Essays in Development Economics

by

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Abstract

Chapter 1 looks at the empirical estimation of the welfare impacts of bargaining. Bargaining for retail goods is common in developing countries, but rare in the developed world. The welfare implications of this difference are theoretically ambiguous—if bargaining is a low cost form of price discrimination, it may lead to greater trade and welfare and even approximate the optimal incentive compatible outcome. However, if bargaining imposes large utility costs on the participants, then a fixed price may be preferable. I develop the tools to resolve this question, specifying a model of repeated trade with asymmetric information adapted to the context of bargaining, and developing a dynamic structural estimation technique to infer the structural parameters of the market. I then apply these techniques to the market for local autorickshaw transportation in Jaipur, India, using data I collected over 2008-2009.

Chapter 2 carries out the first comparison of production function parameters estimated by structural techniques with those estimated via randomized instrumental variables using a unique dataset and field experiment performed by De Mel, McKenzie, and Woodruff (2008). In the context of a simple model of a household firm, I discuss the coefficients that each approach estimates, and the assumptions necessary to interpret those coefficients as the structural parameters of the model. I find that the values of structural and experimental estimators that most plausibly estimate the same parameters are indeed statistically and economically similar, suggesting that in some contexts structural models of production functions may be effective in recovering the parameters of production functions in the context of developing markets. These parameters may then be used to address questions relating to firm productivity and capital allocation that are both central to the study of firms in development, and potentially difficult to identify using randomized variation alone.

Chapter 3 documents an attempt to overcome the challenges of police reform in the Indian state of Rajasthan, evaluated through a series of RCT (Randomized Control Trials). Four reform interventions were implemented in a randomly selected group of 162 police stations across 11 districts of the state: (1) weekly duty rosters with a guaranteed rotating day off per week; (2) a freeze on transfers of police staff; (3) in-service training to update skills; and (4) placing community observers in police stations. To evaluate these reforms, data was collected through two rounds of surveys (before and after the intervention) including police interviews, decoy visits to police stations, and a large scale crime survey—the first of its kind in India. The results suggest that two of the interventions, the freeze on transfers and the training, do have the potential to improve the police effectiveness and public image. The other reforms showed no robust effects, an outcome that may be due to their incomplete implementation.

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Finally, I owe my faith in the possibility of improving the world through a greater understanding of the social sciences to my parents. They have inspired me with their own example, and lovingly supported me through the long path at led to this research.

Contents

1 Introduction

A defining feature of developing societies is the informality of their markets. Individuals, firms, and governments frequently lack the ability or the incentives to enforce contracts and commit to fixed trading rules. In the name of development, governments and aid agencies have often pushed to formalize these markets, bringing them closer to those in the developed world. Yet little is known of the actual costs or benefits of informal market institutions. This paper examines a particularly widespread case: bargaining for retail goods. It develops the theoretical and empirical tools to compare bargaining to a counterfactual formal market mechanism–fixed prices–and applies these techniques to the specific case of the market for autorickshaws in Jaipur. More broadly, the choice to promote fixed prices versus bargaining is one that is faced by governments and firms in many contexts both in and out of emerging markets. The techniques developed in this paper allow us to analyze and inform this decision, and open the path to a broader study of the shift to more formal market structures in the process of development.

Bargaining is a ubiquitous feature of markets in the developing world, yet little is known about its implications for the efficient functioning of markets and social welfare. Is bargaining a drag on the economies of developing countries, imposing high transactions costs and reducing trade? Or is it an efficient means of bilateral price discrimination, allowing gains from trade to occur between buyers and sellers who would never have participated in a fixed-cost market? Economic theory suggests that the welfare effects are a priori ambiguous: If the right equilibrium is played, bargaining has the potential to yield as much surplus as the best incentive compatible mechanism. Ausubel, Cramton, and Deneckere (2002) show that, for buyer and seller distributions with monotone hazard rates, any ex-ante efficient division of the gains from trade is implementable in an alternating offer bargaining game. However, there is no guarantee that the equilibrium actually being played will yield an efficient outcome. If bargaining imposes high costs and traders are homogeneous, then a fixed price may generate greater overall surplus. Furthermore, the selection of the equilibrium mechanism may be influenced by factors not determined by the market such as the penalties to deviation. For instance, taxis use fixed meter rates in American cities partly because policy makers have chosen and enforced a fixed price, thus making any attempt to deviate by either passengers or drivers costly in terms of transactions costs and possible fines. In order to evaluate the costs and benefits of mandating a fixed price (or any other mechanism), the policy maker must know the structural parameters of the market: the costs of bargaining, and the distribution of valuations of the players.

This paper carries out the estimation of these parameters and subsequent policy evaluation, including the first structural analysis of a two-sided incomplete information bargaining model in the literature. I use data collected on series of individual bargaining offers and bargainers' decisions to purchase a ride or walk away from the autorickshaw (local transportation) market in Jaipur, India. The data combines an audit survey with an experimental approach. Some observations were collected from bargaining sessions using surveyors who were the full residual claimants of any financial gains from bargaining and could spend as much time as they wanted searching for a good price. These observations serve to identify the range and probability of the counter-offers that bargainers anticipate after each offer. Another set of observations were collected in which surveyors posing as potential passengers gave randomized counter-offers to the drivers' offers. These observations expand the range of the observed data, and provide a useful test that drivers' counter-offers are not influenced by unobserved signals. This data collection strategy allowed for a much greater degree of control and homogeneity on unobservables than would have been possible using purely observational data.

Using the set of bargaining sequences collected in these data, I back out the player's valuations and bargaining disutility, and consider the counterfactual policy of giving drivers and passengers the option to pay a fixed price for an autorickshaw ride instead of entering the bargaining market. This models the creation of a pre-paid taxi stand- an institution that already exists at certain airports or taxi stands in India. Choosing a per-kilometer meter rate that would set drivers ex-ante indifferent between taking passengers by bargaining or fixed rates, I find that introducing the option of the fixed price raises welfare by 28% due to high value, high bargaining disutility passengers switching to the fixed price mechanism. However, even at the optimal price for passengers (holding drivers indifferent) 63% of the buyers in my sample would remain in the bargaining market, suggesting that bargaining may still hold substantial benefits for large segments of the population.

In contrast to the auctions literature, where structural estimation of valuations is common, structural models of bargaining are rarely specified for two major reasons: First, the data required on individual bargaining offers are not frequently collected. Second, even if the data were available, the recovery of structural parameters from bargaining offers poses several challenges: Bargaining is an inherently dynamic process–for instance a low-ball early offer by the buyer will affect the course of the whole bargaining interaction. Any estimation must incorporate these dynamic considerations, and techniques for the analysis of dynamic games have become available only recently (Aguirregabiria and Mira 2010). Furthermore, the players' valuations of the good, and their expectations of their opponent's valuations, are crucial to the outcome of the bargaining but unobserved to the econometrician. The presence of these unobserved state variables makes many of the standard methods of estimating dynamic games inapplicable. Finally, unlike similar auctions problems, there exists no canonical closed form bargaining model that incorporates a realistic set of market features, and those that do exist and contain sufficient richness to approximate real-world bargaining often feature multiple equilibria.

To address these challenges I employ an estimation strategy motivated by Bajari, Benkard, and Levin (2007) and Pakes, Ostrovsky, and Berry (2007)'s approaches to dynamic games. Without solving for a game theoretical equilibrium of the bargaining game, I use the data on bargaining sequences to estimate players' beliefs on their opponents' strategies (e.g. the probability of accepting an offer, or making any of a discrete set of counter-offers, or exiting) at every bargaining round. Using these action probabilities, I can, through backwards induction, calculate a bargainer's expected payoffs for any action conditional on the agent's true cost or value of the good, and his or her costs of bargaining. These payoffs in turn imply the player's best responses, and yield predictions for the actions taken at each state of the game. Finally, for each buyer (seller) observed in the data, I estimate the set of valuations (costs) and bargaining disutility that rationalize the observed series of offers or exit/accept decisions actually taken by that individual. The outcome is a fully non-parametric estimate of the players' distributions of values (costs) and bargaining disutility.

The source of the data, and the context of the models presented in this paper is the market for intracity transportation by autorickshaw (a type of mini-taxi) in India. While the market and bargaining game are discussed in further detail in section 5, it may be useful to summarize the procedure of bargaining for an autorickshaw ride in order to provide a motivating example for the theory and empirics to follow. Potential passengers make an initial decision of whether to take an autorickshaw ride or not. If they elect to participate in the market, they stand by the side of the road and hail passing autorickshaws. The driver stops and demands where the passenger would like to go. Upon learning the destination, the driver makes an initial offer, and buyer and seller exchange offers at approximately 20 second intervals. The bargaining terminates when one party accepts the other's last offer, or exits the bargaining[1]. If either party exits from the negotiations,

[1] In the data, passengers who are residual claimants unilaterally exit the bargaining only 11% of the time, perhaps because the driver can usually catch up to a passenger in order to make a final offer while the passenger is walking

the passenger waits to hail another autorickshaw and the process repeats. The trips in question are short (2-8 km.), with a cost of between 30 and 80 rupees ($\approx$\$.75-2.00). Bargaining interactions are fairly quick–from 2 to 9 rounds, and passengers rarely encounter the same driver twice. All participants have extensive experience in bargaining.

Section 2 summarizes the various literatures upon which this paper draws for its theoretical and empirical components.

Section 3 specifies the economy in which the bargaining and trade of goods takes place. I consider a market in which buyers are randomly matched with sellers whose valuations they do not know. Whether trade occurs, and at what cost to the market participants, depends on the bilateral mechanism used to allocate the goods; I model the payoffs from bargaining or fixed prices.

Section 4 describes the estimation of the structural parameters necessary to evaluate the welfare implications of the different mechanisms.

Section 5 presents the data and results of the estimation. Reduced form specifications, although confounded by various types of selection bias, provide suggestive evidence regarding the bargaining game and the trade-offs faced by the players when considering making an offer. The structural estimation yield bounds on the distributions of valuations for buyers and sellers.

Finally, section 6 combines the results of the theory from section 3 with the parameters of section 5 and evaluates the welfare consequences of bargaining versus fixed prices. Initially I focus on the supply side of the market, estimating the fraction of drivers who would be willing to provide rides at the official, government sanctioned meter rate and at another rate proposed by some of the drivers themselves. Then, using the valuations estimated for the surveyors to represent the buyer population, I investigate the counterfactual policy of introducing the option to purchase an autorickshaw ride for a fixed price without bargaining, and derive its welfare implications.

2 Literature Review

This paper builds on a wide variety of existing literature: the theoretical framework is drawn from the dynamic mechanism design literature, combined with a specification of the extensive form and payoff functions from the game theoretical literature on bargaining, and an estimation technique adapted from the dynamic structural games literature. Here I review the sources from which I have adapted specifications and results, and finally highlight the areas in which my approach differs from the few previous structural approaches to bargaining.

Two subfields of the market design literature examine the welfare implications of alternative market mechanisms in economic environments with repeated bilateral trade. The first, typified by papers by Rubinstein and Wolinsky (1985), Fraja and Sakovics (2001), and Satterthwaite and Shneyerov (2007), focuses on the relationship between equilibrium prices and amounts of trade and the transactions costs for the traders. These papers consider an economic environment similar to mine, in which traders have imperfect information on each others' valuations, incur some utility cost from each interaction, and participate in the market over several periods. However, their focus is on the conditions under which decentralized markets converge to a Walrasian single price and often abstracts away the details of the specific bilateral mechanism. A second literature, originating in the classic paper by Myerson and Satterthwaite (1983) focuses on the efficiency, or lack thereof, of specific mechanisms for bilateral trade. In more recent work, Athey and Miller (2007) consider an economy similar to that examined in this paper, where buyers and sellers draw new valuations each period, and continue to participate in the market once they have completed a trade. These papers

away, whereas passengers cannot make a final offer to the driver after he has driven away. See table 3 for complete data on exit/accept probabilities at each bargaining round.

resemble this study in their focus on evaluating the efficiency of alternative market mechanisms, with the distinction that the mechanisms considered in this paper, bargaining and fixed prices, are chosen for their empirical relevance rather than their theoretical efficiency.

The bargaining mechanism itself is the subject of a vast literature. Starting with Rubinstein's canonical model of alternating offers bargaining with full information (Rubinstein 1982), a substantial body of work on bargaining with two-sided imperfect information has developed, most recently reviewed by Ausubel, Cramton, and Deneckere (2002). Unfortunately, this literature has not generated a canonical model comparable to Rubinstein's, perhaps due to the multiplicity of equilibria in models with sufficient detail to be realistic. Another ambiguity in the imperfect information bargaining literature is how to model the uncertainty in players' payoff functions. While the majority of research assumes that players are uncertain as to each other's valuations for the good, a separate stream of the literature (Rubinstein 1985, Bikhchandani 1992) assumes that the uncertainty pertains to the opponent's discount factor, while yet another models the possibility that the opponent might be an irrational type (Abreu and Gul 2000). Finally, although most research has focused on a multiplicative discount factor, bargaining could also be modeled as having a fixed per-round transactions cost as in Rubinstein (1982). Perry (1986)shows that this formulation will lead to immediate agreement unless players are uncertain of their opponent's fixed bargaining cost.

Despite the lack of consensus on a realistic model of incomplete information bargaining, two main strands of literature on empirical bargaining have emerged, one concentrating on bargaining experiments in the lab and another (substantially smaller) analyzing the outcomes of real-world bargaining. Lab experiments on bargaining, as surveyed by Roth (1995) set up the environment assumed by a specific model of bargaining, and test whether the outcomes of the bargaining performed by study participants match the predictions of the model. The results of studies in which players have full or partial knowledge of each other's valuations have generally rejected the quantitative and often also the qualitative implications of game theoretic models of bargaining, often in favor of fairness norms such as a 50-50 division of the gains from trade. Fairness effects should be much less important in experiments with two-sided incomplete information, however these are almost nonexistent. The only study, Yan and Lu (2008), tests the model of Abreu and Gul (2000) and finds preliminary support for it. As many authors have noted, bargaining with incomplete information is an area under-researched in the lab.

Empirical studies of bargaining are also remarkably rare. In contrast to the highly model-driven experimental literature, the majority of studies of real world bargaining compare reduced form outcomes of empirical bargaining with the broad implications of game theoretic bargaining. Several papers have examined bargaining in retail transactions. Morton, Zettelmeyer, and Silva-Risso (2004) compare survey data on buyer characteristics and price/purchase outcomes from car dealerships, and find that buyer characteristics are correlated with outcomes in ways predicted by bargaining theory, for instance that better informed, more patient buyers pay less. Ayers and Siegelman (1995) use an audit survey methodology similar to my own to test for discrimination against blacks and women in bargaining for cars. The only work to examine the actual series of bargaining offers as well as the only study of bargaining in developing countries of which I am aware is by Jaleta and Gardebroek (2007), who examine the effect of buyer and seller characteristics on the spread between buyer and seller initial offers and the amount by which the seller is willing to decrease his initial offer.

The work most similar to this study in its approach is the series of papers by Sieg (2000), Watanabe (2004), and Merlo, Ortalo-Magne, and Rust (2008)that use bargaining data to estimate structural models of medical malpractice legislation and the housing market. All three authors explicitly model the bargaining process as a game with a specific, unique equilibrium, and then use a maximum-likelihood or simulated moment-based approach to infer structural parameters of the particular model specified. In particular, Merlo, Ortalo-Magne, and Rust (2008) consider a strategy similar to mine of using bidding functions estimated from the data, but ultimately prefer a more structural model of bidding due to endogeneity concerns. Endogeneity is less of a concern in my estimation due to the presence of data generated by randomized surveyor bids, which can be used to test for the endogeneity of driver's bids with respect to the passenger's previous offer.

This study differs from previous work in several ways. First, while my approach is structural, I do not claim to know which equilibrium of a specific game theoretic model of bargaining is being played in the market, nor do I explicitly specify the process of Bayesian updating that the players carry out during the game. Instead, I estimate the best response probabilities from whatever equilibrium is being played in the data, and use these estimated probabilities, the structural payoff functions and the extensive form of the bargaining game to infer the structural parameters of the bargaining model. Second, while other studies have examined the effect of observable signals (such as race or gender) on bargaining, I focus on a market in which, arguably, the most important aspects of preference heterogeneity are unobservable. Third, I maintain the assumption that the traders are fully rational. While there is strong evidence to the contrary from lab experiments, it is unclear to what degree this would apply to real-world bargaining[2], and the assumption of rationality seems to be an appropriate starting place. In future work I plan to examine various behavioral or rule-of-thumb strategies using the same data.

3 Bargaining vs. Fixed Prices: Theory

The autorickshaw transport market falls into a class of markets in which professional sellers sell multiple types of goods to casual buyers interested in purchasing only one specific item. Other examples include a shopkeeper selling many kinds of products to consumers who are each shopping for one special product, or a consultant providing different services to different clients. Each buyer (henceforth referred to using the feminine pronoun) has a permanent valuation v for the good she wants to purchase, and this valuation is known to her before she encounters a seller. Sellers, because they sell a different good to each buyer, have a buyer-specific cost c, that is only known to the seller once he meets the buyer. Thus buyers keep the same valuations across negotiations with different sellers, while sellers draw new valuations for each buyer they encounter.

Buyers and sellers have different future outcomes if trade occurs. Sellers are professional traders, and always return to the market and begin negotiations with a fresh buyer at the conclusion of a successful or unsuccessful trade. Buyers, on the other hand, are temporary participants in the market. Upon completion of a successful sale, they exit the market permanently. However, if negotiations with a given seller fail to result in trade, the buyer seeks out another seller.

At the beginning of the game, sellers decide once and for all whether to enter the market by comparing the expected returns to the trader profession with some exogenous outside option. In contrast, a new round of buyers enters the market each period to replace those that have traded in the past period and exited the market. Entering buyers first decide whether to participate or not in the market; if not they exit and are replaced next period. Buyers who do elect to participate remain in the market until they have successfully completed a trade. Due to the stationarity of the matching process outline below, once they have entered the market buyers will continue to participate until they successfully trade.

I examine the welfare implications of alternative mechanisms under the assumption that the distribution of buyers in the market has reached a steady state. That is, the cohort of buyers

[2]Indeed, the most commonly cited behavioral strategy, an even division of the gains from trade, is impossible in an environment with two-sided imperfect information where agents do not know the gains from trade.

who make a purchase and exit the market is replaced by an identical group of new buyers entering the market. This assumption, shared by similar work by Fraja and Sakovics (2001), abstracts away from any transitional dynamics, a issue that is outside the scope of this paper. Note that if different types of buyers have different probabilities of purchasing then the distribution of buyers participating in the market will differ from the fundamental distribution of entering buyer types, an issue I address in further detail below. Buyers who decide not to participate in the market exit instantly and are also replaced the next period.

Once entering buyers and sellers have decided to participate in the market, they engage in a process of costly search at the end of which they are randomly matched one-to-one with each other. If the number of buyers and sellers is not equal, some traders will be unmatched and will continue to the next period and have another chance at matching. Matched sellers learn their costs of providing the good, either engage in bargaining or trade at a fixed price (depending on which mechanism is in place in the market), and buyers who have purchased their desired good exit the market. Discounting occurs, new buyers enter, and the process begins again. Figure 1 depicts the market timeline in graphical form.

3.0.1 Type Distributions

The theoretical bargaining literature considers multiple dimensions of incomplete information: players' costs or valuations (c or v), their multiplicative bargaining time costs, or their per-round fixed costs of bargaining (k) may all be unknown to their opponents. In the context of autorickshaw bargaining, it is reasonable to assume that there may be more than one dimension of uncertainty, and this issue is explored in the estimation section. However, for expositional purposes, I set up the model with only unobserved heterogeneity in the costs and valuations of players. Additional dimensions of heterogeneity could be added at some cost in notation.

Sellers are assumed to be ex-ante identical, with free entry into the market. Sellers will thus enter the market until their ex-ante expected utility from market participation, $\mathbb{E}_{c,v}\left[U_S\left(c,v\right)\right]$ is equal to their outside option w:

$$\mathbb{E}_{c,v}\left[U_S\left(c,v\right)\right] = w \tag{1}$$

where expectations are taken over both the distribution of potential buyer values and sellers' own future costs since sellers' costs are only realized upon meeting with a buyer. Define this (exogenous) distribution of seller match-specific costs as.

$$c \quad \sim \quad g_S\left(\cdot\right) \text{ on } \left[\underline{c},\bar{c}\right]$$

Buyers participate in the market if their interim expected utility ($\mathbb{E}_c\left[U_B\left(c,v\right)|v\right]$) is greater than their outside option y:

$$\mathbb{E}_c\left[U_B\left(c,v\right)|v\right] \geq y$$

where expected utility is conditioned on v because buyers know their own valuations before choosing to participate in the market. Let the mass B_0 of buyers who enter the market each period have a fundamental distribution of valuations

$$v \sim f_B\left(\cdot\right) \text{ on } \left[\underline{v}_0,\bar{v}\right]$$

Assuming that a buyer's expected utility under the mechanism is increasing in the valuation of the good, define $\underline{v}$ to be the buyer type or set of types indifferent between entry and remaining out of the market: $\mathbb{E}_c\left[U_B\left(c,\underline{v}\right)|\underline{v}\right] = 0$. While the distribution of entering buyers' valuations is exogenous to the mechanism, the steady state distribution of buyers participating in the market at any given time depends the market mechanism, since some trading rules might cause certain types of buyers to accumulate in the market awaiting an acceptable trading partner. Let this (endogenous) distribution of buyer costs be $g_B(\cdot)$ on $[\underline{v}, \bar{v}]$.

Matching of buyers and sellers occurs randomly, and match probabilities are uncorrelated with trader types both within and across periods. If the number of buyers in the market is B and the number of sellers is S, then buyers and sellers are matched with probabilities $\mu_B(S, B)$ and $\mu_S(S, B)$. Match probabilities for each side of the market are weakly increasing in the number of potential trading partners and weakly decreasing in the number of other buyers or sellers, i.e. $\frac{\partial \mu_B}{\partial S} \geq 0$, $\frac{\partial \mu_B}{\partial B} \leq 0$, and likewise for sellers' μ_S. Let $p(c, v)$ be the probability that trade occurs after matching between a buyer with value v and a seller with cost c. Then the mass of buyers in the market, B, is pinned down by the steady state condition:

$$B\mu_B \mathbb{E}_{c,v}[p(c,v)] = B_0(1 - F_B(\underline{v})) \tag{2}$$

which sets the number of buyers exiting the market (on the LHS) equal to those entering (on the RHS).

3.0.2 Utilities

Each interaction, players receive utility from two sources: First, they expect to trade the good with some probability $p(c, v)$. Second, in the course of the interaction, they receive (or lose) some utility $x_i(c, v)$. The $x_i(c, v)$ may represent either a payment received from or made to their trading partner, or any other utility cost incurred in the course of the interaction. The function through which these additional gains and losses from trade are related to the types of the traders depends on the mechanism used to allocate the goods in the market; below I outline the form of $\{p(c, v), x(c, v)\}$ for the specific cases of bargaining and fixed price mechanisms.

As is standard in the mechanism design literature, I assume that the traders' interim per-period utility functions are separable in utility from acquiring or selling the good and other costs or gains accrued during the trading process:

$$\begin{aligned} \text{Seller: } \mathbb{E}_v[u_S(c,v)|c] &\equiv \mathbb{E}_v[-cp(c,v) + x_S(c,v)|c] \\ \text{Buyer : } \mathbb{E}_c[u_B(c,v)|v] &\equiv \mathbb{E}_c[vp(c,v) + x_B(c,v)|v] \end{aligned}$$

where for both players expectations are taken over the distribution of their opponents' types in the market, $g_S(c)$ and $g_B(v)$.

I consider only stationary mechanisms. This constraint, combined with the assumptions of IID matching and a steady state distribution of buyers, ensures that buyers will never chose to exit the market without trading since their expected future gains from trade are unaffected by any previous failed negotiations.

Let κ_S and κ_B denote the search costs that sellers and buyers, respectively, incur at the beginning of each period and let δ denote the players' discount factor. A seller draws a new c cost each period, and remains in the market after trade. His ex-ante dynamic utility is

$$\begin{aligned} \mathbb{E}_{c,v}[U_S(c,v)] &= -\kappa_S + \mu_S \mathbb{E}_{c,v}[u_S(c,v)] + \delta \mathbb{E}_{c,v}[U_S(c,v)] \\ \mathbb{E}_{c,v}[U_S(c,v)] &= \frac{1}{1-\delta}(-\kappa_S + \mu_S \mathbb{E}_{c,v}[u_S(c,v)]) \end{aligned} \tag{3}$$

where the free entry condition ensures that $\mathbb{E}_{c,v}[U_S(c,v)] = w$. Thus the market mechanism has no effect on the welfare of sellers who, in expectation, earn their outside options. It does, however, affect the number of sellers that the market can sustain.

A buyer has the same value every period she remains in the market, and exits the game permanently whenever trade occurs. Her ex-ante expected utility is

$$\mathbb{E}_c\left[U_B(c,v)\,|v\right] = -\kappa_B + \mu_B \mathbb{E}_c\left[u_B(c,v)\,|v\right] + \left(1-\mu_B \mathbb{E}_c\left[p(c,v)\,|v\right]\right)\delta \mathbb{E}_c\left[U_B(c,v)\,|v\right]$$

$$\mathbb{E}_c\left[U_B(c,v)\,|v\right] = \frac{-\kappa_B + \mu_B \mathbb{E}_c\left[u_B(c,v)\,|v\right]}{1-\delta\left(1-\mu_B \mathbb{E}_c\left[p(c,v)\,|v\right]\right)} \tag{4}$$

The total utility of the buyers who enter in period t is the sum of the market participants' and non-participants' utilities:

$$W_B^t = B_0\left(F_B(\underline{v}) * y + (1-F_B(\underline{v}))\,\mathbb{E}_{c,f_B(v)}\left[U_B(c,v)\,|v \geq \underline{v}\right]\right)$$

Since new buyers enter every period, the total utility from the buyer side of the market is

$$\begin{aligned} W_B &= \sum_{t=1}^{\infty} \delta^t W_B^t \\ &= \frac{B_0}{1-\delta}\left(1-F_B(\underline{v})\right)\mathbb{E}_{f_B(v)}\left[\mathbb{E}_c\left[U_B(c,v)\right]|v \geq \underline{v}\right] \end{aligned} \tag{5}$$

Total welfare in the market is then $W = w + W_B$, which is a function of both the market mechanism $\{p(c,v), x(c,v)\}$ and the structural parameters, $\{f_B(v), g_S(c), \kappa_S, \kappa_B, \delta, B_0\}$. Given estimates of these parameters from any market, the social welfare generated by the status quo mechanism can be calculated, as can that of any other potential market design. I focus on the comparison between bargaining (the status quo mechanism) and an alternative fixed price market design.

