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Graduate School of Arts and Sciences

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"Essays in Development Economics and Labor Economics'

presented by ***Jean Nahrae Lee***

candidate for the degree of Doctor of Philosophy and hereby certify that it is worthy of acceptance.

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Date: November 17, 2009

Essays in Development Economics and Labor Economics

A dissertation presented

by

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to

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Essays in Development Economics and Labor Economics

Abstract

This dissertation consists of three essays in Development Economics and Labor Economics.

The first essay uses data from Brazilian industrial plants to estimate the extent to which employment spillovers between geographically and economically proximate industries lead to larger changes in employment than would be predicted by national trends in Brazilian municipalities. Using establishment-level data from Brazil, we estimate the extent to which firm- and industry-level employment and entry decisions respond to plausibly exogenous changes in the employment decisions of geographically and economically proximate firms between 1995 and 2005. Our results suggest the existence of economically and statistically significant effects of municipality-level predicted trends in other industries on the employment and entry decisions of individual firms.

The second chapter estimates the impact of changes in state statutes, which in addition to laws passed at the federal level in the United States, specify procedures for

summoning and determining the eligibility of jurors to serve on criminal juries in state courts. This paper uses a series of changes in state laws governing the compilation of lists of eligible jurors to attempt to identify the impact of increasing the participation of African Americans and other minorities in jury service on the racial composition of admissions to prison. Evidence exploiting the variation in timing of these law changes suggests that the reforms resulted in a 5 to 6 percentage point drop in the share of new admissions to prison accounted for by non-whites, consistent with the existence of racial discrimination in the deliberation of criminal cases.

The third chapter uses original survey data as well as administrative data on sales from a distributor for a large multinational firm producing household goods to estimate the returns to additional investments made by small retail establishments in western Kenya. Standard textbook models suggest risk-adjusted rates of return should be equalized across activities within firms, and across firms. We find unexploited investments in inventory which would yield an average annual real marginal rate of return of 113 percent, well above rates of return to debt and equity both in Kenya and in international markets. A second approach, using administrative data on whether firms purchased enough to take advantage of quantity discounts from wholesalers, suggests a lower bound on rates of return of at least 117 percent per year. We reject the hypothesis that the marginal rates of return are equal across shops.

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Chapter 1. Cross-Industry Spillovers in Employment: Evidence from Brazil (with Daniel R. Carvalho)

1.1 Introduction

Interlinkages in production and employment decisions across firms and industries may give rise to variation in both the scope and speed of economic development across places (Banerjee and Duflo, 2005; Jones and Olken, 2007)[1]. These interlinkages may be particularly strong within cities or regions, encouraging the formation of geographic clusters of economic activity.

M1O, 28]

The advantages of proximity may take many forms, including knowledge spillovers leading to higher productivity (Marshall, 1890; Jacobs, 1969; Moretti, 2004; Greenstone, Moretti and Hornbeck, 2008; Kremer, 1993)[2]; lower transport costs to producers of inputs or consumers (Krugman, 1991)[3]; better ability to enforce contracts with more proximate producers or financial intermediaries; a thicker market for producers of intermediate goods (Ciccone and Hall, 1996) [18]; better quality of the worker-firm match in thicker labor markets; lower risk for both workers and firms, along with the ability to insure through longer-term contracts, financial institutions, or informal arrangements (Lucas, 1988) [37]; and shared amenities that may be location-specific or increasing in population density (Banerjee, 2004; Glaeser, Kallal, Scheinkman and Shleifer, 1992; Davis and Weinstein, 2002)[4]. A recent paper by Acemoglu and Dell (2009)[5] also suggests the quality of institutions as an important factor in explaining the extent to which incomes vary across space.

For example, knowledge spillovers and location-specific amenities are widely cited as the driving factors behind the phenomena of the rapid development of high-tech industries in places such as Bangalore or Hyderabad (Manova and Shastry, 2006) [39]. Knowledge spillovers may occur through collaborative development of technological advances, as well as the spread of new technologies and managerial best practices through informal interactions between workers in similar industries in geographically proximate places. They may also occur through job transitions across employers in which mobile employees from one firm spread scientific ideas or organizational and managerial strategies across firms in related industries in the same region.

The externalities across workers and firms described above may amplify under-

[2] [40, 27, 42, 25, 31]
[3] [32, 33]
[4] [9, 24, 20]
[5] [3]

lying differences in factor endowments across regions or allow for multiple equilibria when location is not uniquely determined by fundamentals, more so in the absence of adjustment costs. These mechanisms may also contribute to explaining the historical persistence in the locations of industrial production. Proximity to natural resources or other fundamentals could theoretically determine the long-run location of production; however, some development experiences seem to follow from accidents of history[6].

We develop an empirical methodology to quantify the magnitude of spillovers in employment across industries within municipalities and apply it to Brazilian data. Using a dataset on the universe of manufacturing establishments in Brazil, we construct a Bartik-style instrument that combines the cross-industry variation in growth of Brazilian industries between 1995 and 2005 and the cross-municipality variation in pre-period industry composition to test for spillovers across industries located in the same municipality. This period was characterized by a sudden shift in exchange rate regimes in 1999, leading to large changes in exports relative to 1995 and large swings in formal employment in Brazilian industries over that period.

We find evidence for economically significant spillovers in employment across industries. More precisely, the employment of manufacturing firms seem to change in response to arguably exogenous shifts in employment in other industries located in the same municipality. These results are robust across several specifications. We test and find little evidence to support alternative explanations for our results, such as measurement error in our industry variables. The estimated agglomeration spillovers appear only over sufficiently long time horizons to suggest that they reflect causal effects rather than common shocks at the municipality level. Additional results also

[6]Adjustment costs may break the long-run indeterminacy in the location of physical capital predicted by some models as a consequence of equalization of rates of return across places.

suggest that these effects do not reflect spillovers through income effects and consumer demand.

We also find that predicted employment increases in other industries are related to an net increase in the number of establishments, suggesting that employment changes appear both within existing businesses and in changes in the number of market participants within an industry.

Note that this paper focuses on the interlinkages in employment decisions across firms and industries, without using data on the capital structure or output of firms. Theoretically, firms could adopt new technologies or change the mix of labor and capital inputs to production in response to productivity or price changes. We develop a method for analyzing the employment decisions of firms and industries that may be more robust to variation in input and output prices, with panel data on firms over time. One limitation of this approach is that it is derived from the optimizing behavior on the part of firms; however, the results should still hold when relatively small effective price or productivity changes lead to departures from optimal choices of inputs.

We also do not directly examine other production externalities that may directly affect employment, affect the choice of production technology, or influence wages through compensating differentials that would be necessary to retain workers. A recent paper by Lipscomb and Mobarak[7] examines the relationship between industrial production and water quality in Brazil by estimating how county boundaries matter for measures of water quality within the same basin.

Section 2 provides a brief discussion of employment in Brazil during the period which we study in this paper. Section 3 outlines a theoretical framework for the paper, Section 4 describes the data and 5 empirical methods, and Section 5 describes

[7] [36]

the results and discusses preliminary robustness checks. Section 6 concludes and discusses directions for future work.

1.2 Employment Changes in Brazil

We use data on employment at the industry, firm and establishment level to test for local effects of national-level changes in employment in manufacturing industries between 1995 and 2005.

Figure 1-1 shows changes in employment by industry over this period for a subset of industries, indexed to initial levels in 1995. Percentage changes in employment vary widely, with some industries decreasing or increasing employment levels more than 25 percent within the decade.

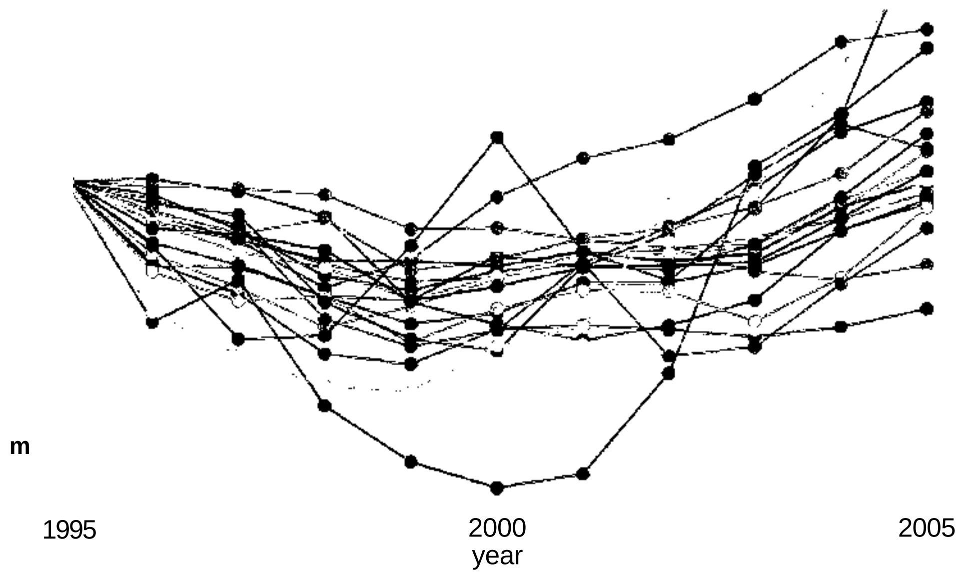
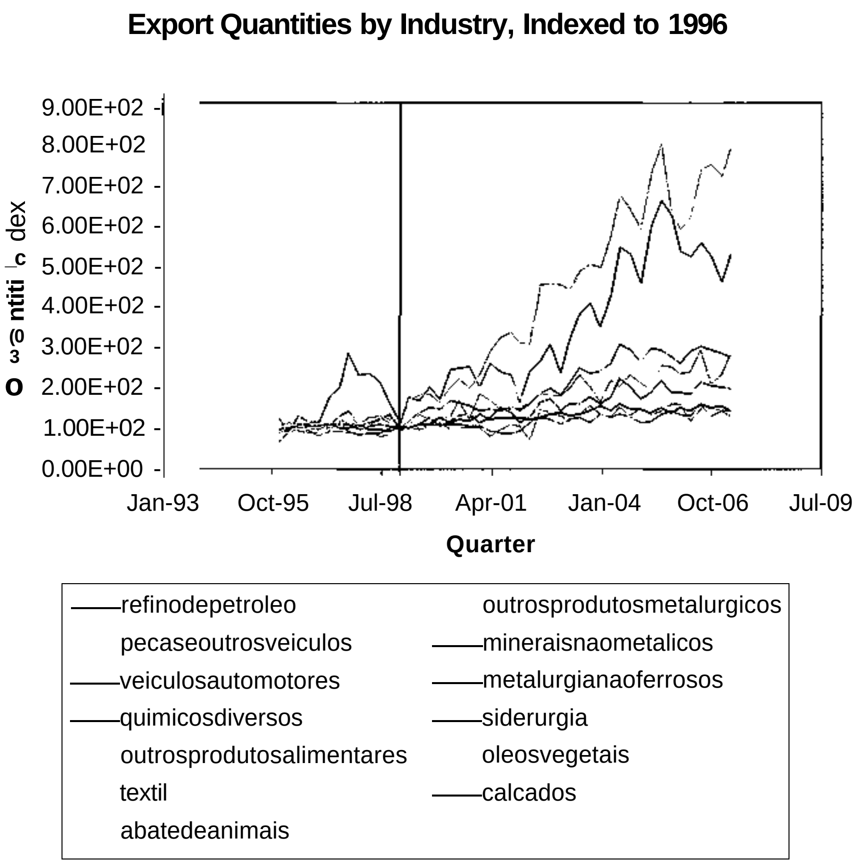


Figure 1-1: Employment by 2-digit manufacturing industry, indexed to 1995

Most industries decreased employment levels prior to a sudden change in the exchange rate with respect to the US Dollar in 1999. The large, unexpected exchange rate devaluation in January 1999 led the real to more than double in value relative to the US Dollar (see Figure 1-2), and was followed by large percentage increases in the quantity of exports in certain industries, possibly reflecting low initial levels of production for export (see Figure 1-3).

: Figure 1-2: Exchange rate (Real vs. US dollar)



: Figure 1-3: Exports by industry for a subset of manufacturing industries

Prior to this exchange rate devaluation, Brazil experienced a period of hyperinflation, followed by an exchange rate regime characterized by a crawling peg to the US dollar. This exchange rate policy, initiated in 1994 as part of an economic stabilization plan, was maintained through the end of 1998 with small, controlled adjustments to the exchange rate. A new floating exchange rate was instituted following the resignation of Brazil's central banker in January 1999, in response to nonpayment of debts from state governments in Brazil to its national government.

A major shift in exchange rate policy, this floating exchange rate may have also been coupled with changes in interest rates and access to finance in Brazil and in the region, leading to aggregate shocks to production and employment reflecting the cost of borrowing to finance ongoing operations.

1.3 Theoretical Framework

Much of the literature on cities and agglomeration focuses on either total output or productivity estimates. In these data, we observe only employment, and do not have measures 4of either total output or capital stock.

In a related paper, Lee (2009) develops a simple theoretical framework to motivate the empirical specifications used in this paper by relating employment levels to output and compute comparative statics when there are small adjustments in the relevant parameters.

Changes in the exchange rate may have led to fluctuations in effective input and output prices - if capital and labor are complementary, then firms may readjust input bundles or the level of production, leading to changes in employment levels.

Suppose that each firm has a production function that uses both labor and a CES aggregate of other inputs (capital goods):

$$Yi — AiLi \ (\ / \ any-$$

where Ai is a firm-specific productivity factor, Li is total employment, $x^\wedge$ is the quantity of each other input used. Suppose also that output prices, wages and prices of inputs vary across municipalities. Then firms solve:

$$\max PmAiLf \ (\ / \ x^\wedge \qquad IMm'-'i \qquad I \ Pjm-E^{1}_{V}$$

For simplicity, assume symmetry across input goods. This then simplifies to:

$$\max P_m AjL" \ (Ni-<>x\backslash \ - \ w_m Li \ - \ Np_m Xi$$

Taking first order conditions and taking logs, we can find an expression for the relationship between total capital inputs $(N_x)i$ and employment:

$$\log a + \log(Nx)i - \log Li = \log w_m - \log p_n \qquad :i)$$

We can then substitute this into the production function to obtain an expressions for total output and employment as a function of prices and parameters:

$$\log Y_x = \log Ai + (a + 1) \log Li + \log w_m - \log p^\wedge \qquad \frac{26-1}{1-6} \qquad \log N — \log a \quad 2)$$

and

$$\log Li = \frac{}{a}\log A \frac{}{a}\log w_m + \frac{-}{a} logp_m + \frac{-}{a}\log P_m - \frac{26-1}{a(1-0)} \log iV \qquad \textbf{(3)}$$

Prices, wages and the productivity parameter A, may vary with the level of output and employment in other industries, which motivates looking at the effect of changes in employment in all other industries on employment in own firm or industry:

$$\frac{d\log L,j}{^{d}(^{l}\circ gT,\&i^{L}j)} \;\_\; \frac{1}{<*} \frac{dlogAj}{d(\backslash og\ J\hat{}j\&Lj)} \quad \frac{2}{a} \frac{dlogw_m}{d\{\backslash ogJ2j\hat{}i\ ^{L}o)} \quad \frac{1}{a} \frac{d\backslash ogp_m}{^{rf}(^{lo}sEjyi\ ^{L}i)}$$

$$\frac{1}{a} \frac{d\backslash ogP_m}{d(\backslash og'\pounds_j\hat{}_iL_j)}$$

This also suggests some comparative statics - if the relevant mechanism is a thicker local market for intermediate inputs, this should show up as lower input prices ($\log p_m$). If the relevant mechanism is search costs, I think (but am not sure) that wages should be lower on average, because with less friction in labor markets, the average search time should be lower and match quality higher. If the relevant mechanism is transport costs, I'm not really sure how that would go, because it would show up in both the output price and the input prices?

Note that this supposes that the choice of technology is stable (the number of varieties used in production doesn't change) and that the underlying parameters of the model do not change with *J2j^i Lj)*-

The degree to which economic activity occurs in cities may be somewhat surprising, given that proximity to other firms and people may bid up prices for certain goods or factors of production, such as labor or land. Higher prices on some inputs must be offset by either lower costs on other inputs, productivity improvements, or other benefits that follow from proximity to other firms.

It is possible to show from here that if productivity rises, employment should as well, both inframarginally and as captured by the entry decisions of firms.

One thing this is useful for is to note that with this production function, in the absence of some kind of cost of starting up a new plant, there should be no effects of TFP improvements on entry. However, if there are, then there should be. Then net profits are $(P_m — (1 + oi))Yi — C$, and Yi conditional on producing at all should be increasing in A_{iy} the parameter indexing productivity.

Finally, without directly observing input prices, this suggests that our data may not be enough to look at the spillovers in firm-specific productivity Ai. I haven't though through this, but I think there's probably a similar way to look at total profits and relate that to the entry and exit decisions of firms? We have some results on the number of establishments by municipality and industry, etc. Here is one way of looking at entry decisions in this framework - profits for the firm are given by:

$$n = P_m Yi - w_m Li - N p_m x$$

We can rewrite the first order conditions, equations (1) and (2), as:

$$\frac{aP \quad Y}{T} = Wm$$

and

$$P_1 \; Y_{m^1 1} \quad N_{Pr},$$

We can then write profits as:

$$\begin{aligned} \text{n} &= PnYi\text{-}aYi\text{-}Yi \\ &= (P_m\text{-}(l + a))Y_t \end{aligned}$$

1.4 Data and Empirical Strategy

We use annual data on employment for establishments in manufacturing industries in Brazil, constructed from the RAIS (see Data Appendix for more detail). We aggregate these data to the industry-municipality level for some of the analysis.

Table 1-1 presents the summary statistics for the sample of firms/muncipalities used in our analysis. The sample includes approximately 30,000 firms located across approximately 3500 municipalities in Brazil.

TABLE 1-1: Summary Statistics by Firm-Municipality-Year

Local Firm Employment	148.42
	(385.00)
Local Firm Employment Growth	-0.01
	(0.34)
Total Firm Employment	958.32
	(2717.76)
Share of Firm to Municipality Total Employment	0.10
	(0.24)
Share of Firm to Municipality Industry Employment	0.26
	(0.31)
Total Firm Number of Municipalities	7.77
	(18.70)
Observations	273675

Note: Observations here are Firm/Municipality/Year. Variables are averaged over all observations in the sample over the period 1995-2005.
Numbers in brackets are standard deviations over that same sample and period.
The sample consists of firms which had average total formal employment, over the years the firm existed in this period, above 50 employees.

It is worth noting that while firms represent a small fraction of their industry employment at the national level, on average they represent 10 percent of the local municipality employment, and an even larger fraction of their industry local employment.

We combine national-level changes in employment by industry with municipality-level variation in the initial distribution of firms to predict such changes between 1995 and 2005.

We predict employment for each municipality-industry-year from the base year share of employment in each industry in each municipality interacted with the national level of employment in each industry-year, excluding own municipality[8].

njt

For a firm i in municipality m, industry j and at time t, $Y_{m}jt$ provides a measure of the expected municipality employment level in the firm's own industry.

For each firm/municipality, we then construct a yearly measure of the predicted employment for firms in all other industries in the same municipality by then summing these predicted employment over all industries in each municipality-year excluding the industry of firm i.

**m—jt*   *_/__1* **mkt*

k^3

[8]In future work, we also plan to follow Autor and Duggan (2004) [8] in excluding own municipality from national trends in the construction of this Bartik-style instrument [11]. However, note that in our sample, no single municipality accounts for an important share of national employment in any of the industries under consideration, and we predict that our results are robust to the exclusion of this adjustment.

We first check the "first stage" by regressing the log of municipality-level manufacturing employment on the log of these predicted employment changes:

$$ln(Y_{mt}) = a_m + 6_t + (3\text{-}\ ln\{Y_{mt}) + \mathrm{e}_{\mathrm{mi}}$$

We then explore the reduced form relationship between employment for firm *i* and the predicted employment of other industries in the same municipality by regressing the log of employment for firm *i* on a full set of firm/state fixed effects, year arbitrary shocks, the log of predicted employment in own industry and the log of predicted employment in other industries:

$$ln(Y_{imjt}) = eti + d_t + Pi\text{-}\ ln(Y_{mjt}) + f3_2 \bullet ln\{Y_{m\_jt}) + e_{imjt}$$

The coefficient of interest is *fa-* This coefficient tells us the average additional growth experienced by firms in an industry when the other industries in the same city are predicted to expand by 100 percent.

In our main specification, we use this predicted employment for other industries in the same municipality as an instrument for the actual employment of other industries in the same municipality. More precisely, we estimate:

$$ln\{Y_{imjt}) = ati + 9_t + /5\mathrm{i} \bullet ln(Y_{mjt}) + f3_2 \bullet ln(Y_{m\_jt}) + e_{imjt}$$

using $Y_m\text{-}j_t$ as an instrument for $Y_m\text{-}j_t$. The coefficient of interest here is also $/5_2$, which now tells us the average additional growth experienced by firms in an industry when the other industries in the same city expand by 100 percent.

The identification of this effect comes from comparing firms which are located near different industries, which experience expansions and contractions (at the national level) in different points in time. Most of the variation reflects differences in magnitudes of changes, not necessarily different timing of changes in employment trends.

