.6 percentage point increase in college attendance at age 20 for minorities, and a 5.2 percentage point increase for students from low-income families. A one σ increase in math or ELA achievement is also associated with a 1.2 percentage point decrease in teen pregnancy among both minority women and women from low-income families. The impact of the being offered admission to the Promise Academy in the survey sample is 0.466σ in eighth grade math and 0.132σ in eighth grade ELA. Using the average correlation across minorities and low-income

[33] Sanbonmatsu et al. (2011) and Gennetian et al. (2012) report MTO results ten to 15 years after random assignment. The results are qualitatively similar to those reported by Kling, Liebman, and Katz (2007).

families from Chetty, Friedman, and Rockoff (2011), these estimates imply that the test score effect alone would lead to a $(5.4 \cdot (0.466 + 0.132)) = 3.3$ percentage point increase in college enrollment and a $(1.2 \cdot (0.466 + 0.132)) = 0.7$ percentage point decrease in teen pregnancy. Using the ITT estimates in Table 5, this implies that the eighth grade test score increase can explain $((3.2/14.1) \cdot 100) = 23.1$ percent of the college enrollment effect, and $((0.7/12.1) \cdot 100) = 6.1$ percent of the pregnancy effect.

We can perform a similar exercise using the correlations identified within the lottery losers. Following Chetty, Friedman, and Rockoff (2011), we estimate correlations based on math and reading scores from grades four through eight. We stack observations such that each row is a unique student-subject-grade combination, and identify the correlation between scores and outcomes after controlling for our full set of demographic variables and a cubic in previous year's test scores. The correlations that we identify are larger than those estimated by Chetty, Friedman, and Rockoff (2011), though not large enough to explain the reduced form effects on non-test score outcomes. A one σ increase in math or ELA test scores is associated with a 12.4 percentage point increase in college enrollment, a 6.9 percentage point reduction in teen pregnancy, and a 1.5 percentage point reduction in the likelihood of being incarcerated. These correlations imply that the eighth grade test score increase can explain $(12.4 \cdot (0.466 + 0.132)/14.1 \cdot 100) = 52.6$ percent of the college enrollment effect, $(6.9 \cdot (0.466 + 0.132)/12.1 \cdot 100) = 34.1$ percent of the pregnancy effect, and $(1.5 \cdot (0.466 + 0.132)/4.1 \cdot 100) = 20.8$ percent of the incarceration effect. Large standard errors on the cross-sectional estimates means that we cannot rule out much larger and smaller impacts.

4.5.3. Other Mechanisms

Our results up until this point suggest that the Promise Academy investments drive the impact on non-test score outcomes, but that the impacts are significantly larger than what would be implied by the cross-sectional relationship between test scores and later outcomes. This section considers three alternative mechanisms: (1) non-cognitive skills, (2) social networks, and (3) economic preference parameters.

A large body of evidence suggests that non-cognitive skills, such as self-esteem, locus of control, and persistence, are correlated with later outcomes. Self-esteem is thought to influence teenage pregnancy and drug use (Stewart et al. 1995, Kalil and Kunz 1999, Cornelius et al. 2004), although there is considerable disagreement on these points (McGee and Williams 2000, Paul et al. 2000). Persistence, as measured through the 8-item scale we use in this paper, is associated with educational attainment and fewer career changes among adults and increased GPA and reduced grade retention among adolescents (Duckworth and Quinn 2009). Heckman et al. (2006) show that self-esteem and locus of control are related to earnings, incarceration, and teen pregnancy. We test this mechanism by administering the Rosenberg self esteem index, which asks respondents to rate the extent to which they agree to a series of 14 statements such as "I certainly feel useless at times" and "At times, I think I am no good at all" (Rosenberg 1965). Youth were also asked to answer questions from the Rotter Locus of Control instrument, which measures the extent to which respondents believe they control events in their lives (Rotter 1966).