3.1 The Fixed Price Mechanism

Fixed prices (or a fixed per-kilometer price in the case of a market for transportation) have the advantage that they are simple, satisfy ex-post IR, IC and budget-balance constraints, and are a clear policy alternative to bargaining since they have been adopted in many markets. While a fixed price is not, in general, the optimal mechanism, Athey and Miller (2007) show that in a stationary environment where all constraints must hold ex-post, the optimal mechanism approaches a fixed price under a wide range of parameter values. In the notation that follows, let η be an exogenously determined fixed price or rate, determined by a regulator or by market forces outside of the economy in question. Traders in the fixed price market pay nothing if they are not matched, and the buyer pays the seller η if trade occurs:

$$x_S(c,v) = -x_B(c,v) = \begin{cases} \eta & \text{if } c \leq \eta \leq v \\ 0 & \text{otherwise} \end{cases}$$

Buyers whose value for the good is below the fixed price will never enter, and those who do enter will trade with probability one, conditional on being matched with a seller whose cost is below η. Since matching is random, the distribution of buyers' valuations in the market $g_B(v)$ will be a truncation of the fundamental distribution of entering buyers' valuations $(f_B(v))$. Similarly, sellers will always make a sale if it is profitable for them to do so, since they gain the same continuation value regardless of whether trade occurs.[3] Trade occurs with probability

$$p(c,v) = \begin{cases} 1 & \text{if } c \leq \eta \leq v \\ 0 & \text{otherwise} \end{cases}$$

[3]Here I assume that traders are not capacity constrained in the short-term. An alternate formulation would be to interpret the seller's cost c as including the opportunity cost of any sales that might have been lost by trading with the current buyer.

Substituting these fixed price trade probabilities and prices into equations 3 and 4 yields the sellers' ex-ante expected utility and the buyers' interim expected utility. These utility functions, together with equations 1 and 2, generate a system of non-linear equations that can be solved to yield the equilibrium masses of sellers and buyers in the market and hence the overall surplus W^{η} for a given fixed price η.

A weakness of fixed prices, and all mechanisms that require traders to walk away from gains from trade, is that these may not be sustainable if traders can communicate or learn some information about each other's types. For instance, buyers may encounter sellers for whom $c < v$, so that there are gains from trade, but if $c > \eta$ trade will not occur under the fixed price mechanism. If the buyer can somehow learn c, and the chance of punishment for deviation from the fixed price is low–as it is in many cases of decentralized trade–traders may prefer to make a side deal and divide the surplus of $v - c$ rather than search for new trading partners. In this case the fixed price will not be a feasible mechanism. This example highlights the difficulties in switching from a bargaining mechanism to a fixed price when traders can credibly reveal their costs and valuations, and punishment is unlikely. If expectations change slowly, immediately after a fixed price is introduced the market may still contain many agents who cannot afford to trade at the fixed cost but still can generate gains from trade. If other traders expect this broad distribution of types, then they may be willing to deviate from the fixed price to increase trade and the fixed price equilibrium will be impossible. On the other hand, in markets where fixed price equilibria are firmly established, only buyers and sellers with valuations (costs) above (below) the price will be present in the market. Because traders will expect these limited distributions, it may be impossible (or very costly) for a trader to credibly signal a valuation (cost) below (above) the price, and the fixed price equilibrium will be sustainable.

3.2 The Bargaining Mechanism

Many bargaining models exist that yield functions relating trader types with trade probabilities and expected payoffs $\{p(c,v), x(c,v)\}$, analogous to those derived above for the model of fixed prices. However, these functions will depend on the specific equilibria of the game being played, which may not be possible to determine a priori in the case of multiple equilibria. Thus instead of following the game theoretical bargaining literature and solving analytically for the full set of beliefs and strategies for each type, this section specifies only the extensive form and payoff functions of the bargaining game, without solving for a specific equilibrium. Later, I use these two elements, together with the pattern of actions observed from the specific equilibrium that is played in the data, to estimate $\{p(c,v), x(c,v)\}$, thereby avoiding the problem of multiple equilibria.

The extensive form is standard in the bargaining game: bargainers alternate taking actions, with the seller taking the first action and the buyer deciding whether to exit the bargaining, accept the seller's previous offer, or make a counteroffer from one of a set of discrete possible offer amounts[4]. Henceforth I refer to these exchanges of offers as a bargaining "rounds", to distinguish them from trade "periods" which represent a whole set of interactions between buyers and sellers in the multilateral market economy, each potentially including many bargaining rounds.

Formally, in bargaining round t player i chooses action a_{it} out of the set of possible actions A_{it} where:

$$a_{it} \in A_i\left(x_{-i(t-1)}, x_{ij(t-2)}\right) = \begin{cases} \chi & \text{exit} \\ \alpha & \text{accept player } -i\text{'s offer} \\ x_j \in X_i\left(x_{-i(t-1)}, x_{i(t-2)}\right) & \text{counteroffer } x_j \end{cases}$$

[4]While the assumption of discrete actions is appropriate for the autorickshaw market, it is not without loss of generality on both game theoretic (Muthoo 1991) or econometric grounds .

The set of feasible actions depends on the previous offers due to the monotonicity of the bargaining game: sellers (buyers) can never rescind an earlier offer and raise (lower) their asking price.[5] Nor can sellers (buyers) make a counteroffer below (above) their opponent's last offer, since in it would always be more profitable for them to accept that offer instead. For brevity of notation, let $A_{it} \equiv A_i\left(x_{-i(t-1)}, x_{i(t-2)}\right)$ and $X_{it} \equiv X_i\left(x_{-i(t-1)}, x_{i(t-2)}\right)$.

The players' per-round payoffs π_i depend on their actions and their beliefs over their opponents' types. I define these posterior distributions of beliefs as $h_S\left(v|\left\{x_\tau\right\}_{\tau=1}^{t-1}\right)$ for the seller and $h_B\left(c|\left\{x_\tau\right\}_{\tau=1}^{t-1}\right)$ for the buyer, where $\left\{x_\tau\right\}_{\tau=1}^{t-1}$ denotes the past history of offers.

Following the concept of Markov Perfect Equilibrium, let the state variable s_{it} to be the vector of payoff-relevant state variables for player i and time t:

$$
\begin{aligned}
s_{St} &= \left\{x_{S(t-2)}, x_{B(t-1)}, h_S\left(v|\left\{x_\tau\right\}_{\tau=1}^{t-1}\right), c\right\}, \text{ for } t \text{ odd} \\
s_{Bt} &= \left\{x_{B(t-2)}, x_{S(t-1)}, h_B\left(c|\left\{x_\tau\right\}_{\tau=1}^{t-1}\right), v\right\}, \text{ for } t \text{ even}
\end{aligned}
$$

Player have a fixed utility cost k of making a single bargaining offer, in contrast with κ_i the search cost of seeking out a new trading partner and entering into a fresh bargaining interaction. Since bargaining rounds are extremely short in the autorickshaw market–offers occur at roughly 20-second intervals–I do not include a multiplicative time cost of bargaining in the payoff function. For contexts in which bargaining takes longer, for instance negotiations between a firm and a union, these time costs may be more appropriate and could easily be incorporated into the model instead of or in addition to an additive cost of bargaining. Note that I do not impose any restrictions on the correlation between k and c and v.

With these variables defined, the seller's per round payoffs for each action can be written as

$$
\begin{aligned}
\pi_S\left(a_{St} = \chi | s_{St}\right) &= \delta \mathbb{E}_{c,v}\left[U_S\left(c, v\right)\right] \\
\pi_S\left(a_{St} = \alpha | s_{St}\right) &= x_{B(t-1)} - c + \delta \mathbb{E}_{c,v}\left[U_S\left(c, v\right)\right] \\
\pi_S\left(a_{St} = x_j | s_{St}\right) &= \Pr\left(a_{B(t+1)} = \chi | x_j, s_{St}\right) \delta \mathbb{E}_{c,v}\left[U_S\left(c, v\right)\right] \\
&\quad + \Pr\left(a_{B(t+1)} = \alpha | x_j, s_{St}\right)\left(x_j - c + \delta \mathbb{E}_{c,v}\left[U_S\left(c, v\right)\right]\right) - k_S
\end{aligned} \tag{6}
$$

Within-round payoffs for the buyer are similar, and reflect the fact that the buyer exits permanently after trade:

$$
\begin{aligned}
\pi_B\left(a_{Bt} = \chi | s_{Bt}\right) &= \delta \mathbb{E}_c\left[U_B\left(c, v\right)|v\right] \\
\pi_B\left(a_{Bt} = \alpha | s_{Bt}\right) &= v - x_{S(t-1)} \\
\pi_B\left(a_{Bt} = x_j | s_{Bt}\right) &= \Pr\left(a_{S(t+1)} = \chi | x_j, s_{Bt}\right) \delta \mathbb{E}_c\left[U_B\left(c, v\right)|v\right] \\
&\quad + \Pr\left(a_{S(t+1)} = \alpha | x_j, s_{Bt}\right)\left(v - x_j\right) - k_B
\end{aligned} \tag{7}
$$

Note that every seller action ultimately results in their returning to the market to seek another customer. Accordingly, I subtract the sellers' continuation value $\mathbb{E}_{c,v}\left[U_S\left(c,v\right)\right]$ from the payoff of each action, thereby normalizing the value of exiting the bargaining to zero. Buyers do not receive the continuation value of remaining in the market if trade occurs; however, when estimating buyers' valuations from only their bargaining offers, $\delta \mathbb{E}_c\left[U_B\left(c,v\right)\right]$ is not separately identified from

[5]While I impose monotonicity a priori, it can be shown that this condition will hold in virtually all models of bargaining.

the value of the value of the ride. I therefore normalize the driver's type-specific continuation value to zero and estimate the surplus net of the continuation value, or $\hat{v} = v - \delta\mathbb{E}_c\left[U_B(c, v)|v\right]$.[6]

Because players only make offers every other round, I write their payoffs from their opponent accepting or rejecting a counteroffer as part of their within-round payoffs, although these payoffs are incurred in the next round and are subject to the utility cost of bargaining. The first four rounds of the bargaining game extensive form are shown graphically in figure 2, with a particular series of offers highlighted for illustrative purposes. At each round of bargaining, the payoffs are written for accepting (α) and exiting (χ). Nodes at which the seller may take an action are colored black, and buyer's nodes are colored white.

An agent's beliefs on his opponent's type enter through the expected payoffs of making a counteroffer, $\pi_i(a_{it} = x_j|s_{it})$. For instance, when assessing the probability that his opponent will exit after an offer of x_j, the seller must account for both his uncertainty over the type of his opponent, and the potentially mixed strategies played by each type of buyer. Thus to calculate the probability that an offer of x_j results in opponent exit, he integrates his beliefs on each buyer type's exit probability over the distribution of buyers that he expects at state s_{it}:

$$\Pr\left(a_{B(t+1)} = \chi|x_j, s_{St}\right) = \int \Pr\left(a_{B(t+1)} = \chi|s_{B(t+1)}(x_j)\right) h_S\left(v|\{x_\tau\}_{\tau=1}^{t-1}\right) dv \qquad (8)$$

where $s_{B(t+1)}(x_j)$ denotes the buyer state that would occur in the next round if the seller counteroffers x_j; note that this contains v, the buyer's unobserved type over which the seller integrates. The expected opponent probability of acceptance is defined analogously.

The dynamic nature of the bargaining game enters through the opponent counteroffer probabilities, since these are, from the player's perspective, the probabilities of transitioning to each future state. I define these conditional state transition probabilities as $\Psi\left(s_{i(t+2)}|s_{it}, a_{it}\right)$; note that due to the alternating offer form of the game, bargainers are concerned with their transition to a state two rounds ahead. For instance, for the seller, the probability of transitioning to state $s_{S(t+2)} = \left\{x_j, x_{B(t+1)}, h_S\left(v|\{x_\tau\}_{\tau=1}^{t+1}\right), c\right\}$ after making an offer of x_j is

$$\Psi\left(s_{S(t+2)}|s_{St}, a_{it} = x_j\right) = \Pr\left(a_{B(t+1)} = x_{B(t+1)}|s_{St}, x_j\right)$$

If the player accepts or exits $\Psi\left(s_{i(t+1)}|s_{it}, \alpha\right) = \Psi\left(s_{i(t+1)}|s_{it}, \chi\right) = 0$, and the bargaining interaction ends. I define the choice-specific utility of taking action a as

$$u_i(a|s_{it}) = \pi_i(a|s_{it}) + \int \left(V_i\left(s_{i(t+2)}\right) - k\right) d\Psi\left(s_{i(t+2)}|s_{it}, a\right)$$

where $V_i(s_{it})$, player i's dynamic utility of state s_{it} is then equal to the value of their highest payoff action

$$V_i(s_{it}) = \max_{a \in A_{it}} \{u_i(a|s_{it})\}$$

Having defined the extensive form and payoff functions of the bargaining game, I do not specify or solve any particular game theoretical equilibrium for the players' beliefs or strategies. Instead my estimation technique will estimate players' beliefs from the cross sectional distribution of opponent actions observed in the data and take their strategies to be the actions maximizing their payoff functions conditional on these beliefs. Consistent with the dynamic games literature, I make three broad assumptions about the equilibrium :

[6] Assuming a one-to-one correspondence between v and $\hat{v}$, the buyer's dynamic utility can be rewritten to include this normalization as: $\mathbb{E}_c\left[U_B(c, v)|v\right] = \frac{1}{1-\delta}\left(-\kappa_B + \mu_B\mathbb{E}_c\left[u_B(c, \hat{v})|\hat{v}\right]\right)$ Intuitively, one can think of reframing the passenger's utilities such that they now expect to participate in the market forever, but in each period gain only the difference between their absolute valuation and outside option.

- Single Equilibrium: All players in the market are playing the same equilibrium.
- Rational Expectations: Player's beliefs on their opponents' actions are correct along the equilibrium path.
- Optimizing Behavior: Player's strategies at each state maximize their expected payoffs subject to their beliefs.

These assumptions are similar to Fudenberg and Levine (1993)'s concept of "Self-Confirming Equilibrium". Intuitively, one can imagine that if players have participated long enough in the market they will learn about their opponent's distribution of actions on the equilibrium path. Since players only observe equilibrium play, the observed actions correspond to their beliefs, which are then "self-confirming".

4 Estimation

I employ the techniques of the dynamic games literature (Aguirregabiria and Mira 2007; Pakes, Ostrovsky, and Berry 2007) to estimate the parameters of the model outline above from data on the series of offers, counter-offers, and accept/exit decisions across multiple bargaining interactions. At every state, I calculate the players' expected utility of each potential action: accept, exit, or counteroffer, and then estimate the parameters such that, at each state, the actions most frequently taken in the data are those with the highest calculated payoffs. The algorithm follows series of distinct steps:

1. In the first stage, I estimate the state-specific probabilities of opponent actions that players would face after making each potential counteroffer.
2. Next, given a set of candidate parameters for each state I calculate the expected payoff of every action–accept, exit, or counteroffer– by backwards induction:
   - At nodes when it is the opponent's turn to act, I use the first stage estimates of the opponent action probabilities to calculate expectations over opponents' future actions.
   - At nodes when it is the player's turn to act, I use the values of the future states, together with a specification of action-specific utility shocks, to calculate the player's own future best response actions.
3. Once calculated, the action-specific utilities imply a set of probabilities that players take each action in each state. I maximize a dynamic logit likelihood function, using random coefficients to model the distribution of players' types, to fit the sequences of actions to in the data to the probability of these sequences implied by the model. Note that for each set of candidate parameters in the process of maximizing the likelihood function I must recalculate the action-specific utilities by repeating the backwards induction in step 2.

Individual-Specific Fixed Effects The most direct and intuitive approach to estimation of the players' types is to use the series of actions made by each individual bargainer to infer the parameters, or set of parameters that would rationalize each of their actions in the bargaining interaction as a best response. For instance, if a seller takes some action a_{St}, then by revealed preference his cost and bargaining disutility must be such that his expected utility $u(a_{St}|s_{St};c,k)$of that action is greater than that of any alternative action a'_{St} he might have taken at that point:

$$u(a'|s_{St};c,k) - u(a_{St}|s_{St};c,k) \leq 0 \;\forall\; a' \neq a_{St} \in A_S(s_{St})$$

Thus if player i takes a total of T_i actions, then the set of parameters that rationalize all those actions as best responses are the solution to the following minimization problem:

$$\{c_i,k_i\} = \text{argmin}_{c_i,k_i}\left\{\sum_{t=1}^{T_i}\sum_{a'\in A_{it}} \min(u(\hat{a}_{it}|s_{it};c_i,k_i) - u(a'|s_{it};c_i,k_i),0)^2\right\}$$

which imposes a quadratic loss function on all binding best-response inequalities. By repeating this estimation for the seller in each bargaining interaction, a set of $\{c_i,k_i\}$sets can be computed (and likewise $\{v_i,k_i\}$sets for the buyer) which provide an estimate of the distribution of costs, values and bargaining disutility.

This estimation approach has the advantage that it imposes no additional distributional or parameteric assumptions on the data beyond those implicit in the payoff functions, extensive form, and first round estimation of the players' beliefs. However, due to the shortness of the bargaining interactions (players make at most 5 offers), the individual valuations are likely to be estimated with substantial noise. As discussed in the results section, prima facie evidence of this noise is the fact that (as is common in the moment inequality literature) at least one best response inequality is typically binding even at the point in the parameter space that best rationalizes the data. This noise will then bias the empirical CDF of the estimated types, both expanding its support and flattening the distribution. Despite this bias, the non-parameteric distributions can deliver valuable insight into which parametric distributions might best fit the data in a random effects framework.

Random Coefficients Logit A random effects approach to estimation provides an alternative to the individual estimation of types that is consistent, but at the price of imposing a known distributional form on the buyer and seller values, costs, and bargaining disutility. Motivated by the shapes of the non-parametric distributions generated by the fixed effects estimation, I model the distribution of types as bivariate log-normal in $\{c,k\}$ (or$\{v,k\}$ for buyers), with the mean and standard deviation of c and v estimated separately for each trip distance, and the mean and standard deviation of k assumed to be the same regardless of the length of the journey.

Random effects estimation also requires more structure to be put on the data in the form of idiosyncratic shocks to the values of the bargainers' choices. As in many applications of dynamic discrete choice models, I follow Rust (1987), in assuming that the per-round utility from each action is hit by an additive extreme-value type 1 distributed shock, and that these shocks are IID across choices and across shocks:

$$\tilde{\pi}_i(a_{it}|s_{it},\varepsilon_t) = \pi_i(a_{it}|s_{it}) + \varepsilon(a_{it})$$

These action-specific shocks are private information–conditional on the player's own actions they do not affect her opponent's actions–but they known to the agent prior to taking an action in each round. This error structure is by far the most commonly adopted in applied dynamic models (Aguirregabiria and Mira 2010), since it allows for closed form solutions to the action probabilities and generates a concave log likelihood. In the context of bargaining for autorickshaw rides, these shocks can be interpreted as a player's idiosyncratic belief that making a given offer will yield a relatively good outcome, or that the returns to accepting the opponent's previous offer or exiting the bargaining may be temporarily high, perhaps due to the passing of another autorickshaw or potential customer.

Agents' probability of taking an action a_{it} is the familiar conditional logit formula

$$\Pr(a_{it} = a|s_{it}) = \frac{\exp(u_i(a, s_{it}))}{\sum_{a' \in A_{it}} \exp(u_i(a', s_{it}))} \tag{9}$$

and the interim expected value of each state, now including the action-specific shocks, can be expressed in closed form as

$$\tilde{V}_i(s_{it}) = \log\left(\sum_{a \in A_{it}} \exp(u_i(a, s_{it}))\right)$$

Finally, let the dynamic payoff of choice a_{it}, now including the value of the shocks, be

$$\tilde{u}_i(a|s_{it}) = \tilde{\pi}_i(a_{it}|s_{it}, \varepsilon_t) + \int \left(\tilde{V}_i\left(s_{i(t+2)}\right) - k\right) d\Psi\left(s_{i(t+2)}|s_{it}, a\right)$$

First Stage: Opponent Action Probabilities The key ingredient to the backwards induction are the player's beliefs on his opponent's actions at each node. These beliefs incorporate both the posterior on the opponent's type, and each type's strategies. For instance, when making an offer of x_j, the seller estimates the probability that his offer will lead to the buyer exiting by integrating over her posterior on buyer's valuation as in equation 8. The econometrician cannot directly evaluate equation 8, since it contains the posterior distribution $h_i(\cdot)$, which is unobserved. Attempting to solve an analytic solution for $h_i(\cdot)$ would lead to the all the problems of multiple equilibria discussed in section 2.

Instead, following the dynamic games literature (Aguirregabiria and Mira 2007; Bajari, Benkard, and Levin 2007), I rely on the assumption of a single equilibrium and estimate the players' beliefs on opponent types and strategies from the distribution of counteroffers observed at each state in the data. The critical feature of the bargaining game that permits this approach is that the unobserved component of the state vector, c or v, is also unobserved to the bargainer's opponent, so players know that their opponents will take actions based only on the history of offers (which is observable to the econometrician). For example, the seller's expectation of the probability that the buyer exits after he counteroffers x_j (from equation 8) can be calculated using the empirical distribution of the buyer's actions following that counteroffer:

$$\begin{aligned}\Pr\left(a_{B(t+1)} = \chi|x_j, s_{St}\right) &= \int \Pr\left(a_{B(t+1)} = \chi|s_{B(t+1)}(x_j)\right) h_S\left(v|\{x_\tau\}_{\tau=1}^{t}\right) dv \\ &= \Pr\left(a_{B(t+1)} = \chi|x_j, \{x_\tau\}_{\tau=1}^{t-1}\right)\end{aligned}$$

Estimating these opponent action probabilities raises an issue common to many dynamic structural papers: if the state space is large relative to the data (as it is in bargaining games) the action probabilities estimated by a simple count estimator will be very noisy and contain many values of 0 or 1. For instance, if a given state is observed only once in the data, estimating beliefs by the simple action probabilities in the data would imply that players expect their opponents to carry out the action performed at that state in data with 100% probability. To generate more continuous beliefs, I follow Aguirregabiria and Mira (2002) and estimate a first stage multinomial logit model to smooth the action probabilities:

$$\Pr\left(a_{-i(t+1)} = a|s_{-i(t+1)}(x_j)\right) = \frac{\exp\left(\theta_a \mathbf{q}\left(s_{-i(t+1)}(x_j)\right)\right)}{\sum_{a' \in A_{it}} \exp\left(\theta_{a'} \mathbf{q}\left(s_{-i(t+1)}(x_j)\right)\right)}$$

where $\mathbf{q}\left(s_{i(t+1)}\left(x_j\right)\right)$ is a vector of linear and squared state values and interactions, and θ_a is a vector of coefficients for action a. This estimation procedure is repeated for each bootstrap sample when calculating the standard errors,

Backwards Induction While the probabilities of opponent actions are identified by the observed actions in the data, the player's own best response at a given state cannot be estimated in the same manner because it depends on the player's own type, which is unobservable to the econometrician but (unlike the opponent's type) known the player herself. Thus the observed distribution of actions does not coincide with the player's own expectations of her actions once she reaches that state. I must therefore calculate the best responses at each node in the game tree as a function of the valuation (or cost) and bargaining disutility.

I do this through the backwards induction technique outlined above. As an illustration of the algorithm, consider the example of estimating the action-specific payoffs for the seller. Assume for simplicity that all bargaining interactions last at most T rounds, and that all possible states and actions are observed in the data. Assume, furthermore, that it is the seller's turn to act in round T. Then in round T the seller faces the choice of whether to accept or exit and selects the choice with the highest value. The value of each last-round state s_{iT} is then

$$\bar{V}_i\left(s_{iT}\right)=\int_{\varepsilon}\max\left\{\tilde{\pi}_i\left(a_{iT}=\chi|s_{iT},\varepsilon_T\right),\tilde{\pi}_i\left(a_{iT}=\alpha|s_{iT},\varepsilon_T\right)\right\}d\Gamma\left(\varepsilon\right)$$

Round $T-2$ is the seller's previous action. In this round the seller considers exiting, accepting, or making one of the remaining feasible offers. Each of these offers would generate an action by the opponent, ultimately resulting in some exit, accept, or continuation value, $\bar{V}_i\left(\tilde{s}_{iT}\right)$. The opponent action probabilities estimated in the first stage are then plugged into the backwards induction to calculate the expected payoffs of the counter-offers, and the value of each state in round $T-2$ is determined by the value of the best response action. Backwards induction continues analogously in round $T-4$,...,1 until the value of each state has been calculated.

Empirical States Empirical estimation of the bargaining game necessitates some reduction of the dimension of the state space. Since players' posteriors about their opponent's type are the same after identical histories of offers, the $h\left(v|\left\{x_\tau\right\}_{\tau=1}^{t-1}\right)$ term in the state vector can be replaced with just the history of offers itself, $\left\{x_\tau\right\}_{\tau=1}^{t-1}$. However, including the whole history as a state would be infeasible due to the very large number of implied states[7]. Thus I proxy the whole past history of offers with just the last two offers and the round itself, t, a simplification which is consistent with the equilibrium in many models of bargaining as a Markov perfect equilibrium. Also included in the state vector is a measure of the distance of the trip in discrete kilometers, d, and the unobserved type of the player, c or v. Thus the full empirical state vector, denoted $\tilde{s}_{it}$, is composed of the five elements

$$\begin{aligned}\tilde{s}_{St}&=\left\{x_{S(t-2)},x_{B(t-1)},t,d,c\right\}\text{, for } t \text{ odd}\\ \tilde{s}_{Bt}&=\left\{x_{B(t-2)},x_{S(t-1)},t,d,v\right\}\text{, for } t \text{ even}\end{aligned}$$

Final Round States The previous discussion of the backwards induction process assumed that all states are observed and that the game had a fixed terminal round T. In reality, neither of these conditions are likely to hold. As the theoretical literature recognizes, bargaining games can, in

[7]On the order of $\sum_{t=1}^{T}|A|^t$ if there are T periods per bargaining interaction, and $|A|$ possible actions per period.

principle, continue for infinitely many rounds. Although any finite sample of data will only contain a maximum of rounds T, at time T the players themselves could have chosen to counteroffer and thus extend the game to $T+1$, or to have taken an action at an earlier round that would have led to $> T$ rounds. Infinite horizon models are commonly estimated in the dynamic structural literature, thanks to the presence of some stationarity assumption. However, even imposing a Markov-perfect equilibrium, the bargaining game is only stationary conditional each player's posterior distribution of the other's type which is not observed.

As discussed in the context of the state variable, the past history of actions (or in this case a subset of the past actions plus a time index) may be substituted for the posterior in the state variable, but with this substitution stationarity is lost. Thus the number of potential states (where now the state depends directly on t) becomes infinite, and the analysis must address the problem of unobserved states.

However, while the unobserved states cannot be measured directly from the data, some structure can be placed on their values. Just as the theory literature often uses conjectures on off-equilibrium path actions to focus on specific equilibria, I can estimate structural parameters using both "optimistic" and "pessimistic" priors as to the type of the other player.