Next, we allow for a more flexible specification by allowing the year effects to differ across industries and across states. We estimate:

$$ln(Y_{imjt}) = a_t + 7_{it} + \backslash_{st} + (3i \bullet ln(Y_{mjt}) + /3_2 \bullet ln(Y_{m\_jt}) + e_{imjt}$$

where s indexes the state containing municipality j. Again, we instrument for $Y_m\text{-}j_t$ using $Y_m\text{-}j_t$. In this specification we restrict identification further, by comparing firms in the same state and sector, but located near different industries, which experience expansions and contractions (at the national level) in different points in time.

We also characterize the timing of the effect. We implement this by using our approach to estimate the effects of changes in the employment of other industries in the same municipality on changes in employment at firm i over 1, 3, 5 and 7-year horizons. We use the following specification:

$$ln\{Y_{imjt}) - /\mathrm{n}(\mathrm{Y}_{\mathrm{imj}}(_{\mathrm{t}}\_\mathrm{j})) = 0^* + \mathrm{ft} \bullet (ln(Y_{mjt}) - \mathrm{M\hat{\ }W(t\text{-}o)})$$
$$+(3_2 \bullet (ln(Y_{m\text{-}jt}) - \mathrm{Zn(y_m\_j(t\_j)))} + e_{imjt}$$

where $ln(Y_m\_j_t) — ln(y_m\text{-}j\{_t\text{-}i))$ is an instrument for $ln(Y_m\hat{\ }j_t) — ln(Y_m\_j(_t\_i))$. The estimation of this equation for different time horizons allows us to understand how long does it take for firms to adjust in response to expansions by other industries in the same municipality.

1.5 Results

We first show that our approach leads to a strong predictor for the local employment of industries across municipalities. We then report the results using this predictor to estimate how firms' local employment growth responds to expansions in the local employment of other industries in the same municipality. In the second part, we report the importance of these effects for different time horizons. We then present and discuss several checks to refine and test the robustness of our results. We then discuss effects of predicted employment changes in other industries on the number of firms operating in a given industry in a municipality.

1.5.1 National and Local Employment Trends

The first basic question that we address is whether our approach actually leads to a strong predictor for the local employment of industries across regions. Table 1-2 reports the estimation of equation (3) with the log of municipality employment as the outcome. We are simply testing how changes over time in the predicted employment for all industries in a municipality are correlated with the actual overall employment in that municipality. We are controlling for fixed differences across municipalities and year fixed effects, so identification comes from comparing municipalities with industries that experienced different shocks (at the national level) in a given point in time. The point estimate implies that a predicted change of 100 percent in the employment of a given municipality is associated with a statistically significant actual change of 56.6 percent.

TABLE 1-2: "First Stage"
Dependent variable: Log(Employment)

	(I)
Log(Predicted Employment)	0.566***
	(0.033)
Constant	-0.008***
	(0.002)
Municipality Effects	Yes
Year Effects	Yes
Observations	22158
R-squared	0.140

Robust standard errors in parentheses, clustered at municipality level.
* significant at 10%; ** significant at 5%; *** significant at 1%

1.5.2 Cross-industry Employment Effects Within Municipalities

Column (1) of Table 1-3 reports the reduced form effects on the local employment of firms in a given industry, using our approach to predict the employment of other industries in the same municipality. The result is based on the estimation of equation (4). The estimated effect is a statistically significant expansion of 5.1 percent on the average employment of firms in a given industry in response to a predicted expansion of 100 percent in other industries.

Column (2) of Table 1-3 reports the IV estimator based on this approach. More precisely, we estimate equation (5) using the log of predicted employment in other industries as an instrument for the log of actual employment in other industries. There is an estimated average expansion of 16.5 percent in the local employment of firms in a given industry in response to an actual expansion of 100 percent in the employment of other industries.

This result suggests the existence of economically and statistically important agglomeration spillovers. In the absence of such spillovers, an expansion in the demand for labor and other immobile factors in a given industry should bid up their prices and reduce the growth of firms in other industries.

TABLE 1-3: Reduced Form Estimates

Dependent variable: Log(Employment)

	(I)	(II)	(III)	(IV)
	Industry-Municipality		Firm-Municipality	
	Reduced Form	IV	Reduced Form	IV
Log(Predicted Employment in Own Industry)	0.602***	0.718***	0.051***	0.070***
	(0.032)	(0.035)	(0.009)	(0.010)
Log(Predicted Employment in Other Industries)	0.175***	0.237***	0.047***	0.165***
	(0.015)	(0.023)	(0.017)	(0.043)
Constant	-0.043***	-1.844***	-0.000	-1.510***
	(0.002)	(0.171)	(0.000)	(0.457)
Industry-Municipality Effects	Yes	Yes	No	No
Firm-Municipality Effects	No	No	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Observations	53984	51565	239111	232604
R-squared	0.094		0.021	

Robust standard errors in parentheses, clustered at municipality level.

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

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1.5.3 Timing of Effects

Our first strategy to refine the evidence on the importance of spillovers is to look at the timing of the effects. To the extent that our results are actually driven by agglomeration spillovers they should be particularly important over longer time horizons. We implement our approach for different time horizons by estimating equation (6) with different time intervals.

Columns (1) to (4) of Table 1-4. A present the reduced form results based on this approach. Columns (1) to (4) of Table 1-4.B present the IV estimates. The results reveal that the impact of expansions to other industries in the same municipality are especially important over longer horizons. Indeed, firms in a given industry do not experience economically or statistically significant higher growth over one year in response to expansions in other industries. On the other hand, over a horizon of five years, firms in a given industry are estimated to expand on average by 26 percent in response to an expansion by 100 percent of other industries. It is also worth noting that the economic magnitudes of the effects increases over longer time horizons, but becomes stable after 5 years.

Together, these results provide additional support for the importance of agglomeration spillovers.

The mechanisms through which these employment spillovers across sectors may operate include productivity effects generated by knowledge sharing or scale effects that influence innovation, straightforward demand effects that can be empirically characterized by examining input-output interlinkages across firms and industries, transport costs or costs of contract negotiation and enforcement that explain the persistence of local production relationships across firms, and insurance motivations.

TABLE 1-4.A: Reduced Form Estimates Over 1, 3, 5 and 7 Year Horizons

Dependent variable: *Log(Employment$_t$)* - *Log(Employment$_t$-x)*

		(I) 1 year	(II) 3 years	(III) 5 years	(IV) 7 years
A	*Log(PredictedEmploymentinOwnIndustry$_t$)*	0.030***	0.045***	0.064***	0.075***
		(0.008)	(0.008)	(0.013)	(0.016)
A	*Log(PredictedEmploymentinOtherlndustriest)*	0.000	0.050***	0.093***	0.114***
		(0.008)	(0.016)	(0.025)	(0.032)
Constant		-0.001	-0.000	-0.075***	-0.099***
		(0.006)	(0.012)	(0.017)	(0.016)
Firm-Municipality Effects		Yes	Yes	Yes	Yes
Year Effects		Yes	Yes	Yes	Yes
Observations		185831	113302	66694	35470
R-squared		0.006	0.012	0.014	0.011

Robust standard errors in parentheses, clustered at municipality level.

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

TABLE 1-4.B: Instrumental Variables Estimates Over 1, 3, 5 and 7 Year Horizons

Dependent variable: *Log(Employmentt)* - *Log(Employment$_t$~x)*

	(I)	(II)	(HI)	(IV)
	1 year	3 years	5 years	7 years
A *Log(PredictedEmploymentinOwnIndustry$_t$)*	0.030***	0.055***	0.084***	0.100***
	(0.007)	(0.008)	(0.013)	(0.017)
A *Log(PredictedEmploymentinOtherlndustriest)*	0.010	0.152***	0.257***	0.260***
	(0.026)	(0.037)	(0.043)	(0.043)
Constant	-0.001	-0.004	-0.064***	-0.073***
	(0.005)	(0.010)	(0.009)	(0.012)
Firm-Municipality Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Observations	180530	110137	64882	34552
R-squared	0.006	0.007		

Robust standard errors in parentheses, clustered at municipality level.

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

to

^

1.5.4 Net Entry of Establishments

We also estimate the effect of local predicted employment changes on the net number of establishments operating within a given industry in a municipality. We find that there are substantial and statistically significant effects of predicted employment changes on net entry of establishments, in both levels and logs (Table 1-5, results in logs not shown).

TABLE 1-5: Reduced Form Estimates
Dependent variable: Number of Firms Operating in 2-digit Industry by Municipality

	Reduced Form (I)	IV (II)
Log(Predicted Employment in Own Industry)	0.931***	0.986***
	(0.263)	(0.265)
Log(Predicted Employment in Other Industries)	0.873***	0.871***
	(0.078)	(0.078)
Constant	4.478***	-3.234***
	(0.591)	(0.480)
Industry-Municipality Effects	Yes	Yes
Year Effects	Yes	Yes
Observations	47812	47812
R-squared	0.001	0.111

Robust standard errors in parentheses, clustered at municipality level.
* significant at 10%; ** significant at 5%; *** significant at 1%

1.5.5 Robustness Checks

One potentially important concern with our results is the possibility of measurement error in our industry variables. Even after conditioning on industry controls, firms located near a given industry might be economically closer to that industry. If this is the case, our results could be simply reflecting the possibility that close industries experience similar shocks at the national level.

The timing of the effects goes against this interpretation, since it is not clear why this mechanical correlation should be particularly important for longer time horizons and not important at all over the horizon of one year.

A second strategy to deal with this concern is to compare the estimates across specifications that include different controls for firms' own industries. Columns (1) and (2) of Table VI respectively present the IV estimates from equation (5) with and without the control for the firms' own industry. The estimated magnitudes become larger when we add the controls for the firms' own industry. This is exactly the opposite that we would expect if the results were driven by measurement error in the industry variables.

TABLE 1-6: Robustness Checks, Log of Firm/Municipality Employment

	(1)	(2)	(3)	(4)	(5)
Log (Employment in Other Industries)	0.135***	0.145***	0.141***	0.149***	0.077**
	(0.035)	(0.037)	(0.037)	(0.039)	(0.037)
Log (Predicted Employment in Own Industry)		Yes	Yes	Yes	Yes
Log(Other Firms Employment in Same Industry/State)			Yes		
Firm Municipality (Plant) Effects	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes		
Industry /Year Effects				Yes	Yes
State/Year Effects					Yes
Observations	250618	232604	230896	232604	232604
R-squared	0.02	0.02	0.02	0.02	0.03

Robust standard errors in parentheses, clustered at municipality level.
* significant at 10%; ** significant at 5%; *** significant at 1%

to

As an additional robustness check, we present the results including state/year and sector/year fixed effects. The results are based on the estimation of equation (6). The addition of those fixed effects restricts the identification of the results only to comparisons across firms in the same industry and states. Another approach to control for differences across firms in their location and industry is to simply include the average employment of other firms in the same industry, state and year as a control in the estimation of equation (5).

Columns (3) to (5) of Table 1-5 present the IV estimates based on these approaches. The results across a variety of specifications support the existence of economically and statistically significant agglomeration spillovers.

Finally, we test the importance of spillovers through consumer aggregate demand in explaining our results. As local expansions in other industries translate into higher wage income, this can lead can to an expansion in local consumer aggregate demand for local goods. To the extent that some manufacturing firms are producing local goods, this could expand the demand for their goods. Additionally, an expansion in the demand for local services could amplify this effect. A central prediction from agglomeration spillovers driven only by this story is that spillovers should be particularly important in municipalities where manufacturing represents a sizeable fraction of the local economy. One way to measure this local importance of manufacturing is to look at the fraction of the working population employed in manufacturing.

In non-reported results we first test if the estimated spillovers are more important in municipalities where manufacturing corresponds to a greater fraction of the total labor force. We found no economically or statistically important evidence that this is the case. Additionally, we found economically and statistically important effects even when we restricted our sample to municipalities where manufacturing represents a very small fraction of the labor force. Together, these results suggest that our findings are not mainly driven by consumer aggregate demand spillovers.

1.6 Conclusion and Further Work

In this paper we develop an empirical methodology to quantify the magnitude of spillovers across industries within municipalities and apply it to Brazilian data. We document the existence of economically important spillovers in the growth decisions of firms. Firms grow substantially more in response to expansions by other industries in the same municipality. Our results support the importance of theories predicting that agglomeration spillovers can explain the spatial concentration of economic activity by amplifying underlying differences in factor endowments across regions or generating multiple equilibria when location is not uniquely determined by fundamentals.

These results highlight the importance of learning about the underlying structural sources explaining agglomeration spillovers. What is the relative importance of factors such as knowledge externalities and transportation costs in explaining them? How do they actually lead to agglomeration spillovers? What is the relative importance of spillovers across and within industries? Our approach can be extended to address these questions. We can test if our effects are particularly important in human capital intensive industries or in places with high transportation costs, for example. We can also test if our effects are particularly important for industries producing similar goods. More broadly, we can use our approach to estimate the importance of spillovers across several pairs of industries to test the relative importance of competing theories which predict agglomeration spillovers. We believe this is a very fruitful area for future research.

Data Appendix

Employment Data

We use an administrative database to construct aggregated annual measures of employment by plant, firm, industry, and location. This database (RAIS) is administered by the Brazilian Ministry of Labor. All firms formally hiring workers in Brazil are required to provide information to the Ministry of Labor for this registry.[9]

The underlying dataset contains unique plant and firm identifiers, and information on the plant's sector of production, as well as a rich set of information on individual workers. For each worker in the dataset, the data include a unique worker identifier, educational attainment in nine categories, an occupation code, as well as dates of accession and separation.[10]

We use an aggregated version of these worker level records to construct annual plant, firm-by-municipality, and industry-by-municipality measures of employment. We also construct these measures disaggregated by education category.

For any given plant and year, we tracked all workers that worked in that plant/year and computed the fraction of the year that each individual worked at the plant. We then aggregate this for all existing workers. The unique plant and firm identifiers allow one to both track firms and plants over time, as well as track plants to firms at any given period. Finally, we construct measures of the total number of plants operating in each firm and industry in each municipality and year.

We do not include years prior to 1995 due to the existence of very high inflation prior to this period. Firms are included in the sample if they had average total employment above 50 workers over this sample period, and also produced results

[9]Daniel Carvalho, one of the authors of the paper in this chapter, carefully constructed these aggregated measures with the generous support of IPEA researchers and staff in Brazil.

[10]Refer to Melo (2008) for more detail about the underlying dataset.

without this size restriction. We track those firms and all their plants over all the sample period. Firms drop out of the data only when they leave the social security registry, which can happen either due to true exit (bankruptcy or acquisition) or due to a change in the firm tax code (unique firm identifier).

Population and Municipality Characteristics

We complement the employment data with data on municipality characteristics from the 2000 Census. Municipalities are uniquely matched based on the Brazilian system of municipality codes. The main variable from the Census of interest for our analysis is the overall size of the municipality working force. Together with the information on manufacturing employment, this allows us to measure the importance of manufacturing in a given municipality.

Chapter 2: Do Jurors Discriminate? Evidence from State Juror Selection Procedures

2.1 Introduction

African Americans comprised 46 percent of the prison population in the United States in 2000, while only accounting for 12 percent of the total population (Bureau of Justice Statistics, 2000; United States Census Bureau, 2000)[11]. Part of this disproportionate rate of incarceration reflects differences in crime rates, types of crime committed and arrest rates (Arvanites and ASher, 1998; Tonry, 1995)[12]. However, in addition, it is a commonly held belief that racial discrimination in the judicial system contributes

[11] [16, 48]
[12] [7, 47]

significantly to the observed discrepancy in incarceration rates for whites and African Americans (Cole, 2000; Kennedy, 1998)[13]. While most observers would likely agree that some form of discrimination on the basis of race occurs in the criminal justice system, little empirical work sheds light on whether discriminatory behavior accounts for a quantitatively significant portion of the observed racial incarceration gap. This analysis takes a step towards assessing the magnitude of the contribution of discrimination to aggregate patterns in incarceration.

Discriminatory actions on the part of a number of actors in the judicial system, including judges, prosecutors, and even defense attorneys, may contribute to this observed disparity in outcomes. I focus on the role of trial jurors[14]. The possibility that juries may make racially biased decisions has attracted considerable attention from both researchers and the popular media, especially in the context of capital trials (Blume, 2004)[15]. Discrimination by jurors would likely have substantial welfare implications for a much broader set of defendants, however - both those who face a jury trial and even some who do not. While only a small percentage of all convictions are obtained through jury trials, the right to trial by jury in criminal cases is constitutionally guaranteed, and the expected outcome before a jury may change the context in which plea bargains are made, affecting defendants whose cases do not ultimately go to trial. The estimates presented in this paper, which focus on a specific set of actors, could arguably be viewed as a lower bound on the total extent to which discrimination contributes to the observed racial incarceration gap.

I use variation in the timing of changes in state laws governing the compilation of lists of eligible jurors to attempt to identify the impact of increasing the share

[13][19, 30]

[14]Trial juries, or *petit* juries, are responsible for deciding to convict or acquit defendants. In some jurisdictions, they are also responsible for sentencing. Grand juries are responsible for issuing indictments.

[15][15]

of non-white jurors on outcomes for nonwhites relative to whites in the criminal justice system. Prior to these changes, jury commissioners, town leaders or civilian jury committees could exercise a great deal of discretion in compiling master lists of eligible jurors.[16] In principle, discretionary systems were meant to facilitate the construction of "blue-ribbon juries" comprised of "men of recognized intelligence and probity" (Abramson, 2000)[17]. In practice, these systems also facilitated the near total exclusion of African-Americans and other minorities from jury service in some counties (Kennedy, 1998)[18]. After these changes, master lists were required to be selected at random from publicly available lists such as lists of registered voters and drivers or tax rolls.

In the benchmark specification, I estimate a differences-in-differences specification exploiting variation in timing of the adoption of random selection across states that changed their laws between 1975 and 1999 to test for an effect of changes in the composition of lists of eligible jurors on the nonwhite share of total new admissions to prison. The procedural changes appear to have lowered the nonwhite share of admissions to prison by over 5 percentage points, a finding that is robust to the inclusion of a rich set of controls. An analysis of the dynamic effects of the law changes reveals a time pattern of treatment effects consistent with a causal interpretation - there are no significant differences in the nonwhite share of admissions to prison in the years leading up to the law changes, and a statistically and economically significant reduction immediately following. I also find suggestive evidence that the nonwhite share of admissions to prison dropped more in states with a higher share of nonwhites as a fraction of the total population, although the standard errors are large in some

[16]For example, in the extreme case of the "key-man" system, names of potential jurors were collected from community organizations and church leaders at the discretion of often exclusively white "key-men" identified by jury commissioners and court officials.

[17][2]

[18] [30]

specifications.

Taken together, these findings suggest that increasing the share of nonwhite potential jurors led to a decline in the nonwhite share of admissions to prison, consistent with an own-race bias due to differences in either preferences or information.

There are several challenges in evaluating the impact of these legislative mandates and in assessing whether the estimated effects reflect racial discrimination.

First, there are limited outcome data available over a sufficiently long time series, and those that data that are available are clearly flawed. Due to limited availability of data on conviction rates conditional on going to jury trial by state and race, the analysis focuses on the effect of these laws on the nonwhite share of new admissions to prison. Although this measure includes both admissions resulting from all convictions, rather than just those from jury trials, it arguably may be the most relevant outcome as changes in the expected conviction probability may influence the terms of plea bargains or the seriousness of charges even in cases that do not ultimately go to trial. To the extent that the data contain measurement error that is classical, the flaws in the data collection will result in larger standard errors but not bias the point estimates.

Second, the timing of the law changes may be endogenous in the sense that the passage of these laws may have been driven by improvements in the racial climate that also contemporaneously changed the racial composition of admissions to prison, or they may have been bundled with other civil rights reforms that would affect the nonwhite share of admissions to prison. I try to address this by assessing the sensitivity of results to including more flexible controls for time trends, by examining the timing of the impacts of the law changes, and by testing for changes in racial attitudes in the General Social Survey (GSS) and the American National Election Studies (ANES). I also present some qualitative evidence that these law changes were not high profile political issues, and if anything, were more often bundled with minor procedural reforms in state courts rather than with civil rights-related legislation. There is no evidence that these law changes were coincident with changes in the racial composition of arrests or with changes in racial attitudes in the general population.

One challenge that I am unfortunately not able to address in detail here is to attribute the estimated changes in the demographic composition of admissions to prison to specific shifts in the demographic composition of eligible jurors. To my knowledge, data on jury lists and jury participation across states and over time do not exist, and thus the analysis here is conducted under the maintained assumption that these laws did indeed lead to an increase in the diversity of jury pools in terms of race, gender, and socioeconomic status, as claimed in secondary sources such as Kennedy (1998) [30]. I present suggestive evidence that changes in the racial composition of the jury pool may have been empirically important relative to changes along other dimensions, but this aspect of the analysis is speculative at best.

