All in the Family: Kinship Pressure and Firm–Worker Matching *

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March 20, 2025

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Abstract

The modal firm in low- and middle-income countries has no non-family-member employees. While this fact is often attributed to informational or contractual frictions, an alternate view is that pressure to offer financial assistance to extended family in the form of employment may distort employers' hiring decisions. I design three field experiments—conducted with both urban firms and agricultural employers in Zambia—to test whether such pressure impacts firm hiring and examine its productivity implications. In the first experiment, urban firms are offered the chance to receive a 3-month subsidy to hire a full-time permanent employee. A subset of firms is then randomized into a treatment providing them plausible deniability in their hiring decision: receipt of a poster that suggests the firm may not have been eligible for the subsidy had it hired a relative. The treatment increases the probability that the firm chooses to hire a nonfamily employee, rather than a relative, by 11 p.p. (20%). In the second experiment, I show that it is socially very costly for employers to hire a nonrelative: When firms have plausible deniability, the subsidy required to induce a firm to choose a nonrelative over a relative falls substantially, to the equivalent of 4% of total monthly profits. This effect is concentrated among firm owners who report higher redistributive pressure at baseline. In the third experiment, I show that in maize shelling, a common agricultural job where productivity is measurable, a worker is likelier to shirk when working for a family employer, leading relatives to be 7% less productive than nonrelatives. Consistent with moral hazard problems being more acute with relatives, employers across samples report that firing a relative would lead to social penalties. Similarly, in the urban sample, the treatment effects are larger among firms that report more difficulty verifying worker effort at baseline. These findings suggest that social pressure to hire relatives may distort the composition of employment and productivity in developing countries.

*I am thankful for support and advice from Supreet Kaur, Ted Miguel, Ned Augenblick and Frederico Finan. Luisa Cefala, Madeline Duhon, Stefano DellaVigna, Benjamin Faber, Matt Lowe, Jeremy Magruder, Nick Tsivanidis and David Wu provided helpful feedback. I gratefully acknowledge financial support from the Center for Effective Global Action (CEGA) through its its Development Economics Challenge and Psychology and Economics of Poverty initiatives, Private Enterprise Development in Low- Income Countries, the UC Berkeley Strandberg Fund and the Weiss Family Fund for Research in Development. Amanda Wong provided excellent research assistance. This study received IRB approval from UC Berkeley and UNZAREC. This study was registered in the AEA RCT registry, AEARCTR-0011328.

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1 Introduction

Few businesses in low- and middle-income countries (LMICs) hire employees outside their family network (Jayachandran, 2021). This fact is often attributed to contracting frictions and missing information markets, as hiring relatives might alleviate information asymmetries or reduce the scope for moral hazard (Chandrasekhar et al., 2020).

This paper proposes that some employers face pressure to offer financial assistance to extended family members in the form of employment, a hypothesis with potentially different productivity implications. This theory is consistent with a literature in anthropology (Akyeampong et al., 2014) and a long economics literature showing that individuals in LMICs face redistributive pressure from their kinship network to share income (Dupas and Robinson, 2013; Jakiela and Ozier, 2016; Goldberg, 2017; Squires, 2018; Di Falco et al., 2018; Fiala, 2018; Carranza et al., 2022; Di Falco et al., 2019; Riley, 2022).

In this paper, I test whether such pressure impacts employers' hiring decisions, quantify how costly it is for employers to tell family members that they will not hire them, and, finally, estimate the returns to employers from hiring relatives as opposed to nonfamily employees. Since family pressure is not randomly assigned, I use a set of field experiments to manipulate the amount of pressure employers face when they hire workers.[1] I vary this pressure by offering some firms *plausible deniability* in their hiring decisions: Specifically, some firms receive a poster that suggests the firm may not have been eligible for the subsidy had it hired a relative.[2] I then measure whether firms that have plausible deniability to hire outside their family change whom they decide to hire.