- The "optimistic" outcome of a counter-offer is that the opponent is revealed to be a "soft" type, and immediately accepts the counter offer. Thus (for the seller)

$$u^{\max}(x_{St}) = x_{St} - c - k$$

  and analogously for the buyer,

$$u^{\max}(x_{Bt}) = v - x_{Bt} - k$$

- The "pessimistic" outcome of any counter-offer is it leads to the opponent deciding that the player is a "hard" type, and immediately exiting:

$$\text{Seller: } u^{\min}(x|\tilde{s}_{St}) = -k$$
$$\text{Buyer:} u^{\min}(x|\tilde{s}_{Bt}) = -k$$

Note the slight asymmetry between the two bounds: while the "optimistic" outcome gives the best possible payoff conditional on the action, the "pessimistic" payoff is still greater than, for example, if the player had made the offer then wasted her time making several more offers before her opponent exited. Nevertheless, the "pessimistic" scenario is consistent with the assumptions on off-equilibrium path actions used in, for instance, Chatterjee and Samuelson (1988).

In practice I apply these bounds only to the values of actions taken in the final round ($T = 9$) of the game and calculate the values of all other states (whether observed or not) by the backwards induction procedure described above.

Likelihood Function Assuming a bivariate log-normal distribution of individual costs/valuations and bargaining disutility, I solve for the parameters of the distribution by maximum likelihood. Letting μ_c^d, σ_c^d denote the mean and variance of the log normal distribution of driver costs for a trip of distance d, and $\mu_{k,S}, \sigma_{k,S}, \rho_S$ denote the (distance invariant) mean and variance of the bargaining disutility and its correlation with the cost, then the full parameter vector for the seller is $\theta_S = \left\{\mu_c^1, \sigma_c^1, \ldots, \mu_c^D, \mu_{k,S}, \sigma_{k,S}, \rho_S\right\}$. The buyers' parameter vector, θ_B, is defined analogously, with valuations also estimated distance by distance.

Define N to be the number of bargaining interactions observed in the data, and T_{in} to be the number of rounds that player i may take an action in bargaining interaction n (as always, i indexes the buyer or seller). Following Judd (2006) I integrate the likelihood over the $\{c, k\}$and $\{v, k\}$distributions using Gauss-Hermite cubature, creating a two dimensional $M = 16$ node grid with parameter values θ_{im} at node m. Defining $\omega(\theta_{im})$ to be the appropriately rescaled Gauss-Hermite weight of node m, the full likelihood is then

$$\mathcal{L}_i(\theta) = \prod_{n=1}^{N} \sum_{m=1}^{M} \omega(\theta_{im}) \prod_{t=1}^{T_{in}} \Pr(a_{itn} | s_{itn}; \theta_{im})$$

where $\Pr(a_{itn}|s_{itn};\theta_{im})$ is the logit action probability, as defined in equation 9. Note that the likelihood makes use of the fact that a player's type is constant throughout the T_{in}actions that he takes in the bargaining interaction.

5 Data

The data for this study comes from the market in local transportation by autorickshaw in Jaipur, India. An autorickshaw is a form of three-wheeled mini-taxi, officially capable of carrying three passengers (although often far more in practice) in a semi-enclosed back seat. Autorickshaws are the primary means of rented transportation in Jaipur, a city of approximately 3.2 million people. Although in Jaipur they are technically outfitted with meters, during the survey period of January 2008 to January 2009 the meter rates had become surpassed by inflation and no autorickshaw driver ever used the meter. There were no police or government efforts to enforce the meter, and all prices were set by negotiation.

In addition to the universal prevalence of bargaining, at least three factors make the autorickshaw market an ideal test case for the economic analysis of bargaining. First, autorickshaw rides are very homogeneous conditional on observables. Given the day, time, and physical appearance of the autorickshaw (all of which are observable), a ride from point A to B is the same (in expectation) regardless of which driver provides it. Second, the autorickshaw market is an excellent candidate for potential policy interventions since a fixed price per kilometer or other non-linear price schedule is a feasible policy alternative. In many Indian cities similar to Jaipur (e.g. Ahmedabad, or Bangalore) virtually all autorickshaw rides are priced by the meter. Many other cities have set up "pre-pay" autorickshaw stands, where rides from the stand to various destinations each have a specific, pre-determined price. This variation in market equilibria suggests that it is differences in local government policy rather than in the underlying structural parameters that determines whether a city is in a bargaining or fixed-price mechanism. Third, the price of a ride is low enough so that data can be collected on actual transactions under the control of the researcher.

In order to collect the data, surveyors followed two protocols:

- In "real" bargaining, surveyors were told to travel through a pre-assigned series of waypoints (for instance, from point A to B, to C, then back to A), and given a lump sum of money to pay for the travel. Any of the payment that they did not spend on the autorickshaw fare was theirs to keep, and once they had finished their day's assigned circuit they were free to return home. Thus the surveyors' opportunity cost of money and value of time in terms of money should have been similar to what it would have been had they been bargaining on their own.[8] However, unlike real bargainers, the surveyors were required to complete their trips by

[8]Because of concerns that surveyors might take buses between waypoints, or negotiate with a single autorickshaw driver for the entire route supervisors were stationed at waypoints to monitor the surveyors.

autorickshaw, thus the "market entry" stage of the model is inapplicable to them, and their values of rides may not be representative of the general population who choose to travel by autorickshaw.

- In "scripted" bargaining, surveyors were assigned to stand in specific locations and were given a written bargaining protocol consisting of a destination and a sequence of pre-determined counteroffers. They then hailed passing autorickshaws, requested a ride to the destination and bargaining with the driver according to the protocol. If the driver were to accept a counteroffer, the surveyor invented an excuse not to take the ride. Although they had no personal stake in the outcomes, surveyors were instructed to act as if they were bargaining in a realistic manner so that drivers would themselves respond as naturally as possible. These scripted bargaining sequences were both cheaper and faster to collect than the real bargaining interactions. They also allowed the driver's action probabilities to be measured more accurately in states that rarely occurred in the real bargaining data.

  The data from these scripted interactions can also offer a test of endogeneity of the state variable. A potential concern in estimation using only real bargaining is that drivers are somehow able to signal their types to passengers in ways not captured by their previous offers. These signals would then influence passengers' offers and introduce biases into the drivers' conditional action probabilities. For instance, if one type of driver can signal a minimum passenger counteroffer below which he commits to exit, passengers may respond by shifting their counter-offers upward or exiting themselves. These drivers will then never be forced to make good their threat, and their valuations will be misestimated. The scripted bargaining is free from this problem, since surveyors' offers cannot be correlated with any statements made by drivers. The disadvantage is that they provide no information about the passenger's bargaining habits. In all analysis that follows, the passenger's offers from the scripted bargaining interactions are dropped except insofar as they serve as the state variables for the driver's choices.

Immediately after the conclusion of the bargaining, surveyors wrote down the series of offers made by the drivers and themselves, and noted the duration of the whole interaction (in seconds). They also recorded the model and quality of the autorickshaw, as well as details about the environment such as whether other drivers had attempted to interrupt the bargaining, the weather, and the time of day.

In August 2010, approximately 18 months after the conclusion of the bargaining data collection, more data was collected on the characteristics of the drivers themselves. To avoid selection bias, surveyors hailed autorickshaw drivers from the streets and administered a short questionnaire in exchange for a small payment (10 rupees) to encourage compliance. Despite the 18 months gap between the driver survey and the bargaining, the qualitative features of the autorickshaw market remained essentially the same in terms of the market structure, timing, and bargaining process. The results of this survey, presented in table 2 below, are used to derive other parameters about the choices of autorickshaw drivers to enter the market, in particular their potential wages outside of the autorickshaw market.

The characteristics of the surveyors employed to collect the data has greater significance in this experimental setup than in many others, since their actions are essential for predicting the distribution of counteroffers and exit/accept probabilities that drivers face after each possible offer. In order to reduce the number of state variables as much as possible, surveyors were chosen to be homogenous on characteristics that could be observed by the drivers. Surveyors hired for this project were all males, between the ages of 20 and 35 and dressed in similar casual clothing. All had finished 10th grade, and some had several years of college or had graduated. All made the same salary of rs. 200/day, in addition to whatever they earned from bargaining. Finally, all surveyors took autorickshaws routinely as part of their normal personal transportation, often on exactly the same routes as assigned those for this research. Thus the maintained hypothesis that surveyors know the distribution of driver types and counter-offers and the equilibrium being played is reasonable in this environment.

Summary statistics Table 1 presents summary statistics on bargaining for autorickshaw rides. In total, 2993 bargaining interactions were recorded, of which 2369 (79%) were conducted with the surveyor making offers from a predetermined list, and 624 (21%) in which the surveyor was the residual claimant of any gains from bargaining. Of the real bargaining interactions, 67% resulted in the surveyor actually taking the ride. The average duration of a bargaining interaction was 5 rounds, which implies two exchanges of offer/counteroffer between the players, and a final accept/exit decision by the driver. Bargaining was over quickly–interactions usually lasted less than a minute. If the interaction did not lead to trade, the average wait for another potential driver was almost 4 minutes. Some surveyors had to wait substantially longer for the next ride–the 90th percentile of the times between autorickshaws was 7:27 minutes. Trips were chosen to average about 5 kilometers, with the longest being 8.06 kms., and the shortest 1.65 kms. Finally, the data on offers suggests the role of bargaining in dividing the gains from trade between buyers and sellers: successful interactions had an average final price of rs. 41 between the driver's initial offer of rs. 56 and the passenger's average counter-offer of rs. 35.

The set of seller offers observed in the data is from 20 to 100 rupees, increasing at 5 rupee intervals, and the range of offers for buyers is from 15 to 65 rupees, again increasing at 5 rupee intervals. I define the set of possible actions in the backwards accordingly.

Table 2 presents the results of the interviews with drivers. On average, drivers were 35 years old and had about 10 years of experience driving an autorickshaw. The mean driver had attended school for over 5 years, although a substantial number (31%) reported having never attended school at all. 46% rented the rickshaw, and of those who rented, the average daily payment was 177 rupees. The median and mode rental rate, with 52% of the observations, was rs. 200, with substantial concentrations at rs. 150 (22%) and rs. 100 (8%). Average daily profit was rs. 267 (around \$5.80 at the exchange rates during the time the data was collected), with rs. 151 being paid for fuel costs. Interestingly, despite the substantial rental costs of an autorickshaw, drivers who rented their autorickshaws made only 88 rupees less than owners, due to the fact that their revenues were higher.

Most drivers work full-time in the autorickshaw market. The average driver worked 6.55 days per week, and the median driver works every day. Each day, the average driver works around 10 hours, in which he transports, on average, slightly over 8 passengers or groups of passengers. Very few (<1%) of drivers also have other jobs, but 8.44% share their autorickshaws with other drivers. Since these drivers do not work significantly fewer hours or days per week than those who do not share their autorickshaws, they are presumably passing the autorickshaw to other drivers who work a night shift.

Informal conversations with autorickshaw drivers suggest that there is substantial variation in drivers' individual costs for a given ride. Many drivers operate primarily in a certain neighborhood, and return there after delivering a passenger. Thus their cost of a trip in the direction of their "home base" is substantially lower than a trip elsewhere. Similarly, drivers often have regular passengers that they transport daily–for instance taking children to school. A trip in the direction of the driver's next scheduled pickup would have a low cost, since the driver would be headed in that direction anyway. Finally, the 46% of drivers that rent or share their autorickshaws and must return them at specific times. If the return time is approaching, the costs to taking a long ride away from the drop off point will be idiosyncratically high.

Different passengers will also have highly variable willingness to pay for the same ride, depending on their urgency, their wealth, and other factors. Anecdotal evidence suggests that, as theory predicts, drivers acknowledge the variability of their passengers' valuations and adjust their bargaining accordingly. This may explain the experience of many foreigners who are quoted (relatively) exorbitant initial offers for retail goods by Indian merchants.

Reduced form evidence from the bargaining interactions also suggests that traders' actions shift their opponents' posterior distributions on their types. The most natural test of bargaining behavior is whether players respond to relatively high or low offers by accepting or exiting the bargaining. Figure 3 shows the probability that a player exits the bargaining conditional on the opponent's previous offer (top and bottom lines show 95% confidence interval). Higher passenger offers decrease drivers' probability of exit, and increase passenger exit probability (except in round 2, when no passenger ever exited).

Figure 4 shows the relationship between previous offers and the probability a player accepts their opponent's offer. As expected, higher offers increase the probability of drivers accepting and decrease passenger's acceptance probability. The sole exception is in the third round where the relationship between driver acceptance and passenger previous offers appears somewhat bell-shaped, although the confidence intervals are large. Perhaps drivers interpret a high second round offer as on opportunity to extract even greater surplus. Note that in both figures the results in rounds 4 and 5 are on the selected sample of drivers and passengers who did not exit in the earlier rounds.

Understanding the reduced form effect of an offer on the potential counteroffers is more challenging because the two selection effects of acceptance and exit now operate within the round as well. On the acceptance side, all driver (passenger) offers below (above) the preceding offer are mechanically unobserved, because they would lead to accepting the ride. For example, if the passenger makes a high offer to the driver, the driver's counteroffer conditional on not accepting the passenger's offer must be even higher. This can be overcome, as I do below, by examining the empirical CDFs of counteroffers with a mass point equal to the fraction of bargainers who accepted located at the value of the previous offer. The rest of the CDF shows the relative probability of offers conditional on not accepting, and by comparing these portions of the CDF conditional on different values of the opponent's previous offer we can determine whether, for instance, drivers counteroffer differently after a low offer that they rejected than after a higher offer that they would also have rejected. There is no analogous direct method of dealing with the differences in exit rates conditional on the previous offer since it is unclear (without additional structure) what offer exiting players would have made had they not exited.

Subject to this caveat, Figure 5 displays how the distribution of counter-offers depends on the preceding offer conditional on not accepting or exiting. Each panel contains three kernel weighted[9] CDFs, each corresponding to a different cross section of the distribution of previous opponent offers. Each panel tells a similar story: conditional on accepting players respond to high offers by making high counteroffers. In each case the CDF of the counteroffers made to a previous offer in the 80th percentile is to the right of the 50th percentile previous offer CDF, which is (weakly) to the right of the 20th percentile. The vertical portions of the CDF on the low end of the driver's CDFs and high end of the passenger's CDFs are caused by the mass points of accepting drivers and passengers.

[9]A Gaussian kernel was used, with bandwidth of $\sigma\left(\frac{4}{3*N}\right)^{1/5}$ where N is the sample size and σ is the standard deviation of the variable being smoothed. For the CDFs, both the initial offers and the counteroffers were smoothed using this kernel.

Taken together, these graphs suggests that players in this market face the classic bargaining trade-off: Passengers can choose to make a high offer and get a higher probability of a favorable counter-offer at the risk of the driver leaving and then being forced to wait for a new autorickshaw, or make a low offer and get a high probability of rapid acceptance at the cost of lower surplus. Drivers face the same trade-off in reverse when it is their turn to make an offer. Interestingly, figures 3-5 suggest that these mechanisms may work slightly differently for the two sides of the market. Since they are more mobile, drivers seem to punish/reward low or high passenger offers more through the exit/accept decision, whereas passengers may be more responsive in the counter-offer dimension.

These patterns are broadly consistent with the signalling equilibria in many game theoretical models of bargaining. Drivers with high costs of providing the good or low costs of bargaining make high initial offers to signal their types (since low cost/impatient drivers would not want to take this risk), and passengers update their beliefs and modify their bargaining strategies accordingly.

6 Results

The techniques and data described above can be used to calculate both the distribution of costs of the drivers and the valuations of passengers, representing the supply and demand sides of the market. However, the costs and valuations recovered from this specific data collection strategy have different external validities: the costs are representative of the population of drivers, since drivers approached surveyors essentially at random and were unaware that their bargaining opponents were recording their offers until after the bargaining was complete. Thus the supply side is representative of the true market supply. The demand curve represented by the estimated passenger valuations, on the other hand, is specific to the context of the data collection for this paper. The surveyors represent only a small portion of the potential market for autorickshaw rides, and are thus unlikely to be representative of the general market for autorickshaw rides.

Due to these differences in external validity, I split the results section into two parts: In the first section, I analyze the distribution of drivers' cost and the supply curve implied by these costs. Here, I am able to examine the impact of policies such as enforcing the official meter rate, a policy in fact carried out by the government shortly after the study period concluded. In the second section I interpret the valuations of the surveyors literally, and derive the optimal fixed price and implied welfare outcomes if the surveyor valuations were those of the full population.

6.0.1 Estimated Parameters

The fixed effects estimation results for the drivers are displayed in figures 6, 7, and 10. Figure 6 displays histograms containing the distribution of driver costs for the four most common trip distances in the sample, 3, 4, 5 and 6 kilometers. Bootstrapped 95% confidence intervals are displayed as lines above the bars. The costs are imprecisely estimated, an outcome which may be due to the non-parametric nature of the estimation technique. Nevertheless, the distribution of costs appears reasonable–driver costs increase for higher distances, while their variance remains roughly the same. Note that the range of values between the lower and upper bounds on the identified set of driver costs is so small that it is not visible on the graph. This is due, in large part, to the fact that the moment inequalities are binding for the large majority of the sample. This suggests substantial noise in the inequalities, perhaps coming from imprecise estimates of the drivers' beliefs on their potential passenger's future actions. Since this uncertainty is incorporated into the bootstrapping (the first stage estimation of beliefs is repeated for each bootstrapped sample), the imprecision of the beliefs may also be contributing to the large confidence intervals on the value distribution.

Figures 7 and 10 display estimates of drivers' bargaining disutility k_S. While the majority of values are in the range of 0-2 rupees, the estimation suggests that over 10% of drivers have bargaining disutilties of greater than 4 rupees per offer–a relatively large amount considering that the mean price of a ride taken in the data is in the range of 35-55 rupees (depending on distance), and on average 5 offers are made per bargaining interaction. Finally, figure 10 shows the three dimensional histogram of drivers' cost and bargaining disutility for rides of 4 kilometers. The results show a strong negative correlation (r = -.82, p = .00001) between driver's costs and dislike of bargaining: drivers with lower costs (and thus more potential surplus) are estimated to have higher additive costs of bargaining. Interestingly, this result is consistent with a multiplicative discount factor, which would also generate this same type of correlation.

The results of the random effects logit estimation of the drivers' parameters are presented in table 4. As expected, mean costs reported in box A are generally increasing in the distance of the journey, although 5 kilometer rides appear to have both a very high mean and variance, and costs appear concave with respect to distance. This may be due to differences in drivers' outside options after trips to farther, more remote destinations. Estimates are reasonably precise, especially for the most common distances of 4-7 kilometers. The correlation between driver costs and bargaining disutility is displayed in the third column of table 4. As in the fixed effects estimation, it is negative but of a much smaller magnitude and very noisily estimated. This suggests that perhaps the log-normal distribution cannot capture the exact features of the underlying distribution of types, in particular the . Box B presents the estimated driver disutility of bargaining. It is substantially lower than that of the fixed effects estimation (.31 versus 1.3 rupees per offer) although imprecisely estimated.

The equivalent parameters of the distribution of passenger valuations are displayed in table 5. Strikingly, mean passenger valuations are often estimated to be lower than drivers' costs for the equivalent distance, although the large estimated standard deviations of the passenger's log-normal valuations create some overlap between the cost and valuation distributions at each distance. Passenger bargaining disutility is estimated extremely imprecisely under bivariate log-normal distribution, with several large outliers in the bootstrapping generating extremely large standard errors. Finally, passenger values do not, on average show a substantial correlation with bargaining costs, although here too the standard errors are large.

6.1 Fixed meter rates and driver utility

A policy change by the Government of Rajasthan provides a useful opportunity to test whether the estimated driver cost distributions are in fact reasonable. The month after the final bargaining data was collected (February, 2009) the Jaipur Road Transport Officer (the road safety enforcement agency) implemented a policy to force autorickshaw drivers to travel by the meter rate. Police set up checkpoints and drivers found to be travelling with meters turned off were fined and threatened with confiscation of their licenses. This initiative was met with strong resistance from the drivers, many of whom went on strike and demonstrated outside government buildings. At the time, the official meter rate was 11 rupees for the first kilometer, and 6 rupees for each additional kilometer. Spokesmen for the drivers' association demanded an increase of 4 rupees for the first kilometer, and one rupee for each subsequent kilometer.

Table 6 shows the fractions of drivers whose estimated costs are below the meter rate, for both the official February, 2009 meter rate, and that proposed by the drivers. The results suggest that under the official meter rate relatively few drivers would be willing to accept passengers on trips of distances in the 3-6 kilometer range. For instance, I estimate that only 18.4% of drivers would have idiosyncratic cost shocks low enough to be willing to take passengers on a typical 4 kilometer journey. Although the standard errors are large, the fraction of drivers willing to accept the meter rate is bounded below 60% for distances of 3, 4, 6, and 7 kilometers. Drivers' preferred rates would, naturally, allow a substantially larger proportion of the drivers to operate profitably, and suggest that drivers would be willing to accept virtually any trip of 7 or 8 kilometers. For shorter distances, however, a substantial fraction of drivers remain might be unwilling to travel. Note that, because drivers' costs are passenger specific, this does not imply that these high cost drivers will never accept a passenger. This fraction represents those drivers whose costs are high because the proposed trip is in the opposite direction of where they are travelling at the moment, or who have other, transient reasons to turn down a passenger.

Given the estimated costs and bargaining disutility of drivers in the bargaining status quo, I can investigate the question of what alternative meter rates would in fact ensure that the drivers continue to participate in the market. In accordance with standard taxi or autorickshaw meters, I allow the price to have both a lump sum "meter down" component η_1 and variable per-kilometer rate η_2; to conserve notation, let the vector $\eta = \eta_1 + \eta_2 d$ for distance d. I then solve for the price that sets drivers ex-ante utility under the fixed price regime with price η equal to their expected welfare under bargaining

$$\frac{1}{1-\delta}\left(-\kappa_S + \mu_S \mathbb{E}_{c,d}\left[\eta - c\right]\right) \quad = \quad \mathbb{E}_{c,v,d}\left[U_S\left(c,v\right)\right]$$

In a partial equilibrium framework, where both drivers' matching probabilities μ_S and their search costs and discount rates remain unchanged by the switch to fixed prices, the price that would make drivers indifferent between the fixed price market and the bargaining status quo is implied as the solution to

$$\eta : \; \mathbb{E}_{c,d}\left[\eta - c\right] = \mathbb{E}_{c,k,d}\left[V\left(t=1; c,k,d\right)\right]$$

where $V\left(t=1;c,k,d\right) = \mathbb{E}_v\left[u_S\left(c,k,v\right)|c,k,d\right]$ is the drivers' dynamic utility of the first round of the bargaining game for a journey of distance d, net of the continuation value of remaining in the market after trade occurs or does not occur.

Since autorickshaw meter prices have both an intercept (first kilometer price) and slope (price for subsequent kilometers), there is a continuum of prices that satisfy this condition. Figure 11 plots these prices on the lower, dashed line, with the higher, solid line showing the set of prices that would set driver's expected fixed price profits equal to the nominal amount they received from passengers, not including any disutility costs. Both price schedules are substantially higher than the existing government rate of 11+6rs./km, and confirm that drivers' welfare would decrease from enforcing the official rate unless a substantial number of new passengers entered the market. Interestingly, the drivers' suggested rate of 15+7rs./km lies very close to the set of prices which would keep drivers indifferent, in utility terms, to the current bargaining system. While these results cannot be interpreted too strongly given the partial equilibrium nature of the analysis, they suggest that the distribution of costs recovered for the drivers may indeed reflect the true parameters.

6.2 The Pre-Paid Autorickshaw Stand

While the drivers' valuations are informative about the supply side of the market, a full comparison of the welfare implications of bargaining versus fixed prices must include the distribution of valuations and bargaining costs of the passengers as well. However, as mentioned earlier, the survey design can provide information only on the parameters of the valuations of the surveyors themselves, who are both a restricted portion of the distribution of passengers, and are taking rides because they are required to do so for their jobs.

Subject to these caveats, I consider the counterfactual policy of allowing passengers the option to purchase an autorickshaw ride at a fixed meter price instead of beginning bargaining with a driver. This option, known as the "pre-paid stand", currently exists in some Indian airports and train stations, although primarily for the taxi transportation markets. Passengers choose to take the pre-paid autorickshaw at price η instead of bargaining if

$$v - \eta \geq \mathbb{E}_c \left[U_B \left(c, v \right) | v \right] + \kappa_B$$

where the search cost is added back to the passenger's expected utility since the passenger could choose to immediately engage in bargaining with one of the drivers waiting at the autorickshaw stand. Under the partial equilibrium assumptions that passengers' outside bargaining option remains the same, their choice to purchase the fixed price autorickshaw ride can be expressed in terms of the estimated valuation $\hat{v}$ as

$$\hat{v} - \eta \geq V_B \left(t = 1; \hat{v}, k \right)$$

where $V_B \left(t = 1; \hat{v}, k \right) = \mathbb{E}_c \left[u_B \left(c, v \right) | v, k \right]$ is the passenger's first round dynamic utility from the bargaining game.[10]

I solve for the optimal pre-paid price, subject to the constraint that drivers are indifferent (in ex-ante utility) between going to the pre-paid stand and seeking a new bargaining interaction. This condition ensures that drivers themselves would be willing to pick up passengers at the fixed rate, while avoiding the issue of congestion and long waits at the auto stand. While a price that does not satisfy this constraint might yield higher overall surplus (for instance one in which drivers wait longer to find a passenger at the stand than to find a bargaining passenger) the lack of information about the matching function makes evaluation of this case difficult given the available data. I thus solve for

$$\begin{aligned} \eta^* =& \text{argma}x_\eta \mathbb{E}_{\hat{v}} \left[max \left\{ \hat{v} - \eta, \ V_B \left(t = 1; \hat{v}, k \right) \right\} \right] \\ \text{subject to } \mathbb{E}_c \left[\eta - c \right] =& \mathbb{E}_{c,k} \left[V \left(t = 1; c, k \right) \right] \end{aligned}$$

Searching through the set of prices yields an optimal price of rs. 8.7 for each kilometer and 0 fixed meter down costs. At these prices, passengers' mean per-period surplus would be 28% higher than their surplus without the option of using the autorickshaw stand. However even with the option of the pre-paid stand, 63% of passengers would still prefer to make remain in the bargaining market, suggesting that, at least for the sample of passengers in the data, bargaining remains a valuable option despite its costs.

Several factors suggest that these welfare estimates may be a lower bound. First, if relative to the full population of buyers, the sample buyers had low valuations of the ride and were relatively patient (as might be expected given their incentive structure) then higher valuation passengers with greater bargaining disutility would stand to gain more from the introduction of the fixed price autorickshaw stands. Second, if certain types of buyers choose the fixed price over bargaining, drivers will have better information regarding the types of buyers who choose to remain in the bargaining market. This extra information may improve the efficiency of the bargaining outside option, albeit at some transfer of surplus from passengers to the now better informed drivers.