These findings relate to the empirical literature on discrimination, in particular the literature on discrimination in the criminal justice system. Previous work has found evidence for discrimination in a wide range of contexts, including but not limited to labor markets, marriage markets, and sports. Bertrand and Mullainathan (2004) [13] find that resumes randomly assigned to have black names receive 50 percent fewer callbacks than those with white names; similarly, Pager (2005) [45] finds that among matched experimental applicants, blacks received substantially fewer job offers, adding experimental evidence to the extensive literature on racial discrimination in the labor market. Price and Wolfers (2007) [46] find NBA referees call more fouls on players of the opposite race, all else equal. In the area of crime and criminal justice, Donohue and Levitt (2001) [23] find that increases in the share of black police officers coincide with increases in white arrests and vice versa, while McCrary (2007)

[41] finds evidence for modest effects of the imposition of court-ordered hiring quotas on the racial composition of arrests. In a recent working paper, Abrams (2006) [1] find evidence for racial biases in sentencing by judges by exploiting the random assignment of cases to judges. *A priori*, it seems likely that jurors, who face no career or reputational incentives to act in a non-discriminatory manner, may be more prone to discriminate than judges, prosecutors or law enforcement officers. Iyengar (2007)[19] finds evidence that juries may be more racially biased than judges by examining a Supreme Court decision that shifted the authority to impose sentences from judges to juries in capital cases in 13 states. Finally, a large literature in social psychology finds evidence that similarity between juror and defendant characteristics generally leads to a bias in favor of the defendant. However, these studies largely rely on mock jury experiments, qualitative evidence, or small samples of cases which provide a characterization of cross-sectional patterns as in Devine (2001)[20].

The remainder of the paper is organized as follows. Section 2 gives a brief outline of jury selection procedures and describes the law changes examined in this paper. Section 3 presents a simple theoretical framework for understanding the possible effects of this policy change. Section 4 discusses the data and Section 5 presents the main results. Section 6 concludes and outlines directions for further work.

2.2 Background

Jury Selection Procedures

While jury selection procedures vary from state to state, they share several common features across states. The initial pool of eligible jurors is contained in a master list, typically compiled by jury commissioners and district clerks. Names of potential

[19] [26]
[20] [22]

jurors are drawn from this list, and summonses are mailed to those jurors who are drawn. Summoned prospective jurors appear before a judge, and can be excused due to unnecessary hardship. The remaining potential jurors are assigned at random to jury panels for each trial, and are sent to a "voir dire" to be considered for jury service for a particular trial. In most states, jurors may be examined by defense and prosecution attorneys or by a judge. During this process, they may be dismissed "for cause" due to conflicts of interest or preexisting knowledge of the case, or without cause through peremptory challenges[21].

This paper focuses on laws which limited the ability of jury commissioners and district clerks to manipulate the composition of the jury pool by excluding women, African Americans and the poor from the master jury list. Discrimination could occur at each of these stages of jury selection - e.g., there is at least anecdotal evidence that peremptory challenges are used to strike black jurors in cases with black defendants, particularly in capital cases (Liptak, 2007). However, there is some reason to believe that exclusion at stage of compilation of juror lists was significant relative to discrimination at later stages in reducing the representation of nonwhites on juries relative to their population share. A 1972 survey of jury commissioners, district clerks, state attorneys, defense attorneys and judges in 325 counties in the South with large African American populations found that self-reported race shares at different stages (jury list, jury box, jury) indicate that a large fraction of the disparity between population shares and jury service race shares materialized at the stage of the compilation of the jury list [12].[22]

[21]The number of peremptory challenges available to defense and prosecution attorneys is limited by state law, although the limit varies across states.

[22]These survey results should be viewed with caution, given nonresponse rates and biases in self-reported data, although the conclusion that the disparity largely appears at the stage of the jury list may be robust to this if misreporting is similar across jury commissioners, attorneys, and judges.

Law Changes

A sweeping procedural change occurred in Federal courts as a result of the passage of the 1968 Jury Selection and Service Act, which required the approximately 60% of districts still allowing discretion in the selection of eligible jurors to switch to random sampling from lists of registered voters [35]. While a small number of states adopted random selection before 1968, most states slowly switched over following the passage of the Federal Jury Selection and Service Act and a 1975 Supreme Court decision which required state courts to do the same [2]. As of 1980, sixteen states still retained policies which allowed for discretion in the selection of eligible jurors. As of 2004, only four states allowed for discretion in the summoning of potential jurors [43, 44]. Despite the disproportionately low representation of nonwhites on voter rolls, and the opportunities provided by peremptory challenges to strike nonwhite jurors from juries later in the jury selection process, it seems plausible that the number of nonwhite jurors would be higher under random selection than under the key-man system.

While there were often prohibitions against discriminatory jury selection in state laws, there is some evidence that these laws were difficult to enforce in the absence of specific statutory requirements limiting discretion in procedures such as the compilation of lists of eligible jurors. One legal scholar found that between 1935 and 1975, the Supreme Court heard on average one case per term regarding discriminatory jury selection procedures, and usually ruled in favor of the defendant [49]. The persistence of such cases into the 1970's shows that discriminatory practices continued even though there was a clear precedent that they would be ruled unconstitutional [2].

Even laws mandating the use of specific source lists alone, without supplementary legislative mandates specifying that potential jurors should be selected at random from them, appear to have left substantial room for discretion in the compilation of lists of eligible jurors[23]. In the coding of state laws, I follow the taxonomy in the Bureau of Justice Statistics' "State Court Organization" publications and focus on two aspects of the laws governing compilation of the master list: whether they specify a source list (such as the voter registration list or list of registered drivers), and whether they require random selection from these lists rather than giving a substantial amount of discretion to jury commissioners, clerks, or jury commissions comprised of citizens or civil servants. States are coded as having adopted "random selection" if the laws specify source lists and require random selection from those lists.

Data on jury participation are scarce, but Supreme Court cases provide a rich (albeit unrepresentative) source of anecdotal evidence about the impact of key-man jury selection procedures on the composition of the pool of eligible jurors. For example, in 1947, the Court reversed the death sentence of a man who was convicted in Lauderdale County, Mississippi because out of 12,511 African Americans in the county at the time, only 25 were eligible to serve on juries. Even more shockingly, no African American had served on a jury in that county in the previous 30 years (Patton v. Mississippi, 332 US 463) [2]. In another example, in 1975 the Fifth Circuit Court of Appeals ruled that the disparity between the 51 % population share of Quitman County and the 24 % share on the list for trial juries, when added to evidence showing that this disparity appeared only after the stage of the process where discretion could be exercised, provided prima facie evidence of discrimination (Foster v. Sparks, 1975).

Discussion of the procedures governing compilation of jury lists appears infrequently in the academic literature on discrimination, although they are featured prominently in more recent histories of discrimination and the criminal jury [30, 5].

[23]Benokraitis (1982) found that in such states, jury commissioners and district clerks reported using personal knowledge to select potential jurors, or consulting acquaintances to eliminate a significant number of jurors from the lists based on reports of "character" and "intelligence" [12].

It also does not appear that these law changes were contemporaneous with the passage of broader state-level civil rights legislation, although they were in some cases bundled with procedural reforms such as the institution of one day-one trial systems of jury service. In addition, these law changes do not appear to have generated attention in the popular media at the time they were enacted, suggesting that they were not politically salient issues and leaving open the possibility that they do not simply reflect general improvements in race relations that would independently drive changes in the racial composition of crime and admissions to prison.

In addition to this qualitative evidence, below I present some tests of the endogeneity of these law changes to improvements in racial attitudes in the general public or in law enforcement, as well as placebo tests using data from Federal courts. While it is still possible that both the passage of these laws and the corresponding changes in the racial composition of admissions to prison were jointly driven by unobserved factors, these tests provide some confidence that the most obvious of these mechanisms may not be at play.

2.3 Theoretical Framework

The effect of random sampling on the nonwhite share of admissions to prison is theoretically ambiguous. However, the most intuitive prediction would be that increasing the representation of nonwhites on jury panels would lower conviction rates for nonwhites relative to whites and thus would lower the nonwhite share of admissions to prison through two channels: through a direct impact on conviction probabilities conditional on reaching trial, and through the effect this change in conviction probability may have on the treatment of cases at earlier stages in the process. Given that a small share of cases actually go to trial, it would perhaps be surprising to observe a quantitatively significant effect of changes in jury selection procedures on rates of admission to prison without considering the indirect effect that changes in these procedures may have at earlier stages, for example by changing the threat points for negotiations between defense attorneys and prosecutors over the terms of plea bargains.

To illustrate these two effects, consider a toy model in which the prosecutor's objective is to maximize expected punishment possible given three possible outcomes: acquittal, which involves a punishment of 0; plea bargain punishment *L* (which is for now assumed to be exogenously given, and assumed to be a punishment other than inprisonment); and conviction (resulting in imprisonment) *H* . Let *p* be the probability that the jury will convict, and *C* be the cost to the prosecutor of taking the case to trial with probability distribution *F()*. The prosecutor will then take a case to trial if $p \cdot H — L > c$, or with probability $F(p \cdot H — L)$, and the defendant will be sent to prison with probability $p \cdot F(p \cdot H — L)$. The effect of an exogenous shock to *p* is given by:

$$\frac{dPr\hat{}soned)}{dp} = R\,— \quad + p \quad ff \quad R\,—$$

Both terms are positive, so a negative shock to p will unambiguously reduce the share of defendants who are imprisoned. The first term captures the direct effect of a change in p on the probability of imprisonment. The second term captures the indirect effect: prosecutors may be more willing to agree to plea bargains if the probability of conviction goes down. This is a crude toy model, and in reality prosecutors could adjust on a number of margins, including the severity of the charge and sentencing, but it captures the basic intuition behind why changes in jury composition may have substantial effects on imprisonment even though a small share of cases go to trial.

The discussion to this point has taken for granted that random sampling would decrease the probability of conviction for nonwhites, but it is plausible that the effect of moving to random sampling could increase conviction probabilities for nonwhites.

First, some argue that all-white juries convict insufficiently frequently or impose less stringent sentences in cases involving African American defendants and African American victims [29]. Consistent with this hypothesis, Blume (2004) [15] finds that black defendants convicted of murdering black victims are underrepresented on death row given the share of black defendant-black victim murder cases among all murder cases, and that this shortfall is larger in the South than in the rest of the country. Given that both violent and nonviolent crimes are most often intraracial, introducing more nonwhites into the pool of eligible jurors could then in theory *raise* the rate of nonwhite admissions to prison.

Second, random sampling could have brought less educated whites as well as more nonwhites into the pool of eligible jurors. These less educated whites could be more biased jurors than the "men of probity" who supposedly served as jurors under the key-man system. Third, to the extent that these changes were mitigated by the use of peremptory challenges to remove nonwhite jurors from jury panels, these results suggest that the inclusion of a small number of nonwhite jurors on a jury panel may heavily influence trial outcomes. This may reflect the fact that jury verdicts in most jurisdictions must be unanimous, so the dissent of a single juror would be sufficient to prevent conviction. Finally, given that only a small share of criminal cases go to jury trials, it may be the case that changes to the pool of jurors would have no effect or a very small effect on admissions to prison, if the effect of a change in the expected conviction rate on upstream decisions such as plea bargaining is small.

2.4 Data

The timing of the *de jure* changes were obtained from each state's annotated state code. The year in which each state changed its policy (to the best of my knowledge), as well as a list of states that never changed their policies appear are summarized in Table 2-1 and documented in more detail in Appendix A. The date of the law change was inferred from four cross sections from secondary sources that document the laws governing source list compilation in 1977, 1980, 1983, 1998 and 2004; from notes to the relevant codes in current and superseded versions of the annotated state code for each state; and from state-specific secondary sources in some cases. States which adopted random selection from public source lists in 1975 or before are coded as having changed "before 1975" and are included in the regressions as controls, as are states that never changed their procedures.

TABLE 2-1
Timing of Law Changes

Early Adopters State	Changed in Sample Period (After 1975) State	Year	Do Not Change by 2004 State
All others	Kentucky	1976	Georgia
	New York	1977	Oklahoma
	Virginia	1977	South Carolina (2006)
	Alabama	1978	Tennessee
	Florida	1979	
	Arkansas	1981	
	Massachusetts	1982	
	West Virginia	1986	
	Missouri	1989	
	New Hampshire	1992	
	Louisiana	1995	
	Connecticut	1997	

Data on admissions to prison were obtained from the Bureau of Justice Statistics' National Corrections Reporting Program (NCRP) series for 1986-1999 and from the Bureau of Justice Statistics' "Race of Prisoners Admitted to State and Federal Institutions in the United States, 1926-1986" for 1975-1985.

The National Corrections Reporting Program datasets contain individual level information on admissions to prison, including a limited set of demographic characteristics (age, sex, race, and education), the most serious charge, the maximum time to be served, whether the individual is being newly committed to prison, and the county in which the sentence was imposed. For this analysis, I aggregate these data up to state-year cells. Since race for nonwhites was only coded as "nonwhite" or "other" in some years, I code all admissions data in that fashion, rather than focusing on African American admissions to prison.

The NCRP data begin in 1983, and data quality is especially poor for the first few years. Thus, to extend the time series of admissions shares by race, I use data from the "Race of Prisoners Admitted to State and Federal Institutions in the United States, 1926-1986" on aggregate admissions by race, state and year from 1975-1985. The data are sparse prior to 1975, with 0 state-year observations in 1971, 1972 and 1973.

Although the best available, both series are seriously flawed, with missing observations for many state-year pairs and obviously incorrect data (potentially due to nonreporting) in others. The analysis excludes data from Alaska, Hawaii, Connecticut and Louisiana due to very poor data quality[24]. State-year pairs with fewer than 100 new admissions to prison were dropped from the analysis as well. Even after dropping these obviously flawed state-year observations, there is a large amount of

[24]Unfortunately, Louisiana and Connecticut changed their procedures in the sample period, so the elimination of these states results in the loss of two experiments. For further discussion, see Appendix F.

year to year variation in the numbers of admissions to prison that does not appear to reflect real variation. This analysis thus focuses on the nonwhite share of admissions to prison rather than the absolute numbers of prisoners admitted or number of prisoners admitted per population. Given the questionable data quality, the findings in this paper should be interpreted with some caution (although if the measurement error in the outcome variable is classical, this should result in larger standard errors but not bias the estimates). Summary statistics for the data used in this analysis appear in Table 1-2.

Data from the FBI Uniform Crime Reports (UCR) on arrests by race were obtained for 1980-1999 through the National Consortium on Violence Research.

The uneven coverage across states and over time reduces the number of law changes that can be used to identify the treatment effect from 12 to 7 in the specification without controls, and to only two states in the specification with the most comprehensive set of controls.

TABLE 2-2
Summary Statistics

Variable	Mean	Standard Deviation (overall)	
Nonwhite Share of New Admissions to Prison	0.40	0.20	832
Nonwhite Population Share	0.13	0.09	798
ln(State Population in Thousands)	15.10	1.00	833
ln(State # of Prisons per Capita)	0.571	0.68	798
Non-discretionary Concealed Handgun Law	0.33	0.47	833
ln(Income per Capita in $2000)	10.05	0.17	833
ln(Police per 1000 Capita), Lagged One Year	0.94	0.21	833
Unemployment rate	0.06	0.02	833
Nonwhite Share of Arrests	0.28	0.16	619
Beer Consumption in Gallons per Capita	23.45	4.40	833
Poverty Rate	13.26	3.94	687
AFDC generosity, Lagged 15 Years	6856.64	2716.67	570

2.5 Results

Estimates using the variation in timing of laws mandating random selection suggest that the institution of statutory requirements that lists of eligible jurors be selected at random led to a 5 to 6 percentage point drop in the nonwhite share of admissions. Pooling the data, a weighted least squares estimate of the difference-in-differences specification shows that rates of admission to prison for non-whites were lower in years following the passage of such laws, and that this finding is robust to the inclusion or exclusion of a rich set of controls (Table 2-3, columns I through IV, includes state and year fixed effects)[25]. I estimate the following specification for the nonwhite share of admissions to prison:

$$\frac{NonwhiteadmissionSit}{FotaladmissionSit} = a + p * Random\ Selection_{it} + \sum_t m * Year_t$$

$$+ \sum_i \lambda_i * State_i + Controls_{it} + e_{it}$$

where the standard set of controls include an indicator that is equal to 1 if the outcome data are not from the NCRP, the nonwhite population share, the log of the state population in thousands, the log of the number of prisons per capita lagged one year, whether or not the state has a non-discretionary concealed handgun law, the log of state income per capita in $2000, the log of police per capita lagged one year, the unemployment rate, beer consumption in gallons per capita, the poverty rate, AFDC generosity lagged 15 years, and the nonwhite share of arrests.

For the benchmark specification including state and year effects the full set of controls (column IV of Table 2-3), this finding is robust to controlling for time trends

[25]Weights reflect the total number of admissions for each state-year cell. Robust standard errors are reported, and clustered at the state level.

more flexibly by adding linear state-specific trends or region-year fixed effects (Appendix Table D). This provides confidence that the estimated treatment effect does not reflect differences in trends in adopting and nonadopting states, any differences in unobserved factors that trend linearly over time with states, or region-specific time varying unobserved factors. The estimate is stable across specifications. The estimate is also robust to weighting by state population and qualitatively robust to being estimated with OLS (Appendix Table E).

TABLE 2-3
Weighted Least Squares Differences-in-Differences Estimates
Dependent variable: Nonwhite Share of New Admissions to State Prisons

Variable	(i)	(II)	(III)	(IV)
Random Selection	-0.058**	-0.054***	-0.054***	-0.062***
	(0.022)	(0.016)	(0.012)	(0.018)
Data from Bureau of Justice Statistics Series	-0.028**	-0.002	0.016	0.000
	(0.013)	(0.056)	(0.083)	(0.000)
Nonwhite Population Share		-0.320	-0.507	-0.305
		(0.488)	(0.775)	(1.153)
InfState Population in Thousands)		-0.117**	-0.045	0.050
		(0.052)	(0.084)	(0.103)
ln(State # of Prisons per Capita), Lagged One Year		-0.012	-0.022	-0.003
		(0.029)	(0.029)	(0.028)
Non-discretionary Concealed Handgun Law		0.014	0.010	0.003
		(0.010)	(0.010)	(0.010)
ln(Income per Capita in $2000)		0.403*	0.280	0.186
		(0.212)	(0.207)	(0.214)
ln(Police per 1000 Capita), Lagged One Year		0.124*	0.125	0.058
		(0.073)	(0.076)	(0.054)
Unemployment Rate		0.256	0.270	-0.403
		(0.458)	(0.449)	(0.562)
Beer Consumption in Gallons per Capita		-0.005	-0.005	0.000
		(0.003)	(0.004)	(0.006)
Poverty Rate			-0.002	-0.002
			(0.001)	(0.002)
Nonwhite Share of Arrests			0.174**	0.150*
			(0.072)	(0.076)
AFDC generosity, Lagged 15 Years				-0.000
				(0.000)
Constant	0.299***	-2.393	-2.078	-2.437
	(0.020)	(2.408)	(2.445)	(2.081)
State and Year fixed effects	Yes	Yes	Yes	Yes
Observations	832	797	619	505
R-squared	0.900	0.953	0.961	0.973

Robust standard errors in parentheses, clustered at state level. * significant at 10%; ** significant at 5%; *** significant at 1%. Observations are weighted by total admissions to prison in each state-year cell.

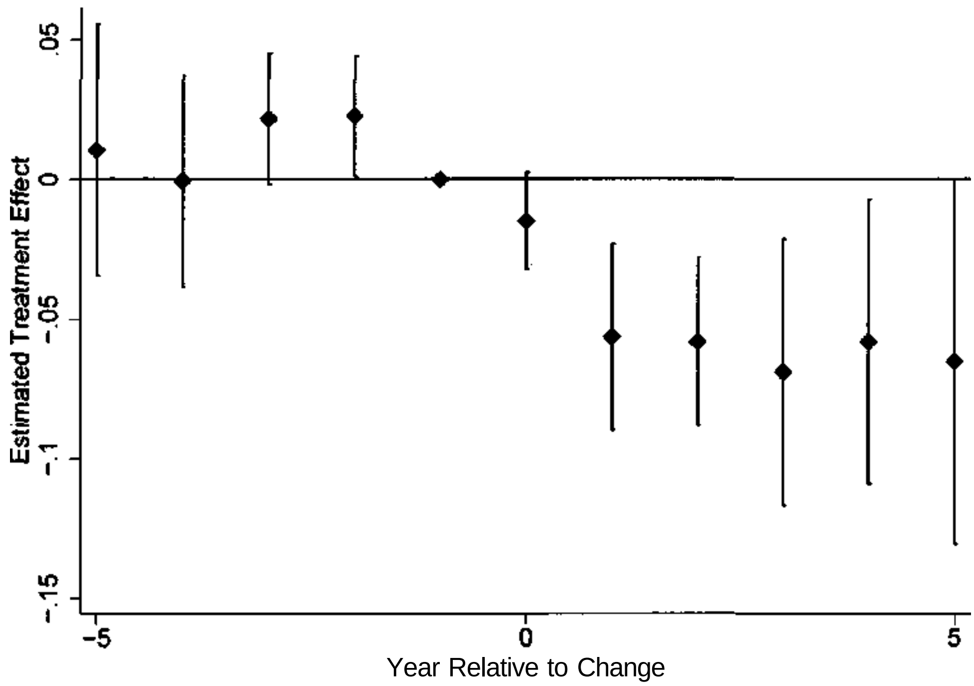
One unusual feature of the set of law changes I am able to analyze with these data is that a large number of states were treated shortly before the beginning of the dataset. If the treatment effect increases over time - for example, because imprisonment may increase the returns to future criminal activity or decrease the returns to licit economic activity - then β, the decrease relative to the average trend for the largely already treated "control" states, will underestimate the true treatment effect.