I run three separate experiments to test this hypothesis. The first experiment tests whether family pressure impacts hiring under naturalistic hiring conditions. The second and third experiments have more involved protocols to extract further information about how employers make hiring decisions under pressure and measure the productivity impacts of hiring family and nonfamily employees.

While the experiments differ, all three broadly follow the same design: Employers receive a subsidy to hire an employee so that I can observe their hiring decisions.[3] Employers are then randomized into one of two conditions. In the control group, employers are told that they will receive a poster stating that they received a subsidy and selected the person hired. In the plausible deniability group, employers are told that they will receive a poster stating

[1] A common approach in the literature is for researchers to vary whether an individual takes an action publicly or privately and then measure how the action changes in response to others observing it (Bursztyn and Jensen, 2015, 2017; Bursztyn et al., 2017; Breza et al., 2019; Carranza et al., 2022). This design is infeasible in my setting since who is hired is almost always observable by others.

[2] For this intervention to be effective, hiring norms must be such that a business owner is punished if she *chooses* to hire a nonrelative for a job that is desired. I show evidence that this is indeed the case in my context.

[3] The process that I follow to offer subsidies generally follows the protocol of De Mel et al. (2019), who also offer subsidies to businesses to hire an employee.

that they received a subsidy but that some employers were unable to use the subsidy to hire their preferred candidate. The poster in the second group makes it easier for employers to tell their family that they would not have received the subsidy had they hired a relative. To ensure that the treatment does not involve deception, the employers in both treatment conditions are sometimes offered a take-it-or-leave-it subsidy to hire an employee who is not their preferred candidate.[4] I then measure the likelihood that the employer chooses to hire an employee related to her with the subsidy across treatment conditions.

In my first experiment, the "urban field experiment," I begin by testing whether social pressure alters hiring decisions for full-time employees among a sample of 253 small urban enterprises in Zambia. Firm owners are told that they might receive a 3-month subsidy to hire an employee and should identify a candidate for the job.[5] To create variation in plausible deniability, I give employers in the control group a poster stating (among other things) that they were involved in selecting the employee. In the plausible deniability treatment, business owners receive a poster stating that some employers receive subsidies for employees they do not select, which allows employers to tell their family they would not have received the subsidy had they hired a relative.[6] I measure how the introduction of this poster changes whom employers choose to hire.[7]

The primary finding from this experiment is that plausible deniability decreases firm owners' likelihood of hiring a family member: For employers who receive the plausible deniability poster, the likelihood of hiring from their family declines by 12 p.p. (20%) ($p = 0.06$). There is also suggestive evidence that employers in the plausible deniability treatment are able to select more productive employees. The employees selected by employers in the plausible deniability treatment are expected to earn higher wages and are likelier to have some secondary education.

To measure the magnitude and incidence of these family pressure costs, I turn to my second field experiment, the "urban mechanism experiment," conducted with 619 urban employers. This experiment broadly follows the same design as the urban field experiment, with two key deviations: First, before I introduce the variation in plausible deniability, employers are asked to identify a family and a nonfamily employee for the subsidy. Second, after treatment assignment, I offer employers varying subsidies for family and nonfamily employees and use the Becker–DeGroot–Marschak method to estimate how much larger of a subsidy employers must be offered to be induced to hire a nonfamily employee. Comparing this wedge across treatment arms allows me to quantify the pressure cost associated with hiring nonfamily

[4] The control group's poster is also adjusted in such cases so as not to involve deception.

[5] Urban employers in this setting typically hire employees for medium-term durations (median = 6 months).

[6] To ensure that the treatment does not involve deception, employers are sometimes offered the subsidy for an employee of their choosing and other times for another jobseeker. The poster received by the control group is adjusted in cases where the control group does not select the employee.

[7] In this experiment, the subsidies are then implemented probabilistically, ensuring that the experiment is feasible but the choices are incentive compatible.

employees.