Panel A of Table 12 presents results of the impact of the Promise Academy on these non-cognitive skills. If anything, Promise Academy students report lower non-cognitive skills than the control group. Lottery winners score 0.138σ (0.110) lower on the Rosenberg self esteem index, and 0.254σ (0.113) lower on Duckworth and Quinn's (2009) short grit scale, though only the latter is statistically significant. Lottery winners have Locus of Control scores that are 0.041σ (0.107) higher, but the estimate is not statistically different than zero.[34]

The second mechanism we explore is the impact of the Promise Academy on traditional economic preference parameters such as risk aversion and discount rate. These measures are the common determinants of decision-making in economic models and have been linked to a variety of later outcomes (Borghans et al. 2008). Discount rates and risk aversion are measured by asking youth to make choices through a fixed series of comparisons to infer an indifference point

[34]The negative impact of the Promise Academy on self esteem and grit may be the result of different reference points regarding hard work and perseverance (Heine et al. 2002, 2008). To partially test this theory, we correlate grit scores with Woodcock-Johnson math scores in the treatment and control groups. The correlation between grit and Woodcock-Johnson math scores is 0.24 in the control group but -0.07 in the treatment group. This pattern of results is inconsistent with the idea that treatment changes reference points by an equal amount, but could be explained by a more complex story in which reference points change more for more students with higher ability.

(Hardisty et al. 2011). For discount rates, youth were asked whether they would prefer that \$40 to be mailed to them later that day or for a larger amount to be mailed in one month. The amount was then varied until the student changed her answer or reached the extreme value of either \$42 or \$55. For risk aversion, youth were given a choice between a job that paid \$600 with probability one and a second identical job that paid \$1,200 with probability 0.5 and a value less than \$600 with equal probability. The latter value was then altered until a student changed her answer or reached an extreme value of either \$150 or \$540. To maintain consistency with the rest of our results, we report results for both discount rate and risk aversion in standard-deviation units.

Winning the lottery to attend the Promise Academy has no detectable effect on discount rates. Lottery winners have discount rates that are only 0.045σ (0.110) higher.[35] Conversely, the Promise Academy does seem to alter risk aversion in its students, as lottery winners report 0.248σ (0.103) higher Pratt-Arrow measures than lottery losers.

The final mechanism we explore is the importance of changes in peer quality. A large literature suggests that outcomes are heavily influenced by one's peers (Sacerdote 2000, Fergusson et al. 2002, Boisjoly et al. 2006, Carrell et al. 2009, Deming 2011). We measure peer networks by asking youth about the attitudes of their peer group on crime and educational attainment. Academic peer quality was measured by asking youth to how important it is for their friends to study, stay in school, and attending class regularly. Risky behavior peer quality was measured by asking youth whether their friends use drugs, drink alcohol, smoke cigarettes, steal, fight, or are in a gang. We use these responses to create summary indices of peer networks.

[35]Over a third of the sample selected the highest discount rate category, preferring \$40 now to \$55 in one month, implying an annual discount rate of over 4,000 percent. We also find no impact of the Promise Academy on choosing the highest discount rate category, or choosing a rate above the median.

Table 4.12
Impacts of the Promise Academy on Possible Mechanisms

	CM	ITT	LATE
Panel A. Non-Cognitive Measures	(1)	(2)	(3)
Self Esteem Index	0.000	−0.138	−0.224
	(1.000)	(0.110)	(0.173)
	255	403	403
Grit Index	0.000	−0.254**	−0.402**
	(1.000)	(0.113)	(0.177)
	250	398	398
Locus of Control	0.000	0.041	0.067
	(1.000)	(0.107)	(0.166)
	254	398	398
Panel B. Discount Rates and Risk Aversion			
Discount Rate (σ)	0.000	0.045	0.073
	(1.000)	(0.110)	(0.171)
	257	404	404
Risk Aversion (σ)	0.000	0.248**	0.400**
	(1.000)	(0.103)	(0.162)
	256	404	404
Panel C. Social Networks			
Academic Activities in Network	−0.003	0.094	0.148
	(0.754)	(0.077)	(0.118)
	252	398	398
Risky Behaviors in Network	0.001	−0.009	−0.015
	(0.574)	(0.069)	(0.105)
	252	398	398