Further evidence that this estimate can be interpreted as causal is provided by examining the timing of the effect. I estimate coefficients on leads and lags of the policy change to trace out the effect of the change over time:

$$
\begin{aligned}
\frac{NonwhiteAdmissions_{it}}{TotalAdmissions_{it}} = \; & \alpha + \sum_{k=2}^{5} \delta^{k} * PreRandomSelection^{k}_{it} \\
& + \delta^{>5} * PreRandomSelection^{>5}_{it} \\
& + \sum_{k=0}^{5} \beta^{k} * PostRandomSelection^{k}_{it} \\
& + \beta^{>5} * PostRandomSelection^{>5}_{it} \\
& + \sum_{t} \gamma_t * Year_t \\
& + \sum_{i} \lambda_i * State_i + Controls_{it} + \epsilon_{it}
\end{aligned}
$$

where $PreRandomSelection^{k}_{it}$ is a dummy that takes on a value of 1 for observations that are k years preceding the passage of a random selection law, and $PostRandomSelection^{k}_{it}$ takes on a value of 1 for observations that are k years following the passage of a random selection law.

Figure 2-1 shows that there is no difference in the years leading up to the law change, but a significant decline in the nonwhite share of new admissions in the years immediately following the law change. I cannot reject the hypothesis that the leads of the law changes are jointly equal to zero (p-value = 0.32), but can reject the hypothesis that each of the lags is individually equal to zero at the 10 percent level, and the hypothesis that they are jointly equal to 0 at the 1 percent level (p-value = 0.002). Table 2-4 reports the coefficients for this regression:



: Figure 2-1: Treatment Effect by Year Relative to Law Change (Weighted Least Squares Regression, Including Full Set of Controls and State and Year Fixed Effects, 95 Percent Confidence Intervals Shown). Omitted Category is One Year Prior to Law Change.

TABLE 2-4

Dependent variable: Nonwhite Share of New Admissions to State Prisons

Variable	(i)	(ii)	(in)	(IV)
> 5 years before change	0.074***	0.067**	0.026	0.038
	(0.026)	(0.029)	(0.022)	(0.031)
5 years before change	-0.023	-0.033	-0.010	0.011
	(0.047)	(0.048)	(0.023)	(0.023)
4 years before change	-0.011	-0.004	-0.020	-0.001
	(0.034)	(0.037)	(0.023)	(0.019)
3 years before change	0.026*	0.031	0.013	0.022*
	(0.013)	(0.018)	(0.014)	(0.012)
2 years before change	-0.013	-0.015	0.013	0.023**
	(0.031)	(0.035)	(0.014)	(0.011)
1 year before change	0	0	0	0
Year of change	-0.042	-0.036	-0.013	-0.015
	(0.033)	(0.035)	(0.012)	(0.009)
1 year after change	-0.067	-0.056	-0.049***	-0.056**'
	(0.045)	(0.044)	(0.016)	(0.017)
2 years after change	-0.044	-0.037	-0.029	-0.058**'
	(0.038)	(0.036)	(0.027)	(0.015)
3 years after change	-0.084**	-0.066*	-0.083***	-0.069**'
	(0.036)	(0.038)	(0.029)	(0.024)
4 years after change	-0.031	-0.023	-0.037*	-0.058**
	(0.026)	(0.025)	(0.020)	(0.026)
5 years after change	-0.064*	-0.042	-0.069**	-0.065*
	(0.034)	(0.037)	(0.030)	(0.033)
> 5 years after change	-0.055**	-0.044**	-0.057**	-0.093**'
	(0.021)	(0.021)	(0.022)	(0.025)
Basic Controls	No	Yes	Yes	Yes
Poverty Rate and Nonwhite Arrest Share	No	No	Yes	Yes
AFDC Generosity, Lagged 15 Years	No	No	No	Yes
State and Year fixed effects	Yes	Yes	Yes	Yes
Observations	561	537	427	346
R-squared	0.925	0.928	0.946	0.960

Robust standard errors in parentheses, clustered at state level. * significant at 10%; ** significant at 5%; *** significant at 1%. Observations are weighted by total admissions to prison in each state-year cell.

The move to random selection likely resulted in more diverse jury pools on a number of dimensions including but not limited to race, such as gender, education and income. In order to test whether the observed treatment effect can be attributed to changes in the racial composition of juries rather than to these other changes, I estimate a differences-in-differences-in-differences specification which exploits the fact that the effect of the policy change should have been greater in states with a higher share of non-whites. Suppose that in every state, prior to random sampling, a very small number of non-whites would have served on juries. Random sampling would have produced a greater change in the composition of juries in those states in which African Americans comprise a larger share of the population.

The differences-in-differences-in-differences specification takes the form:

$$\frac{Nonwhiteadmissionsu}{Totaladmissions}_{it} = a + /? * RandomSelection\wedge$$
$$+7 * Random Selection\wedge * N onwhitePopulationS hare_{it}$$
$$+ y\wedge f]t * Year_t + 2\_\backslash \backslash * Statei + Controls_{it} + e_{it}$$

(Table 2-5, columns I through IV) and

$$\frac{Nonwhiteadmissionsu}{T \quad otaladmissionsit} \quad a + /3 * RandomSel\ ection_{it}$$
$$+7 * RandomS\ election\wedge * NonwhitePopulationShare_{it}$$
$$+// * RandomS\ election\wedge * N\ onwhitePopulationS\ haref_t$$
$$+ \sum_t \wedge Vt* \quad Year_t$$
$$+ 2\_]_i \backslash * Statei + Controlsu + e_{it}$$

where in the second set of specifications, the controls include the square of the non-white population share (Table 2-5, columns V through VIII).

TABLE 2-5
Weighted Least Squares Differences-in-Differences-in-Differences Estimates
Dependent variable: Nonwhite Share of New Admissions to State Prisons

Variable	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Random Selection	-0.038	-0.043	-0.042	-0.043	-0.015	0.006	0.003	0.001
	(0.031)	(0.026)	(0.036)	(0.045)	(0.036)	(0.033)	(0.030)	(0.038)
Nonwhite Population Share	-0.643	-0.207	-0.398	-0.154	-1.995	0.440	-0.409	-0.983
	(0.934)	(0.524)	(0.794)	(1.109)	(1.601)	(1.355)	(1.851)	(2.968)
Random Selection*Nonwhite Population Share	-0.161	-0.109	-0.132	-0.202	-0.532	-0.960**	-0.972**	-1.050**
	(0.215)	(0.194)	(0.337)	(0.437)	(0.550)	(0.401)	(0.406)	(0.461)
Nonwhite Population Share Squared					2.883	-2.182	-1.379	-0.102
					(2.614)	(2.484)	(2.927)	(3.894)
Random Selection*Nonwhite Population Share Squared					1.389	2.961**	3.290*	3.383*
					(1.605)	(1.237)	(1.761)	(1.797)
State and Year fixed effects								
Observations	797	797	619	505	797	797	619	505
R-squared	0945	0953	0961	0.973	0.946	0.954	0.961	0.973

Robust standard errors in parentheses, clustered at state level. * significant at 10%; ** significant at 5%; *** significant at 1%. Observations are weighted by total admissions to prison in each state-year cell.

7 and /u. are the coefficients of interest. Table 2-5 shows that states with larger non-white populations experienced larger declines of non-white admission rates to prison relative to whites than those with smaller non-white populations as a result of the switch from the keyman system to random selection, although 7 is only statistically significantly different from 0 in the specifications including the quadratic term. The magnitudes suggest that the effects were substantially larger in states with larger nonwhite population shares. The negative coefficient on the quadratic suggests a declining marginal impact in the nonwhite population share. These features should be interpreted with some caution, because of the small number of law changes involved.

This additional interaction clarifies the interpretation of the difference-in-difference results. It is clear that if nonwhite jurors were prone to convict nonwhites at a higher rate than whites in order to better enforce the law in their communities, then states with larger shares of nonwhites should have experienced increases or smaller drops in admissions to prison. In light of this result, we can rule out that story in favor of one in which juries with more whites result in higher rates of admissions to prison for non-whites - these results can be interpreted as evidence for some type of discrimination by race.

To shed light on the relative importance of the direct effect on conviction probabilities and the indirect effect through the induced changes at stages preceding trial, I test separately for an effect of the law changes on rates of admissions for nonviolent and violent crime (Table 2-6). Individuals are classified as having been admitted for a nonviolent or violent crime on the basis of the most serious charge for which they were admitted.

TABLE 2-6
Weighted Least Squares Differences-in-Differences Estimates
Dependent variable: Nonwhite Share of New Admissions to State Prisons

Variable	Admissions for Nonviolent Offenses				Admissions for Violent Offenses			
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Random Selection	-0.080***	-0.078***	-0.081***	-0.075***	-0.017	-0.025	-0.028	-0.031
	(0.022)	(0.022)	(0.023)	(0.024)	(0.020)	(0.021)	(0.021)	(0.023)
Basic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Poverty Rate and Nonwhite Arrest Share	No	No	Yes	Yes	No	No	Yes	Yes
AFDC Generosity, Lagged 15 Years	No	No	No	Yes	No	No	No	Yes
State and Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	511	511	501	501	504	504	494	494
R-squared	0.933	0.940	0.945	0.945	0.942	0.950	0.951	0.951

Robust standard errors in parentheses, clustered at state level. * significant at 10%; ** significant at 5%; *** significant at 1%. Observations are weighted by total admissions to prison in each state-year cell.

The total decline in the nonwhite share of admissions should reflect both the direct effect of the change in procedure on the conviction probability conditional on going to trial, and the indirect effect induced by this on plea bargaining and other aspects of case processing at earlier stages of the process. A larger share of violent crime cases go to jury trial, so assuming a common effect of changing racial composition on conviction probability for the two categories of crimes, the treatment effect should be larger for violent than nonviolent crimes if the direct effect dominates. However, admissions to prison for nonviolent offenses may be more elastic to changes in conviction probabilities if alternative punishments such as parole are viewed by prosecutors as better substitutes for imprisonment in cases involving nonviolent offenses than for those involving violent offenses. The estimated effect is larger and more statistically significant for nonviolent offenses, suggesting that a substantial portion of the reduction in the nonwhite share of admissions to prison may reflect the strategic response of prosecutors following a decline in the conviction probability.

Although I do not directly observe plea bargaining here, Kuziemko (2006)[26] finds evidence for an analogous effect on plea bargaining following the 1998 reinstitution of capital punishment in New York state using case-level data. She finds evidence that the ability to pursue a death sentence led to a 3 percentage point jump in the probability that a murder defendant would plead guilty, a 26 percent increase relative to baseline, as well as a 4 percentage point drop in the probability that a murder defendant would be offered a charge bargain. These are large effects given that death notices were issued in fewer than 8 percent of first degree murder cases. Her work suggests that changes in the expected severity of punishment can substantially affect plea bargaining; it seems reasonable to believe that changes in the expected probability of conviction in a jury trial would do the same.

[26] [34]

While these estimates are an improvement over the existing literature, this analysis is subject to the usual critiques of panel data analyses that exploit variation in states' policies. The most difficult critique to address is that the timing of the states' policy changes may be endogenous to the outcome of interest. If the factors that determine states' policies are additive and constant over time, then including state fixed effects removes the endogeneity problem. The addition of more flexible controls for time trends can absorb any unobserved differences that may create differential trends in states that change their policies relative to those who do not. However, if the unobserved factors change contemporaneously with the laws - if for example, the law changes were caused by improvements in attitudes towards nonwhites, or if states changed their policies anticipating a future decrease in the probability of guilt conditional on arrest for non-whites relative to whites - then the panel estimates will be biased [14].

I test whether there were contemporaneous improvements in attitudes towards nonwhites using data from the American National Election Studies. The ANES surveys have been administered nationally every two years since 1948, and in addition to questions about voter participation and politics, includes questions on issues such as race. The set of questions varies over time. Between 1976 and 1994, the ANES asked whether respondents thought that the government should enforce school integration. Between 1986 and 1998, the ANES asked respondents whether they were for affirmative action in hiring and promotion, whether they thought that blacks "had gotten less than they deserved", and whether they thought that blacks should get no special favors. These questions do not capture the aspects of attitudes towards nonwhites that would be most relevant for their treatment in court, especially those that focus on the role of government in ameliorating racial differences, but they are likely to be capture some information about prevailing attitudes about race relations .

Using a difference-in-difference specification including state and year fixed effects and robust standard errors clustered at the state level, I test whether these measures of racial attitudes changed discontinuously at the time of the passage of laws mandating random selection:

$$RaceRelationsI\ indicator\ ij_t = ot + P * RandomS\ election\ j_t + \underset{t}{\backslash.}\ Vt * Year_t$$
$$+ \underset{3}{2\_]}\ \hat{}j * Statej + ControlSij_t + t\hat{}_t$$

where here i indexes the individual in state j and survey year t, and the controls include race, gender, dummies for five income categories, age, and age squared. There is no evidence that these indicators moved in a direction favorable to nonwhites at the time of the law changes, and some evidence that they deteriorated relative to the rest of the country at those times (Table 2-7.A). These results should be regarded as only suggestive, since the ANES sample sizes in any given year are small and the survey is not designed to be representative at the state level.

Using the same specification, I test for contemporaneous changes in attitudes in a number of indicators of racial attitudes from the GSS (Table VII.B). These also provide no evidence that racial attitudes could drive both the observed changes in the racial composition of admissions to prison and the law changes.

[27]The ANES includes questions about the racial composition of coworkers, neighborhoods and friends, but unfortunately these questions were discontinued prior to the period studied in this paper.

TABLE 2-7.A
Weighted Least Squares Differences-in-Differences Estimates
Dependent variables: Indicators of Racial Attitudes From ANES

Variable	(i) Favor School Integration	(H) Favor Affirmative Action	(HI) Blacks Have Gotten Less Than Deserved	(IV) Blacks Should Get No Special Favors
Random Selection	-0.089***	0.001	0.535**	-0.087
	(0.028)	(0.069)	(0.245)	(0.054)
White	-0.228***	-0.271***	0.785***	-0.544***
	(0.025)	(0.030)	(0.107)	(0.089)
Male	-0.020	-0.001	0.058	-0.059*
	(0.012)	(0.009)	(0.034)	(0.034)
Income in 17 to 33 percentile	-0.006	-0.068***	0.050	0.003
	(0.018)	(0.018)	(0.038)	(0.055)
Income in 34 to 67 percentile	-0.052***	-0.118***	0.181***	0.000
	(0.018)	(0.021)	(0.052)	(0.046)
Income in 68 to 95 percentile	-0.061***	-0.142***	0.224***	0.099**
	(0.015)	(0.019)	(0.045)	(0.046)
Income in 96 to 100 percentile	-0.015	-0.102***	0.200***	0.299***
	(0.028)	(0.031)	(0.070)	(0.079)
Age	-0.006***	0.001	-0.006	0.015**
	(0.002)	(0.002)	(0.005)	(0.007)
Age squared	0.000**	-0.000	0.000	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.304***	0.061	2.062***	2.750***
	(0.056)	(0.088)	(0.320)	(0.170)
State and Year fixed effects	Yes	Yes	Yes	Yes
Observations	7330	7301	7601	7638
R-squared	0.071	0.126	0.101	0.082

Robust standard errors in parentheses, clustered at state level. * significant at 10%; ** significant at 5%; *** significant at 1%. Observations are weighted by total admissions to prison in each state-year cell. The outcome in column I is equal to 1 if the respondent thinks the government should ensure school integration and 0 if not. The outcome in column II is equal to 1 if the respondent is for affirmative action in hiring and promotion and 0 if not. The outcomes in columns III and IV are coded on a scale from 1 to 5, where 1 indicates Agree Strongly, 2 indicates Agree Somewhat, 3 indicates Neither Agree nor Disagree, 4 indicates Disagree Somewhat, and 5 indicates Disagree Strongly.

TABLE 2-7.B

Weighted Least Squares Differences-in-Differences Estimates

Dependent variables: Indicators of Racial Attitudes From GSS

Variable	(i) Interracial Marriage	(ii) Blacks Dinner	(in) Blacks Shouldn't Push	(IV) Favor Segregation	(V) Blacks in Neighborhood	(VI) Blacks Close By	(VII) Blacks at Home	(VIII) Favor Busing
Random Selection	0.027	0.152*	0.046	0.091	CT086*	-0.009	-0.017	-0.015
	(0.026)	(0.079)	(0.110)	(0.054)	(0.048)	(0.100)	(0.017)	(0.014)
White	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Male	0.009*	-0.039**	-0.161***	-0.039***	-0.015**	-0.017	0.021**"	0.027***
	(0.005)	(0.018)	(0.026)	(0.014)	(0.005)	(0.016)	(0.008)	(0.009)
Age	-0.002**	-0.015***	-0.018***	-0.017***	0.001	0.000	-0.000	0.011***
	(0.001)	(0.003)	(0.004)	(0.003)	(0.001)	(0.003)	(0.001)	(0.001)
Age squared	-0.000***	0.000***	0.000	0.000*	0.000**	0.000	0.000**"	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Constant	1.782***	2.862***	2.869***	3.331***	1.413***	1.265***	1.803**"	1.044***
	(0.045)	(0.142)	(0.225)	(0.111)	(0.066)	(0.242)	(0.043)	(0.049)
Income Group Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State and Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17065	4641	10261	13365	25608	9983	13628	13162
R-squared	0.177	0.083	0.161	0.140	0.100	0.027	0.064	0.064

Robust standard errors in parentheses, clustered at state level. * significant at 10%; ** significant at 5%; *** significant at 1%. Observations are weighted by total admissions to prison in each state-year cell

OS

In addition, I perform a similar analysis using the nonwhite arrest share as the outcome variable and find no evidence for an effect of random selection laws on the nonwhite arrest share (Table 2-8). To the extent that the nonwhite arrest share may be correlated with attitudes among police officers towards nonwhites or changes in the racial composition of the police force, this suggests that the estimated effect was not driven by changes in attitudes towards nonwhites at other points in the justice system.

TABLE 2-8
Weighted Least Squares Differences-in-Differences Estimates
Dependent variable: Nonwhite Share of Arrests

Variable	(I)	(II)	(III)	(IV)
Random Selection	0.006	0.001	-0.001	0.008
	(0.013)	(0.014)	(0.014)	(0.015)
Data from Bureau of Justice Statistics Series	-0.067***	0.009	0.006	0.000
	(0.012)	(0.027)	(0.024)	(0.000)
Nonwhite Population Share		-1.032	-0.938	-1.845
		(0.691)	(0.635)	(1.631)
ln(State Population in Thousands)		-0.041	-0.040	0.079
		(0.074)	(0.070)	(0.140)
ln(State # of Prisons per Capita), Lagged One Year		-0.029	-0.028	-0.033
		(0.018)	(0.017)	(0.020)
Non-discretionary Concealed Handgun Law		-0.005	-0.005	-0.008
		(0.014)	(0.014)	(0.016)
ln(Income per Capita in $2000)		0.292**	0.246*	0.054
		(0.140)	(0.123)	(0.177)
ln(Police per 1000 Capita), Lagged One Year		0.053**	0.051**	0.028
		(0.022)	(0.022)	(0.021)
Unemployment Rate		-0.119	0.021	-0.178
		(0.352)	(0.337)	(0.310)
Beer consumption in Gallons per Capita		-0.005	-0.005	-0.007
		(0.004)	(0.004)	(0.005)
Poverty Rate			-0.003*	-0.005**
			(0.002)	(0.002)
AFDC Generosity, Lagged 15 Years				-0.000
				(0.000)
Constant	0.128***	-1.708	-1.200	-0.938
	(0.028)	(1.935)	(1.728)	(2.204)
State and Year fixed effects	Yes	Yes	Yes	Yes
Observations	619	619	619	505
R-squared	0.941	0.949	0.950	0.951

Robust standard errors in parentheses, clustered at state level. * significant at 10%; ** significant at 5%; *** significant at *1%*. Observations are weighted by total admissions to prison in each state-year cell.

As an additional rough check for the endogeneity of changes in state laws, I generate an indicator for "early adopter" that is equal to 1 if the state changed its policy by 1975. I then regress (linear probability model) this on various 1975 state characteristics, including income per capita, the share of the state population that is nonwhite, arrests per 1000 population, police per 1000 population, total population, and AFDC generosity. These are shown in Table 2-9.

TABLE 2-9

Dependent variable: Indicator for Policy Change by 1975	
Nonwhite Population Share	0.544
	(0.639)
ln(State Population in Thousands)	0.158**
	(0.073)
ln(State # of Prisons per Capita), Lagged One Year	-0.145
	(0.195)
Non-discretionary Concealed Handgun Law	0.100
	(0.200)
ln(Income per Capita in $2000)	-1.179*
	(0.639)
ln(Police per 1000 Capita), Lagged One Year	0.254
	(0.517)
Unemployment Rate	-0.155
	(3.552)
Observations	50
R-squared	0.210

Standard errors in parentheses

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

2.6 Conclusion and Interpretation

I find that randomly selecting eligible jurors from publicly available lists resulted in fewer nonwhite admissions to prison in states, and some evidence that this was more pronounced in states with more nonwhites as a share of their populations. This result is consistent with a story in which juries with fewer nonwhites are more rather than less likely to convict nonwhite defendants.