In this second experiment, I replicate the finding from the urban field experiment that access to the plausible deniability poster leads employers to choose family members as employees less often: When employers are offered the same subsidy for each employee, they are 16 p.p. (25%) less likely to choose to hire a relative. Consistent with the hypothesized mechanism, the treatment effects are concentrated among employers who report more social pressure from their family at baseline: These employers reduce their hiring of family employees by 20 p.p. when offered plausible deniability, while those who do not report family pressure do not change the identity of whom they hire across treatments. In addition, I find suggestive evidence that this pressure may lead to less productive employer–employee matches. Employers who perceive that their family employee would exert less effort for them than other employers reduce their hiring of family employees by approximately 30 p.p.

I then estimate how costly it is for employers to tell family members that they did not hire them for a job by measuring the difference in the amount of subsidy I need to offer employers for them to hire a nonrelative in the control and plausible deniability conditions. For employers with plausible deniability, the amount of subsidy they must be offered to select a nonrelative declines by 38 Zambian kwacha (ZMW; $p = 0.01$). This difference is meaningful, corresponding to 7% of the median subsidy offered in the experiment or 4% of the median firm's monthly profits. This suggests that firms are willing to forgo hiring nonfamily employees whom they expect to increase their profits substantially to avoid telling their family that they did not hire a family member.

Finally, in the "rural mechanism experiment," I estimate the returns to hiring family versus nonfamily employees. I conduct this experiment with employers hiring workers for a common agricultural task, maize shelling, in which productivity is measurable and contracts are only several days long, making it feasible to provide subsidies to all employers. To conduct this experiment, I follow broadly the same protocol as in the urban mechanism experiment, with two key deviations. First, given the level of literacy in this population, I cannot use a poster to introduce variation in plausible deniability for employers. Therefore, rather than transmitting the information about how hiring decisions are made with a poster, I verbally transmit the information about whether the employer was responsible for the hiring decision to the employees. Second, after the experiment, I randomize most employers into randomly receiving a subsidy, for either the selected relative or anonrelative, and measure the productivity effects of their hiring family as opposed to nonfamily employees.

In the study's next set of findings, I again replicate the results from the prior two sets of experiments. Employers with less plausible deniability for their choice are 26% likelier to hire a relative ($p < 0.01$), with the effect concentrated among employers who report a higher likelihood of facing social sanction and who expect their family employee to exert less effort when working for them. I estimate a large pressure cost, with employers on average willing

to forgo 8 ZMW, approximately 20% of the *daily wage*, to avoid telling a relative that they did not choose to hire her. Again, I find that these effects are concentrated among employers who report facing redistributive pressure at baseline.[8]

I then measure the productivity impacts of firms' hiring of relatives by comparing output for employers randomly assigned to receive a subsidy, for either their relative or their nonrelative. Relatives produce 7% less output ($p = 0.04$). Moreover, the output of family employees is 7% likelier to show defects ($p = 0.06$) (i.e., it is of lower quality), implying that it is likely to lead to a lower sales price.[9] This suggests that hiring pressure may have meaningful impacts on employers' productivity by leading them to hire less productive employees.

Relatives' lower productivity might stem from the selection of worse types or from moral hazard that leads employees to work less well when matched with family employers. To evaluate each of these explanations, I offer family and nonfamily employees a contract to shell maize in return for a piece rate that I pay them. In this task, I find no productivity difference between employees related and those unrelated to the employer. This suggests that the lower productivity estimated when employees work for family employers is driven not by underlying differences in ability but instead by their working less hard for family employers. This is consistent with both the treatment effect heterogeneity that I find in all three experiments and survey evidence that I collect among employers, which shows that almost half expect that family employees work less hard for them than they do for nonfamily employers.

I then consider a set of alternate interpretations of these results. Using variations in the basic experiment design, I show that 1) the signaling motive for hiring family employees that I identify in my experiments is perceived as a cost, not a benefit, by the majority of employers and 2) the results are not driven by the selection of employees who would not be the marginal employee outside the experiment. Taken together, these results provide some of the first evidence for a social pressure mechanism contributing to the prevalence of family businesses in low-income countries.