[10] For passengers, who make the second offer, calculating mean expected interim utility requires first averaging over driver initial offers:

$$\mathbb{E}_c \left[u_B \left(c, v \right) | v, k \right] = \sum_{x_j \in X_{S1}} \Pr \left(x_{S1} = x_j \right) V_B \left(x_j, t = 2 | \hat{v}_n, k_n \right)$$

7 Conclusion

This paper has applied the theories of market design and bargaining and the empirical techniques of dynamic structural estimation to the comparison of markets with either bargaining or fixed prices. Unlike earlier papers on empirical bargaining that have tested the reduced-form implications of bargaining models, I have used the structure of the extensive form of the bargaining game to calculate payoffs and hence the structural parameters of the model. However, in contrast with the laboratory bargaining literature, I do not specify a particular game theoretical model of bargaining. Instead, following the literature on the estimation of dynamic games, I estimate the equilibrium strategy functions from the data and then solve for the parameters that imply that players' actions are optimal given their opponents' expected responses. These techniques, originally developed to analyze firm entry and exit, have rarely been applied outside that context and this study represents their first application to the field of bargaining.

In the specific case studied–the market for local transportation in Jaipur, India–giving traders the option to avoid bargaining and purchase a ride immediately at a fixed price is shown to increase overall welfare by allowing high value buyers to opt out and avoid incurring the disutility of bargaining. However, even with the option of a fixed price, the majority of buyers are estimated to have valuations and bargaining disutility such that they would prefer to remain in the bargaining market, suggesting that, at least for the buyers in this sample, flexible mechanisms such as bargaining retain substantial value. Generalizing these results to consider the counterfactual of a complete switch to fixed prices would require more information about buyers' and sellers' entry choices into the market and matching probabilities, and is a natural next step for future research.

More broadly, structural parameters of bargaining models are of great interest in a variety of settings, for instance in determining the cost of firm/union wage negotiations, or the transactions costs to purchasers of new homes. The choice between bargaining and fixed prices is itself relevant in many other contexts, including markets in developing countries for automobiles, or negotiations between health-care providers and insurance companies. Given data on the series of bargaining offers, the estimation performed in this paper for the autorickshaw market could be applied to any market with bargaining, and the same analysis could be undertaken. Structural analyses of this type have proven to be a robust and useful tool for the analysis of other market mechanisms, in particular auctions, and this paper has taken the first step toward applying them to the rich set of questions in the field of bargaining.

Tables

Table 1.1: Summary Statistics of Bargaining

	Mean	Median
Number of Bargaining Interactions	2993	
Real bargaining	624	
Scripted bargaining	2369	
Percentage ending in trade:	66.51%	
Number of periods per interaction:	4.93	5.00
	(1.25)	
Total duration of an interaction: (seconds)	:55	:49
	(:36)	
Length of time between autorickshaws	3:49	2:25
	(5:14)	
Distance of trip (kms)	5.03	4.87
	(1.22)	
Driver initial offer	56.26	60.00
	(10.70)	
Passenger counteroffer	34.68	35.00
	(8.50)	
Final price if trade occurs	41.29	40.00
	(7.73)	

All details of bargaining calculated from real bargaining interactions. Standard errors in parentheses

Table 1.2: Summary Statistics of Drivers

	Mean	Median
Total number of driver interviews	678.00	
Driver age	37.44	36.00
	(10.27)	
Years spent as autorickshaw driver	10.30	8.00
	(8.51)	
Percentage renting the autorickshaw	0.35	7.50
	(8.13)	
Rental rate	166.97	186.67
	(52.42)	
Daily revenue	383.22	400.00
	(115.43)	
Daily profit (not including rental)	243.66	200.00
	(96.76)	
Percentage renting the autorickshaw	46%	
Rental rate	177.17	200.00
	(44.48)	
Daily revenue	494.31	500.00
	(130.19)	
Daily profit (not including rental)	267.15	250.00
	(103.97)	

Standard deviations in parentheses

Table 1.3: Players' Actions in Each Bargaining Round

			Action		
Round	Player:	N	Accept	Counteroffer	Exit
1	Driver	624	0%	100%	0%
2	Passenger	624	0%	100%	0%
3	Driver	617	1%	99%	0%
4	Passenger	523	10%	83%	8%
5	Driver	435	44%	34%	22%
6	Passenger	165	46%	37%	16%
7	Driver	68	65%	20%	15%
8	Passenger	15	73%	20%	7%
9	Driver	3	100%	0%	0%

Actions reported only for bargaining interactions in which the surveyor was the residual claimant.

Table 1.4: Estimated Driver's Parameters - Log-normal Types:

Distance	Mean	Std. Deviation	Correlation with bargaining dis-utility
2 km	41.86	0.25	-0.01
	(5.48)	(6.90)	(0.16)
3 km	42.18	3.03	-0.12
	(2.80)	(13.20)	(0.15)
4 km	46.14	4.09	-0.15
	(2.98)	(1.96)	(0.11)
5 km	66.14	13.85	-0.37
	(4.17)	(1.83)	(0.09)
6 km	56.49	2.11	-0.06
	(3.18)	(2.37)	(0.10)
7 km	49.99	6.71	-0.23
	(5.43)	(6.11)	(0.13)
8 km	56.08	10.80	-0.34
	(4.24)	(11.86)	(0.15)

Table 1.5: Drivers' Bargaining Disutility

Mean	Std. Deviation
0.31	0.11
(0.17)	(0.19)

Table 1.6: Estimated Passengers' Parameters - Log-normal Types:

Distance	Mean	Std. Deviation	Correlation with bargaining dis-utility
2 km	31.38	18.05	0.00
	(12.31)	(8.72)	(0.21)
3 km	25.61	9.31	0.00
	(8.64)	(3.95)	(0.22)
4 km	46.14	12.20	0.00
	(4.57)	(4.57)	(0.13)
5 km	49.30	6.81	0.00
	(4.79)	(3.84)	(0.10)
6 km	49.86	6.10	0.00
	(1.74)	(1.44)	(0.07)
7 km	56.17	12.76	0.00
	(5.98)	(6.99)	(0.10)
8 km	87.88	37.96	0.00
	(19.84)	(21.66)	(0.25)

Table 1.7: Passengers' Bargaining Disutility

Mean	Std. Deviation
0.57	2688.75
(2.88E+06)	(2.70E+21)

Table 1.8: Percentage of drivers with costs below meter rate

	Official meter rate			Drivers' proposed rate		
	Price	Cost- high bound	Cost- low bound	Price	Cost- high bound	Cost- low bound
2 km	rs. 23	27.78%	55.56%	rs. 29	61.11%	66.67%
		(14.67%)	(14.98%)		(12.78%)	(12.94%)
3 km	rs. 29	13.66%	13.66%	rs. 36	42.44%	42.44%
		(5.00%)	(4.97%)		(11.44%)	(11.40%)
4 km	rs. 35	18.60%	18.60%	rs. 43	55.14%	55.14%
		(9.99%)	(10.04%)		(8.34%)	(8.37%)
5 km	rs. 41	38.75%	38.75%	rs. 50	74.79%	74.79%
		(16.74%)	(16.74%)		(20.18%)	(20.13%)
6 km	rs. 47	32.60%	32.60%	rs. 57	66.89%	66.89%
		(9.61%)	(9.62%)		(6.39%)	(6.38%)
7 km	rs. 53	31.78%	31.78%	rs. 64	95.33%	96.26%
		(12.32%)	(12.42%)		(4.92%)	(5.02%)
8 km	rs. 59	45.24%	46.03%	rs. 71	99.21%	99.21%
		(13.84%)	(13.65%)		(1.78%)	(1.78%)

Bootstrapped standard errors in parentheses.

Figures

Figure 1-1: Market Timeline

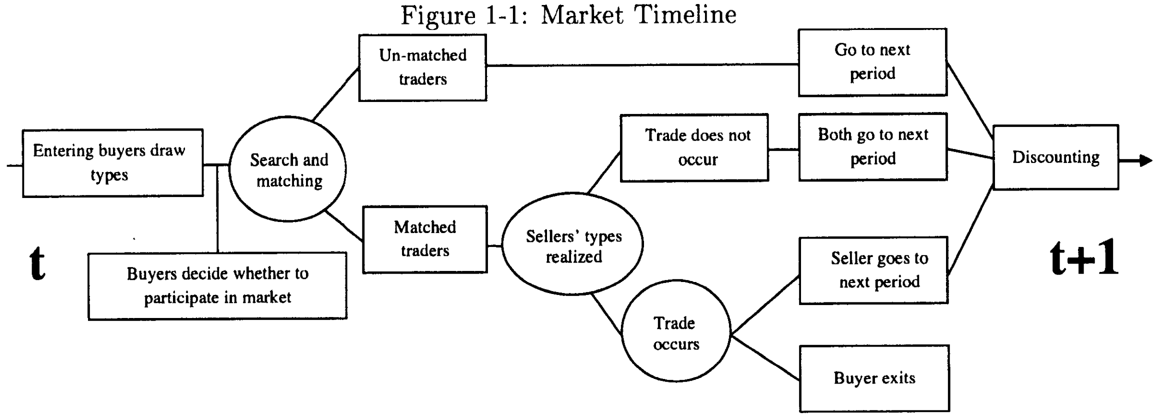


Figure 1-2: Extensive Form

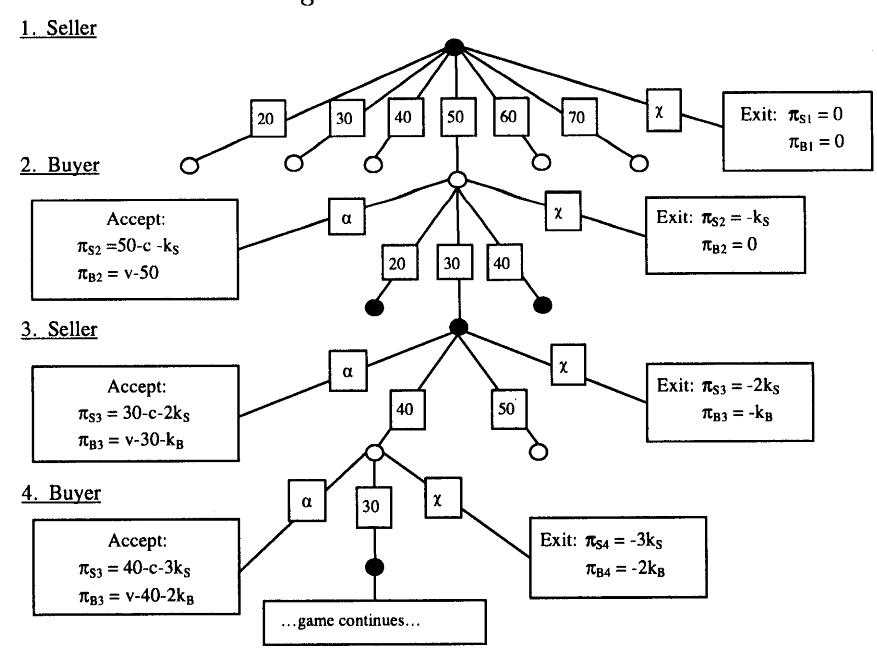


Figure 1-3: Exit Probabilities

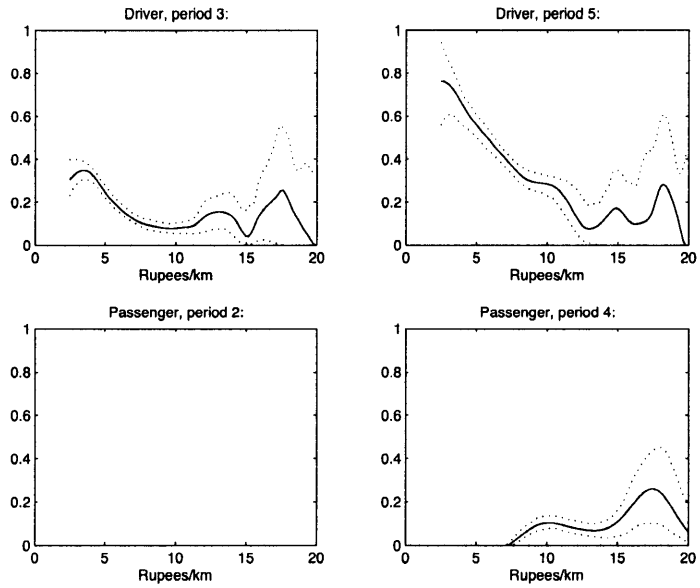


Figure 1-4: Accept Probabilities

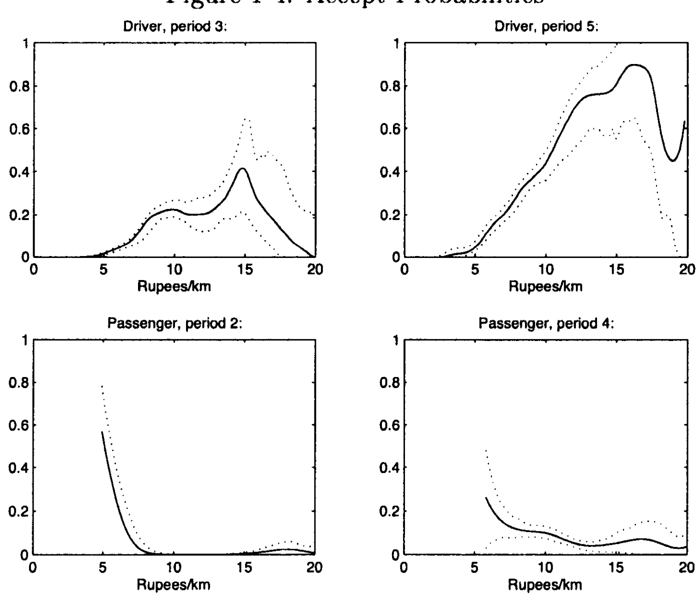


Figure 1-5: Counteroffer Probabilities

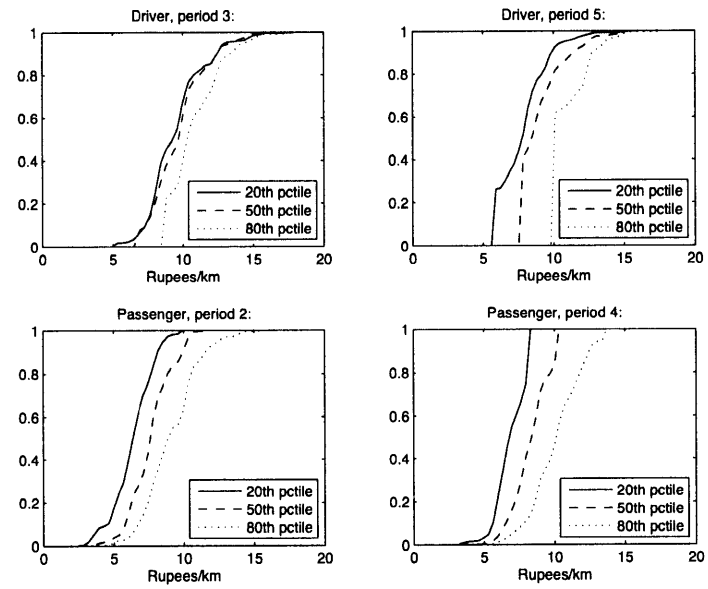


Figure 1-6: Driver Costs

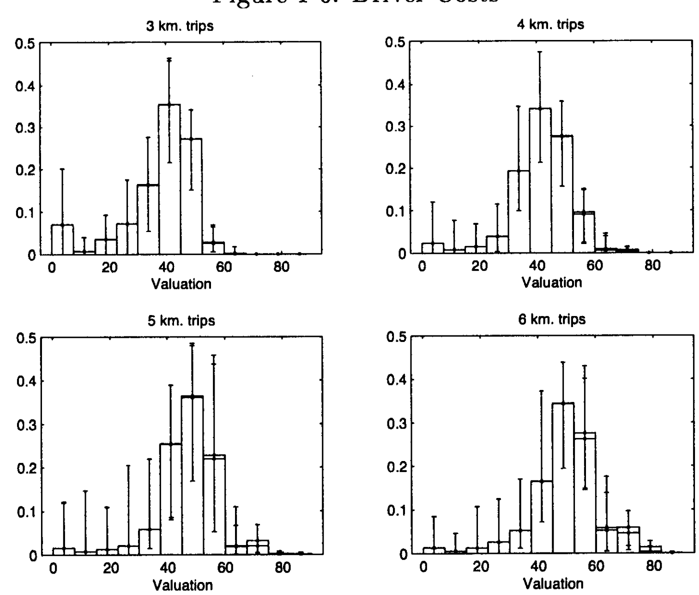


Figure 1-7: Driver Bargaining Disutility

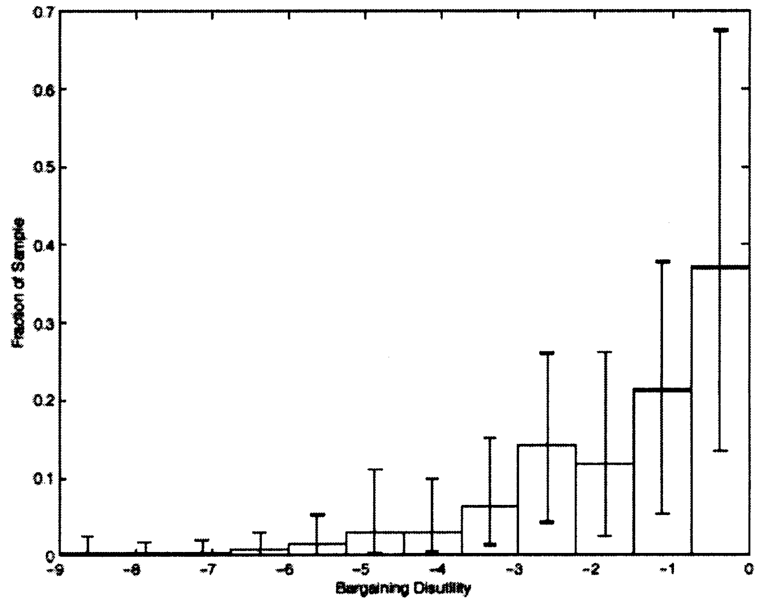


Figure 1-8: Passenger Valuations

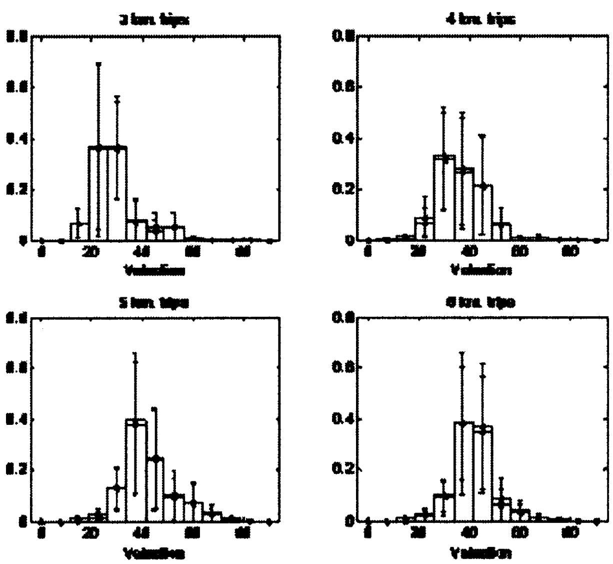


Figure 1-9: Passenger Bargaining Disutility

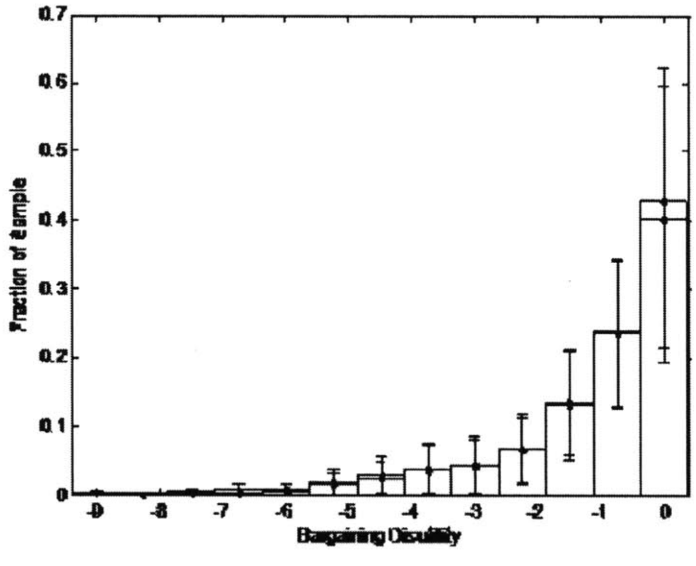


Figure 1-10: Driver Costs and Bargaining Disutility - 4 km Trips

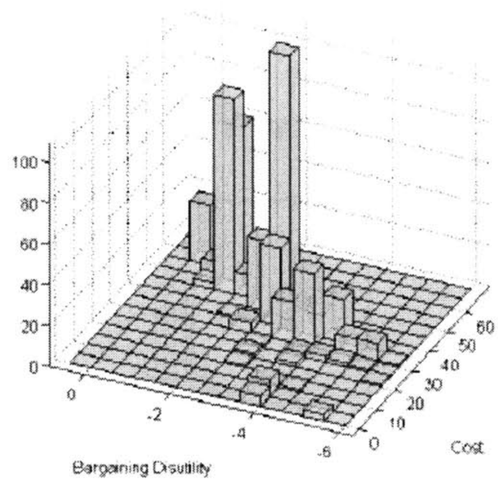
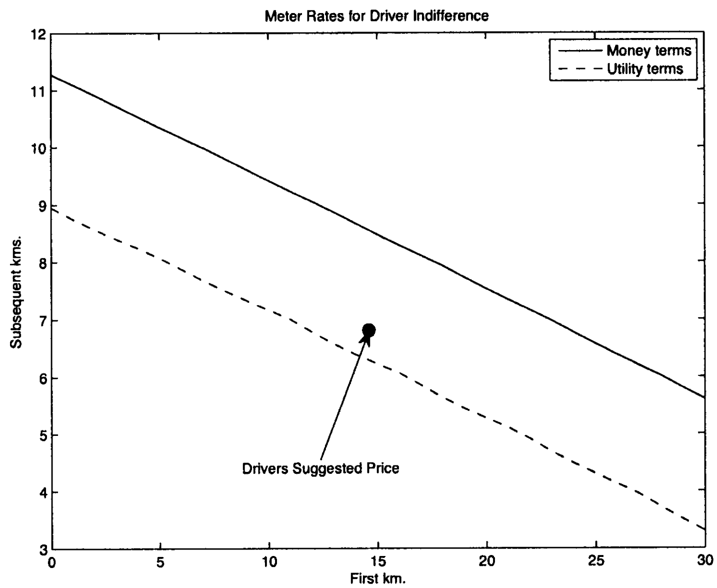


Figure 1-11: Set of Prices such that Drivers are Indifferent Between Fixed Price and Bargaining:



Chapter 2

Experimental vs. Structural Estimates of the Return to Capital in Microenterprises

Introduction

The estimation of production functions, in particular the measurement of the returns to capital, has a long history and a wide scope. Topics as diverse as the macroeconomic study of growth, the role of competition in driving productivity, and the effect of market imperfections on output all rely on a model of how firms transform inputs into outputs, and may depend critically on the parameters of that model. The importance of the issue, and the difficulties inherent in the estimation, have given rise to a substantial literature on structural estimation techniques, recently reviewed in Ackerberg, Benkard, Berry, and Pakes (2007). This paper contributes to this literature by comparing the production parameters estimated using these structural techniques with those derived from an instrumental variables regression of output on capital stock, with capital instrumented by random cash shocks to the firm. I make use of a unique dataset and randomized experiment carried out by De Mel, McKenzie, and Woodruff (2008b) (henceforth DMW) in which the authors implemented a program through which randomly chosen Sri Lankan microenterprises were given grants of various sizes (either in cash or physical capital or materials) and their subsequent production activities were carefully recorded over a two-year period. This data allows a comparison between structural estimators that may be applicable to a wide range of observational data (but depend upon stronger assumptions) and the results of randomized trials that is robust to a wide range of potential sources of bias.

The population of microenterprises in Sri Lanka is a highly specific one–De Mel, McKenzie and Woodruff surveyed only firms with less than about $1000 in capital (not including land or buildings)–and the success or failure of the structural estimators in this context does not imply they might not perform differently in another context, for example one in which markets are better developed but inputs are less flexible. Nevertheless, the parameters of production functions of microenterprises are of significant interest in their own right. The broad role of microenterprises in developing countries is the subject of a lively debate–one perspective argues that they may be high productivity firms held back by credit constraints or other frictions, while another view is that informal enterprises serve as a low-returns safety net for individuals excluded from the formal sector (Porta and Shleifer 2008). A broader knowledge of the production functions of microenterprises is clearly an important factor in this debate, and hence in informing policy towards the informal sector. Another source of interest in the returns to capital of microenterprises lies in its implications for the functioning of markets. As discussed in Banerjee and Duflo (2009), with perfectly functioning capital markets all firms should have the same risk-adjusted return to capital, which in turn should be equal to the interest rate. Estimating the extent and sectors in which this prediction does not hold true may then inform us of the extent of capital market imperfections in the broader economy.

Despite the broad concensus on the importance of understanding production functions, estimation has proved difficult for a variety of reasons. First, since unobserved shocks to productivity will affect both output and a firm's choice of inputs, a naive regression of production on input use may lead to biased coefficients. Second, if productivity shocks are serially correlated and firms exit the market after a negative shock, then any dataset of active firms will only contain those with relatively positive productivity shocks, again potentially biasing the results. Finally, a third challenge lies in the potential collinearity of variable and dynamic inputs. If firms choose variable inputs (e.g. labor or raw materials) as a deterministic function of dynamic state variables (e.g. capital), then the effect of these variable inputs may be impossible to separate from the dynamic inputs (Bond and Soderbom 2005).

These challenges have inspired a substantial literature on estimation techniques to recover the true parameters of production functions. One strand, beginning with Olley and Pakes (1996) has employed a control function approach to include proxies for a firm's productivity shock and propensity to exit within the estimation equation. Another approach, culminating in Blundell and Bond (2000) applies dynamic panel techniques to production function estimation, using lagged differences of variable and dynamic inputs as instruments in a GMM framework. Finally, DMW's approach advances the literature on instrumental variables estimates of production functions, a literature that has previously been hindered by the lack of plausible instruments with substantial interfirm variation (Ackerberg, Benkard, Berry, and Pakes 2007).