This result is suggestive but not conclusive, and could be improved upon in several ways. In further work, I plan to use micro data on jury panels to test specifically for the influence of the demographic characteristics of jurors on trial outcomes.

More broadly, a limitation of the results is that they cannot distinguish between taste-based discrimination and statistical discrimination. These two theories of discrimination would have different welfare implications and may suggest differing policies. Nor can they distinguish between various models of statistical discrimination.

It is straightforward to show that discriminatory preferences could produce an elasticity of non-white convictions with respect to the composition of the pool of eligible jurors. In this case, an intervention to achieve the socially optimal incarceration rate by providing for more or less representation of nonwhites on juries may be warranted (such as the abolition of peremptory challenges, which in many states either defense or prosecution lawyers can use to dismiss jurors without cause). The socially optimal conviction rate could be a function of parameters that differ across racial groups, such as the elasticity of crime with respect to deterrents, or the probability of guilt conditional on arrest. In this case it is unclear from observing an elasticity whether the share of nonwhite jurors should be increased or decreased.

However, in spite of these limitations, the empirical evidence presented in this paper can speak to two broad areas of policy debate. First, they demonstrate that discrimination may contribute in a quantitatively significant way to the observed aggregate differences in incarceration rates for blacks and whites. While much previous research has focused on the role of race in capital cases, these account for an very small share of the total case load and observations made about capital crimes may not generalize to cases involving less serious crimes if high profile or highly emotionally charged cases involving murders inspire more biased decisionmaking than would occur in lower profile cases, or if race is seen as more salient in these types of cases than in those involving less serious offenses. Second, this analysis suggests that policies that allow for discretion at various stages of case processing may have important distributional implications or "disparate impact". These should be taken into account as well as overall social welfare considerations when considering whether policies allowing for discretion should be implemented[28].

[28]Although note that policies that limit discretion, such as mandatory sentencing laws for drug violations, may also have different implications for different demographic groups, and so "rules" are not necessarily more neutral in their treatment of race than discretionary regimes.

Appendix A: Timing of State Law Changes

State	Year of change	Statute and Act	Van Dyke (1977), *Jury Selection Procedures*	*State Court Organization, 1980*	*State Court Organization, 1993*	*State Court Organization, 1998*
		§12-16-57, §12-16-58; Acts 1978, No. 78-594	*Selection:* Discretion, exercised by a 3-member citizen jury commission *Source lists:* List of voters, tax rolls, telephone directories, etc.	*Selection:* Random selection. *Source lists:* Voter registration list, telephone directory	*Selection:* Random selection. *Source lists:* May include voter registration, drivers' license, and other lists.	*Selection:* Random selection. *Source lists:* May include voter registration, drivers' license, and other lists.
	Before 1975	§09.20.050, §09.20-060; §2.06 ch. 101, SLA 1962	*Selection:* Random selection. *Source lists:* List of actual voters, tax rolls, list of persons with trapping, hunting, and fishing licenses, sometimes drivers' lists.	*Selection:* Random selection. *Source lists:* List of actual voters, tax rolls, list of persons with trapping, hunting, and fishing licenses, sometimes drivers' lists.	*Selection:* Random selection. *Source lists:* All applicants for permanent fund income.	*Selection:* Random selection. *Source lists:* All applicants for permanent fund income.
	Before 1975		*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists, and other lists determined by Supreme Court.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists, and other lists determined by Supreme Court.
Arkan		§16-32-103; Acts 1981, No. 687	*Selection:* Discretion, exercised by a 3 to 12-member citizen jury commission appointed by a circuit judge. *Source lists:* List of registered voters.	*Selection:* Discretion, exercised by a 3 to 12-member citizen jury commission appointed by a circuit judge. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters.

State	Year of change	Statute and Act	Van Dyke (1977), *Jury Selection Procedures*	*State Court Organization, 1980*	*State Court Organization, 1993*	*State Court Organization, 1998*
	Before 1975		*Selection:* Random selection. *Source lists:* List of registered voters, supplemented in some counties with drivers' license lists.	*Selection:* Random selection. *Source lists:* List of registered voters, supplemented in some counties with drivers' license lists.	*Selection:* Random selection. *Source lists:* List of registered voters, customer mailing lists, utilities customers, and drivers' license lists.	*Selection:* Random selection. *Source lists:* May include, in addition to other lists, customer mailing lists, telephone directories, utilities customers, lists of registered voters, and drivers' license lists.
Colorado	Before 1975	§13-71-107 through §13-71-109	*Selection:* Random selection (Uniform Jury Selection Act). *Source lists:* Registered voters lists, lists of drivers' licenses, city directories.	*Selection:* Random selection (Uniform Jury Selection Act). *Source lists:* Registered voters lists, lists of drivers' licenses, city directories.	*Selection:* Random selection (Uniform Jury Selection Act). *Source lists:* Registered voters list, supplemented with drivers' license lists and city directories.	*Selection:* Random selection (Uniform Jury Selection Act). *Source lists:* Registered voters list, supplemented with drivers' license lists and city directories.
Connecticut		§51-221, §51-222	*Selection:* Discretion, exercised by town civil servants. *Source lists:* List of registered voters and city directories.	*Selection:* Discretion, exercised by town civil servants. *Source lists:* List of registered voters and city directories.	*Selection:* Discretion, exercised by town civil servants. *Source lists:* List of registered voters and drivers' license lists.	*Selection:* Discretion, exercised by town civil servants. *Source lists:* Tax rolls, lists of registered voters, and drivers' license lists.
Delaware	Before 1975	CCP §4502 through 4511	*Selection:* Random selection, but from districts that are not uniform, and favor rural over urban areas. *Source lists:* List of registered voters; Volunteers are accepted.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters, may be supplemented with other lists.	*Selection:* Random selection. *Source lists:* List of registered voters; may be supplemented with other sources.

State	Year of change	Statute and Act	Van Dyke (1977), *Jury Selection Procedures*	*State Court Organization, 1980*	*State Court Organization, 1993*	*State Court Organization, 1998*
		§40.01; Laws 1979, 79-235	*Selection:* Discretion, exercised by county commissioner or 2-member citizen jury commissions. (In Dade county and a number of other counties, random selection from voter lists. *Source lists:* No particular source.	*Selection:* Discretion, exercised by county commissioner or 2-member citizen jury commissions. (In Dade county and a number of other counties, random selection from voter lists. *Source lists:* No particular source.	*Selection:* Random selection. *Source lists:* Drivers' license lists and volunteers.	*Selection:* Random selection. *Source lists:* DHSMV database and volunteers who execute an affidavit at the office of the clerk.
Georgia	No change		*Selection:* Discretion, exercised by a 6-member jury commission appointed by a judge. *Source list:* List of registered voters, tax digest, and personal acquaintances.	*Selection:* Discretion, exercised by a 6-member jury commission appointed by a judge. *Source list:* List of registered voters, tax digest, and personal acquaintances.	*Selection:* Discretion, exercised by board of jury commissioners. *Source lists:* List of registered voters, sometimes supplemented with other sources if necessary.	*Selection:* Discretion, exercised by board of jury commissioners. *Source lists:* List of registered voters, supplemented with other sources if necessary to assure a fairly representative cross-section.
	Before 1975		*Selection:* Random selection. *Source lists:* Lists of registered voters, plus optional supplemental lists, including taxpayers' and drivers' lists.	*Selection:* Random selection. *Source lists:* Lists of registered voters, plus optional supplemental lists, including taxpayers' and drivers' lists.	*Selection:* Random selection. *Source lists:* Lists of registered voters, plus optional supplemental lists, including taxpayers' and drivers' lists.	*Selection:* Random selection. *Source lists:* Lists of registered voters, supplemented with other lists such as tax rolls and drivers' license lists.
	Before 1975	§2-206 and §2-207	*Selection:* Random selection (Uniform Jury Selection Act). *Source lists:* Registered voters' list, drivers' license lists, electric utilities' lists.	*Selection:* Random selection (Uniform Jury Selection Act). *Source lists:* Registered voters' list, drivers' license lists, electric utilities' lists, city directory, and tax rolls.	*Selection:* Random selection (Uniform Jury Selection Act). *Source lists:* Lists of registered voters, supplemented with names from other lists such as utilities' lists, tax rolls, city directories, drivers' and license lists.	*Selection:* Random selection (Uniform Jury Selection Act). *Source lists:* Lists of registered voters, supplemented with names from other lists such as utilities customer lists, tax rolls, drivers' license lists, and motor vehicle registration.

	Year of change	Statute and Act	Van Dyke (1977), *Jury Selection Procedures*	*State Court Organization, 1980*	*State Court Organization, 1993*	*State Court Organization, 1998*
	Before 1975	§305/1	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists, and Illinois Disabled Person cardholders.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists, and Illinois Disabled Person cardholders.
	Befon 1975	§33-28-5-13, §33-28-5-14	*Selection:* Random selection. *Source lists:* List of registered voters, occasionally supplemented by the tax rolls.	*Selection:* Discretion, exercised by court-appointed commissioners (random selection in Lake County). *Source lists:* List of registered voters, and tax rolls.	*Selection:* Random selection. *Source lists:* Lists of registered voters, supplemented with other lists such as utilities customers, tax rolls, city directories, drivers' license lists, etc.	*Selection:* Random selection. *Source lists:* Lists of registered voters, supplemented with other lists such as utilities customers, tax rolls, city directories, drivers' license lists, etc.
	Befon 1975		*Selection:* Random selection. *Source lists:* List of actual voters.	*Selection:* Random selection. *Source lists:* List of actual voters, city directories, and other lists.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists. May use other lists.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists. May use other lists.
	Before 1975		*Selection:* Random selection. *Source lists:* Lists of registered voters and/or state census rolls.	*Selection:* Random selection. *Source lists:* Lists of registered voters and/or state census rolls.	*Selection:* Random selection. *Source lists:* Lists of registered voters, drivers' license lists, and (sometimes) lists of state-issued nondrivers' identification cards.	*Selection:* Random selection. *Source lists:* Lists of registered voters, drivers' license lists, and (optionally) lists of state-issued nondrivers' identification cards.
Kentucky		§29A.80; L. 1976 ex s, c. 22, §12 (effective 1977)	*Selection:* Discretion, exercised by a 3-member citizen jury commission appointed by a judge. *Source lists:* List of registered voters and tax lists.	*Selection:* Random selection. *Source lists:* List of registered voters and other tax rolls.	*Selection:* Random selection. *Source lists:* List of registered voters and drivers' license lists.	*Selection:* Random selection. *Source lists:* List of registered voters and drivers' license lists.

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	Year of change	Statute and Act	Van Dyke (1977), *Jury Selection Procedures*	*State Court Organization, 1980*	*State Court Organization, 1993*	*State Court Organization, 1998*
			Selection: Discretion, exercised by a 5-member citizen jury commission appointed by a judge or the governor. *Source lists:* No particular source.	*Selection:* Discretion, exercised by a 5-member citizen jury commission appointed by a judge or the governor. *Source lists:* No particular source.	*Selection:* Discretion, exercised by a 5-member citizen jury commission. *Source lists:* No particular source.	*Selection:* Random selection. *Source lists:* List of registered voters and drivers' license lists.
	Before 1975		*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists.	*Selection:* Random selection. *Source lists:* List of registered voters, volunteers. May be supplemented with names from lists specified by the Supreme Judicial Circuit, ID card holders.
Maryland	Before 1975	§8-102 and §8-104	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters, and other lists as provided for by jury selection plan.	*Selection:* Random selection. *Source lists:* List of registered voters, and from such additional sources permitted by jury selection plans.
Massachusetts			*Selection:* Discretion, exercised by town officials and county officials followed by personal interviews. *Source lists:* Police census lists.	*Selection:* Discretion, exercised by town officials and county officials followed by personal interviews. *Source lists:* Voter registration lists, police census lists, and state census list in Middlesex county.	*Selection:* Random selection. *Source lists:* Numbered resident files.	*Selection:* Random selection. *Source lists:* Numbered resident files.
Michigan	Before 1975	§600.1310	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists, and personal identification cardholders.	*Selection:* Random selection. *Source lists:* Drivers' license lists, personal identification cardholders' lists.

State	Year of change	Statute and Act	Van Dyke (1977), *Jury Selection Procedures*	*State Court Organization, 1980*	*State Court Organization, 1993*	*State Court Organization, 1998*
		§593.31, and §593.37; Laws 1977, c. 286, § 1, §7	*Selection:* Random selection. *Source lists:* Lists of registered voters and sometimes city directory.	*Selection:* Random selection. *Source lists:* Lists of registered voters and sometimes city directory, telephone directory, drivers' license lists, utilities' lists, and welfare rolls.	*Selection:* Random selection. *Source lists:* Lists of registered voters and sometimes city directory, telephone directory, drivers' license lists, utilities' lists, and welfare rolls.	*Selection:* Random selection. *Source lists:* Lists of registered voters and drivers' license lists.
Mississippi		§13-5-1 through §13-5-12; Laws 1974, ch. 378 (effective 1975)	*Selection:* Random selection. *Source lists:* Lists of registered voters.	*Selection:* Random selection. *Source lists:* Lists of registered voters.	*Selection:* Random selection. *Source lists:* Lists of registered voters.	*Selection:* Random selection. *Source lists:* Lists of registered voters.
		Something	*Selection:* Random selection in the major cities, discretion in the less populated counties. *Source lists:* List of registered voters, tax rolls, other sources.	*Selection:* Random selection in the major cities, discretion in the less populated counties. *Source lists:* List of registered voters, tax rolls, telephone directories, other sources.	*Selection:* Random selection. *Source lists:* "One or more public records."	*Selection:* Random selection. *Source lists:* "One or more public records" plus drivers' license lists, voter registration lists on request.
			Selection: Random selection. *Source lists:* Tax rolls.	*Selection:* Random selection. *Source lists:* Voter registration lists. and tax rolls.	*Selection:* Random selection. *Source lists:* Voter registration lists.	*Selection:* Random selection. *Source lists:* Voter registration lists.
Nebraska	Before 1975		*Selection:* Random selection. *Source lists:* List of actual or registered voters.	*Selection:* Random selection. *Source lists:* List of actual or registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists.
Nevada	Before 1975		*Selection:* Random selection. *Source lists:* List of registered voters, sometimes supplemented.	*Selection:* Discretion, exercised by county commissioners or jury commissioners. *Source lists:* Voter registration list.	*Selection:* Discretion, exercised by county jury commissioners. *Source lists:* Voter registration list, other lists.	*Selection:* Discretion, exercised by county jury commissioners. *Source lists:* Voter registration list, other lists.

State	Year of change	Statute and Act	Van Dyke (1977), *Jury Selection Procedures*	*State Court Organization, 1980*	*State Court Organization, 1993*	*State Court Organization, 1998*
New Hampshire	1992	§500A; L 1992 33:1 (effective 1993)	*Selection:* Discretion, exercised by town selectmen. *Source lists:* No particular sources.	*Selection:* Discretion, exercised by town selectmen. *Source lists:* No particular sources.	*Selection:* Discretion, exercised by town selectmen. *Source lists:* "Town lists."	*Selection:* Random selection. *Source lists:* Drivers' license lists.
New Jersey	Before 1975		*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists.	*Selection:* Random selection. *Source lists:* Drivers' license lists, list of registered voters, tax rolls and homestead rebate filers.
New Mexico	Before 1975		*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists.
		§16-506; L. 1977, c. 316, §2	*Selection:* Random selection, followed by a personal interview. *Source lists:* Lists of registered voters, telephone books, tax rolls, and other sources. Volunteers.	*Selection:* Permanent jury list maintained, and supplemented by random selection, followed by a personal interview. *Source lists:* Lists of registered voters, telephone books, tax rolls, and other sources. Volunteers.	*Selection:* Random selection. *Source lists:* Lists of registered voters, telephone books, tax rolls, and other sources. Volunteers.	*Selection:* Random selection. *Source lists:* Lists of registered voters, telephone books, tax rolls, and other sources. Volunteers.
North Carolina	Before 1975	§9-2	*Selection:* Discretion, exercised by a 3-member citizen jury commission appointed by 3 local officials. *Source lists:* Lists of registered voters and tax rolls.	*Selection:* Random selection. *Source lists:* Registered voter list, utilities' lists, and tax rolls.	*Selection:* Random selection. *Source lists:* Registered voter list, drivers' license lists, and other sources "deemed to be reliable".	*Selection:* Random selection. *Source lists:* Registered voter list, drivers' license lists, and may use other sources deemed reliable.

	Year of change	Statute and Act	Van Dyke (1977), *Jury Selection Procedures*	*State Court Organization, 1980*	*State Court Orgo.niza.tion, 1993*	*State Court Organization, 1998*
North Dakota	Before 1975		*Selection:* Random selection {Uniform Jury-Selection Act). *Source lists:* Lists of actual voters and drivers' licenses.	*Selection:* Random selection (Uniform Jury Selection Act). *Source lists:* Lists of actual voters and drivers' licenses.	*Selection:* Random selection (Uniform Jury Selection Act). *Source lists:* Lists of actual voters, drivers' license list, tax rolls, utilities customers' lists, etc.	*Selection:* Random selection (Uniform Jury Selection Act). *Source lists:* Lists of actual voters, drivers' license list, tax rolls, utilities customers' lists, etc. which the Supreme Court designates.
Ohio	Before 1975		*Selection:* Random selection, followed by personal interviews conducted by a 2-member citizen jury commission, appointed by judges and representing the 2 major political parties. *Source lists:* List of registered voters.	*Selection:* Random selection, followed by personal interviews conducted by a 2-member citizen jury commission, appointed by judges and representing the 2 major political parties. *Source lists:* List of registered voters.	*Selection:* Random selection, followed by personal interviews conducted by a 2-member citizen jury commission. *Source lists:* Voter registration lists and drivers' license lists.	*Selection:* Random selection, followed by personal interviews conducted by a 2-member citizen jury commission. *Source lists:* Voter registration lists and drivers' license lists.
	No change	Title 38, §18	*Selection:* Discretion, exercised by a jury commission composed of civil servants, or — at the discretion of the presiding judge - random selection. Oklahoma and Tulsa counties select randomly. *Source lists:* List of registered voters.	*Selection:* Discretion, exercised by a jury commission composed of civil servants, or — at the discretion of the presiding judge - random selection. Oklahoma and Tulsa counties select randomly. *Source lists:* List of registered voters.	*Selection:* Discretion exercised by jury commission except in Oklahoma and Tulsa counties. *Source lists:* Drivers' license lists, and volunteers.	*Selection:* Discretion exercised by jury commission except in Oklahoma and Tulsa counties. *Source lists:* Drivers' license lists, and volunteers.

	Year of change	Statute and Act	Van Dyke (1977), *Jury Selection Procedures*	*State Court Organization, 1980*	*State Court Organization, 1993*	*State Court Organization, 1998*
Oregon	Before 1975	§10.215	*Selection:* Random selection. *Source lists:* List of registered voters. (Statute also authorizes use of tax lists).	*Selection:* Random selection. *Source lists:* List of registered voters, and city directory.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists, and other lists deemed appropriate by the Supreme Court.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists, and other lists deemed appropriate by the Supreme Court.
Pennsylvania		Ch. 45 of Judicial Code; 1980, PL 266, No. 78	*Selection:* Random selection, followed (in Philadelphia) by some personal interviews. *Source lists:* List of registered voters.	*Selection:* Random selection, followed (in Philadelphia and Allegheny counties) by some personal interviews. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:*	*Selection:* Random selection. *Source lists:* Voter registration lists. May supplement with lists such as telephone directory, city directory, tax rolls, persons participating in any state, local or federal program, school census list, and volunteers.
Rhode Island	Before 1975	§9-9-1 throug §9-9-14.1	*Selection:* Random selection, followed by personal interviews. *Source lists:* List of registered voters.	*Selection:* Random selection, followed by personal interviews. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:*	*Selection:* Random selection. *Source lists:* Voter registration lists, sometimes motor vehicle registration lists.
South Carolina		§14-7-110 through 140	*Selection:* Discretion, exercised by a jury commission composed of civil servants. *Source lists:* List of registered voters.	*Selection:* Discretion, exercised by a jury commission composed of civil servants. *Source lists:* List of registered voters.	*Selection:* Discretion, exercised by a jury commission composed of civil servants. *Source lists:* List of registered voters, drivers' license lists.	*Selection:* Discretion, exercised by a jury commission composed of civil servants. *Source lists:* List of registered voters, drivers' license lists.
South Dakota	Before 1975	§16-13-9	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters, supplemented with drivers' license lists.	*Selection:* Random selection. *Source lists:* List of registered voters, supplemented with drivers' license lists.