Why does this pressure persist when alternate transfer mechanisms—for instance, hiring the most productive employee and then making larger cash transfer payments to family members—seemingly offer benefits for all parties? Moreover, how does this mechanism accord with our common understanding that relationships alleviate contracting frictions? While a full discussion of these questions is beyond the scope of this paper, I provide two pieces of evidence suggesting that this norm may be sustained by divergences between employers' and other family members' beliefs about family employees' productivity. While approximately half of firm owners expect that family employees work less hard for them than they would for nonfamily employers, *almost all* employers expect their *family* to believe that their family em-

[8] This large cost is plausibly due to the mechanism used to disseminate the information about hiring choices.

[9] This lower quantity equilibrium does not appear to be the result of employers optimally choosing a lower-quantity, lower-wage contract when hiring relatives; the treatment effect on the wage paid to relatives is statistically insignificant.

ployee would work *harder* for them than for a nonfamily employer. Moreover, the treatment effects in the urban mechanism experiment are concentrated in jobs where the employer states that it is "easy to make excuses for poor performance," making it harder to verify employee effort. Taken together, these findings suggest that hiring pressure might be concentrated in cases where there are disagreements in the family about an employee's performance and where it is difficult to update beliefs about this.

This paper extends a literature in development related to the role of redistributive pressure in distorting investment, saving, and labor supply decisions. A long experimental literature shows that individuals take costly actions to hide income, while heterogeneity in field experiments suggests that the effect of new saving technologies is particularly important for those who face redistributive pressure (Dupas and Robinson, 2013; Jakiela and Ozier, 2016; Goldberg, 2017; Squires, 2018; Di Falco et al., 2018; Fiala, 2018; Di Falco et al., 2019; Riley, 2022). However, there is limited field evidence for the effects of kinship pressure on margins beyond savings and investment, with the exception of a recent paper that documents its impacts on labor supply (Carranza et al., 2022). I contribute to this literature by highlighting a new and economically important margin through which such pressure might manifest—hiring choices—and by enacting a field experiment to quantify this pressure and explaining how it might persist.

This paper also speaks to a development literature on the functioning of labor markets in LMICs and the existence and microfoundations of frictions in these markets (Breza et al., 2019; Kaur, 2019; Breza et al., 2021). It also builds on two recent contributions to the literature that highlight the social and redistributive purposes of work (Hussam et al., 2022; Macchi and Stalder, 2023). This paper extends these papers by proposing an alternate mechanism for redistribution, namely, employment, and documenting the productivity losses associated with work-related transfers.

This paper also builds on a sizable body of behavioral literature exploring the influence of social pressure and signaling motivations on economic behavior in a variety of domains (Bursztyn and Jensen, 2015; Bursztyn et al., 2017; DellaVigna et al., 2012; Karing, 2018). It also contributes to a related literature documenting how "wiggle room" and uncertain mappings from actions to outcomes can lead to sizable changes in behavior (Dana et al., 2007). This paper contributes to this literature by applying this insight to an important domain and estimating the productivity losses associated with this social pressure.

Finally, this paper also advances a sizable behavioral literature documenting that social ties across workers may distort workplace effort and team selection (Bandiera et al., 2005, 2009, 2013). I contribute to this literature by extending the focus from ties among workers to ties across employers and employees and therefore introduce a possible distortion arising from social ties to the long literature considering the reasons for network-based hiring (Beaman and Magruder, 2012; Beaman, 2016; Pallais and Sands, 2016; Heath, 2018; Chandrasekhar et

al., 2020). By blending these two literatures, I offer a complementary explanation for some hiring from social ties, namely, that this hiring reflects social norms—an argument that has been advanced by Bertrand and Schoar (2006).