DMW resolve this difficulty by giving randomly selected firms lump sums of money or physical materials or capital, then using these random grants as instruments for capital in the production function. Firms were divided into four groups, receiving either 10,000 LKR in cash, 20,000 LKR cash, 10,000 LKR worth of materials or capital, or 20,000 LKR of materials or capital. This randomized approach is uniquely feasible in DMW's setting of microenterprises in Sri Lanka, since the firms are small enough (all had total non-building/land capital of less than 100,000 LKR) that the sums of money randomly allocated by the researchers represent a substantial cash shock.

This paper compares the randomized and structural estimation techniques using the same dataset of microenterprises used in DMW. Despite concerns of potential misspecification of both estimators, I find the results to be broadly similar, suggesting a role for structural estimation of production functions to reveal more insights into firms in developing economies. Section 2.1 develops a simple and standard model of firm production, and introduces the structural and IV estimators in the context of this model. Section 2.3 reviews the techniques for instrumental variables and structural estimation of production functions and discusses their application in the context of incomplete markets. Finally, section 2.4 provides a brief overview of the data, including some summary statistics that may inform the models, then presents and contrasts the results of each of the estimators.

2.1 Dynamic Firm Production

In order to motivate the estimation procedures, as well as to assist in intepreting the resulting parameters, this section lays out a model of a micro-entrepreneur solving a dynamic consumption and investment problem subject to random productivity and liquidity shocks[1]. The entrepreneur maximizes her discounted lifetime flow of consumption $\mathbb{E}\left[\sum_{t=0}^{\infty}\beta^t u\left(c_t\right)\right]$ where $u\left(c_t\right)$ is an increasing and concave utility function. She enters each period with some financial assets $A_t$and physical capital K_t, then earns the return on the assets $\left(1+r\right)A_t$ and operates his business, earning profits of π_t, defined as

$$\pi_t = F\left(K_t, X_t, \omega_t\right) - pX_t$$

where $F\left(K_t, X_t, \omega_t\right)$ is the production function transforming capital, K_t and a vector of static inputs, X_t (e.g. materials, labor, fuels, etc.) into a final good sold at a price normalized to 1[2]. The definition of profit used here and throughout the paper does not include the rental cost of capital employed, rK_t, a formulation that seems to correspond to that used by the surveyed entrepreneurs, only 1.9% of whom reported paying any rent for machinery or equipment in a given period.

A random period-specific productivity shock, ω_t is realized at the beginning of each period but unknown to the entrepreneur in advance. I allow these productivity shocks to be serially correlated, thereby including, at the extreme case of $\omega_t = \omega_{t-1}$, the possibility of firm-level fixed effects. Entrepreneurs also receive an IID liquidity shock Z_t, which could represent an unexpected cash transfer such as an inheritance, or in this context, the randomized transfer from the researchers.

[1]While all the substantive insights of the model also hold in a static setting, the identification of the structural estimators in section 2.3 depends critically on the timing of productivity shocks and capital accumulation across periods. Hence for consistency I present the firm's problem as dynamic in this section as well.

[2]The model presented here does not allow entrepreneurs to shut down their firms and exit the market. Introducing this option would change little of substance for the conditions determining capital and input use for those firms selecting to remain in the market (Pakes 1996) while substantially increasing the notation. The exit choice is discussed further in section 2.3, and the structural techniques discussed there allow for this possibility.

Finally, at the end of the period the owner of the firm invests in capital that is realized next period, and chooses how much to save and consume. Capital depreciates between periods at rate δ and thus evolves according to

$$K_{t+1} = (1-\delta)\, K_t + i_t$$

where i_t is the investment made in period t. Rewriting the entrepreneur's problem in recursive form yields

$$V(A_t, K_t, \omega_t) = \max_{X, K_{t+1} A_{t+1}} u(c_t) + \beta E\left[V(A_{t+1}, K_{t+1}, \omega_{t+1}) \mid \mid \omega\right]$$

subject to

$$c_t = F(K_t, X_t, \omega_t) - pX_t + (1+r)\, A_t - A_{t+1} + K_{t+1} - (1-\delta)\, K_t + Z_t$$

where the potential for serial correlation in the productivity shocks, ω_t, necessitates their inclusion in the vector of state variables. Solving for the entrepreneur's choice of investment and savings leads to the standard Euler equation and solution for the marginal return to capital,

$$\frac{u'(c_t)}{E\left[u'(c_t) \mid \omega_t\right]} = \beta(1+r) \tag{2.1}$$

$$rE\left[u'(c_{t+1}) \mid \omega_t\right] = E\left[\left(F_1'(K_{t+1}, X_{t+1}, \omega_{t+1}) - \delta\right) u'(c_{t+1}) \mid \omega_t\right] \tag{2.2}$$

$$F_2'(K_t, X_t, \omega_t) = p \tag{2.3}$$

In the case of perfect insurance markets, the condition determining investment (equation 2.2) reduces to

$$\mathbb{E}\left[F_1'(K_{t+1}, X_{t+1}, \omega_{t+1}) \mid \omega_t\right] = r + \delta$$

and investment is chosen to set the future expected returns to capital equal to the depreciation-adjusted interest rate. The current period liquidity shock does not affect the entrepreneur's choices of capital or inputs, and DMW's randomized one-time grants of cash to firms would be allocated primarily to savings, a prediction not born out in the data where only 12% of grant recipients chose to save the amount they have been given. Recipients of grants in the form of materials or capital might increase their production if they could not resell their granted stocks, but returns would be below the market interest rate, a prediction that DMW argue conflicts with the results found in the data.

While cash shocks should have no impact on production if markets are complete, the productivity shock directly affects the choice of X_t and, if these shocks are serially correlated, also the choice of next period's capital stock. Pakes (1996) shows that if the production function has increasing differences in K_t and ω_t, then investment will be monotonically increasing in the ω_t shock. OLS estimates of the return to capital generated by naive regressions of output on observed K_t and X_t will typically be biased upwards by the endogeneity of the capital choice-the original observation that inspired the literature on structural estimation of production functions.

The simple conditions derived above are founded upon the assumption of complete credit markets, an assumption that DMW (among others) argue is unlikely to hold for the microenterprises found in the Sri Lankan data. More plausibly, these firms face some constraints on their borrowing, constraints that I model as the limiting the total inputs and investment purchased for the business to being less than some multiplier λ of the firm's current assets, capital, and liquidity shock:

$$i_t + pX_t \leq \lambda \left(A_t + K_t + Z_t\right) \tag{2.4}$$

If this constraint binds, the convenient separation between the entrepreneur's cash shocks and the production process no longer holds. In particular, investment is determined by the condition

$$\mathbb{E}\left[u'\left(c_{t+1}\right) F_1'\left(K_{t+1}, X_{t+1}, \omega_{t+1}\right) \mid \omega_t\right] = (r+\delta)\,\mathbb{E}\left[u'\left(c_{t+1}\right) \mid \omega_t\right] + \frac{\mu_t - \beta\left(1-\delta\right)\mathbb{E}\left[\mu_{t+1} \mid \omega_t\right]}{\beta}$$

where μ_t and μ_{t+1} are the Lagrange multipliers on the current and future credit constraints.

The current period cash shock Z_t now affects capital choice by loosening the current period credit constraint, thereby decreasing the Lagrange multiplier, decreasing the optimal future returns to capital, and increasing current period investment. If there is any heterogeneity across firms, either in the current productivity shock or in the degree to which firms are credit constrained, the effects of the random grant on investment will vary across firms in a manner correlated with the returns to capital.

A similar condition defines the firm's choice of static inputs,

$$F_2'\left(K_t, X_t, \omega_t\right) = p\left(1 + \frac{\mu_t}{u'\left(c_t\right)}\right) \tag{2.5}$$

whose marginal return may also be set above cost if credit constraints are binding. Again, the random grant will increase input use, potentially to a heterogenous extent across firms, if there is any heterogeneity in K_t, A_t, or ω_t.

2.2 Reduced Form and IV Estimation of Production functions

Perhaps the main benefit of the randomized experiment of giving grants to microenterprises is that it allows for a direct and robust reduced form regression of firm profits on the size of the random grant given to the firm,

$$\pi_{it} = \gamma_0 + \gamma_1 Z_{it} + \varepsilon_{it}$$

where (assuming for the moment that there is continuous variation in the grant amounts, Z_{it}) the γ_1 coefficient expresses the return to additional funds. Expressing this coefficient in terms of the model developed above, I fully differentiate profits with respect to Z_t,

$$\begin{aligned}\frac{d\pi_{t+1}}{dZ_t} &= \frac{d}{dZ_t}\left(F\left(K_{t+1}, X_{t+1}, \omega_{t+1}\right) - pX_{t+1}\right) \\ &= \frac{\partial F\left(K_{t+1}, X_{t+1}, \omega_{t+1}\right)}{\partial K_{t+1}}\frac{\partial K_{t+1}}{\partial Z_t} + \frac{\partial F\left(K_{t+1}, X_{t+1}, \omega_{t+1}\right)}{\partial X_{t+1}}\frac{\partial X_{t+1}}{\partial Z_t} - p\frac{\partial X_{t+1}}{\partial Z_t} \qquad (2.6)\end{aligned}$$

Equation 2.6 makes clear that the reduced form parameter differs substantially from the literal form of the return to capital $\partial F\left(K_t, X_t\omega_t\right)/\partial K_t$. In particular, even if the returns to fixed capital are zero, credit constrained firms' will still rise after receiving the grant due to increased purchases of materials, inventories, or labor. DMW report that 57% of entrepreneurs chose to purchase inventories or raw materials, consistent with a scenario in which returns to additional fixed capital may be low relative to the return to materials. Finally, recall that under complete markets one should expect γ_1 to be exactly zero, since any cash windfall should be saved rather than invested in the firm–that DMW find γ_1 to be both economically and statistically significant indicates the presence of substantial market imperfections.

The γ_1 coefficient, or $\frac{d\pi_{t+1}}{dZ_t}$, is an important parameter for policy purposes: it is revealing of the total extent of market imperfections, and, as DMW note, it indicates the total amount that microenterprises would benefit from a government subsidy or a microfinance loan. Furthermore, the robust estimation of $\frac{d\pi_{t+1}}{dZ_t}$ that comes directly from a randomized trial could not easily be derived from through structural estimation, since it depends on the entrepreneur's investment policy function which is itself a function of many unidentified parameters such as δ, β, λ, the form of the utility function, etc. However, knowledge of γ_1 tells us little about the underlying production functions of the microenterprises. To isolate the direct effect of capital on profits, DMW specify a 2SLS regression of profits on capital (in levels), with capital instrumented by the Sri Lankan rupee value of the randomly allocated grant to the firm, and including firm and year fixed effects,

$$\begin{aligned}\pi_{it} &= \beta_0 + \beta_1 K_{it} + \eta_i + \psi_t + \varepsilon_{it} \\ K_{it} &= \alpha_0 + \alpha_1 Z_{it} + \zeta_i + \phi_t + \varepsilon_{it}\end{aligned}$$

where as usual it is assumed that firms' reported profits does not include the rental rate on their fixed capital.

Under certain conditions discussed in detail below, the coefficient β_1 from this regression corresponds to the full differential of profits with respect to capital: the change in firms' profits with respect to an increase in capital, including the effects of any changes in labor or static inputs due to that change in capital. Under complete markets the first stage should be insignificant if Z_{it} is a pure cash grant, but assuming some effect of Z_{it} on K_{it} due to optimization error or because some grants were given in the form of capital, we should expect that $\beta_1 = r$ since firms should borrow to purchase capital until its returns in terms of profits are equal to the interest rate[3].

[3]Under complete markets the partial and full derivatives of profits with respect to capital should be equal, since all

The relationship between the β_1 parameter and the return to capital becomes substantially less clear under incomplete markets. The randomized grant program provides a single instrument, one which affects not only capital choice but also other endogenous variables such as the amount of labor and intermediate inputs purchased. Using profit as a dependent variable resolves this issue under the assumption of perfect labor and intermediate input markets, since the coefficients of a regression of revenue on the static inputs should simply be equal to cost of these inputs, which can then be subtracted from both sides of the equation to yield DMW's prefered specification with profit (not including capital costs) as the dependent variable,

$$\begin{aligned} Y_{it} &= \beta_0 + \beta_1 K_{it} + \beta_2 X_{it} + \eta_i + \gamma_t + \varepsilon_{it} \\ Y_{it} - pX_{it} &= \beta_0 + \beta_1 K_{it} + (\beta_2 X_{it} - pX_{it}) + \eta_i + \gamma_t + \varepsilon_{it} \\ \pi_{it} &= \beta_0 + \beta_1 K_{it} + \eta_i + \gamma_t + \varepsilon_{it} \end{aligned}$$

In the absence of perfect intermediate input markets, or if firms are credit constrained in their choice of X_{it} as in the model in section 2.1, $\beta_2 \neq p$, and it is the shadow costs of inputs that must be subtracted from output to yield consistent estimates of β_1. While DMW carefully consider these issues when subtracting the value of the entrepreneurs' own wages from their reported profits, it is unlikely the entrepreneurs themselves went to the same pains to account for the shadow cost of other static inputs when reporting their own profits. The IV coefficient on capital stock is then

$$\begin{aligned} \hat{\beta}^{IV} &= \frac{\operatorname{cov}(\pi_{it}, Z_{it})}{\operatorname{cov}(K_{it}, Z_{it})} \\ &= \beta_1 + (\beta_2 - p)\frac{\operatorname{cov}(X_{it}, Z_{it})}{\operatorname{cov}(K_{it}, Z_{it})} \end{aligned}$$

Substituting the return to intermediate inputs from equation 2.5 for β_2 and cancelling yields

$$\hat{\beta}^{IV} = \beta_1 + \left(\frac{\mu_t}{u'(c_t)}\right)\frac{\operatorname{cov}(X_{it}, Z_{it})}{\operatorname{cov}(K_{it}, Z_{it})}$$

which shows that the bias in the IV parameter is an increasing function of the Lagrange multiplier on the credit constraint. Thus under a standard model of credit constraints the 2SLS regression of profits on only capital instrumented by the random grant does not yield the true return to capital.

There are several possible means to deal with this ommitted variable bias. The solution chosen by DMW is to introduce fixed effects to control for the time invariant component of $(\beta_2 - p)\, X_{it}$. However, this method will only work if variable input use, X_{it} is relatively constant within a firm,

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other inputs are used to the point where their marginal benefit is exactly equal to their cost, and thus by the envelope theorem a change in capital stock should have no indirect effects. Under incomplete markets, and in particular in the case outlined in section 2.1 where the credit constraint and hence the degree of market imperfection depends on the capital stock, this equivalence will no longer hold.

which does not appear to be the case for the Sri Lankan microenterprises. A more effective technique would be to introduce another instrument for X_{it}. Since DMW distributed four different types of grants, this approach is, in principal, feasible if at least two of the grant types induced independent variation in K_{it} and X_{it} and can then be used as seperate instruments. Unfortunately, as is clear from table 2.3, the first stage regressions of input use on dummy variables for the different grant types are sufficiently noisy that is impossible to statistically distinguish the effects of the different types of grants on different inputs despite point estimates that are often substantially different. Though one may still attempt to use the different grants to identify the effects of different inputs seperately, the standard errors on the resulting second stage estimates will be large due to the collinearity induced by the similar instruments (see, for example, tables 2.6 and 2.7).

Another potential difficulty in the interpretation of the IV estimate of the returns to capital arises when there is heterogeneity in the response of individual agents to treatment programs. As DMW themselves point out, under heterogeneous treatment effects IV and OLS estimators generally converge to different probability limits, corresponding to differently weighted averages of the underlying structural parameters. In particular, if the model is correctly specified, the coefficients from the LP approach correspond to OLS estimates of the returns for capital controlling for the unobserved productivity shock, while the coefficients reported by DMW are the result of 2SLS using the experimental capital grant as an instrument.

These issues are particularly relevant in this context, since many authors (Banerjee and Duflo 2009) have argued that heterogeneity in the returns to capital is a central feature of markets in developing countries. Indeed, the substantial literature on capital misallocation is founded on this premise, and it seems a plausible hypothesis in the context of Sri Lankan microenterprises where financial institutions to redistribute capital to the most productive firms may be lacking. DMW investigate the possibility of heterogeneous returns, but argue that these returns appear to be uncorrelated with the changes in firms' capital levels induced by the random grants. In support of this argument they show that the percentage of the grant invested appears uncorrelated with variables that predict higher returns, for instance proxies for entrepreneur ability, risk aversion, or past profit/capital stock ratios. While reassuring, these results are not entirely robust to the type of serially correlated productivity shocks ω_t presented in section 2.1, which would affect both current output and future capital investment due to the random grant.

To investigate these issues further, consider the a regression of profits on capital, controlling for other inputs and (assuming it can be observed) the productivity shock,

$$\mathbb{E}\left[\pi_{it} \mid X_{it}, \omega_{it}\right] = a + b\mathbb{E}\left[K_{it} \mid X_{it}, \omega_{it}\right]$$

The standard OLS coefficient is,

$$\hat{b}^{OLS} = \frac{\operatorname{cov}\left(\pi_{it}, K_{it} \mid X_{it}, \omega_{it}\right)}{\operatorname{var}\left(K_{it} | X_{it}, \omega_{it}\right)}$$

Following Yitzhaki (1996), the OLS coefficient can be shown to be a weighted average of the returns to capital on profits. Leaving off the conditioning on X_{it} and ω_{it} for conciseness, he shows that,

$$\hat{b}^{OLS} = \frac{1}{\text{var}\left(K_{it}\right)} \int_{-\infty}^{\infty} \frac{\partial \mathbb{E}\left[\pi_{it}|K\right]}{\partial K} w^{OLS}\left(K\right) dK$$

where the OLS weighting function $w^{OLS}\left(K\right)$is

$$w^{OLS}\left(K\right) = \int_{K}^{\infty} \left(\kappa - \mathbb{E}\left[K\right]\right) f_K\left(\kappa\right) d\kappa$$

In the standard case in which the returns to capital are homogeneous, $\frac{\partial \mathbb{E}[\pi_{it}|K]}{\partial K} = \beta$, and the estimator returns the standard OLS result $\hat{b}^{OLS} = \beta$. Alternatively, it is possible that the firm's returns to capital are uncorrelated with their capital levels, for instance in the case where productivity is a IID random variable, and in this case OLS returns $\hat{b}^{OLS} = \int_{-\infty}^{\infty} \frac{\partial \mathbb{E}[\pi_{it}|K]}{\partial K} dK = \bar{\beta}$. Otherwise, the OLS estimator returns a weighted average of the heterogeneous returns, with greater weights on observations with K_{it} values closer to the mean.

A similar set of calculations yields the IV estimator

$$\begin{aligned} \hat{\beta}^{IV} &= \frac{\text{cov}\left(\pi_{it}, Z_{it} \mid X_{it}, \omega_{it}\right)}{\text{cov}\left(K_{it}, Z_{it} \mid X_{it}, \omega_{it}\right)} \\ &= \frac{1}{\text{cov}\left(K_{it}, Z_{it}\right)} \int_{-\infty}^{\infty} \frac{\partial \mathbb{E}\left[\pi_{it}|Z\right]}{\partial Z} \int_{Z}^{\infty} \left(\zeta - \mathbb{E}\left[Z\right]\right) f_K\left(\zeta\right) d\zeta dZ \end{aligned}$$

In general $\hat{\beta}^{OLS} \neq \hat{\beta}^{IV}$, although in the two special cases considered above, homogeneous returns and returns uncorrelated with capital levels, the two estimators will converge to the same results.

A canonical (albeit very specific) case in which many of the problems of heterogeneity are easily resolved is when firms' output has constant elasticity with respect to inputs, as is the case for a Cobb Douglas production function. If all firms have the same Cobb Douglas production function, a log transformation of output and all inputs will remove any heterogeneity due to capital misallocation, and the structural parameters of the production can be recovered directly from the coefficients on the log inputs (assuming any endogeneity issues are resolved). In light of this result, the most robust test of the structural estimators may be to compare IV and structural regressions specified in logs; nevertheless heterogeneity remains a concern since it is possible that firms may be heterogeneous both in returns to capital and in the elasticity of revenue with respect to capital

2.3 Structural Estimation of Production Functions

Starting with Olley and Pakes (1996) a substantial literature has developed on structural estimation of the parameters of production functions using control functions to account for unobserved productivity shocks. In this section I review these techniques as applied to the problem of estimating the production functions of the Sri Lankan microenterprises, largely following the presentation of Levinsohn and Petrin (2003) (henceforth LP) in Ackerberg, Caves, and Frazer (2006)[4] (henceforth ACF). For brevity of notation, firms' production functions are modeled as Cobb Douglas, and intermediate inputs are limited to labor l_{it} and materials m_{it}. Output, y_{it}, is then

$$\log\left(F\left(K_t, X_t, \omega_t\right)\right) = \quad y_{it} = \quad \beta_k k_{it} + \beta_l l_{it} + \beta_m m_{it} + \omega_{it} + \varepsilon_{it} \tag{2.7}$$

where ω_{it} is, as described above, a productivity shock observed by the firm, and ε_{it} is an IID shock to output unobserved by the firm before output is realized, potentially representing measurement error. The assumption of a Cobb Douglas functional form is not essential to the estimation technique, and will be relaxed in the empirical section.

Consistent with the model presented above, firms are assumed to choose capital in a previous period ($t-1$ or earlier), and observe the current period productivity shock at the beginning of each period. I further augment the dynamics of the model developed in section 2.1 to allow firms the option to exit the market after learning their productivity realization, ω_t. Firms will avail themselves of this exit option if the current productivity shock is low enough that the expected returns of remaining in business are below the sell-off value of the firm. Following Olley and Pakes, the solution to the firm's exit decision can then be modelled as an indicator function χ_t equal to one if the firm remains in business, and a cut-off value of the productivity shock $\underline{\omega}_t\left(k_t\right)$ such that

$$\chi_{it} = \begin{cases} 1 & \text{if } \omega_{it} \geq \underline{\omega}\left(k_{it}\right) \\ 0 & \text{otherwise} \end{cases}$$

The selection on unobservables inherent in the firms' exit decision introduces another potential source of bias. Olley and Pakes show that if firms' gains from staying open during a bad shock are increasing in the size of their capital stock, then $\underline{\omega}\left(k_{it}\right)$ will be decreasing in k_{it} and low productivity realizations will only be observed for firms with large k_{it}. The ensuing negative correlation between ω_{it} and k_{it} will thus bias downward the coefficient on capital.

After the exit decision has been made, firms that decide to remain in the market simultaneously select labor and material inputs. LP's estimation technique is based on the observation that in perfect markets the firm's choice of intermediate inputs

$$m_{it} = \iota\left(k_{it}, \omega_{it}\right)$$

will be monotonically increasing in the productivity shock under a wide variety of conditions. However, with credit constraints or other market imperfections, this montonicity may only hold condi-

[4]The assumptions underlying the original estimation technique developed by Olley and Pakes are such that the method may only be used on observations containing non-zero investment. Since only 11% of the firm-survey rounds in the DMW Sri Lankan firm dataset satisfy this condition, I focus on techniques developed by Levinsohn and Petrin (2003) and Ackerberg, Caves, and Frazer (2006) that do not impose this requirement.

tional on other variables[5]. For instance, in the model in section 2.1, equation 2.5 shows the choice of inputs X_{it} to be a function not only of K_{it} and ω_{it}, but also of the marginal utility and the Lagrange multiplier. While in this particular model expanding the intermediate input function to include A_{it} and Z_{it} would restore conditional monotonicity, the broader point is that monotonicity cannot be taken for granted in models of imperfect markets.

Maintaining the monotonicity hypothesis (while bearing this caveat in mind), the intermediate input function can be inverted to generate a proxy for unobserved productivity as a function of the observed choices of capital and intermediate inputs

$$\omega_{it} = \iota^{-1}(k_{it}, m_{it}) \tag{2.8}$$

While the form of the $\iota^{-1}(\cdot)$ function is generally unspecified, it can be approximated by a nonparametric function of capital and intermediate inputs, $g(k_{it}, m_{it})$ (either using a high order polynomial or a local linear regression) and this proxy function can be substituted for the productivity shocks in equation 2.7 to yield

$$\begin{aligned} y_{it} &= \beta_k k_{it} + \beta_l l_{it} + \beta_m m_{it} + g(k_{it}, m_{it}) + \varepsilon_{it} \\ &= \beta_l l_{it} + \Phi(k_{it}, m_{it}) + \varepsilon_{it} \end{aligned} \tag{2.9}$$

Given some source of exogenous variation in l_{it} (a point discussed further below), a regression of y_{it} on l_{it} and this non-parametric function of k_{it} and m_{it} yields a consistent estimate of β_l, but leaves β_k and β_m unidentified. These parameters can, however, be identified by the timing assumptions that current period capital stock and previous period variable input choices are uncorrelated with the current period innovation in the productivity shock. Let this innovation be defined as ξ_t, where

$$\xi_{it} = \omega_{it} - \mathbb{E}[\omega_{it} | \omega_{t-1}, k_{it}, \chi_{it} = 1] \tag{2.10}$$

Equation 2.10 demonstrates again the importance of incorporating the exit decision into the estimation procedure, since firms with low current capital stock may be expected to have higher future productivities if they choose to continue operation. OP show that the final term, the expectation of the current productivity shock conditional on the firm's continued operation, can be expressed as a function of the lagged survival probability of the firm, $P_{t-1} = \Pr\{\chi_t = 1 \mid \underline{\omega}_t(k_t), \omega_{t-1}\}$, and the lagged productivity shock,

$$\begin{aligned} \mathbb{E}[\omega_{it} | \omega_{t-1}, k_{it-1}, \chi_{it} = 1] &= j(\underline{\omega}_t(k_t), \omega_{t-1}) \\ &= j(P_{t-1}, \omega_{t-1}) \end{aligned}$$

[5] I thank Michael Peters for bringing this issue to my attention

which follows from the fact that the survival probability can be represented as a function of the productivity cut-off $\underline{\omega}_t(k_t)$ which can then be inverted and the resulting expression for $\underline{\omega}_t(k_t)$ in terms of P_{t-1} substituted into the $j(\cdot)$ function.

LP show that, conditional on coefficients for the capital and materials parameters, both $\hat{\omega}_{it}(\beta_k, \beta_l)$ and $\hat{\xi}_t(\beta_k, \beta_l)$ can be recovered using the results of the first stage estimates: $\hat{\omega}_{it}(\beta_k, \beta_l)$ from the relation

$$\hat{\omega}_t(\beta_k, \beta_l) = \hat{\Phi}(k_{it}, m_{it}) - \beta_k k_{it} - \beta_m m_{it}$$

and $\hat{\xi}_t(\beta_k, \beta_l)$ from the residuals of a regression of $\hat{\omega}_t$ on a nonparametric function $h(\cdot)$ of $\hat{\omega}_{t-1}$ (again by either local linear regression or polynomials):

$$\hat{\omega}_t(\beta_k, \beta_l) = h\left(\hat{P}_t, \hat{\omega}_{t-1}(\beta_k, \beta_l)\right) + \hat{\xi}_t$$

where $\hat{P}_t$ is the predicted survival probabilities from a first stage non-parametric regression of χ_{it} on k_{it-1} and i_{it-1}.