	Year of change	Statute and Act	Van Dyke (1977), *Jury Selection Procedures*	*State Court Organization, 1980*	*State Court Organization, 1993*	*State Court Organization, 1998*
	No change	§22-1-101	*Selection:* Discretion, exercised by a jury commission composed of civil servants. *Source lists:* Various lists.	*Selection:* Discretion, exercised by a jury commission composed of civil servants. *Source lists:* Voter registration list, city directory, utilities' lists.	*Selection:* Discretion, exercised by a jury commission composed of civil servants. *Source lists:* Tax rolls, drivers' license lists, and lists of registered voters (must be supplemented).	*Selection:* Discretion, exercised by a jury commission composed of civil servants. *Source lists:* Tax rolls, drivers' license lists, and lists of registered voters (must be supplemented).
Texa	Before 1975	§62-102	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists.
		§78-46-2; Laws 1986, ch. 119, §2, effective April 24, 1989.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Discretion, exercised by 2 court-appointed jury commissioners from the major parties. *Source lists:* Voter registration list.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists.	*Selection:* Random selection. *Source lists:* Drivers' license lists, and lists of registered voters.
	Before 1975	Jurors R.l	*Selection:* Discretion, exercised by town officials. *Source lists:* Various lists.	*Selection:* Random selection. *Source lists:* Voter registration list, telephone directory, and census.	*Selection:* Random selection. *Source lists:* Voter registration list, drivers' license lists, telephone directory, census, and "any other general source of names".	*Selection:* Random selection. *Source lists:* Voter registration list, drivers' license lists, and volunteers.
Virginia		§8.01-337; Acts 1977, c. 451	*Selection:* Discretion, exercised by a 2 to 9 member citizen jury commission, appointed by a judge. Counties can use a random selection method at the discretion of the chief judge of the circuit. *Source lists:* No particular source.	*Selection:* Random selection. *Source lists:* No particular source list.	*Selection:* Random selection. *Source lists:* Lists of registered voters, and where feasible, city directories, drivers' license lists, tax rolls, and "other such lists as approved by the Chief Judge of the Circuit."	*Selection:* Random selection. *Source lists:* Lists of registered voters, and where feasible, city directories, drivers' license lists, tax rolls, and "other such lists as approved by the Chief Judge of the Circuit."

	Year of change	Statute and Act	Van Dyke (1977), *Jury Selection Procedures*	*State Court Organization, 1980*	*State Court Organization, 1993*	*State Court Organization, 1998*
Washington	Before		*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists.
West Virginia	1986	§52-1-5; L. 1986, c. 94	*Selection:* Discretion, exercised by a 2-member citizen jury commission representing the 2 major parties. *Source lists:* No particular sources.	*Selection:* Discretion, exercised by a 2-member citizen jury commission representing the 2 major parties. *Source lists:* No particular sources.	*Selection:* Discretion, exercised by a 2-member citizen jury commission representing the 2 major parties. *Source lists:* Not less than two of the following:	*Selection:* Random selection. *Source lists:* Not less than two of the following: tax rolls, voter registration lists, drivers' license or chauffeurs' license lists.
			Selection: Random selection, followed by personal interviews conducted by a 3-member citizen jury commission. *Source lists:* List of registered voters.	*Selection:* Random selection, followed by personal interviews conducted by a 3-member citizen jury commission. *Source lists:* List of registered voters.	*Selection:* Random selection. At the option of the county board, either the Clerk of Circuit Court or a panel of 3 jury commissioners screen responses to questionnaires to determine eligibility for service. *Source lists:* Drivers' license lists. May be supplemented with other lists, including but not limited to lists of registered voters, tax rolls, utilities customers, high school graduates, and welfare lists.	*Selection:* Random selection. At the option of the county board, either the Clerk of Circuit Court or a panel of 3 jury commissioners screen responses to questionnaires to determine eligibility for service. *Source lists:* Drivers' license lists. May be supplemented with other lists, including but not limited to lists of registered voters, tax rolls, utilities customers, high school graduates, and welfare lists.

State	Year of change	Statute and Act	Van Dyke (1977), *Jury Selection Procedures*	*State Court Organization, 1980*	*State Court Organization, 1993*	*State Court Organization, 1998*
Wyoming	Before 1975		*Selection:* Random selection *Source lists:* List of registered voters.	*Select-ton:* Random selection *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters.	*Selection:* Random selection. *Source lists:* List of registered voters, drivers' license lists.

Source: State annotated statutes
"Unknown" indicates adopted random selection at unknown date
"No change" indicates retention of some discretion in 1998

Appendix B: Discussion of Imputation of Timing of State Law Changes

Information from five cross-sections detailing the juror list compilation procedures for all 50 states (in 1977, 1980, 1993, and 1998) were combined with information from the annotations to current and superseded state statutes on the years in which laws were amended and the nature of those amendments to infer the years in which the statutes governing juror list compilation were changed to both (1) specify a source for the master list, such as the list of registered voters, and (2) require the list of potential jurors to be drawn randomly from that master list.

For some states, such as Massachusetts and Pennsylvania, information on the year and nature of the policy changes was also available from secondary sources.

Appendix C: Data Sources

Admissions to Prison

Data on admissions to prisons were obtained from two sources. For years prior to 1983, data come from the Bureau of Justice Statistics' "Race of Prisoners Admitted to State and Federal Institutions in the United States, 1926-1986" (ICPSR Study No. 9165). For 1983-2002, data come from the Bureau of Justice Statistics' National Corrections Reporting Program (ICPSR Study Nos. 8363, 8918, 9276, 9402, 9450, 9849, 6141, 6272, 6400, 6823, 6881, 2194, 2448, 2613, 3029, 3339, 3671, and 4052). A small number of clear outliers were removed from the data.

AFDC caseloads

Data on AFDC cases and recipients from 1975-1996 were generously contributed by Rebecca Blank. Her documentation indicates that the 1969-80 were found in Public Assistance Statistics (HEW.) From September 1982-March 1988, they were found in Monthly Benefit Statistics, published by the U.S. Department of Health and Human Services (HHS). Quarterly Public Assistance Statistics (HHS) also published these data from 1981-93. Data for 1996 were acquired electronically from the U.S. Department of Health and Human Services. A modest amount of data cleaning was done on these numbers, typically eliminating obviously incorrect monthly reports with interpolated numbers.

Data on TANF cases for 1997-2004 were obtained from the website of the Department of Health and Human Services' Office of Family Assistance.

Arrests

Data on arrests were obtained from the Federal Bureau of Investigation's Uniform Crime Reporting program county-level datasets (ICPSR Study Nos. 8703, 8714, 9252, 9119, 9335, 9573, 9785, 6036, 6316, 6545, 6669, 6850, 2389, 2764, 2910, 3167, 3451, 3721, and 4009). The UCR Return A files are notoriously flawed so even these data should be regarded with some caution.

Population

Data on population were obtained from the Bureau of the Census, "Intercensal Estimates of the Population of Counties by Age, Sex, and Race (United States): 1970-1980" and "Revised Estimates of the Population of Counties by Age, Sex and Race [United States]: 1980-1989" (ICPSR Study Nos. 8384 and 6031). Data at the state level for 1990-1999 were obtained from the Bureau of the Census website.

Police

Data on police were taken from the Bureau of Justice Statistics' "Expenditure and Employment Data for the Criminal Justice System" series (ICPSR Study Nos. 7618, 8382, 8455, 09162, 09161, 09160, 09396, 06259, 06259, 06579, 06795, 02257, 02840, 03063, 03408, 03409, 03961, 03962, and 04365).

Unemployment, Income per Capita, and Expenditure on Family Assistance Programs

Data on unemployment, income per capita and expenditures on family assistance programs (AFDC/TANF) were obtained from the Bureau of Economic Analysis' Regional Economic Information System State Annual Tables, 1929-2004.

Attitudes

Data on attitudes towards race-related issues were obtained from the American National Election Studes (ANES) Cumulative Data Files, 1948-2002, available as ICPSR Study No. 8475.

APPENDIX TABLE D
Robustness Checks — Weighting
Dependent variable: Nonwhite Share of New Admissions to State Prisons

	Unweighted (OLS)				Weighted by State Population			
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Random Selection	-0.029*	-0.016	-0.032*	-0.040	-0.042*	-0.042*	-0.049***	-0.064**
	(0.016)	(0.015)	(0.016)	(0.029)	(0.023)	(0.024)	(0.014)	(0.024)
Basic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Poverty Rate and Nonwhite Arrest Share	No	No	Yes	Yes	No	No	Yes	Yes
AFDC Generosity, Lagged 15 Years	No	No	No	Yes	No	No	No	Yes
State and Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	832	797	619	505	832	797	619	505
R-squared	0.935	0.940	0.946	0.951	0.901	0.927	0.941	0.954

Robust standard errors in parentheses, clustered at state level. * significant at 10%; ** significant at 5%;
*** significant at 1%.

APPENDIX TABLE E
Robustness Checks — Time Trends
Dependent variable: Nonwhite Share of New Admissions to State Prisons

	State-Specific Trends				Region-Year Fixed Effects			
Variable	)		)	)	00	(VI)	(VII)	(VIII)
Random Selection	-0.002	-0.027	-0.011	-0.035***	-0.055***	-0.042**	-0.057*	-0.055*
	(0.027)	(0.023)	(0.011)	(0.012)	(0.019)	(0.019)	(0.030)	(0.027)
Basic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Poverty Rate and Nonwhite Arrest Share	No	No	Yes	Yes	No	No	Yes	Yes
AFDC Generosity, Lagged 15 Years	No	No	No	Yes	No	No	No	Yes
State and Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	832	797	619	505	832	797	619	505
R-squared	0.914	0.966	0.972	0.984	0.969	0.976	0.981	0.985

Robust standard errors in parentheses, clustered at state level. * significant at 10%; ** significant at 5%; *** significant at 1%. Observations are weighted by total admissions to prison in each state-year cell.

Chapter 3. The Return to Capital for Small Retailers in Kenya: Evidence from Inventories (with Michael R. Kremer and Jonathan Robinson)

3.1 Introduction

Standard textbook economic models suggest that the risk-adjusted rate of return should be equalized across activities within a firm. If capital markets function well, rates of return should also be equalized across firms, both within and even across countries. While it is clear that various frictions interfere with perfect equalization of rates of return across firms, it is not clear how big the departures from this benchmark are, and which departures are most important.

In addition, it is often difficult or impossible to directly measure rates of return to capital, particularly at the margin. In this paper, we take advantage of the structure of the retail industry among a subset of Kenya retailers to measure the rate of return to inventory capital. We are able to identify specific investment opportunities that are available to retail firms and directly compute the return that could be realized from these investments. The data imply very high marginal rates of return on average, and provide evidence for economically and statistically significant heterogeneity in marginal rates of return across shops.

In our first empirical strategy, we directly measure the expected rate of return to an incremental investment in inventory for small retail firms in Western Kenya. We collected detailed panel data on inventory decisions, sales, and stockouts (lost sales in which a customer asks for a product that it out of stock and does not accept a replacement) for a sample of 45 small rural retail firms in 11 towns in Western Kenya. By measuring daily stockouts over a period of several months, we are able to measure the probability that an additional unit of inventory would have been sold in a given time period, had the shopowner bought it at the beginning of the period. In this way, we are able to estimate marginal rates of return to inventory investment by calculating the expected marginal benefit from holding an additional unit of inventory (the markup multiplied by the probability that the marginal unit would sell during the relevant time period), and comparing this to the marginal cost of obtaining an additional unit (the wholesale price multiplied by the cost of financing).

We focus our analysis on cell phone top-up cards, for several reasons. First, phone cards have fixed wholesale and retail prices and negligible storage and depreciation costs, and are not substitutable across brands. Second, phone cards are kept behind the counter in the shops we survey, so lost sales can be measured. Using this approach, we find that on average, a shop in our sample could achieve a real rate of return of 113 percent to a marginal increase in inventory, and the median shop could achieve a real rate of return of 36 percent, much higher than rates of return on debt or equity in either Kenya or international markets. If lost customer goodwill or other sales of complementary goods are significant, this will be a lower bound on the rate of return.

We explore the extent to which these rates of return may reflect high rates of return to capital or behavioral anomalies by separately estimating rates of return to two different brands of phone cards, Celtel and Safaricom, for each shop in the sample. We present some preliminary tests of equalization of marginal rates of return across products within shops. On average, the rates of return for the Celtel and Safaricom products differ, although this appears to be driven by the top decile of the distribution of return. The median rates of return on these products are similar, and we find a rank correlation of 0.38 between rates of return for products of different brands.

If one treats these as estimates rather than bounds, or assumes that all these bounds are equally tight because the cost of lost goodwill and other sales is similar across shops, we can then test whether these marginal rates of return are equal across shops, and estimate the degree of heterogeneity in rates of return under some assumptions about the underlying distribution of rates of return. Using a variety of tests, we reject the hypothesis of equalization of marginal rates of return across shops, suggesting some misallocation of capital in these markets. We find evidence that the standard deviation of the population distribution of annual rates of return may be as high as 171 percent.

Second, we perform a preliminary back-of-the-envelope calculation of bounds on the rate of return to investments in inventory for a much larger population of shops in Western Kenya from a complete database of purchases from a major distributor of retail goods. We infer bounds on the rates of return to investments in inventory that could be achieved if shops shifted the timing of their purchases to take advantage of quantity discounts offered by the distributor. If shops have investment opportunities that exhibit diminishing returns at least locally, the average return on these incremental investments will be a lower bound on the marginal rate of return. Preliminary estimates using this approach suggest that the median firm has unexploited investment opportunities that would yield a real rate of return of at least 142 percent annual.

This paper contributes a novel piece of evidence to a growing empirical literature on marginal rates of return to capital. Lucas (1990)[29] famously noted that the simplest calibration exercise assuming a common aggregate production function suggests that the marginal rates of return to capital must differ dramatically between the rich and poor countries of the world. A recent paper by Caselli and Feyrer (2007)[30] argues that the aggregate country level data on capital share of income, output, capital stock, are consistent with equalization of financial marginal rates of return across countries, after accounting for payments to previously unobserved factors (such as land and natural resources) and differences in prices of investment goods across countries.

The development literature, in contrast, finds evidence for high and variable marginal rates of return to capital. The approaches in this literature are varied and creative, but in general they find annualized marginal rates of return between 30 and 1200 percent, well above typical estimates for the developed world. These studies fall roughly into three categories: revealed preference arguments, cross-sectional production function estimates, and evidence from exogenous shocks to credit access (in the form either of natural experiments due to policy changes or field experiments).

[29] [38]
[30] [17]

The first approach, as in Aleem (1990)[31], notes that marginal rates of return must exceed the high interest rates at which people and businesses are willing to borrow, but may include some borrowing to smooth consumption as well as borrowing for productive investments .

The second method, which is employed in some form by much of the existing literature (as reviewed in Banerjee and Duflo (2004)[32]), uses cross-sectional firm level accounting data to estimate production functions and infer rates of return from the estimated coefficients. These studies typically find evidence of high rates of return: Anagol and Udry (2006)[33] find an annual rate of return of 150 to 250 percent to pineapple cultivation in Ghana. However, while they provide an informative characterization of the economy, these cross-sectional estimates do not provide estimates of marginal rates of return.

Finally, the third strategy exploits natural experiments or randomized field experiments to estimate marginal returns. Banerjee and Duflo (2005)[34] examines policy shocks to directed lending in India and concludes that marginal rates of return to capital exceed 70 percent for those firms affected by the changes. Finally, de Mel, McKenzie and Woodruff (2006)[35] estimate marginal rates of return of 60 percent for microenterprises in Sri Lanka in a field experiment in which the researchers provided grants or equipment valued at approximately one third of annual profits to randomly selected entrepreneurs.

In a recent study, Anagol and Udry (2006)[36] take the elegant approach of using data on prices of used car parts of varying expected lifetimes to infer a 60 percent

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32 [9]

33[6]

34 [10]

35[21]

36 [6]

annual discount rate for taxi drivers in Ghana, although as they note, their estimate may not be directly interpretable as an estimate of the rate of return in a world with imperfect financial markets. This paper is organized as follows. Section 2 discusses the context of the small-scale retail sector in Kenya. Section 3 describes the stockout survey and data, the rates of return implied by the stockout data, and presents a framework for interpretation. Section 4 introduces the distributor data and Section 5 shows that these data imply very high marginal rates of return for a nontrivial fraction of firms. Section 6 concludes.

3.2 The Small Scale Retail Sector in Kenya

The small-scale retail sector comprises a significant share of economic activity in Kenya, particularly in rural areas. Daniels and Mead (1998) estimate that small and medium enterprises with 10 or fewer employees (not including agriculture and mineral extraction industries) comprise 12-14% of total Kenyan GDP, and that a quarter of this contribution comes from the retail trade.

We focus on a category of retail shop in Western Kenya called dukas in Kiswahili, which typically sell a relatively homogeneous set of household products such as perishable and non-perishable foodstuffs, soaps, detergents, cooking fat, sodas, phone cards, and other household items. These shops are ubiquitous in market centers and small towns in the region, and are often located adjacent to or in close proximity to several competing shops.

These enterprises are typically owner-operated, are often operated by women and those with some secondary education, and operate at a small scale. Products are kept behind a counter (and often behind a set of metal bars) and all transactions and transfers of goods are mediated through the store operator. This means that we potentially have information on stockouts, although people may see certain goods out of stock and not ask for them. Phone cards, however, are kept below the counter so that customers are unable to know that they are out of stock without inquiring with the shopkeeper. Operators deal with a number of suppliers for the different goods they sell, but typically do business with a single supplier for each type of good.

Many goods are delivered on a regular schedule. Distributors are based in larger, semi-urban towns, and deliveries are made several times a week, depending on the product. For shops that are located in or near these larger towns, it is possible to restock from the distributor immediately if a stockout occurs or is soon to occur. The firms in this study, however, are located too far from their phone card distributors to make travel for restocking profitable.

However, in some areas, shops are also able to restock certain products by purchasing from a wholesaler that is located nearby. The disadvantage of restocking from these wholesalers is that they offer a smaller discount from the retail price than do distributors.

One feature of the distribution system that complicates our analysis is that goods must be purchased in discrete order sizes. For example, cards must be purchased in packs of ten. For this reason, we calculate the expected profit from holding an additional order of ten cards rather than the return to one marginal card. Future work will explore how this discreteness may affect the analysis.

In this study, we focus our stockout analysis on top-up cards for cellular phone service and our bulk discount analysis on non-perishable food items and household goods (e.g. vegetable cooking fat, soup mix, soap, and margarine). These products differ in their typical method of distribution. For the shops in our sample, non-perishable food items are purchased either from the distributor or from a wholesaler, while phone cards are purchased exclusively from distributors.

Phone cards are a high volume commodity and carried by many shops. There are two brands of top-up cards which are specific to the major cellphone carriers in the region: Celtel and Safaricom. Each brand has several denominations of cards. Celtel cards come in 40, 100, 200, 300, 600, and 1200 Kenyan shilling (Ksh) denominations. A small number of shops also have a technology which allows them to sell cards in arbitrary denominations. Safaricom cards come in 50, 100, 250, 500, and 1000 Ksh denominations. The brands are not substitutable for each other, though there is substitutability across denominations within a brand. Because most consumers buy the smallest available denomination, there is rarely substitution across denominations in the event in which a shopowner runs out of inventory for the desired denomination.

3.3 Estimating Marginal Rates of Return from Stock-outs

3.3.1 Survey Data

The dukas in this study were recruited from a census of small retailers in 11 small towns in Western Kenya: Bumala, Funyula, Matayos, Mayoni, Nambale, Rang'ala, Sega, Sidindi, Shibale, Ugunja, and Ukwala. Shops were eligible to participate in the survey if they sold telephone cards, although a small number of businesses that sold these products but operate primarily as wholesalers were excluded from the sample. In addition, we excluded a small number of larger retail outlets (supermarkets) because they allow customers direct access to goods, so that the shopkeeper would have a difficult time observing and reporting the number of customers lost to stockouts. In total, 104 shops were eligible to participate in the survey in these 11 towns. Fifty-one shops initially refused to participate in the survey, and 8 withdrew from the survey (attributing their wish to discontinue the survey to its frequency, length and repetitiveness). After raising the compensation for participation, we recruited a larger sample of shops of an additional 106 shops to participate in the survey from August to December 2007. The overall participation rate in the expanded sample is 74 percent. Due to the political instability in Kenya, these data have not yet been entered and analyzed. Our results can only be considered valid for the subset of shops that agreed to participate in the survey. However, in order to demonstrate that rates of return are not equalized it is sufficient to show that the rate of return to a particular investment in a well-defined subset of firms differs from that in another set.

In total, the analysis to date includes data on forty-five shops which were surveyed twice weekly about a set of 33 products for a period ranging from three months to one year. The survey collected information about the number of items sold that day, the last time the shop had restocked each item, and the number of customers who had been lost to stockouts for each product.

As noted above, we define the event in which a customer comes to ask for a product that is out of stock and does not purchase a substitute to be a "stockout". Daily data on stockouts for each item were constructed by asking shopkeepers to retrospectively report stockouts for each day since the previous survey. For some products, customers may substitute to another size or brand. To account for this, shopkeepers were asked whether the last customer on each day that requested a product that was out of stock substituted to another size or brand, or left. It was quite rare for customers to substitute to other brands or sizes - substitutions were reported in fewer than 6 percent of cases. In these cases, we set the number of stockouts for that day to zero. This may bias the estimates towards zero, since customers who originally substitute from higher denomination cards to lower denomination cards may buy only one of the lower denomination cards in the event of a stockout, so that even cases in which customers substitute to other brands or denominations may result in lost sales. We plan to gather detailed information on the exact purchases made by customers who request products that are out of stock in a subset of future surveys in order to assess the extent to which this rough cut of the data accurately captures the revenues lost due to stockouts.