Table 4.12 (Continued)
Impacts of the Promise Academy on Possible Mechanisms

	CM	ITT	LATE
Panel D. Sexual Behaviors	(1)	(2)	(3)
Ever Had Sex	0.644	−0.014	−0.023
	(0.480)	(0.051)	(0.080)
	253	398	398
Condom Use	0.809	−0.043	−0.070
	(0.395)	(0.058)	(0.089)
	162	255	255
Other Contraceptive Use	0.466	0.068	0.110
	(0.500)	(0.066)	(0.101)
	161	255	255

Notes: This table reports estimates of the effect of attending the Promise Academy on mediating outcomes. Column (1) reports the mean and standard deviation of each variable for the control group. Column (2) reports ITT estimates of the impact of winning the admissions lottery. Column (3) reports LATE estimates of the impact of ever attending the Promise Academy using a winning lottery number as an instrument. All regressions control for the baseline demographic variables summarized in Table 3, a quadratic of 4th and 5th grade math and ELA test scores, lottery-year indicators, indicators having a sibling enrolled in the same lottery, and a sibling-year interaction term. The sample includes lottery entrants in the survey sample. Results for condom and contraceptive use are restricted to students who report having ever had sex. Self Esteem is constructed from students' responses to ten self-evaluative questions from Rosenberg (1965). Grit is measured by the eight-question Short Grit Scale developed by Duckworth and Quinn (2009). Locus of Control is constructed from students' levels of agreement with four pairs of questions developed by Rotter (1966) and adapted for the NLSY. Academic Activities in Social Network is the average of standardized measures of the importance of studying to friends, the importance of education to friends, the importance of attending class to friends, and the importance of getting good grades to friends. Risky Behaviors in Social Network is the average of standardized indicators for a youth's friends using drugs, smoking cigarettes, having stolen an item worth less than $50 dollars, having stolen an item worth more than $50 dollars, getting in fights, carrying a handgun, or being in a gang. Condom Use is an indicator for using a condom during the last time the student had sexual intercourse. Other Contraceptive Use is an indicator for using a non-condom form of contraception. All variables are standardized to have mean zero and standard deviation one in the control group. See Web Appendix B for additional information on each variable. Heteroskedasticity-robust standard errors are reported in parenthesis. The number of observations is reported below the standard error. ***, **, and * indicate statistical significance with 99%, 95%, and 90% confidence, respectively.

Panel C of Table 12 presents results of the impact of the Promise Academy on peer quality. Lottery winners have peers that are 0.094σ (0.077) higher than lottery losers on our index measuring the relative importance of various academic activities in one's peer group, though the effect is not statistically significant. There is almost no difference between levels of risky behaviors in the networks of winners and losers, with an estimated point estimate of -0.009σ (0.069). Taken together, we interpret these results as suggesting that changes in peer quality are not driving our results, although we cannot rule out changes in other forms of social interaction.

4.6. Conclusion

In this paper, we estimate the impact of attending the Promise Academy in the Harlem Children's Zone on a wide range of human capital investments, risky behaviors, and health outcomes. Youth randomly offered admission to the Promise Academy score higher on nationally-normed math achievement tests, are more likely to enroll in college, less likely to be pregnant in their teens, and less likely to be incarcerated. A comparison of youth living in and outside of the Zone reveal similar impacts on these outcomes.

The education reform movement is based, in part, on two important assumptions: (1) high quality schools can increase test scores, and (2) the well-known relationship between test scores and adult outcomes is causal. We have good evidence that the first assumption holds (Angrist et al. 2010, Abdulkadiroglu et al. 2011, Dobbie and Fryer 2011a). This paper presents the first pieces of evidence that the second assumption may not only be true, but that the cross-sectional correlation between test scores and adult outcomes may understate the true impact of a high quality school, suggesting that high quality schools change more than cognitive ability. Importantly, the return on investment for high-performing charter schools could be much larger than that implied by the short-run test score increases.

A larger sample of schools, longer-term outcomes, and a better sense of the mechanisms generating the observed impacts are all ripe areas for future research.

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