Finally, the capital and intermediate input parameters are chosen to set these productivity innovations to be uncorrelated with current capital stock and past period intermediate inputs, generating the moment conditions,

$$\mathbb{E}\left[\xi_{it}(\beta_k, \beta_l) \cdot \begin{pmatrix} k_{it} \\ m_{it-1} \end{pmatrix}\right] = 0$$

A key element of this estimation strategy is the presence of a source of variation in l_{it} which is independent of the other factors of production, but does not directly affect output or expectations of future returns to labor or capital. Without this, as ACF demonstrate, the β_l coefficient will not be identified in equation 2.9. While ACF are skeptical that such variation exists, it may indeed be present in the context of the Sri Lankan microenterprise data. DMW document that firms frequently purchase materials several months prior to using them in the production process, whereas labor, in particular labor of the business owner herself, may be more flexibly decided after the purchase of inputs and subject to shocks to the business owner's opportunity cost of time.

Nevertheless, in the absence of independent variation in l_{it}, ACF propose an estimation strategy that allows for the alternative, perhaps more realistic production process in which labor is still determined after intermediate input production, but the choice of labor is correlated with the productivity shock. More formally, assume that each period has an intermediate stage, at time $t-b$, when intermediate inputs m_{t-b} are purchased, and that the productivity shock ω_{it} evolves according to a first order Markov process between these stages, i.e.

$$p(\omega_{it}|I_{it-b}) = p(\omega_{it}|\omega_{it-b}) \text{ and } p(\omega_{it-b}|I_{it-1}) = p(\omega_{it-b}|\omega_{it-1})$$

In this case labor use is a function of both the productivity shock, and the time $t-b$ choice of materials, and these factors must now enter into the inversion used to recover ω_t,

$$\omega_t \quad = \quad g\left(k_{it}, m_{it-b}, l_{it}\right)$$

Substituting this new augmented function into equation 2.9 makes it clear that the coefficient on labor is no longer identified in the first stage. However, the second stage may proceed as in the Levinsohn-Petrin procedure, with the productivity shocks recovered, conditional on a set the $\{\beta_k, \beta_l, \beta_m\}$ parameters from

$$\hat{\omega}_t\left(\beta_k, \beta_l\right) \quad = \quad \hat{\Phi}\left(k_{it}, m_{it}, l_{it}\right) - \beta_k k_{it} - \beta_l l_{it} - \beta_m m_{it}$$

and the final moment conditions of

$$\mathbb{E}\left[\xi_{it}\left(\beta_k, \beta_l, \beta_m\right) \cdot \begin{pmatrix} k_{it} \\ m_{it-1} \\ l_{it-1} \end{pmatrix}\right] \quad = \quad 0$$

While these techniques account for several potential sources of bias, they rely on assumptions about the timing and shocks to the production process; (Bond and Soderbom 2005) assumptions that have remained largely untested due to the lack of a clear baseline.

2.4 Sri Lankan Microenterprise Data and Results

DMW provide a detailed description of the data collection and randomized allocation of grants to microenterprises in their main paper, as well as additional details in a companion paper on measuring firm profits (De Mel, McKenzie, and Woodruff 2008a). In this section I briefly review the setting and approach of their experiment, as well as report some summary statistics of particular interest in the context of structural estimation of production functions. Readers interested in learning more are encouraged to refer to the more detailed descriptions in DMW's original work.

DMW's preferred sample contains data on 408 enterprises operating in southern and southwestern Sri Lanka in April 2005. [6] The owner/managers of these firms were selected from a baseline household census, on the criterion that they were self-employed workers outside of agriculture, fishing, transportation, and professional services, between the ages of 20 and 65, had less than 100,000 LKR in capital (about \$1000) net of land and buildings, had no paid employees, and were not directly affected by the December 2004 tsunami. The selected firms were roughly evenly split between retail sales establishments (203 firms), typically small grocery stores, and manufacturing firms (205

[6]The full dataset, including those directly affected by the tsunami, contains 618 firms. I restrict the sample to that used by DMW for ease of comparability.

firms). There is substantial variety in the primary output of the manufacturing firms, with the most common activities being sewing clothes (62 firms), food production (38 firms), spinning lace (36 firms) and making bamboo products (29 firms).

The sample firms were interviewed quarterly for over two years, for a total of nine rounds of data in DMW's preferred sample. In each survey round, the firm owners were administered a detailed questionnaire in which they reported expenditures on materials, labor, inventories and investment, as well as revenues and profits. There is a substantial discrepancy between reported profits and profits implied by reported revenues and expenditure. DMW (2008a) investigate this discrepancy and attribute it largely due to mis-timing of purchasing of materials and inventories versus sales of finished goods.

The richness of the data yields a variety of variables that could serve as proxies for the unobserved productivity shock. All firms report hours of labor used by both the entrepreneur himself or herself, the hours of family labor, and the hours of paid non-family labor. In the presence of credit constraints perhaps the most promising of these is the firm owner's own labor, since this is both very flexible and hence likely to be correlated with current period productivity, as well as relatively unlikely to be constrained by financial frictions. The quantity of materials purchased during the past month is another potential proxy, as is the amount of fuel/electricity used, although both of these may fail the monotonicity condition if highly credit constrained firms with large positive productivity shocks purchase less of them than unconstrained firms with lower productivity shocks. Another potential proxy comes from the survey question that asked entrepreneurs how much of the material inputs purchased during the past month had actually been transformed into outputs and sold during that month: as long as firms use less inputs than their total purchased amount (true in 92% of firm/survey round observations), this quantity may be relatively less affected by credit constraints.

Firm exit occurs relatively frequently in the microenterprise data: of the 385 firms in the DMW sample, there are 73 cases in which firm owners report changing their line of business from what had been their activity during the previous survey firm visit. Exit occurs for a variety of reasons tabulated on table 2.2, the most commonly specified (25 cases) being that the business was making a loss. These may to be cases where, much as in Olley and Pakes' model, firms experiencing negative productivity shocks exit the market. 10 firms exited due to sickness of the entrepreneur, which might be interpreted as a positive shock to the cost of labor (and a shock which, under perfect markets, we would not expect to affect the firm). Exit from one business did not necessarily imply a permanent exit from entrepreneurship: 30 of the 73 entrepreneurs in the sample who exited or switched businesses subsequently began new businesses with some non-land or building based capital.

2.4.1 Results

To establish a baseline against which to compare the IV and structural estimators of production functions, I first estimate OLS regressions of revenue and profits on capital, variable inputs, and other entrepreneur characteristics, and present the results (using only data from firms in the control group) in tables 2.4 and 2.5. The most striking feature of the OLS regressions of revenue on inputs in table 2.4 is the lack of effect of including labor (total hours of family plus hired labor used in the past week) in the regression on the capital coefficient. In specifications with either revenue in levels (columns 1 and 2) or in logs (columns 6 and 7) as the dependent variable, the inclusion of labor in the regression has no significant effect on the capital coefficient, suggesting a lack of complementarities between capital and labor. This result contrasts markedly with the predictions of the canonical Cobb Douglas production function, where the effect of introducing labor into the log-revenue specification (abstracting from concerns about endogeneity or credit constraints) should be to scale the capital coefficient by one minus the coefficient on labor. Results for other variable inputs are more in line with standard predictions: controlling for materials and inventory use in columns 4 and 9 substantially decreases both labor and capital coefficients. Introducing fixed effects reduces the capital and materials coefficients but has little effect on the estimated labor coefficient.

OLS regressions of profits on variable inputs and capital in table 2.5 are qualitatively similar, although of course subject to the same strong caveats of endogenity that make the results difficult to interpret[7]. Again, labor seems almost uncorrelated with capital use, which is strongly correlated with materials. Introducing firm-level fixed effects in columns 5 and 10 substantially reduces the capital coefficient, making it insignificant in the regression on capital in levels. As DMW note, women appear, on average, to earn lower profits than men, and curiously education and owner age are significant and negative in some specifications.

Tables 2.6 and 2.7 present the same regressions, now using dummy variables for DMW's four treatment types (10,000 LKR cash grant, 10,000 LKR in-kind grant, 20,000 LKR cash grant, and 20,000 LKR in-kind grant) as instruments for endogenous inputs. Columns 1, 2, 6 and 7 of each table present regressions of revenues and profits (in levels and logs) on capital, controlling for fixed effects in columns 2 and 7. Both profit and revenue coefficients are substantially greater than the OLS coefficients, although these differences are not always significant due to the large 2SLS standard errors. Including fixed effects (DMW's preferred specification) appears to lower the estimated effect of capital on output; again the differences between fixed effects and OLS coefficients are not significant. Finally, the remaining columns of tables 2.6 and 2.7 document the largely uninformative attempt to use the difference in grant types to seperately identify the effect of different inputs in order to control for potential endogeneity due to the difference between the accounting and shadow

[7] DMW report a substantially lower coefficient on a capital in a regression similar to that in column 2. I find a higher result by including all un-treated firms in the sample (DMW use only data from the first round of data), and by using reported real profits instead of profits adjusted for owner's labor. Using the adjusted profits in the same sample reduces the coefficient to 3.79 (0.558).

cost of inputs. Standard errors are extremely high on all variables, suggesting substantial collinarity in the second stage.

Univariate structural estimation of the effect of capital on profits and revenues (in levels and logs) is presented in table 2.8. Structural results are remarkably stable across specifications and quantitatively similar to the fixed-effects IV and non-fixed effects OLS regressions. Indeed, in no case are the structural estimates statistically different from the IV coefficients, although this is partly due to the large standard errors in the IV[8]. Given the concerns raised in section 2.2 with heterogeneity and potential correlation between the instrument and the residual contribution of materials to profits (net of the materials cost), the most robust IV parameter may be the fixed effects estimate of profit in logs on log capital, 0.310 (0.124). This is roughly 21% lower than full-sample structural estimate of the same parameter, 0.396 (0.039), and statistically indistiguishable from it. Interestingly, the IV coefficient on capital in a regression not including is almost 50% larger than the IV-FE coefficient, which suggests that the fixed effect may indeed be capturing residual variation correlated with the random shock.

Comparing coefficients across columns, it seems that all three proxies yield similar results, with no obvious trends across specifications. Figures 2-1, 2-2, and 2-3 investigate the choice of proxy further, plotting the predicted productivity shock (ω_t) as a function of capital and the candidate proxy variable. Recall that for the inversion in equation 2.8 to recover the productivity shock, the shock must be monotonically increasing in the proxy. This monotonicity is very clear for materials and inventories in figure 2-1, seems to hold for the fraction of materials and inventories purchased in the last month that were used displayed in 2-2, but does not appear to hold for the owner's hours worked in figure 2-3. In light of these results, subsequent specifications use materials and inventories as the proxy for productivity shocks.

The structural estimator, assuming it is properly specified, may be used to estimate the returns to the other inputs to production that cannot be identified with only a single instrument. Table 2.10 contains the results of this estimation for both the LP and ACF estimators. The results in columns 1 and 3, which do not control for entrepreneur characteristics, show little differences between the ACF and LP techniques, with the exception of the labor coefficient which is about twice as big using the ACF technique, although also estimated with substantially more noise. Materials and capital coefficients are very close between estimators and also very similar to the OLS results. This is consistent with LP and Olley and Pakes, who also find that the structural estimates are not typically statistically different from the OLS estimates. Controlling for entrepreneur characteristics substantially decreases all parameters except the capital coefficient, while increasing the standard errors. Women are still predicted to have lower productivity, and the coefficients on age and education are now positive and, in some specifications significant.

Table 2.11 uses the structural estimator to investigate the differences in production functions

[8]Since the Levinsohn & Petrin method and the Ackerberg, Caves, & Frazer technique coincide when the only independent variable is capital, these results tell us little about the choice of structural technique.

between sectors. The firms in the Sri Lanka dataset are divided into three broad sectors, manufacturing, trade, and services, and the elasticities of revenue with respect to capital, labor and materials are estimated seperately for each sector using the Levinsohn & Petrin estimator with materials and inventories as a proxy. Due to the decreased sample size I no longer include controls for entrepreneur characteristics. The result show substantial and occasionally significant differences between coefficients across sectors: as might be expected, firms in services sector (e.g. sewing clothes) have the highest elasticity with respect to materials and labor and a lower elasticity of fixed capital than those in manufacturing and trade, although these differences are not significant perhaps due to the relatively small number of observations of firms in services. Less intuitively, I find that the elasticity of labor is significantly lower in the trade sector, which consists mainly of retail shops (61%). The data is suggestive that returns to scale may be higher in the services sector, although the coefficients are estimated with too much error to reject the null hypothesis of constant returns in all sectors.

Conclusion

The choice of techniques to study the production processes of firms in developing countries ultimately depends upon the question that the researcher wishes to answer. DMW's reduced form estimate of the impact of a cash grant on profits answers a well-defined policy question, a question that would be impossible to answer through structural estimation without a host of additional assumptions on entrepreneur's utility functions, discount factors, and other unidentified parameters. Yet to investigate other questions, such as how structural changes in markets affect firms' productivities, or to delve deeper into questions about capital allocation and productivity differences across firms, a more complete parameterization of firms' production functions is can yield valuable insights. Under certain assumptions, the ACF and LP estimation techniques offer consistent estimates of these parameters, and DMW's Sri Lankan dataset provides a unique opportunity to compare these results with estimates derived from a randomized field experiment. I find the results to be similar in economic and statistical terms, a result that suggests that, under certain circumstances, structural estimators of production functions may be a valuable tool to learning more about production and firm growth in the developing world.

Tables

Table 2.1: Summary Statistics

	Percent of non-missing observations	Mean	Std. Deviation	Median
Profits (real LKR)	96.15%	5970.23	5412.44	4063.31
Revenues (real LKR)	97.69%	22745.97	28368.29	11538.23
Material and Inventory costs (real LKR)*	96.62%	15936.98	23279.77	7448.64
Fuel and Electricity (real LKR)*	69.69%	352.08	607.87	143.71
Materials/Inventories used or sold (real LKR)*	82.30%	11398.54	21457.42	3600
Investment (real LKR)*	10.97%	8062.93	17960.96	2260.01
Capital excluding land and buildings (real LKR)	93.40%	37770.26	41359.90	26000
Own hours worked	100.00%	51.07	25.3	50
Family hours worked	100.00%	20.64	31.32	0
Paid labor hours worked	100.00%	3.97	18.43	0
Firms exiting	100.00%	0.02%	0.15	0

Values in Sri Lankan LKR are deflated by the Sri Lankan CPI to April 2005 values.
Observations with absolute or proportional profit changes in the top 5% dropped.
* Percentage of missing observations include those for which these variables were equal to zero.

Table 2.2: Reasons for Firm Exit

Reason	Frequency	Percent of cases
The business was making a loss	25	34.25%
Sickness or health reasons	10	13.70%
I found a better paying wage job	5	6.85%
To take care of family matters	4	5.48%
A better business opportunity came along	2	2.74%
Other	27	36.99%
Total	73	100%

Exit occurs when the entrepreneur reports changing his or her line of business from the activity being performed at the last survey visit.

Table 2.3: Effect of Treatments on Input Use

Impact of treatment amount on:	Capital stock	Log capital stock	Owner hours worked	Log owner hours worked	Materials and inventories purchased	Log materials and inventories purchased
	1	2	3	4	5	6
10,000 LKR in-kind	4793.802*	0.399**	6.052**	0.132**	-703.263	0.334**
	(2713.579)	(0.077)	(2.856)	(0.061)	(1558.678)	(0.147)
20,000 LKR in-kind	13167.128**	0.715**	-0.581	0.043	6600.362*	0.344**
	(3773.485)	(0.169)	(3.414)	(0.066)	(3855.861)	(0.151)
10,000 LKR cash	10781.247**	0.230**	4.515*	0.092**	2200.728	0.233
	(5139.460)	(0.103)	(2.540)	(0.046)	(1692.130)	(0.162)
20,000 LKR cash	23431.369**	0.533**	2.365	0.041	3465.381	0.418**
	(6685.955)	(0.113)	(3.259)	(0.056)	(2945.594)	(0.134)
Firm fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Wave fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.098	0.203	0.011	0.006	0.05	0.03
N	3156	3156	3379	3155	3320	3265

Capital stock and materials and inventories used are measured in Sri Lankan rupees, deflated by the Sri Lankan CPI to reflect March 2005 price levels. Standard errors clustered at the firm level in parentheses. **p < .05 *p < .10

Table 2.4: OLS Results- Revenue

	Revenue in Levels					Revenue in Logs				
	1	2	3	4	5	6	7	8	9	10
Capital	28.101**	26.596**	4.455**	4.277**	2.556	0.509**	0.471**	0.172**	0.181**	0.156**
	(3.711)	(3.715)	(1.934)	(1.893)	(2.345)	(0.044)	(0.040)	(0.032)	(0.032)	(0.045)
Labor		54.107**	16.964**	15.773**	13.059		0.450**	0.142**	0.140**	0.143**
		(12.285)	(7.177)	(7.061)	(8.872)		(0.070)	(0.049)	(0.045)	(0.041)
Materials			0.774**	0.768**	0.479**			0.527**	0.522**	0.280**
			(0.064)	(0.064)	(0.075)			(0.039)	(0.038)	(0.037)
Owner's age				27.422					-0.009	
				(43.742)					(0.109)	
Owner's education				39.726					-0.214**	
				(120.867)					(0.105)	
Female owner				-2337.027**					-0.149**	
				(832.088)					(0.059)	
Survey round fixed effects	No	No	No	No	Yes	No	No	No	No	Yes
Firm fixed effects	No	No	No	No	Yes	No	No	No	No	Yes
R^2	0.233	0.248	0.665	0.668	0.315	0.298	0.357	0.684	0.695	0.301
N	1714	1714	1714	1714	1714	1713	1660	1638	1592	1638

All regressions run DMW's preferred sample, further selected to include only control firms and firms that had as yet received no grant from the researchers. Dependent variable in columns 1-5 is real (CPI-adjusted) revenue in levels, and in columns 6-10 is real revenue in logs. Standard errors in parenthesis clustered at the firm level. ** p < .05 * p < .1

Table 2.5: OLS Results- Profit

	Profit in Levels					Profit in Logs				
	1	2	3	4	5	6	7	8	9	10
Capital	4.693**	4.310**	2.456**	2.304**	0.268	0.362**	0.331**	0.180**	0.179**	0.116**
	(0.602)	(0.573)	(0.637)	(0.594)	(0.762)	(0.030)	(0.028)	(0.030)	(0.031)	(0.040)
Labor		13.528**	10.403**	9.636**	8.432**		0.343**	0.180**	0.172**	0.203**
		(2.551)	(2.566)	(2.473)	(3.279)		(0.054)	(0.050)	(0.047)	(0.043)
Materials			0.065**	0.063**	0.038**			0.268**	0.264**	0.171**
			(0.013)	(0.012)	(0.011)			(0.032)	(0.030)	(0.032)
Owner's age				-31.778*					-0.209*	
				(16.581)					(0.122)	
Owner's education				-66.164					-0.264**	
				(50.511)					(0.112)	
Female owner				-1509.261**					-0.246**	
				(342.238)					(0.068)	
Survey round fixed effects	No	No	No	No	Yes	No	No	No	No	Yes
Firm fixed effects	No	No	No	No	Yes	No	No	No	No	Yes
R^2	0.105	0.177	0.245	0.279	0.092	0.146	0.299	0.259	0.465	0.118
N	1691	1691	1691	1691	1691	1472	1640	1411	1574	1411

All regressions run on DMW's preferred sample, further selected to include only control firms and firms that had as yet received no grant from the researchers. Dependent variable in columns 1-5 is real profits in levels, and in columns 6-10 is profits in logs. Profits not adjusted for firm owner's own labor or cost of capital. Standard errors in parenthesis clustered at the firm level. ** p < .05 * p < .1

Table 2.6: IV Results - Revenues

	Revenue in Levels					Revenue in Logs				
	1	2	3	4	5	6	7	8	9	10
Capital	47.981**	34.506**	48.857**	19.767	-0.433	0.894**	0.515**	0.905**	0.537	0.153
	(10.048)	(10.818)	(12.015)	(17.491)	(23.580)	(0.157)	(0.149)	(0.156)	(0.444)	(0.706)
Labor			-288.096		209.282			-0.154		1.224
			(245.440)		(251.800)			(1.029)		(1.527)
Materials				0.747*	1.527*				0.338	0.27
				(0.432)	(0.841)				(0.417)	(0.854)
Survey round fixed effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Firm fixed effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
R^2	0.151	-0.083	.	0.674	-0.921	0.21	-0.006	0.195	0.601	-0.317
N	3135	3134	3135	3135	3134	3133	3132	3038	3104	3008

All regressions run DMW's preferred sample. Dependent variable in columns 1-5 is real (CPI-adjusted) revenue in levels, and in columns 6-10 is real revenue in logs. Instruments in all regressions are a set of 4 dummy variables indicating grant status. Standard errors in parenthesis clustered at the firm level. ** $p < .05$ * $p < .1$

Table 2.7: IV Results - Profits

	Profits in Levels					Profits in Logs				
	1	2	3	4	5	6	7	8	9	10
Capital	8.259**	5.162**	8.256**	15.504	0.006	0.594**	0.310**	0.583**	0.486	-0.273
	(1.957)	(2.257)	(1.959)	(10.519)	(6.720)	(0.115)	(0.124)	(0.108)	(0.446)	(0.562)
Labor			0.739		93.333			0.341		0.944
			(40.742)		(76.178)			(0.746)		(1.190)
Materials				-0.192	0.185				0.092	0.608
				(0.269)	(0.219)				(0.415)	(0.728)
Survey round fixed effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
Firm fixed effects	No	Yes	No	No	Yes	No	Yes	No	No	Yes
R^2	0.04	-0.092	0.042	.	-1.26	0.192	-0.016	0.233	0.316	-0.818
N	3103	3102	3103	3103	3102	3103	3102	3011	3076	2983

All regressions run on DMW's preferred sample. Dependent variable in columns 1-5 is real profits in levels, and in columns 6-10 is profits in logs. Profits not adjusted for firm owner's own labor or cost of capital. Instruments in all regressions are a set of 4 dummy variables indicating grant status. Standard errors in parenthesis clustered at the firm level. ** $p < .05$ * $p < .1$

Table 2.8: Univariate Structural Estimation

	Profits in levels			Profits in logs			Revenue in levels			Revenue in logs		
	1	2	3	4	5	6	7	8	9	10	11	12
						Full Sample						
Capital	3.682**	3.980**	4.207**	0.396**	0.413**	0.402**	27.109**	30.397**	29.065**	0.561**	0.598**	0.578**
	(0.836)	(0.786)	(0.726)	(0.039)	(0.041)	(0.038)	(5.064)	(3.912)	(3.767)	(0.065)	(0.060)	(0.054)
N	3032	2401	2951	3028	2401	2943	3061	2410	2977	3056	2410	2969
						Control firms						
Capital	3.649**	4.931**	5.175**	0.367**	0.394**	0.373**	30.830**	31.270**	0.476**	0.547**	0.482**	48.245**
	(1.015)	(0.953)	(0.866)	(0.039)	(0.044)	(0.039)	(4.525)	(4.593)	(5.033)	(0.061)	(0.061)	(0.055)
N	1649	1141	1607	1648	1141	1605	1669	1142	1626	1668	1142	1624
						Treatment firms						
Capital	4.077**	3.877**	4.007**	0.434**	0.434**	0.438**	14.690**	28.950	28.629**	0.634**	0.645**	0.643**
	(1.050)	(0.883)	(0.771)	(0.057)	(0.048)	(0.044)	(9.578)	(4.510)	(5.187)	(0.107)	(0.067)	(0.062)
N	1383	1260	1344	1380	1260	1338	1392	1268	1351	1388	1268	1345
Proxy	Materials and inventories	Fraction of materials used	Owner's labor	Materials and inventories	Fraction of materials used	Owner's labor	Materials and inventories	Fraction of materials used	Owner's labor	Materials and inventories	Fraction of materials used	Owner's labor

Dependent variable in columns 1-3 is profits in levels, in columns 4-6 is profits in logs, in columns 7-9 is revenue in levels, and in columns 10-12 is revenue in logs. Standard errors in parenthesis clustered at the firm level. ** $p < .05$ * $p < .1$

Table 2.10: Structural Estimation of Production Functions

	Revenue in Logs			
	Levinsohn & Petrin estimator		Ackerberg, Caves, & Frazer estimator	
	1	2	3	4
Log capital	0.148**	0.111*	0.146**	0.098
	(0.038)	(0.055)	(0.044)	(0.064)
Log materials	0.566**	0.007	0.579**	0.018
and inventories	(0.058)	(0.112)	(0.087)	(0.154)
Log total labor	0.170**	-0.164	0.277	-0.120
	(0.033)	(0.194)	(0.225)	(0.211)
Log owner's age		0.157*		0.139
		(0.072)		(0.113)
Log owner's		0.655**		0.647**
education		(0.090)		(0.186)
Female		-0.136**		-0.274
		(0.047)		(0.805)
N	2965	2896	2965	2896

Dependent variable is real revenues in logs. Standard errors in parenthesis clustered at the firm level. ** p < .05 * p < .1

Table 2.11: Production Function Parameters by Sector

	Revenue in logs		
	Manufacturing	Services	Trade
	1	2	3
Log capital	0.142**	0.065	0.160
	(0.057)	(0.461)	(0.100)
Log materials	0.528**	0.910	0.543**
and inventories	(0.094)	(1.831)	(0.182)
Log total labor	0.212**	0.280**	0.049
	(0.039)	(0.063)	(0.053)
N	1261	444	1260

Dependent variable is real revenues in logs. Standard errors in parenthesis clustered at the firm level. ** p < .05 * p < .1

Figures

Figure 2-1: Productivity as a Function of Capital, and Materials and Inventories

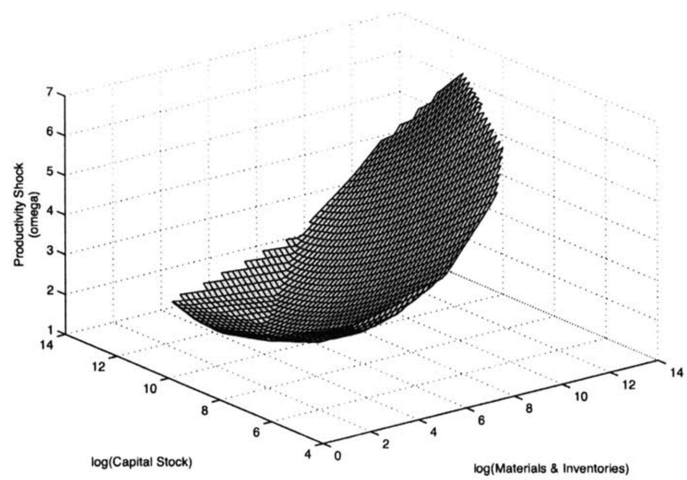


Figure 2-2: Productivity as a Function of Capital, and Fraction Materials and Inventories Used

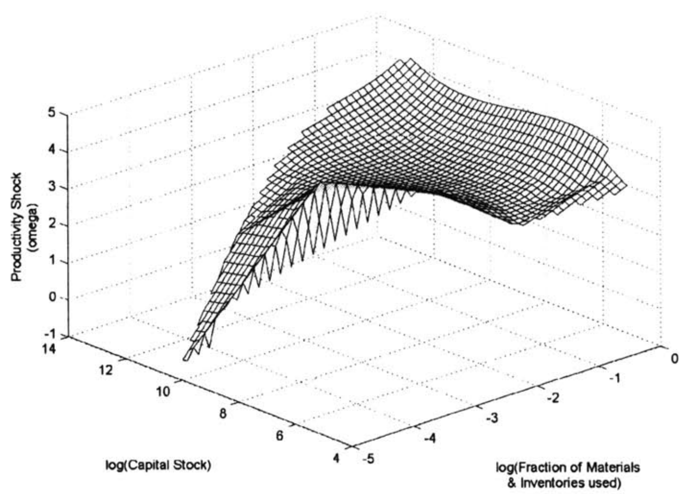
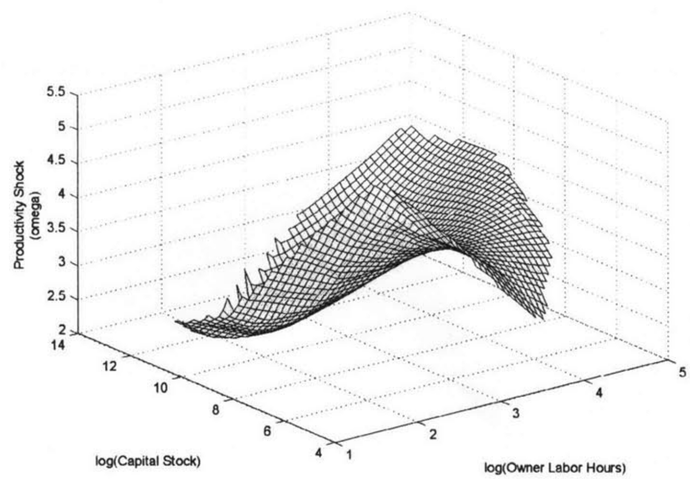


Figure 2-3: Productivity as a Function of Capital, and Hours of Owner Labor



Chapter 3

Making Police Reform Real: The Rajasthan Experiment

Police reform has long been a contentious subject in India. Experts, activists and various police commissions have called for the police to become more effective and accountable to the needs of citizens in a modern democratic society, yet few reform proposals have had much impact on the ground. Broad, systemic initiatives suggesting fundamental changes to the institution of policing have been only incompletely implemented, while other local projects have been implemented on a small scale, but have proven difficult or impossible to sustain and replicate. As a result, the Indian police are not nearly as effective as they could be: problems such as inefficiency, corruption, and an insular police culture adversely affect police performance and create negative public perceptions.