In addition, a subset of shops were given a detailed background survey which gathered information on the shopowner's access to savings and credit, his land, durable good and other asset holdings, transfers he had given and received, and his other sources of income. The survey also included a number of background questions such as the owner's age, sex, ethnicity, educational attainment, literacy, and the size of the owner's family. Since trade credit provided by suppliers may also potentially be an important source of financing, a separate section of this questionnaire focused on the relationship between suppliers and the retailer, especially regarding any credit provided by suppliers. Currently we have background information on only 15 shops, but are in the process of extending the sample to include all shops in the stockout survey, as well as the shops included in the distributor data analysis which will be detailed below.

Data on wholesale and retail prices for all goods were collected from the suppliers. Retail prices deviate somewhat from the prices reported by retailers for some products, but there are likely to be very few deviations in retail prices for phone cards, since the cards are printed with their value. Informal interviews with shopkeepers also indicate that deviations from the retail price are rare.

Since shops are visited by distributors at regular intervals, the relevant horizon over which shop owners decided how much inventory to hold is the interval between distributor visits. We thus aggregate the daily data to shop-product-distributor visit interval observations in order to impute the marginal rate of return. In order to construct the number of stockouts over one of these intervals, we must have data on the stockouts and on the exact dates of distributor visits. As a consequence, we drop observations that belong to an interval in which we cannot construct a complete history of stockouts; we also drop observations that fall in intervals of indeterminate length because the date of a past or future distributor visit is missing.

Table 3-1 displays summary statistics for the sample. We observe each shop for a total of 131 days on average. Stockouts are common, occurring in 10 percent of shop-product-day observations. Figure 3-1 shows the distribution of stockouts on a day for phone cards, conditional on having a positive number of stockouts that day.

TABLE 3-1: Summary Statistics, Phone Card Stockout Survey

	Mean	Variance	N
Average number of days observed (per shop)	219.9	121.3	45
Average number of phone card distributor intervals observed (per shop)	73.6	50.4	45
Average length of phone card distributor interval (days)	3.9	1.6	3311
Average number of phone card products carried	5.0	1.3	45
Average number of stockouts per month	16.6	68.9	45
Probability of purchasing positive number of cards at last distributor visit	0.87	0.34	3311

>10

o

4 6 8 10
Frequency of Stockouts per Day

: Figure 3-1: Distribution of Stockouts of Phone Cards Per Day, Conditional on Having a Positive Number of Stockouts

3.3.2 **Empirical Methods**

These data allow us to directly compute the expected rate of return to buying one incremental unit of inventory.

The net rate of return to holding an additional unit of inventory over the time between distributor visits can be expressed as:

$$\frac{(P^R - P^w)Pr\{uj > x) - ((1 - 5) + c)}{pw}$$

where r is the marginal rate of return on the inventory investment, and P^R and

P are the wholesale and retail prices, respectively, u is the number of customers who want to buy the product, x is the level of inventory, S is the rate of depreciation and c is the cost of storage. The return to holding an additional unit is just the markup multiplied by the probability the marginal good sells less depreciation <5 and the cost of storage c divided by the wholesale price. This calculation implicitly assumes that the firm values unsold cards at the wholesale price at the end of the period, less depreciation and storage costs.

Typically, it is difficult to measure rates of return, since the expected rate of return to an inventory increase depends not only on expected extra sales, but also on product depreciation, storage costs, the risk of theft, and the cross-elasticity of demand with respect to other products. For these reasons, the ideal product to study would be one for which depreciation, storage, and expected theft costs are minimal, and one which is neither a substitute nor complement for other goods sold by shops. For these reasons, we focus on top-up cards for pay-as-you-go cellular phone service, which do not depreciate and take up little storage space. If there is no depreciation and if there are no storage costs, the expression for the return reduces to:

$$r = (P^R - P^w)Pr(u > x)$$

These assumptions are approximately true for phone cards, which do not depreciate other than through inflation and are sufficiently small that the storage costs are negligible. Though theft is possible, no store in our survey reported any theft in the past year. Note that in these stores, phone cards and all other goods (with the possible exception of sodas) are typically kept behind the counter, so that customers do not have access to them unless they request the goods from the shopkeeper. Shops sell no substitutes for these goods other than top-up cards of other denominations, since the cards are specific to cell phone networks. They are unlikely to be strongly complementary to other household goods, but shops may incur some losses of sales of other products if they frequently stock out of phone cards due to a loss of reputation if customers prefer to buy all of their goods in one place. In this case, the estimates we present should be viewed as lower bounds on the actual rate of return.

Taking into account the minimum order size, the marginal rate of return to holding an additional pack of cards over the period of the investment (the interval between distributor visits) in this context is then given by:

$$rAD) = \frac{N \quad E \ \backslash minmaxN_{ijt} - N^*_j \quad 0,\ N^{min} \backslash m,\ D] \ \bullet \ (Pf - Pf)}{3 \quad ' \quad ij}$$

where Tj(D) is the marginal rate of return to the investment over the interval of length D days for shop i; P™ and P^ are the wholesale and retail prices of product j, respectively; $N*j$ is the optimal (and actual) number of units of product j in stock at the beginning of the period; N*^ is the number of customers who come to the store to buy the product (so that $minmaxN$^ — N*^$_t$, 0, N^{mm} is the number of stockouts, capped at the minimum order size); and N™^{m} is the minimum number of units in a purchase from the distributor.

If the length of distributor visit intervals were constant across shops and across time, we could directly compute the expected marginal rate of return over those intervals from our data. In practice, the distributor visit intervals vary both within and across shops. For example, if a distributor visits a shop on Tuesdays and Fridays every week, the data will consist of intervals of three days and intervals of four days.

Note that $r(D) = exp(rD — 1)$, where r is the daily interest rate. One option would be to substitute *exp(riD)* — 1 for in (1) and treat it as a moment condition, and then use a generalized method of moments approach to obtain an estimate of the daily rate of return, Tj.

Instead, we Taylor expand *r(D)* around r = 0 to obtain an estimating equation that is linear in r:

$$
\begin{aligned}
r\{D) \quad & « \quad (exp\{rD - 1)|_{r=0}) + (D \cdot exp\{rD)|_{r=0} + H.O.T. \\
& ss \quad rD
\end{aligned}
$$

Substituting this into equation (5) for *rt(D)* and rearranging, we obtain the following estimating equation:

$$minmaxN_{l3t} - N*_{jv}\ 0,\ N^{mm} = v_r \cdot I\frac{D- \quad P\text{-}j^{w}}{\_} \cdot N^{mm}ijt + e_{ijt}$$

where *e^t* is the error term.

We estimate daily marginal rates of return for each shop using OLS, Poisson, and negative binomial regressions. Our benchmark estimates are the OLS estimates, although Poisson and negative binomial estimates which take into account the count data nature of the outcome variable are also shown in Appendix Table 3-1. We then transform these to annual rates of return.

The OLS and Poisson specifications have an attractive robustness property. Viewed as quasi-maximum-likelihood (QML) estimators, both the OLS and Poisson estimators are consistent even if the distributional assumptions are wrong, as long as the model for the mean of the outcome is correct. If each shop faces a constant marginal rate of return over time, the model of the mean will be correct by construction.

The negative binomial regressions may potentially be preferred to the Poisson regressions because the Poisson regression restricts the mean and variance of the data generating process to be equal, a restriction which is clearly not satisfied in these data - the sample variance of stockouts is an order of magnitude larger than the sample mean. However, the negative binomial regressions are not robust to misspecification of the distribution, and this is a case where making the econometric model more flexible hurts robustness.

To begin to interpret these estimates, we then estimate rates of return separately for each shop and phone card brand, since the standard theory would predict that rates of return should be equalized across products.

If cards are independent of other goods or if all shops face the same reputation costs from stockouts, then we can test for and estimate heterogeneity in marginal rates of return across shops. We do so by first using standard Wald tests of equality based on both the robust covariance matrix and the bootstrapped variance-covariance matrix. However, this test is not invariant to nonlinear transformations and since it relies on asymptotic approximations, may not be appropriate given the small number of shop-product-distributor visit intervals we observe for some shops in our data. Thus we also construct a nonparametric permutation test, in the spirit of a Fisher test, to check whether the observed distribution of estimands is consistent with what we would expect to observe in a world of equalized marginal rates of return. If marginal rates of return are equal across shops, there are no unobserved components of the marginal cost of holding an extra unit and no unobserved marginal benefits that vary across shops, and there is no autocorrelation in shocks to demand for a shop, then we can view the distribution of stockouts for all shops as the empirical distribution of residual shocks to demand for all shops.

Under these assumptions, we can generate distributions of the variation in estimated interest rates that would be realized if shops in fact faced the same interest rate and thus the same distribution of residual shocks to demand. We do this by randomly assigning shop-product-distributor intervals to artificial shops, and generating simulated distributions of estimated interest rates. We then compare the actual distribution of estimated interest rates to the simulated distributions. We generate simulated distributions of the variance of the estimated interest rates, the 90-10 spread, and the Wald test statistic test and compare the statistics for the actual distribution to the simulated distributions. We calculate the probability that the observed distribution of estimated coefficients would be generated at random under the null hypothesis of equal marginal rates of return by comparing the actual statistics (variance, 90-10 spread, and the Wald test statistic) to the empirical distributions of those generated by randomly permuting the shop assignment.

This procedure is robust to some types of correlation in shocks to demand over time. For example, if shocks to demand follow an AR(1) process and shops know this, then they will adjust their expectations accordingly. As a result, the residual shocks to demand for each shop will be uncorrelated over time.

Finally, we estimate the degree of underlying heterogeneity in rates of return in the population by using a random effects model. We estimate the following model:

$$\frac{minmaxNijt \ - \ N*_{jt}, \ 0, \ N^{mm}}{f \quad \underline{DP\text{-}j^{w} \quad \bullet N^{min}ijt}} = \mathrm{r} + Hi + t_{ijt}$$

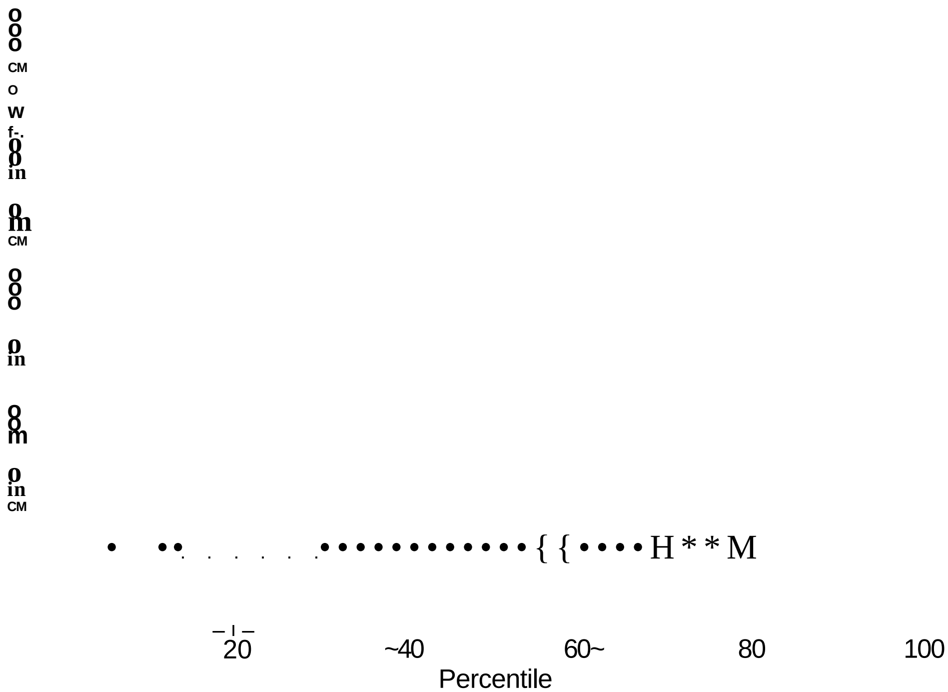
where r represents the average rate of return in the sample and \ii = (rj — r). The object of interest is the standard deviation of /ij.

3.4 Results

The preferred estimated annualized marginal rates of return fall between 0 and 1278 percent in real terms (Figures 3-2, 3-3 and 3-4). The average shop faces an annualized real marginal rate of return of 113 percent (standard error of 21 percent), and the median shop in the data faces an annualized real marginal rate of return of 36 percent. Note that some rates of return are negative, since an estimated nominal rate of return of zero would imply a negative real rate of return. Standard errors for the regressions are robust, which gives the appropriate QML standard errors, and are clustered at the shop level. Standard errors for the annual interest rates were obtained both by applying the delta method to the QML standard errors for the coefficients (Appendix Table 3-1) and by bootstrapping the coefficients (not shown), which yielded similar results.

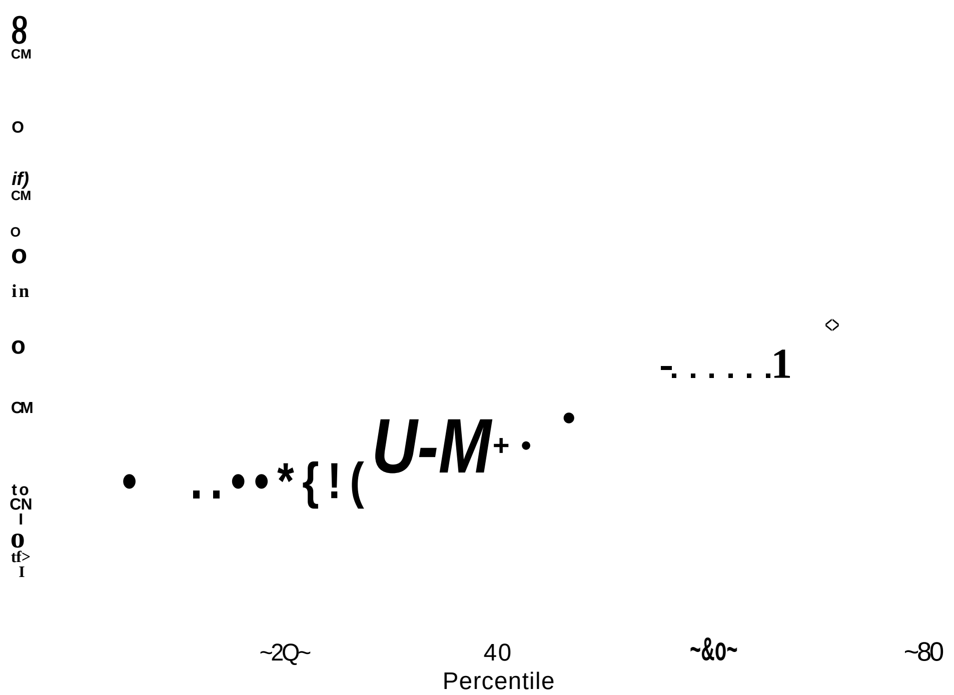
The Poisson and negative binomial regressions in general imply similar interest rates to the OLS regressions (Appendix Table 3-1). The average real rate of return across shops implied by the Poisson estimates is 105 percent, while that implied by the Negative Binomial regressions is 138 percent. The correlation between the OLS point estimates and the Poisson point estimates is 0.92, while the correlation between the OLS and Negative Binomial estimates is 0.70. This is heartening because - viewed as quasi-maximum-likelihood estimators - both the OLS and Poisson estimators should be consistent even if the distributional assumptions are wrong, as long as the model for the mean is correct.

These numbers are roughly consistent with Udry and Anagol's (2006) estimate of the rate of return for non-pineapple crops in rural Ghana, and well above other estimates of the annual rate of return to capital (de Mel, McKenzie and Woodruff, 2006; Banerjee and Duflo, 2004), although both the context and sample composition differ significantly from those studies.

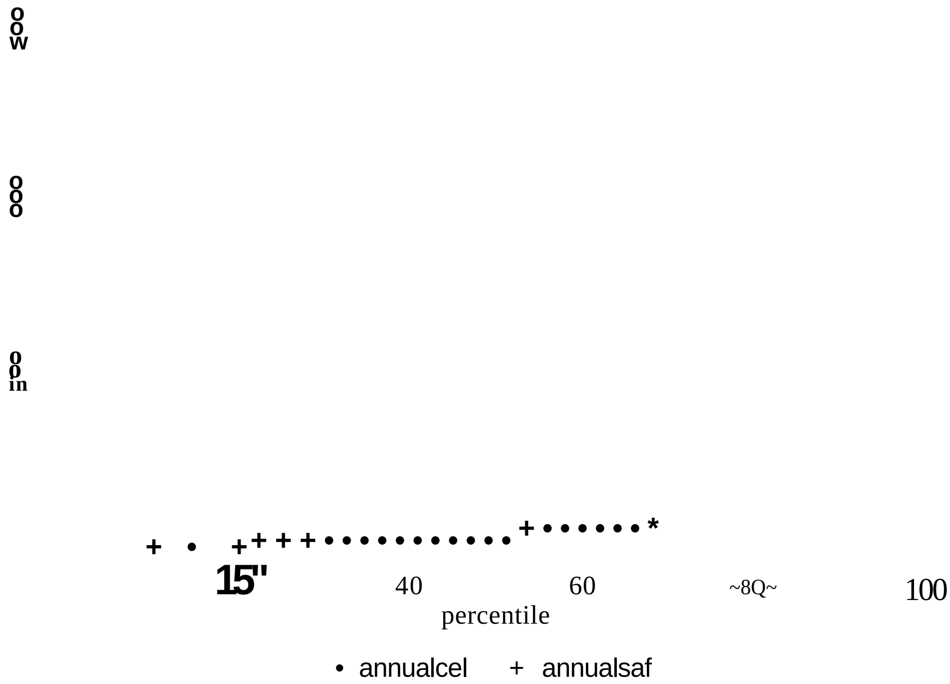


: Figure 3-2: Distribution of Estimated Interest Rates of Return from Survey Data (OLS Regression, Confidence Intervals Shown)

One possibility is that these calculations overestimate the rate of return because customers are willing to intertemporally substitute and return on a later date to purchase a card if a shop runs out of stock. However, in the context we study, such behavior on the part of consumers is not likely to be empirically relevant because there are always a number of competitors nearby (within one hundred feet) who carry the same product, and market level stockouts are rare. We plan to gather data on this



: Figure 3-3: Distribution of Estimated Interest Rates of Return from Survey Data (OLS Regression, Confidence Intervals Shown), to 75th Percentile



: Figure 3-4: Distribution of Estimated Interest Rates of Return from Survey Data by Brand (OLS Regression) directly by surveying both shopkeepers and customers.

Another possibility is that we have not properly accounted for the possibility of theft in our calculations. First, note that stolen cards can be reported to the wholesaler and refunded in the case of theft, limiting the losses to the retailer. In addition, stolen cards are identifiable by serial number (reported on the receipt) and are inactivated and rendered worthless once reported stolen, reducing the value of these goods to a potential thief. Consistent with these institutional features, theft of phone cards appears to be extremely rare.

While the probability of theft is observed to be low, it could be the case that it is increasing sufficiently sharply in inventory to explain the observed frequency of stockouts. Two features of our data suggest that a high marginal probability of theft is unlikely to explain stockouts. First, there is a large range of shop size within our sample: the largest shops in our sample carry a total value of inventory orders an order of magnitude larger than the inventory orders of the smallest shops. However, given that the probability of being robbed is very close to zero for all types of shops, even if the probability of being robbed is monotonically increasing in inventory, then the marginal increase in the probability of being robbed with respect to an increase in inventory must be low on average across the observed range of inventory. However, if shops can make investments in preventing theft, what we observe is equilibrium theft probability as a function of size. A second line of argument relies on the intertemporal variation in stock within shops. There is substantial variation in the value of inventory held by a shop over time, and both the probability of theft at times of high and low inventory are close to zero. However, within shops, the investments made in theft prevention technologies (quality locks or security guards) do not appear to adjust with the relatively high-frequency changes in inventory; thus, the effect of the marginal increase in inventory on the probability of theft must be bounded by something very close to zero, and will not substantially affect our results.

A third possibility is that these stockouts reflect collusion on the part of shopkeepers to each hold low levels of inventories, since it is clearly socially optimal for there to be shop level stock outs but not market level stock outs. The information structure makes it difficult to believe that shops jointly decide how much inventory to hold, given that shopowners do not observe each other's restocking decisions and the stochastic nature of stockouts would make it difficult to verify deviations from any agreement. Direct inquiries confirm this intuition. In addition, the skewness of the within-market distribution of rates of return suggests that shopowners do not collude to hold lower levels of inventory than they would in a decentralized equilibrium - the simplest models of collusion would suggest that all shopowners would agree to reduce inventory and thus that stockouts should be relatively evenly distributed across shops within towns, but in fact the distribution is quite skewed, with some shops frequently experiencing stockouts and others only very rarely. We plan to further explore the degree to which collusion and market structure may influence this measure of rates of return by examining the correlation between the estimated rate of return and the competitiveness of the local market, as proxied for by the number of very local competitors, for example.

These high rates of return do not appear to reflect failures to optimize driven by inattention or any other factor that would result in mean zero measurement error. However, they could be driven by behavioral anomalies that lead to difficulties in saving or systematic mistakes in setting inventory levels.