This paper documents an attempt to overcome the challenges of police reform in the Indian state of Rajasthan, evaluated through a series of RCT (Randomized Control Trials). Four reform interventions were implemented in a randomly selected group of 162 police stations across 11 districts of the state: (1) weekly duty rosters with a guaranteed rotating day off per week; (2) a freeze on transfers of police staff; (3) in-service training to update skills; and (4) placing community observers in police stations. To evaluate these reforms, data was collected through two rounds of surveys (before and after the intervention) including police interviews, decoy visits to police stations, and a large scale crime survey—the first of its kind in India. The results suggest that two of the interventions, the freeze on transfers and the training, do have the potential to improve the police effectiveness and public image. The other reforms showed no robust effects, an outcome that may be due to their incomplete implementation.

Background of Police Reform in India

Like many other developing countries, independent India inherited a police force initially designed to protect the interests of the colonial power. The British authority established an Indian police force in which senior officers were recruited through a competitive examination held in London that was initially open only to the British citizens. Local constables formed the main interface between the public and the police, but they were accountable only to the British colonial establishment they served. Fraternization with the public was discouraged.

The onset of democracy after independence did little to change the colonial culture, while the social, economic and political dynamics of the society have transformed dramatically. The Police Act of 1861 remains in effect, and so do the constraints and handicaps of an outmoded system. In 1977 the National Police Commission (NPC) was formed to provide recommendations for improving the effectiveness, accountability and public image of the police in view of the democratic aspirations of people of independent India. The Commission pointed out several anomalies and recommended progressive suggestions, but, few of its recommendations have been put into practice. Subsequently there have been a series of commissions aimed at reforming various facets of policing, but essentially none of their recommendations have been implemented by the successive governments.

Most recently, in 2005, the Government of India convened the Soli Sorabjee Committee to draft a new Model Police Act to guide the states in view of the changing role of police in Indian democracy. Following the Model act, few states have drafted new police legislation, but none have fully implemented them. In 2006, the Supreme Court of India in its judgment in Prakash Singh vs Union of India directed the state governments to constitute institutions and draft mechanisms for improving the effectiveness and accountability of police. Here too, there has been little implementation. Thus, so far the history of police reform in India is that of a series of dreams but little on the ground changes.

In response to lack of systematic reform and the perceived need for change on the ground, individual officers did take initiative at the local level. In Punjab, Sidhu (2003) implemented weekly day off of the police personnel to reduce their level of stress and improve productivity in districts. Similarly in 2002, the Rajasthan Police introduced a community policing program (Jansahbhagita), to build police public partnership in resolving petty disputes and foster mutual understanding. However, these programs are often dependent on the support and enthusiasm of a single individual or administration, and cease operation after their exit. The lack of broad support may be due to the absence of any objective, substantial data on success; there is little basis to compare the claims of the supporters and detractors of such initiatives.

Objective of the Rajasthan Police Experiment

Against this background, the leadership of Rajasthan Police conceived an innovative approach of police reforms that would depart from the past in two key areas: First, the interventions would be simple and cost effective enough to be implemented in any police station in India, and would not require fundamental changes in the policing legislations or cause extra financial burden to the state. Second, they would be evaluated scientifically and quantitatively, providing tangible data for future policy decisions. To assist in the evaluation aspect, the Rajasthan Police partnered with J-PAL, a research institute based out of the Massachusetts Institute of Technology, USA and the Institute for Financial Management and Research in Chennai, with a long experience of conducting randomized evaluations of economic and social projects in developing countries, and in particular in India. The Rajasthan Police Program aimed at:

1. Assessing the current state of (a) service delivery by Rajasthan Police and (b) police public relations in Rajasthan, with the help of household survey.
2. Identifying means of advancing police efficiency, effectiveness and professionalism through internal police management restructuring.
3. Suggesting measures to develop proper police attitude and communications with citizens.
4. Advising on improving public perception of the police and to secure citizens' willing and active cooperation in all areas of police working.

Selection of Interventions

Preliminary interviews, focus groups, and review of the existing literature indicated four problems that were both serious, and had the potential to be addressed through this the types of reforms described above: First, extremely rapid transfers were causing dissatisfaction among the police ranks and adversely affecting their performance. Second, members of the police felt that the schedule and system of duty assignments within the police station was unfair and overwhelmingly stressful. Third, there seemed to be a major disconnect between the police self image as overworked and unappreciated and the public's perception of the police as high-handed and unresponsive. Finally, many respondents within the police attributed poor performance to lack of police training, both on professional and behavioral dimensions.

These issues have long been plaguing the police system. The National Police Commission (NPC) in 1979 observed that "the threat of transfer is the most potent weapon in the hands of politicians to bend down the police to his will". (15.14) On the one hand it is a source of corruption as police officer pay bribe to politicians and senior officers to retain a good posting (Verma 2003; for Media Studies 2005) on the other police is also perceived as "a partial agency of government in power and find it difficult to play a lawful role in upholding the constitutional rights and liberty of people" (1.20). In response to this widely documented form of corruption and undue pressure, many have suggested regularizing posting periods or a more transparent, systematic posting procedure.

Irrational use of manpower in the police station has been another area of concern directly affecting the productivity and welfare of the personnel (NPC 1979, Sidhu 2003). The staff at the police station has no fixed rest periods and theoretically remains on duty for twenty four hours a day, seven days a week. The provision of a weekly day off for all staff has been advocated by the NPC (3.19). Several states have implemented the weekly day off, including the neighboring states of Gujarat and Punjab.

The allocation of different duties among the staff of police station by the Station House Officers (SHO) have also been found lacking transparency. Often, the complaints of partiality in assigning duties caused resentment and disinterest among the staff, ultimately hampering their overall performance.

A third pressing issue has been the negative image of police. NPC observed that "police partiality, corruption, brutality and failure to register cognizable offences are the most important factors which contribute to this sad state of affairs" (62.4). The lack of accountability to the public, and communication with it, has been a major contributing factor. NPC recommended constitution of State Security Commissions to promote efficiency and accountability. It also emphasized the need to communicate to the public a "clear understanding of the limitations and constraints within which the police has to function" (Initiative 2005). Verma (2005) suggested the creation of "citizens' boards" with extensive powers to investigate complaints against police personnel, and evaluate their performance at the district level. Such experiences have been tried in several developed countries, as well as the ex-Soviet States (Caparini and Marenin 2005) and South Africa (Bruce and Neild 2005).

Another factors contributing the negative public image has been the lack of skills; both 'professional' (investigation methods, etc.) and 'soft' skills (mediation, communication) have frequently been recognized as significant barriers to effective policing. While many have emphasized the need for emphasis on scientific investigation skills (Verma 2005), the traditional attitudes of the police, largely a legacy of the colonial era are also inadequate to command public trust in changed environment. The NPC stated that "It is considered basic and fundamental... that every police officer develops an attitude of courtesy and consideration combined with sympathy and understanding towards any member of the public who comes to him seeking help." (41.09)

In addition to the specific issues identified above, there is a general concern about the reliability of police data on crime. While Indian crime statistics are based exclusively on police station crime registration, there have been serious allegations of non-reporting of crimes. The reporting rate may be low precisely because people are afraid of the police and expect them to be unresponsive. Moreover, the police may actively discourage the reporting of crimes in order to make their record look better. The NPC went to the extent of stating, "The unreliability of crime statistics in India is well known....Whenever a genuine effort was made to register all crime... the figures showed such fantastic jumps as were impossible with any normal increase in one year" (62.10) . Precisely because registration rates are not a reliable source for crime statistics, most countries, including the US, most European countries, and many developing countries, use crimes statistics based on crime victimization surveys (household surveys conducted by independent agency) to measure crime. Better statistics on crime would give the police a greater understanding of where to put in effort in fighting it, and would provide the basis for a more reasoned allocation of ressources and manpower.

Four Interventions of Rajasthan Experiment

The following four interventions, implemented in 162 police stations across 11 districts of Rajasthan for a period of one and half years were designed to address some of the difficulties mentioned above:

1. Freezing of transfers: All administrative transfers of personnel posted in these police stations for less than two years were prohibited during the program period. Exceptions could be made for well documented cases of police misconduct requiring transfer.

2. Weekly day off and Duty roster system: The entire staff in selected police stations (except the SHO) received one day off every eight days. In smaller police stations, where the shortage of manpower might be more acute, the SHO had the option of extending the rotation period up to 15 days.

   In addition, the various police station level duties, such as writing general diary and other paperwork, night patrolling, sentry duty, court liaison, serving summons, etc., were allocated to staff on a rotating basis. The goal was to create a transparent and fair system of work allocation that would lead to lower stress, more flexibility, reduced corruption, better informed constabulary and higher overall productivity.

3. Community Observers: Introduced for the first time in Rajasthan, the community observers were local volunteers chosen to sit in the police station for approximately three hours during its peak operating hours. The observers' sole task was to watch the activities within the police station and become familiar with the duties, procedures and challenges faced by the police. Their comments could be placed in a suggestion box, which could only be opened by a district-level officer. Each selected police station compiled a list of up to 100 community observers, each of whom was to visit the police station only once or twice before suggesting a replacement.

   The community observer program was designed primarily to bridge the gap between police and public perceptions. The objectives were three fold: first, to give a group of citizens firsthand experience with the police in a positive setting; second to encourage them to share their experience with others, and second, to provide a community oversight in the police station; third, the presence of the observer might have encouraged the police to treat complainants with more sympathy and patience: the hours at which the observer was in the station were publicly posted, and potential complainants could thus decide to visit the police stations during those times.

4. In-Service training program: The training had two modules:

   (a) Professional/Investigation Skills: After random selection process, 292 investigation officers (Inspectors, Sub Inspectors and ASIs) were trained for 6 days at the Rajasthan Police

Academy with inputs on improving investigation procedures, such as field techniques and documentation, with emphasis on scientific techniques.

(b) Soft skills: 1540 randomly selected police personnel of all ranks were trained for three days on improving public relations with inputs on 'soft skills' such as communication, mediation, stress management, motivation, team building, leadership, attitudinal change etc.

The eleven districts included Ajmer, Alwar, Barmer, Chittorgarh, Dholpur, Hanumangarh, Jaipur City East, Kota City, Nagaur, Pratapgarh, and Udaipur.

Evaluation Strategy

A central component of this project was the randomized choice of police stations in which the reform interventions was conducted, which made rigorous evaluation possible. Police stations were randomly selected for inclusion in the reform project, using stratification to ensure a balanced sample of the state in terms of size, criminality and population. Table 3.1describes how the police stations were allocated to each evaluation group. Similarly, for the purpose of executing the training intervention, the staff were randomly selected in police stations were not everyone was trained. To measure the spillover effects of training, the percentage of staff trained within each police station is randomized at 0%, 25%, 50% and 100%. This allows us to detect whether the effect of training are different when few or almost all the officers are trained.

To address the concern of Hawthorne effect—the possibility that the police stations participating in the program might change their responses or behavior simply because they are being observed by outside evaluators- an additional group of police stations, the 'Special controls' was also randomly selected. These police stations were not informed of the project, or visited by investigators, until the final end-line survey, at which point they were surveyed like all other stations. No staff members from the special control stations were trained. The Hawthorne effect can be seen in the contrast between the control and special control groups.

As part of the randomization process, five main groups of police stations were formed:

Note that wherever the community observer or weekly off/duty roster interventions were attempted, transfers were also frozen. This was done for two reasons: first, because long tenure of staff is considered necessary to get acquainted with the area and people of the police station jurisdiction and see if this has any effect on their performance and second, due to concerns about attrition.

Implementation and Monitoring

The Rajasthan Police Reform Program was the outcome of the commitment of the senior leadership of state police to improve the overall performance of personnel. The general supervision was done

Table 3.1: Program Design

Group	Number of Police Stations	Program Elements			
		Community observer	Weekly off and duty rotation	No Transfer	Training
1. All Interventions	35	Yes	Yes	Yes	Between 0 and 100% of staff
2. Community Observer + No Transfer	25	Yes	No	Yes	Between 0 and 100% of staff
3. Weekly Off / Duty Rotation + No Transfer	25	No	Yes	Yes	Between 0 and 100% of staff
4. No Transfer	40	No	No	Yes	Between 0 and 100% of staff
5. Control	25	No	No	No	Between 0 and 100% of staff

at the level of State Police Headquarters. The District Superintendents of Police were supervising the overall implementation in their respective districts. Several briefing sessions were undertaken in police stations and feedback collected before the final roll out. For the duration of the intervention, the surveyors of J-PAL conducted a number of random, unannounced visits to all police stations to collect an independent opinion regarding the status of implementation.

Surveys

The surveys covered both public as well as police staff (all ranks posted in the police station) and were carried out in two stages: baseline and end-line. Respondents for the surveys were chosen randomly (the public from voter lists, the police from administrative staff lists) in order to avoid biasing the results by selecting only those favorably or unfavorably disposed towards the police.

Public Surveys

The public image of the police is both one of the most important outcome of the project and the most difficult to measure, since impression are subjective and tenuous. These issues, combined with the fact that extremely few citizens actually interact with the police, necessitated a household survey that would be both in depth—to capture the various aspects of public perception—and large enough to detect a small change in opinion. In total 5,895 households were interviewed: 2,451 in the spring of 2007 then the households that could be re-located were re-interviewed along with 3,443 newly chosen households the autumn of 2008. Within each household, three modules were administered:

- A general crime screener questionnaire administered to the head of the household, inquiring whether any member of the household had been a victim of a crime in the previous year.

- A crime victim survey, administered to each individual crime victim in the household. This survey ascertained greater factual detail about the crime and the victim's satisfaction with police actions.

- An opinion survey, administered to a randomly chosen adult in the household. The individual was asked about his or her contact with the police, and perceptions of police performance and integrity.

The crime-related surveys were broadly based upon the UNICRI's International Crime and Victimization Survey (ICVS), customized to the Indian context in order to make their results comparable with the crime categories of the Indian Penal Code. Like any crime victimization survey, these questionnaires do not capture "victimless crimes" such as gambling or drug use. Also no attempt is made to measure the incidence of domestic violence because of concerns about the accuracy of reporting and the possibility of decreased respondent cooperation.

Police Surveys

To measure the effects of the interventions on the police performance, a survey was conducted of the police staff. One half of the staff of all ranks (in total 2367) in treatment and control police stations was randomly selected to be interviewed by surveyors. They were interviewed on the following aspects:

1. Level of Job- Satisfaction

2. Knowledge of police duties

3. Impact of training

4. Complaints with the police system

Case File Reviews of Investigations

Reviews of the case files of investigation were done to ascertain the impact of professional training on the Investigating Officers (IOs). A total of 982 case files were randomly selected from the project police stations and sent to a group of retired senior police officers for grading in two rounds half investigated before the training and half post training. The retired officers filled out a detailed report about each case in which they graded the performance of the officer on both his actions at the crime scene, and the care and diligence with which the evidence is collected and documents in the case file is prepared. Grades were given on a scale of 1 (worst) to 10 (perfect). In addition to this, the retired officers also recorded whether the IO had used any scientific techniques or not.

Visit of Decoys

To provide an alternative indicator of police performance, a "decoy" surveyor program was developed in which surveyors visited police stations in the treatment and control groups and attempted to register cases for various types of crimes. These visits were unannounced and the surveyor did not disclose his identity except in case it seemed that the police would actually register the case, or if the situation otherwise required that the surveyor disclose his identity, for instance if the police threatened to prosecute him for filing a false case. Immediately after the visit to the police station, the surveyor completed a short form recording his success or failure in registration, the attitudes and actions of the police, and other details such as the total time taken and the names of the officers with whom he interacted.

Status of Service Delivery: Base line Results

One of the prime objectives of the program was to assess the current status of service delivery of the police. This was done with the rigorous quantitative analysis of baseline survey results:

Public Perception of Police

The following highlights the key results on police perception at the baseline:

(i) Interaction with Police

The most striking aspect of the public opinion data is how few respondents actually have experience with the police or ever interacted with them. Only 11% of those surveyed have actually had an interaction with the police in their lives, and only 5% of women have ever spoken with a police officer. Even in urban areas, only 24% of respondents (men) replied ever interacting with the police.

In the absence of personal experience, 71% of those surveyed claimed that they had formed their opinions based on word of mouth conversations with other citizens, with 12% stating that they based their opinions on television and print news sources.

(ii) Perception of Police Conduct

The opinions of the police conduct were ambivalent. On the one hand most citizens felt that the police was generally cooperative: 71% responded that the police were mostly or always helpful, and 65% replied that the police were always or mostly courteous. Yet these positive impressions were balanced by negative perceptions of other aspects of police behavior: 53% of respondents thought that the police work less hard than the average citizen, in sharp contrast with police staff's self-perception of being overworked.

Furthermore, 53% of the respondents stated that law abiding citizens fear the police, while 44% of the population believed that the police are always or usually cruel to those in custody.

(iii) Perception regarding Police Resources

While police officers unanimously called for more funding, manpower, and equipment, for better services, the citizens of Rajasthan do not perceive this shortage of resources: of those interviewed, 61% replied that the police had sufficient personnel, and 68% replied that the police had sufficient money and other resources.

(iv) Reporting of Crime by citizens

Another striking revelation of the survey is the fact that most crime victims never report their incidents to the police. Only 29% of the crime victims we surveyed stated that they had visited a police station to report the crime, and even among those who attempted to report their crimes, 19% did not succeed in registering a case.

While the biggest reason for not reporting a crime was lack of importance (28%) perceived by the victim, substantial numbers of crime victims did not go to the police because they thought that the police were incapable of helping (20%) or unwilling to help (17%). Important cases such as motorcycle theft and assault were registered relatively more frequently (75% and 61%) than theft (12%) and vandalism (17%). It is important to note that the "lack of importance" may reflect the perception that the police would not treat the case seriously even if reported (and may refuse to register it).

(v) Victim Satisfaction

Figure 3-1 reveals a very lukewarm picture in the the baseline data on public satisfaction with the police work during investigations: 38% of the victims are completely unsatisfied, 24% are unsatisfied, and less than a quarter of the victims were either completely satisfied or satisfied . The reasons for victim dissatisfaction are clear: there is a strong perception among crime victims that police are not truly interested in helping them or resolving their case. A total of 50% of victims stated that they were unsatisfied because the police "didn't do anything special" or "didn't take interest" in their case. In contrast, only 6% complained of police corruption, and another 6% complained of discourtesy.

These results suggest that the responsiveness of police is the deciding factor of public perception. The probability that the victims expressed satisfaction with the police was 10% higher when the police simply contacted him or her again after the crime, 23% higher when the police made an arrest, and 51% higher when the police recovered property. Not surprisingly, demand of bribe by the police asked reduced the level of satisfaction by 19%. (All these differences are significant at the 97% or higher level.)

Victims of extortion and hurt or grievous hurt were more satisfied with police work, while victims of eve-teasing and vandalism were less satisfied. Wealthier victims (vehicle owners) were less likely to be satisfied with police efforts than agricultural laborers, who would tend to occupy a lower economic class in society. The gender of the victim (16% of crime victims are women) seems to have no effect on the level of satisfaction.

Police Performance and Morale

A consistent feature of the police survey is a strong secular trend between the baseline and end line surveys. Respondents seemed more willing to be truthful at the endline survey, even if the answers did not fall "in line". For instance, the police appear to have become less satisfied by their jobs, less interested in becoming close to the public and more comfortable with police brutality. While the source of this change is unknown—it may represent a true trend, or simply the result of the police becoming accustomed to the surveyors—this effect should be constant across both treatment and control police stations, and therefore will not affect the accuracy of the evaluation of the results of the project.

Perhaps the most direct means to measure police morale is the simple question, "How satisfied are you with your job as a policeman?" However, when we asked this question at the baseline, an unbelievable 82% of police respondents claimed they could not be more satisfied—a 10 out of 10. Responses at the endline were more varied, with average rating of 7.6 and only 28% claiming to be completely satisfied.

(i) Police Staff Complaints

There was substantial variation in the issues cited by the police staff as the worst aspects of their job. When asked by the surveyors to list what they perceived as the worst aspects of their job, their responses are categorized in the displayed in Figure 3-2. Many of these issues, for instance problems with low pay; low quality housing or poor promotional avenues were outside the scope of the police reform project. However, long working hours and no day off have been major complaints disturbing their morale and performance, and these were addressed by the project.

(ii) Use of Scientific aids in Investigation

Use of scientific aid and collection of evidence is crucial to criminal investigations in all countries. In India it is more important because of two factors: First, plea bargaining is not permissible under the law and most cases are decided on the basis of evidence and witness testimony. Second, during the long period of trial, witnesses often turn hostile and retract their statements, thereby making physical evidence all the more important. However, the use of scientific evidence is far from universal in criminal investigations in Rajasthan. Only 19% of cases investigated in Rajasthan during 2006-2007 made any use of scientific investigation, and most of these were accident cases.

Results of Decoy Survey

The decoy survey provides a complementary source of data on police treatment of crime victims to the household and individual interviews. Its main advantage lies in the fact that choice of reporting a crime is influenced by both, the quality of services of the local police and the confidence of decoy as victim. Crime victims who choose to report their cases are thus an endogenously selected group, which may bias their reports in multiple ways. The crimes themselves are selected to be "important" by the victims, as we saw, which makes it more likely that they will be reported. Furthermore, the recollections of those victims who did register FIRs may be imperfect, and they may selectively recall certain elements and not others.

Overall, police were willing to register FIRs only 54% of the time from the decoy surveyors. However, this rate was quite variable across types of crimes: Figure 3-3 displays the registration rates for various crime types:While the rate at which FIR are indeed filed is quite low, The decoy surveyor's reports on police attitudes towards them corroborate the generally positive opinions from the public opinion survey. In 65% of all decoy visits the surveyor's complaint was immediately addressed by the police staff and less than 5% of the time was the waiting period greater than 15 minutes.

Surveyors also compared the behavior of police staff to the "normally polite" attitude they might expect from the staff in a private bank. They found that 69% of the time the police were polite, and 11% of the time they were "extremely polite". While 60% of surveyors were addressed as "aap" the polite Hindi word, in 14% of cases did the police address "Shrimanji" or "Sir?

Impact Evaluation of the interventions

We now turn to a description of the effects of the interventions. The results suggest significant effects of the training and the freezing of transfers on the perception of the police by citizens, and no effects for the other two interventions.

Public Perception of Police

Since the public opinion survey covered a broad range of qualitative indicators, the fourteen major questions on police performance are aggregated into four broad categories in order to simplify the interpretation and guard against false positive results:

1. Responsiveness of police to citizens

2. Fear of police

3. Corruption

4. Adequacy of police resources[1]

[1] The questions incorporated into the categories were as follows:

Table 3.2 displays the effect of each reform on each category, both using data from the baseline and endline surveys. In order to ease interpretation, the "All Interventions" coefficient is defined as the difference between the police station in which all interventions where implemented and the controls, whereas the other intervention coefficients represent the marginal improvement from implementing that intervention (compared to control only). For instance, since the community observer program was always implemented where transfers were banned, the effect of the program between the police stations with community observers and the controls would be the sum of these Community Observer and No Transfer coefficients. The results are expressed in standardized effect size, with standard errors in parentheses. Stars indicate significant effects, at 10% level (one star) or 5% level (two stars).

Of the 4 categories, the project shows a robustly significant effect at the 95% level only in the "Fear of Police" category. Here, where all interventions were implemented together, there was a significant reduction in the fear of the police. Unpacking the effects of the different interventions, we see that two interventions seem to have had an effect, but going in opposite direction: First, the freezing of transfers seems to have had a substantial positive effect on increasing public trust. And second, this effect appears to be undone by the weekly off and duty rotation program (recall that stations that had the weekly off programs also had the freezing of transfers: so the station that had both just these programs were similar to the control station, while stations that had the transfer frozen, and any of the other program, or the stations that had all the programs, had a lower fear of police).