In order to begin to explore whether these high rates of return reflect behavioral anomalies or genuinely high rates of return to capital, we separately estimate rates of return implied by stockouts of Celtel and Safaricom products for each shop. For this analysis, we restrict attention to the 43 of 45 shops that carry both Celtel and

Safaricom products. We examine rates of return across products and at first glance, the average real marginal rates of return across shops for Celtel and Safaricom products look quite different at 156 and 94 percent, respectively. However, the medians of the rates of return across shops are very similar and as shown in Figure 3-5, the differences in the average rates of return are largely driven by the shops in the top decile of the distribution. The rates of return are also related within shops - the rank correlation between the rate of return on Celtel products and the rate of return on Safaricom products is 0.38. This correlation is consistent with maximization, but may reflect similar mistakes in optimization for both brands of phone cards.

In future work, we plan to run additional tests of whether rates of return reflect optimization by using a difference-in-differences strategy to look at how stockouts respond to wholesale price changes, and by testing whether apparent discrepancies in rates of return across brands are larger for those who might be expected a priori to make more mistakes, such as shopkeepers with less experience or less education.

We find evidence that not only are marginal rates of return to these inventory investments high in this population of businesses, but that they are also heterogeneous across shops. With a standard Wald test based on the robust covariance matrix or on the bootstrapped covariance matrix, we can reject the hypothesis that the estimated interest rates are equal across shops at the 1 percent level. Since the Wald test is not invariant to nonlinear transformations, we perform this test on the estimated coefficients, the daily rate of return, and the annual rate of return, and find similar results.

In addition, using the permutation test described above, we find that the standard deviation of the observed distribution of estimated coefficients falls in the 99th percentile of the simulated distribution of variances (Figure 3-6). At 229 percent, the actual standard deviation of the estimated rates of return falls far above what would be expected if the shops actually faced the same rate of return. Taken together, we interpret these tests as a strong rejection of the hypothesis that the marginal rates of return to these investments are equal for the shops in our sample.

Given that we can reject homogeneity of returns across shops, we next estimate the extent of the heterogeneity with a random effects OLS regression. Some of the variation in the distribution of fixed effects reflects sampling error, so we use a random effects model to estimate how much of this variation reflects real underlying heterogeneity in rates of return in the population of shops. We estimate that the standard deviation of the annual rate of return in the population is 171 percent.

The parametric assumption of normality in both the distribution of rates of return and the error term is almost certainly wrong, and the estimate of the extent of underlying variation changes dramatically with different assumptions about the distribution of random effects or with a random effects Poisson model. However, the conclusion that there is a large amount of underlying heterogeneity in the rates of return is qualitatively robust to the choice of specification - the estimates of the underlying real variation in rates of return are consistently large. This provides evidence for economically significant departures from the equalization of rates of return across firms that would be predicted by the standard model.

As noted above, these results on heterogeneity should be interpreted carefully, as there may be unobserved heterogeneity in the costs of stockouts (such as lost sales of other goods or reputation costs) that could explain some fraction of the differences in rates of return across shops. We plan to test for reputation costs by examining the cross-sectional relationship between the density of competitors in the immediate vicinity of the shop and the imputed rate of return, and also by using a difference-in-differences strategy to estimate the impact of entry and exit of nearby competitors on stockouts. While not interpretable as causal estimates, these correlations would provide some idea of whether reputation costs are likely to be empirically significant in this context.

One additional issue in the current set of results is that our sample included only half of the shops operating in the towns that we studied. However, our key results on both the level and variance are qualitatively robust to sample selection issues. Even making the pessimistic assumption that the nonparticipating shops have a marginal rate of return of zero, the full sample average annual rate of return would still be bounded below by 60 percent. In addition, rejecting the hypothesis of equal rates of return in the sample we do observe is sufficient to reject the hypothesis of equal rates of return in a larger sample.

3.5 Bulk Discount Analysis

3.5.1 Distributor Data

In addition to the stockout survey, we analyze sales data from a major distributor of retail goods in Western Kenya. These data contain detailed records of purchases between January 1, 2005 and January 1, 2007 for purchases that are less than 100,000 Ksh in value, a rough cutoff which excludes very large wholesalers. We observe the name of the shop, date of the purchase, the quantity purchased of each product, the unit prices, the actual prices paid, the Value Added Tax paid, and any discounts received for each purchase. The shop identifiers also include some geographic information.

During this period, the distributor supplied 160 different household goods. While

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Standard Deviation of Estimated Annual Rates of Return for Simulated Shops

: Figure 3-6: Distribution of Standard Deviation of Estimated Interest Rates of Return for Simulated Shops Under Null Hypothesis of a Common Marginal Rate of Return, Stockout Survey Data. Note: The line at 231 percent indicates the actual standard deviation of estimated rates of return.

goods such as eggs, bread, milk and a set of other household goods are distributed separately and not observed in these data, the products in our data appear to comprise a significant share of inventory for small retail shops in the region.

We restrict our analysis to shops that purchase at least 5,000 Ksh worth of goods from the distributor in the first month in the data and that make purchases over a period lasting a minimum of 8 months. There are 585 shops in the data that satisfy this requirement, although we only have sufficient data for a subset of 434 of these to perform our rough first-pass calculation of the rate of return that could be achieved by taking advantage of quantity discounts.

The average shop satisfying these inclusion rules makes 40.7 purchases in the data, and the average length of time between the first and last purchase in the data is 571.4 days. The average shop in the sample invests 20,706 Ksh ($276) per month in products sold by this distributor (although note that the distribution is skewed). Summary statistics for the data appear in Table 3-2. Shops receive a 0.5 percent discount if their total bill including VAT exceeds 5,000 Ksh, a 1 percent discount if their bill exceeds 7,000 Ksh, and a 1.5 discount if their bill exceeds 10,000 Ksh. Figure 3-7 shows some features of the data.

TABLE 3-2: Summary Statistics, Distributor Data

	Mean	Variance	N
Average number of purchases	0.67	26.68	585
Average number of days between first and last purchase in data	571.35	155.17	585
Average purchases per month, Ksh	20706.21	56053.25	585

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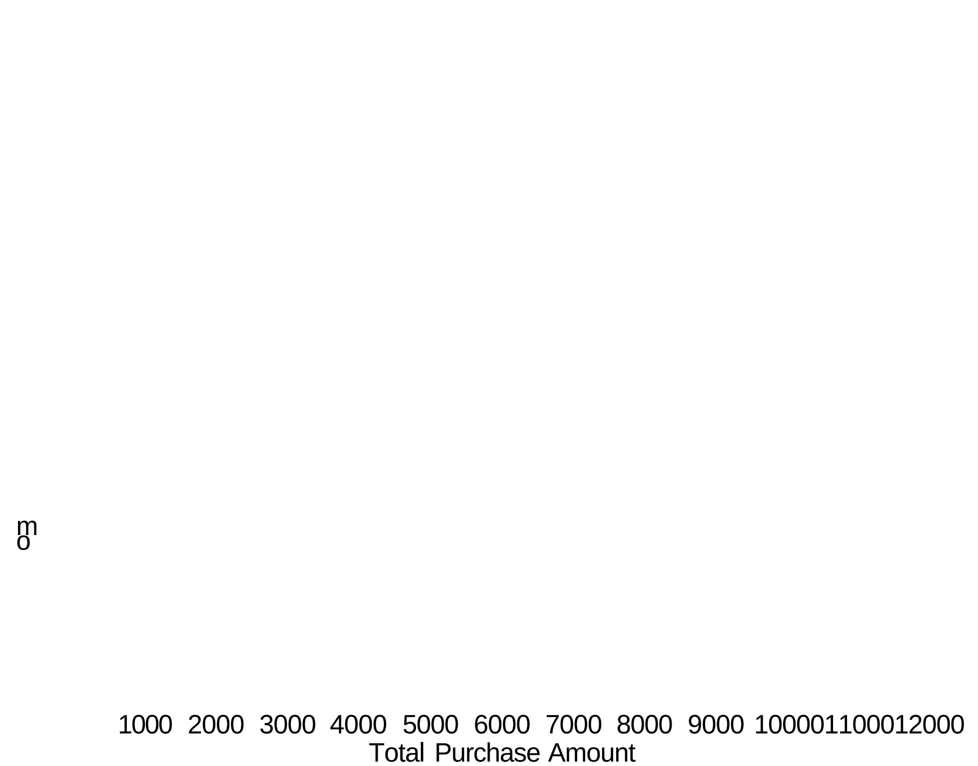
1000 2000 3000 4000 5000 6000 7000 8000 9000 100001100012000
Total Purchase Amount

: Figure 3-7: Distribution of Purchase Sizes in Distributor Data for Shops Satisfying Inclusion Criteria, January 2005 to December 2006 (Ksh)

3.5.2 **Estimates of Marginal Rates of Return from Bulk Discounts**

We use the availability of bulk discounts to infer a lower bound on the average marginal rate of return. Figure 3-8 shows that shops do respond to the availability of bulk discounts by trying to make purchases that just exceed the discount thresholds - there are bumps in the distribution at the cutoffs. However, a substantial fraction of purchases fall in the intervals just below the discount thresholds as well, and shops frequently forgo the discounts they could achieve by buying a larger quantity of goods up front.



: Figure 3-8: Distribution of Purchase Sizes in Distributor Data for Shops Satisfying Inclusion Criteria, January 2005 to December 2006 (Ksh)

We calculate the rate of return that each shop could have realized had it bought goods earlier in order to obtain the bulk discount, given that it would have been able reproduce the same sales pattern going forward as the sales pattern that is empirically realized.

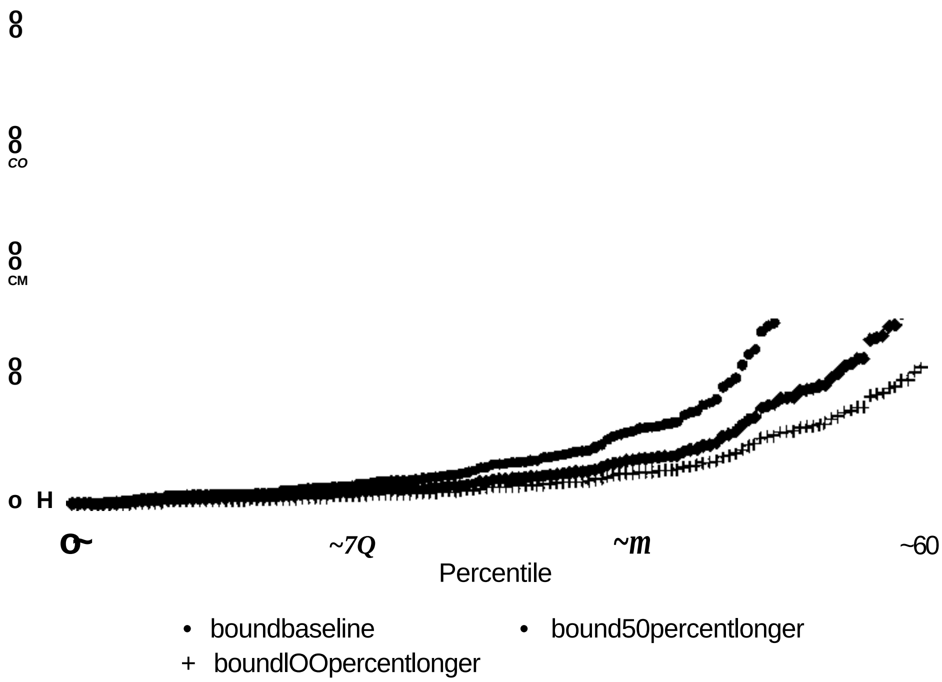
For example, suppose that a shop makes a 4,500 Ksh purchase each month. Given an interest rate of r over a period of a month, their cost of borrowing to get to 5,000 Ksh would be $500 \cdot r$. The benefit would be a discount of $0.005 \cdot 5000$. If they are not borrowing to get to the 5,000 Ksh threshold, this implies that $500 \cdot r < 0.005 \cdot 5000$, or $r > 0.05$. A 5 percent rate of return over one month would be equivalent to an annual rate of 82 percent.

Some shops have low turnover and buy very few goods from this distributor. We thus restrict our sample to shops that purchase at least 5,000 Ksh of goods in the first month they appear in the data and appear in the data for at least 8 months. These shops are generally larger than other shops, and have been in operation longer. To the extent that there are diminishing returns to scale, this sample will have lower underlying rates of return than the unrestricted sample, and the bounds we present should be regarded as lower bounds on the distribution of rates of return for the entire population of retailers. To the extent that larger shops are likely to have been in operation longer, this sample will likely exclude new shops that may not yet have learned to take advantage of the discount, for whom we might calculate a spuriously high bound for the rate of return due to a lack of information about the discounts.

We then search for the date on which they make a purchase that is closest to the next discount threshold. Using subsequent purchases, we then calculate the rate of return they could achieve by increasing the size of the purchase order to meet the next discount threshold. Using this method, we are able to bound rates of return for 434 of the 585 shops satisfying our inclusion criteria.

We find evidence that rates of return for a significant fraction of the shops we study can be bounded at extremely high levels. Figure 3-9 details the distribution of bounds on annual rates of return. For 68 percent of shops, we can bound their rates of return above 50 percent annual. For 54 percent of shops, we can bound annual rates of return above 100 percent. For 24 percent of shops, those that make purchases very close to the discount threshold in our data, we calculate bounds on annual rates of return above 1,000 percent.

There are several caveats to this analysis that should be noted. First, we calculate very high bounds on rates of return at some point in time for these shops, but average rates of return across time may be substantially lower. Second, in the current version of the analysis, we do not account for uncertainty over which products will be in demand. Shops may delay purchasing products until some of the uncertainty becomes resolved. In the medium run, we plan to account for this by analyzing the expected returns to very simple investment rules - for example, the return to increasing the purchase order by equal amounts for the three highest volume products. To the extent that shops have more information, and could have chosen a higher return bundle of goods to buy, this will be a lower bound on the rate of return they could have achieved by increasing the order to the next discount threshold. For now, we note that the finding of high rates of return is not likely to be sensitive to this modification in the calculation - making the crude assumption that shops would have taken 50 percent longer to sell the ex ante optimal product mix than the one they actually do purchase, the median shop in our data would still have a rate of return to this type of investment bounded above 76 percent annual. Under the even more conservative assumption that it would take 100 percent longer to sell off the ex ante optimal bundle, the median shop would have a rate of return bounded above 49 percent annual.



: Figure 3-9: Distribution of Lower Bounds in Distributor Data.

3.6 Conclusion

We use evidence from inventories to provide a novel look at the marginal rates of return to investments available to rural retail enterprises in developing countries. Using detailed panel data on a set of 45 retail shops, we find an average (median) marginal annualized rate of return of 113 (36) percent. With administrative data on a larger sample of shops, we find a lower bound for the marginal annualized rate of return to capital for the median shop of 142 percent.

We also find evidence for substantial heterogeneity in marginal rates of return among these shops - using several tests, we reject the hypothesis that the estimated marginal rates of return are equal across shops.

This suggests the potential gains from improving the allocation of capital may be large. The ability to realize these gains and the policy levers most conducive to doing so depend on the sources of these differences. There are of course multiple potential hypotheses about why rates of return will not be equalized, including the hypothesis that credit constraints prevent small shopkeepers from borrowing to equalize returns with the outside credit market and the hypothesis that behavioral factors limit the ability of small entrepreneurs to equalize rates of return across different items within their firms. In ongoing work we hope to be able to provide some information to help differentiate between these hypotheses by looking at rates of return on different items, comparing rates of return across shops from the "phone card" test and bounds on rates of return from the "reordering" test, and examining correlations between rates of return on inventories as we measure them and other characteristics, such as asset ownership, other sources of income, and educational attainment.

We measure the rate of return to investment in a narrow category of activities, and this is sufficient to reject the standard model. Under stronger assumptions, the rate of return we measure also provides information about the rate of return to a broader set of investments. The marginal rate of return we measure may also reflect the marginal rate of return to capital in a broad swath of rural economic activities if the individuals we study (or the households to which they belong) are diversified and allocate their working capital across a set of productive activities (such as farming, raising poultry, etc). Diversification has important implications for the interpretation of the estimand not only for this reason, but also because if these shopowners are diversified, it may be possible to interpret this rate of return as the social marginal rate of return rather than just the private rate of return. Aggregate stockouts in these market towns are rare and there are typically many shops selling the same goods, so in the context of rural retail shops, the social return to financing the purchase of an additional unit of inventory by any one shop may be close to zero - if a customer finds that one shop has stocked out of a particular product, he will buy from a competitor. However, if shopowners are diversified and participate in a variety of productive activities including some that do not exhibit this zero-sum feature, then the marginal rate of return we measure may reflect the social marginal rate of return to capital as well as the private return.

APPENDIX TABLE 3-1: Annual Real Marginal Rates of Return by Shop, Stockout Survey Data

Shop	OLS	Poisson	Negative Binomial	N
	-0.11	0.36	-1.14	–
	(5.47)	(2.40)	(1.60)	
	-8.58***	-6.76***	-6.40**	77
	(0.50)	(2.19)	(2.61)	
	268.03***	573.49***	1781.69*	30
	(29.34)	(189.81)	(921.19)	
	590.71**	226.10***	286.86**	25
	(277.92)	(73.14)	(116.64)	
	36.29**	93.29***	109.73*	15
	(15.15)	(8.95)	(56.23)	
	5.28	15.63	10.74	64
	(15.51)	(24.28)	(18.61)	
	431.36***	423.33**	327.41**	38
	(91.36)	(192.75)	(138.44)	
	124.75	72.24***	66.09**	149
	(86.18)	(25.79)	(33.63)	
	41.41***	25.47**	18.08	72
	(7.95)	(10.79)	(11.87)	
10	187.65***	174.59***	159.42***	32
	(58.85)	(42.11)	(51.06)	
11	161.72****	174.45**	196.80*	15
	(15.78)	(83.80)	(110.19)	
12	30.76***	31.43***	23.86***	119
	(2.86)	(5.94)	(6.30)	
13	591.52	342.31**	722.93	31
	(382.45)	(147.33)	(661.11)	
14	15.10	36.90	43.81	91
	(11.61)	(25.71)	(28.11)	

15	131.87*** (21.06)	140.48*** (29.84)	139.48*** (34.48)	156
16	77 11*** (22.52)	73.05 (51.69)	79.48 (51.69)	139
17	54.21*** (16.03)	50.72** (19.97)	44.60*** (15.33)	179
18	16.45*** (6.22)	3.36 (7.47)	-1.234 (5.40)	113
19	1.66 (9.07)	29.56** (11.70)	24.13* (13.23)	94
20	15.89 (20.98)	-2.72 (2.53)	-3.16 (2.23)	170
21	69.56* (40.13)	49.56** (16.78)	44.71** (21.34)	157
22	51.63*** (15.53)	67.93*** (18.02)	55.65*** (14.73)	106
23	1.12 (7.22)	20.35 (18.06)	23.43 (18.50)	49
24	-4.15 (3.51)	8.23 (8.84)	8.04 (8.19)	141
25	-8.73*** (0.26)	-3.59 (3.89)	-0.84 (6.44)	157
26	57.51*** (15.17)	74.37*** (23.44)	89.11*** (29.64)	34
27	25.94* (14.00)	43.21** (19.93)	39.27** (19.04)	77
28	-9.00*** (0.00)	-9.00 (0.00)	-9.00*** (0.00)	29
29	419.84*** (28.87)	419.14*** (81.46)	382.75*** (84.72)	62

30	99.52** (44.01)	103.95*** (27.90)	67.65*** (15.17)	32
31	79.46*** (21.60)	133.60*** (31.24)	151.83*** (36.73)	14
32	8.23 (10.21)	18.99 (12.99)	35.18 (36.67)	48
33	3.11 (15.38)	15.70 (22.94)	14.03 (26.62)	56
34	52.48 (39.35)	153.85** (63.89)	128.49** (58.94)	46
35	5.16 (9.00)	1.79 (3.83)	1.65 (2.50)	30
36	51.08 (46.90)	-3.61 (6.59)	-3.45 (6.63)	29
37	3.43 (12.37)	27.70* (15.42)	17.60 (11.62)	19
38	-9.00*** (0.00)	-9.000*** (0.00)	-9.000 (0.00)	35
39	-9.00*** (0.00)	-9.00*** (0.00)	-9.00*** (0.00)	78
40	-0.56 (4.11)	3.59 (6.64)	2.22 (5.58)	81
41	-9.00*** (0.00)	-9.00*** (0.00)	-9.00*** (0.00)	80
42	61.85*** (8.25)	46.23*** (9.30)	33.39*** (9.14)	72
43	62.55 (66.56)	63.34 (56.44)	58.23 (55.31)	46
44	.278.27*** (294.41)	1095.73** (509.47)	1119.18 (680.42)	30

45	44.63***	15.13	11.04	22
	(13.85)	(20.28)	(15.89)	
Average for all shops	121.26***	113.63***	146.84***	3311
	(20.92)	(12.40)	(27.89)	

Robust standard errors clustered at the shop level are reported.
Bootstrapped standard errors yield comparable confidence intervals (not shown).
* significant at 10%; ** significant at 5%; *** significant at 1%.

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