The effect of the freeze on transfers might work through a variety of mechanisms. Perhaps due to less frequent transfers the public became more familiar with the same police staff and hence came to trust them more and fear them less. Alternatively, it is possible that once the police staff remained in a posting for a longer period of time, their behavior changed with respect to the inhabitants of that area and they became more approachable and less intimidating to the population. Many have written about the importance for the police of knowing their "beat"

A possible explanation for the negative interaction with the weekly off/duty rotation might be that the duty rotation, by moving staff from post to post within the police station, prevents the development of public/police familiarity that is the means by which the freeze on transfers reduces

Responsiveness of police to citizens: "How do the police behave with normal citizens?" "Do the police help citizens when required?" "How quick is the police response to distress calls by citizens?"

Fear of police: "Do you think that citizens like you are afraid of the police?" "Are law-abiding citizens afraid of the police?" and how the population thinks of the police, whether "They fear them"?

Corruption: "Would you say that the police in your area are generally honest or generally corrupt?" "Is it necessary to pay the police some money in order to get them do their job?" and "Do policemen themselves violate the law than the average citizen?"

Amount of police resources: "Do the police have enough personnel to do the work required of them?" "Do the police have enough money and resources to do the work required of them?" "Should the size of the police force be increased, decreased, or stay the same?" and "Do you think that the government should spend more money on the police, even if it means spending less on things like education and roads?"

fear of police.

Victim Satisfaction with Police

Table 3.3 shows the results of the project on the probability that the respondent reports being "Satisfied" or "Completely Satisfied" with the police handling of his or her case. The training program shows a large, robust impact on the satisfaction of crime victims. The effect of going from 0% trained officers to 100% is to raise the probability that victims are satisfied with police investigation by between 35 and 50 percentage points, depending on the specification. Since on average only 27% of victims report being entirely or partially satisfied, these changes represent a more than twofold increase in satisfaction. Consistent with the positive findings from the public opinion survey, the estimate of the effect of freezing of is also large and positive, although the relatively small sample size implies that it is only marginally significant.

To unpack the effect of the training on vicim satisfaction, we investigate the impact of the training on what the victim reported about police behavior following the complaint. For instance, the investigation training may have led officers to make more arrests, or to carry out more follow-up visits to the victims. We find a significant impact of the training program on the probability that the police make an arrest: going from 0% to 100% training in a police station increases the probability of making an arrest by 26% (statistically significant at the 5% level). Furthermore, when we generate an index for whether the victim saw police take any positive action at all after the crime, including arrest, property recovery, collecting evidence, interviewing witnesses, etc., the effect of the training on police action increases to 30% (significant at 3% level), or 22% if we don't include arrest as a possible action. Nevertheless, when we do control for these differences, we continue to find a significant effect of the training on victim satisfaction.

Thus it appears that the training programs had two effects on police performance: First, it encouraged the police into more active investigation of crimes, and second that it made crime victims more satisfied with the performance of the police, regardless of whether the police took action or were successful in arresting a suspect or recovering property. This is likely to be due to better treatment of the crime victims by the police (which can probably be attributed to the case review).

These regressions assume that the effect of the fraction of police officers who are trained is linear in the fraction trained, i.e. that when training 50% of officers, the effect is twice as large as when training 25%. In reality, the effect could have been non-linear: for example, it may be necessary to train everyone to see any effect, or it may be sufficient to train a few agents of change. Our design allows us to answer this question, since the fraction of officers trained varied from station to station. Interestingly, we find quite strong evidence that the effect is indeed linear: it seems that what matter is the fraction of police officers who are trained.

Analysis of Police Survey and Case Reviews

Unfortunately, the evaluation of the police survey and case review could not yield reliable results due to the high number of transfers among the police staff (despite the freeze of transfer intervention!). Due to these transfers, only 65 percent of individuals interviewed in the baseline could be re-interviewed in the end line survey, and it is very difficult for us to know whether a particular officer should be attributed to a particular station. As expected, retention in the survey is affected by the no-transfer intervention but as Table 3.5 shows, it varies significantly across the other interventions as well[2].

This attrition raises the possibility that the sample of officers included in the final survey differs across treatments in important but unobservable characteristics. The interventions seem to have changed the composition of the staff in the project stations, potentially introducing biases into the analysis.

In general however, the results of the case reviews show that the higher the rank of an officer, the better quality his or her field investigation and documentation of evidence in case file. The same appears true for use of scientific aid in investigation, although the results are too noisy to be certain. There was also substantial variation across types of crimes: Cases involving physical violence tended to be graded higher: Murder(S 302 IPC), Hurt/Grievous Hurt (S 324/338 IPC), preparation for committing dacoity (S 399 IPC) and the Arms Act, all scored significantly higher than other cases on both field investigation and documentation. Scientific investigation was used more in serious cases, particularly rape (S 376 IPC), and also in cases of tampering with evidence (S 201 IPC).

Analysis of Decoy Survey

Table 3.4 shows the effect of the various program components on the probability that the FIR brought by a decoy surveyor is registered. The first two columns show the effects of the different interventions on the percentage of cases that were registered by the police, and the second two columns show the impact on whether the police were rated as "extremely polite" with the decoy.

The decoy program itself appears to have a substantial impact. The decoy surveyors almost always informed the police that they were being tested, and this knowledge increases the probability of FIR registration by an average of 6 or 7 percent per visit. Though the effect of each extra visit is only marginally significant, there is a significant difference in the probability of FIR registration in stations that had gotten at least 4 decoys (69.4%), versus those for which it was the first decoy (44.6%, P=0.000). Similarly, in the columns examining the impact on politeness, no intervention shows any positive effect except for that of the decoy visits themselves, which appear to increase politeness.

[2] While the relatively high attrition in the weekly off/duty rotation and all interventions groups might be attributed to the absence of staff on the day of the survey due to their being on weekly off, by the time of the end line we find no actual difference in days of leave between the police stations employing weekly off and those that were not.

Thus, the decoy survey program had a substantial effect on both FIR registration and behavior of the police. This is despite the fact that the program was quite explicitly un-linked to any possible sanction: the police officers were warned that decoys could take place, but that the results would not be communicated to their superiors. None of the other interventions were effective. Given the importance of accurate registration of crimes, this suggests that a policy of allowing some organizations to conduct decoys may actually be promising.

Need of an Independent Crime Survey

The low rate of registration in the decoys, plus the fact that many crime victim do not actually bother to report the crime, suggests that using crimes registered in police stations as measure of crime rate is profoundly misleading. Indeed, the analysis of the data of household crime survey conducted during the course of the program reflects public perception of crime better than the official registration of crime at the police stations. The official claim of reduction of crime by the police has no impact on public perception. This is clear from Figure 3-4, which shows no relationship between actual crime victimization and official crime rate, but a positive relationhip between crime victimization and perception of crime (even among non police victims). This underscores the need for an independent crime survey, which could be conducted in conjunction with nationally representative surveys (e.g. National Sample Surveys)

Challenges in Implementation

The program showed some significant outcomes in the short span of 18 months, yet failed to generate robust effect on a broad change in either police performance or public perception of the police. Two of the interventions seemed to have had no effects. Some of the possible reasons stem from difficulty encountered during the implementation of the project, and are a useful warnings for future programs.

Poor implementation of the interventions

Despite the absolute interest and commitment of senior police leadership of the state, the lack of interest on the part of Station House Officers (Chief of Police Stations) was evident. The reasons may be manifold: no direct benefit to SHOs from the intervention, lack of manpower and other resources, unforeseen externalities e.g. the violent Gujjar agitation in the state, series of terror blast in Jaipur etc. Consequently, as the time passed the police stations did not sustain these interventions, which is clear from the discussion below:

Community Observer

A simple and revealing indicator of the implementation of the community observer program was whether surveyors encountered a community observer while visiting the police stations. Each police station in the program designated a specific time during which the observer was available on each day. Ideally surveyors would have met them during their visits. However, in actuality, observers were present only 10.29% of the times. Moreover, the observers did not seem to respect the planned time schedule: surveyors met community observers approximately 10% of the time regardless of whether they visited during the official times or not.

A more detailed record of the presence of community observers can be found in logbooks of observer visits and comments maintained by each police station. These reports varied widely: certain districts appeared to maintain the community observer program at full strength throughout the project while others seem to have given up entirely by the last 3 months. However, these logbook reports need to be taken with a grain of salts: surveyors found that 74% of the visitor logbooks were always or often in the same handwriting, and in 40% of the time the comments seemed to have been written by the police themselves.

Overall, it appears that the community observer interventions was not implemented in a sustained and complete way.

No Transfer

The ban on transfers can be measured primarily through administrative data from personnel records. If the ban on transfers were effective, we should expect to see no transfers occurring in police stations selected for the project, or at least noticeably fewer than in the control group. We matched names of officers from police station staff lists at the beginning of the program with those from the end of the program, and counted any officers not found as having been transferred. Table 3.6 shows that there was indeed a significant reduction in the number of transfers in police stations in the ranks that were targeted (13 percentage point for constables, or 44% and 15 percentage points for inspectors), but transfers during the course of the project were substantial.

The ban on transfers, while somewhat effective for certain ranks, was substantially less than a complete freeze. For the more politically sensitive ranks, Inspectors and Sub-Inspectors, the project made the greatest difference and came furthest from achieving its goal. Yet, we have seen that even this relatively small reduction in transfers produced significant increase in citizens' satisfaction. This is encouraging, and suggest that a stronger commitment to the no-transfer policy could produce even larger improvement.

Weekly off

On Paper the implementation of the weekly off program was much better than the other interventions: 84% of treatment police stations reported executing some type of rotating day off, whereas only 34% of control police stations reported having any scheduled day off. In keeping with program implementation guidelines that allowed a longer rotation period for smaller police stations, the regular day off was not always weekly: 60% of police stations in the project has rotation terms longer than 8 days. In fact, the size of the police station mattered little for the rotation period of the weekly off: police stations with 15 or less staff had an average period of 13.8 days, whereas larger police stations had a period of 11.7 days.

In order to verify that the police station chief was in fact giving days off to his staff, surveyors randomly selected two constables from the staff and interviewed them separately. Surveyors asked the constables to recall the last time that they had had a day off for any reason, including both the weekly off program and any other days off. The results show that the weekly off program did succeed in shortening the time since the last day off by an average of 4.87 days, going from an overall average of 30.03 days in control stations to 25.16 days in treatment stations. However, table 3.7 shows that this difference seems to decrease over the course of the program, eventually disappearing by round 5, our last visit. Since the program was designed to give a day off every 8 days, we can inspect this outcome directly in table 3.8. Once again, the project seems to have been operating in the original rounds, albeit not at full implementation or as well as the police records would suggest. Over time implementation decreased until, at the end of the program period, there is no statistically detectable difference between treatment and control.

Duty Roster

Since the duty roster was implemented in the same police stations as the weekly off, the results might be expected to be similar. Here too, implementation on paper was close to complete, with 91% of SHOs of treatment police stations able to produce a duty roster when asked. However, only 58% of program police stations posted the duty roster on the wall as stipulated by the implementation guidelines, as opposed to 39% of control police stations that had independently created duty rosters.

When surveyors asked constables about the duty rotation, these major differences between treatment and control police stations almost disappeared. Table 3.9 shows the responses over time to questions about the rapidity of changing duties and the amount of knowledge constables have of their future duties. Most notably, in police stations with the duty rotation, constables spent slightly more time at each duty, although this difference is not statistically significant. Constables in police stations with duty rotation did have a significantly higher probability of reporting that they know what their next duty would be, with an overall average of 58% in treatment police stations and 51% in controls. Unlike in the weekly off, this effect appears to have grown stronger over time, with the greatest treatment/control differences in the final periods. Interestingly, there is also a strong downward trend in both treatment and control police stations, perhaps due to the police becoming more familiar with the surveyors. These results suggest that the effect of the duty rotation program may have been to transform a system in which duties were in fact rotating fairly rapidly, albeit on an ad-hoc basis, into one in which transition from one job to another was more predictable. Training The training was the easiest intervention to implement, and there is administrative evidence that it took place as planned. Rajasthan Police Academy records show that 88% of officers selected for training reported to the Academy for training. It is also interesting that this is one intervention that had a significant impact on victim satisfaction. It seem to be low hanging fruit: easy to implement, popular with the constables, and effective.

Policy Recommendations

The fundamental conclusions of the Rajasthan Police Reform Program fall into two major categories: First, several of the interventions demonstrate that it is possible to affect the public image of the police in a relatively short period of time, using an affordable and simple set of interventions. Decoy visits were effective in getting the police to register more crimes. Reducing transfers decreased citizens' fear of the police and increased the satisfaction of crime victims. Training police staff in investigation techniques and public relations skill (soft skills) also increased the satisfaction of crime victims. Furthermore, it appears to have a real impact on the actions of the investigating officers, causing them to arrest more suspects and take more actions subsequent to the registration of an FIR (at least according to the victim's perception). These results suggest that the police system is not completely resistant to change, and that public opinion can be affected in the short term—that practical police reform is possible. Second, the lack of effectiveness of the program on other dimensions carries lessons for the design and implementation of future government sector reform projects. At some level the cause of failure cannot be identified—we cannot see whether the programs that showed little impact would have been successful had they been implemented more rigorously or whether they were flawed in conception. As already discussed, this program was conceived to be executed within the framework of existing condition of police, incentives for the implementation of reforms was not incorporated into the design. Therefore, while the senior police leadership consistently supported the reforms and gave orders for their implementation, police station staff gradually ceased to carry out the program elements, perhaps even going so far as to falsify the community observer records. Since the long term, system-wide benefits perceived by the police leadership were not internalized by the police station SHOs, who only experience the short term costs, the reform project gradually ceased to function over 18 months. This did not affect the training, which was easy to monitor and implement, or the decoy programs, which was run by an outside organization committed to the program. But this was a key limitation for the community observer program and for the weekly off. Future interventions should either not need to rely on the continuing collaboration of the SHO, or make sure it is in their interest to carry them out.

Overall, on the basis of evidence generated by the program in Rajasthan, following policy recommendation is suggested to improve the police performance and public perception: 1. National Crime Victimization Survey: The police reform project of Rajasthan strongly suggested the need to have a national crime survey conducted periodically by an independent agency. This will help in identifying problem areas in terms of incidence and nature of crime as well as taking appropriate policy measures to address them. The household survey of this program provides a template for how this can be done. 2. Freezing of Transfers: Increasing transfer time appears to have significant impact on effectiveness, public relations, as well as decreasing staff grievances. Although this is often presented as a politically difficult reform, this project has shown that it is both feasible and beneficial and as such is recommended as a permanent goal. 3. Training: Both the trainings (Professional Skills and Soft Skills) showed positive effects on public satisfaction and crime investigations. These interventions are straightforward to scale up and should be extended to the remaining Rajasthan Police staff. In fact, encouraged by the results of these trainings, the Bureau of Police Research and Development (BPRD), Government of India granted special sanction to the Rajasthan Police to expand these trainings to the rest of the staff.

More research is needed on how the weekly day off can be implemented in a more sustained way, and whether this would result in improvement in performance (the results are not encouraging so far). The community observer interventions, as designed, does not seem effective as a way to provide accountability to the public. However, the large impact of the decoy visits suggest that an interventions that would provide regular feedback ot the public on whether the police serve their basic need (registering a complaint) could do a lot to change their behavior. This intervention would need to be designed and tested before any more recommendation can be made, however.

On the whole, the Rajasthan Police Reform Program has shown that simple and cost effective interventions at cutting edge level can improve police performance in terms of effectiveness and efficiency. It has further reaffirmed that improving the efficiency of police has the potential to create positive public image, so very essential in contemporary policing. Police has to overcome its stubborn and traditional attitudes and beliefs by creating a positive synergy among the stakeholders.

Tables

Table 3.2: Public Opinion

	Police Responsiveness to Citizens			
All Interventions	-0.02 (.780)	-0.026 (.718)	-0.026 (.71)	-0.034 (.622)
No Transfer	-0.038 (.633)	-0.02 (.802)	-0.018 (.82)	-0.025 (.757)
Weekly Off	0.152 (.063)	0.148 (.084)	0.154 (.072)	0.144 (.088)
Community Observer	0.108 (.209)	0.09 (.317)	0.094 (.306)	0.091 (.319)
Percentage staff trained	0.085 (.22)	0.07 (.324)	0.071 (.312)	0.061 (.367)
	Reducing Police Corruption			
All Interventions	0.084 (.304)	0.089 (.288)	0.089 (.289)	0.076 (.358)
No Transfer	0.003 (.944)	0.036 (.638)	0.036 (.633)	0.023 (.758)
Weekly Off	-0.026 (.711)	-0.046 (.529)	-0.042 (.563)	-0.051 (.483)
Community Observer	0.045 (.49)	0.037 (.573)	0.04 (.545)	0.046 (.491)
Percentage staff trained	0.000 (.976)	-0.031 (.608)	-0.029 (.628)	-0.034 (.565)
	Increase Police Resources			
All Interventions	0.010 (.888)	-0.026 (.662)	-0.028 (.646)	-0.021 (.722)
No Transfer	-0.039 (.612)	-0.045 (.463)	-0.046 (.456)	-0.035 (.565)
Weekly Off	-0.096 (.238)	-0.03 (.563)	-0.035 (.509)	-0.033 (.536)
Community Observer	-0.066 (.479)	-0.024 (.682)	-0.026 (.656)	-0.031 (.585)
Percentage staff trained	0.086 (.255)	0.037 (.479)	0.04 (.45)	0.042 (.422)
		Controls		
Police Station Fixed Effects	Yes	Yes	Yes	Yes
HH Characteristics	No	Yes	Yes	Yes
HH has been victim of crime or arrested	No	No	Yes	Yes
Source of HH information about police	No	No	No	Yes

Table 3.3: Victim Satisfaction

Whether Respondent Reported being "Satisfied" or "Completely Satisfied" with Police Investigation			
All Interventions	-0.13	-0.09	-0.07
	(0.45)	(0.49)	(0.61)
No Transfer	0.16	0.16	0.17
	(0.32)	(0.23)	(0.20)
Weekly Off	-0.11	-0.02	0.02
	(0.41)	(0.88)	(0.89)
Community Observer	-0.27	-0.27	-0.36
	(0.22)	(0.23)	(0.10)
Percentage staff trained	0.38*	0.35*	0.50**
	(0.02)	(0.01)	(0.00)
		Controls	
Police Station Fixed Effects	Yes	Yes	Yes
Type of Crime	No	Yes	Yes
HH Characteristics	No	No	Yes
N	489	488	463

Table 3.4: Decoy Survey Outcomes

Decoy Surveyor's Reported Outcomes of Case Registration				
	FIR Registered		Politeness of Police	
All Interventions	-0.18	-0.16	-0.20	-0.15
	(0.38)	(0.39)	(0.06)	(0.29)
No Transfer	-0.06	-0.04	-0.04	-0.01
	(0.32)	(0.37)	(0.36)	(0.68)
Weekly Off	0.04	0.04	0.03	0.04
	(0.46)	(0.42)	(0.35)	(0.20)
Community Observer	0.05	0.05	0.00	-0.01
	(0.42)	(0.45)	(0.92)	(0.84)
In Project	-0.14	-0.13	-0.15	-0.11
	(0.48)	(0.46)	(0.13)	(0.44)
Percentage staff trained	0.00	0.00	0.02	0.02
	(0.93)	(0.98)	(0.43)	(0.53)
Number of previous decoy	0.06	0.07	0.04	0.03
visits to police station	(0.11)	(0.06)	(0.06)	(0.22)
Time (in months)	0.02	0.02	-0.03*	-0.03
	(0.59)	(0.54)	(0.03)	(0.14)
		Controls		
Crime story used, whether staff suspected decoy, surveyor name	No	Yes	No	Yes
N	837	834	832	829

Table 3.5: Attrition

	Percent Missing from Baseline	95% Confidence Interval	
All Interventions	37%	32%	42%
No Transfer	31%	26%	35%
Weekly off / Duty rotation	37%	31%	43%
Community Observer	27%	22%	33%
Control	44%	38%	50%

Table 3.6: Police Staff Transfers

Rank	Percentage Transfered		
	Treatment PS (No Transfer)	Control PS	Difference:
Inspector	49%	64%	15%
Sub. Inspector	48%	68%	20%
Asst. Sub. Insp.	28%	30%	2%
Head Constable	28%	34%	6%
Constable	17%	30%	13%
All Ranks	21%	33%	12%

Table 3.7: Weekly Off Implementation - I

Days since last day off					
Police Station type:	Round 1	Round 2	Round 3	Round 4	Round 5
No weekly off	29	32	28	31	30
Weekly off	21	22	25	26	32
Difference:	8	11	3	5	-2

Table 3.8: Weekly Off Implementation - II

Percentage of staff who had a day off in last 8 days:					
Police Station type:	Round 1	Round 2	Round 3	Round 4	Round 5
No weekly off	25%	22%	22%	23%	23%
Weekly off	46%	31%	36%	29%	27%
Difference:	21%	9%	14%	6%	4%

Table 3.9: Implementation of Duty Roster

Number of days constable has been doing current duty					
Station type:	Round 1	Round 2	Round 3	Round 4	Round 5
No duty rotation	4.05	3.97	6.27	4.17	2.70
Duty rotation	5.58	4.51	5.33	4.50	2.78
Difference:	1.53	0.54	-0.94	0.33	0.08
Whether constable knows what his next duty will be:					
No duty rotation	77%	68%	40%	39%	37%
Duty rotation	83%	72%	43%	46%	54%
Difference:	6%	4%	3%	7%	17%

Figures

Figure 3-1:

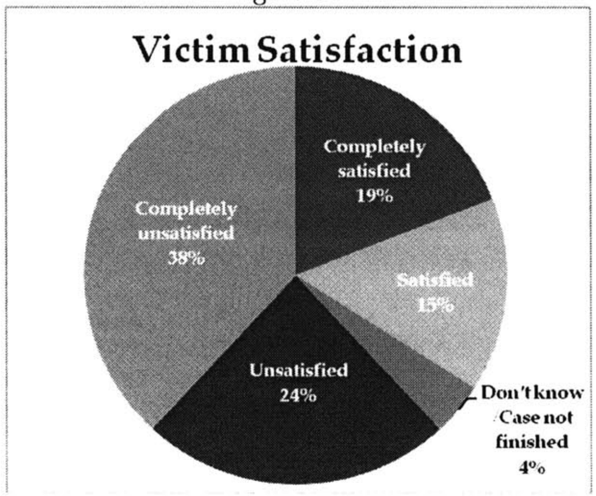


Figure 3-2:

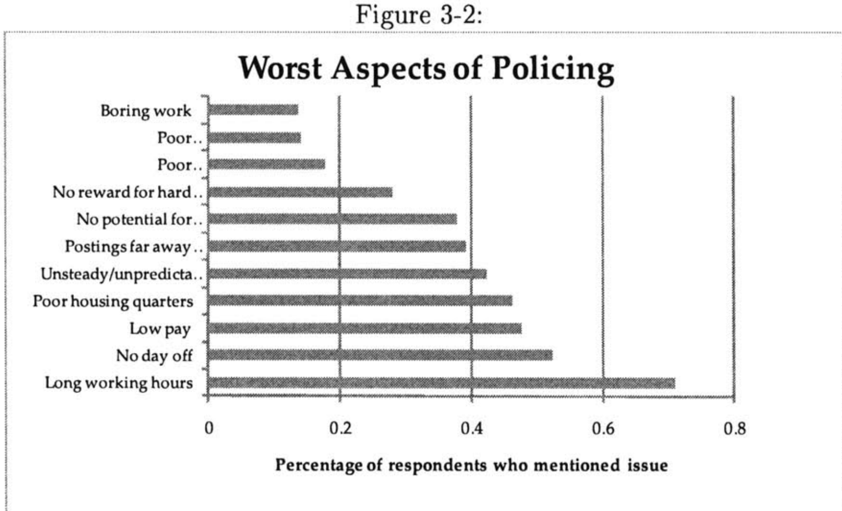


Figure 3-3:

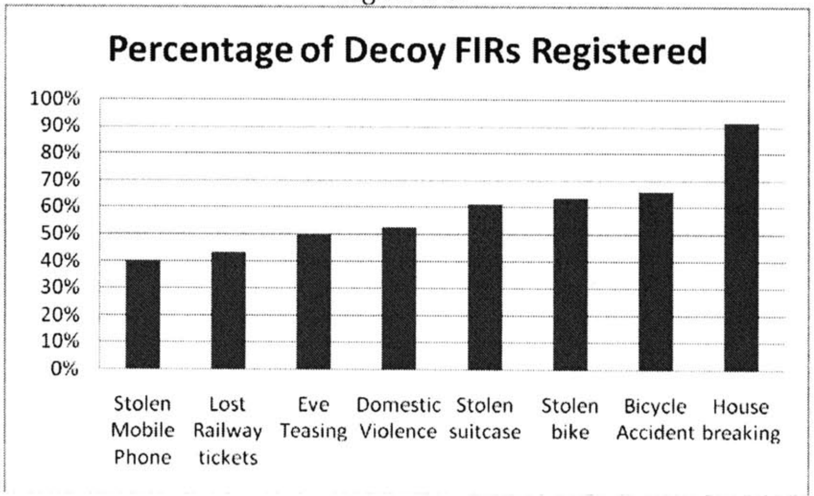
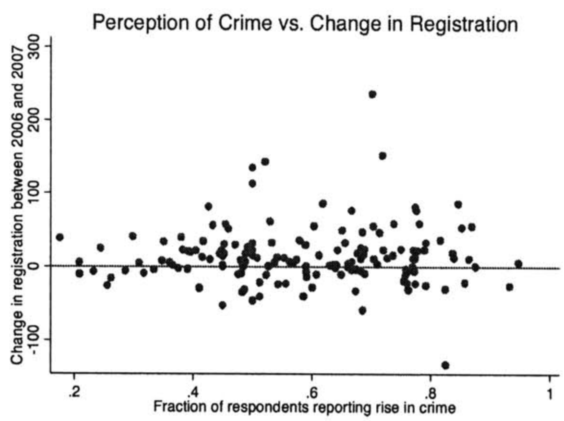
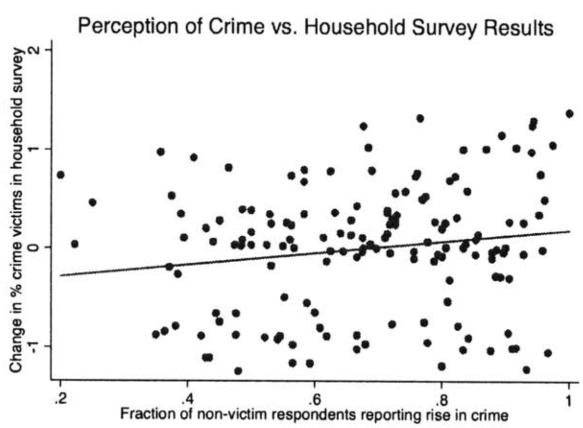


Figure 3-4:





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