The remainder of the paper proceeds as follows. Section 2 outlines the context of the experiments. Section 3 describes the design of the three experiments. Section 4 provides summary statistics and explains the empirical strategy. Section 5 presents the results of the urban field experiment. Section 6 presents the results of the urban mechanism experiment. Section 7 presents the results of the rural mechanism experiment. Section 8 discusses alternate explanations for the empirical patterns. Section 9 discusses what leads to the persistence of the pressure to hire. Section 10 concludes.

2 Setting

Urban Samples

The experiments with urban employers focus on owners of micro, small and medium-sized enterprises in Chipata and Katete— two urban centers—in Eastern Province, Zambia. In February 2023, we conducted a listing of all markets located in these two cities.

Labor Markets and Hiring in Context

Attachment between workers and firms is relatively short in the context that we study here. Among firms with employees, the mean length of tenure of a current employee at a firm is 30 months, with the 25th percentile and median being 6 and 24 months, respectively. The average employee in the sample is paid around 600 ZMW per month (approximately 30 USD).

Cost does not appear to be a major factor in the decision to hire family versus nonfamily: The average wage paid for relative and nonrelative employees is the same (though approximately 10% of relative employees are unpaid, while the proportion of nonrelative unpaid employees is lower).

In line with findings from prior literature, relatives do appear to be likelier to be hired for particular kinds of jobs. Consistent with the results of Bloom et al. (2013), they are 12 p.p. likelier to be hired for a job in management and 30 p.p. likelier to be hired for jobs involving money, and their likelihood of being hired is increasing in employers' ranking of the trust requirements of the job. However, relatives are hired approximately 30% of the time even for jobs that do not involve management, money or lower levels of trust, suggesting that other channels might also explain why such individuals are hired.

The most common relatives hired are not immediate family members: 22% are nephews, and 20% are cousins. These groups are followed in hiring frequency by more immediate relations: 17% are children of the owner, and 13% (8%) are the owner's brother (sister).

There appear to be no significant differences in the types of jobs relatives hold by distance of the kinship relation to the employer.

Rural Sample

The rural experiment focuses on farmers in villages around Chipata districts who hire workers to shell their maize after harvest.

Labor Markets and Hiring in Context

Shelling of maize is a common task in villages that farmers hire workers to do, conditional on the farmers' having a large harvest. Manual shelling of maize involves several steps: i) removing husks, if the maize has not already been dehusked; ii) shelling the maize, which typically involves beating the maize husks to knock the maize off of the cob; and iii) cleaning the maize, which involves sifting maize shells out from dust, rocks and other debris that may have gathered among the shells.

Manual maize shelling can be completed in one of two ways. The maize can be put into a bag and beaten. This method speeds up the process of knocking the shells off the cob but increases the likelihood that the shells may be damaged[10] and of the grains being broken, with broken grains commanding lower prices and being more susceptible to pest and insect attacks (Hodges and Maritime, 2012a,b). Alternatively, the maize can be shelled slowly by hand. This process is less likely to damage the maize but requires effort and is time consuming (Nsubuga et al., 2021). Because of these differences in how the task can be completed, measuring both quantity and quality of the output is important.

Labor markets for this task are governed by short-term contracts: The median length of an employment contract for such work is just 3 days. Hiring of relatives is common. Among those employers who stated that they hired an individual to shell their maize in the prior year, 76% reported that they had hired at least one relative to do so. Contracts are often agreed by an employer searching for a worker 1–2 days before needing her, and the work is usually completed at the household of the employer. The modal contract in the setting is a half-day contract, with the worker starting in the morning and finishing around lunch time. Workers are paid either for a half-day's labor or by the volume of maize shelled—although there is no general prevailing wage in the village and the terms are generally bargained between employer and employee.

[10] Estimates of the postharvest losses attributable to such shelling procedures range from 1% to 6%.

3 Experiment

Treatment Overview—Family Pressure and Hiring Choices

The key hypothesis in this paper is that employers face social pressure when choosing whom to hire as an employee and that this pressure influences their hiring decision. To test this, I create an intervention that offers some employers more plausible deniability for not hiring a particular person. I then measure how employers who know that they can make excuses for not hiring someone changes their hiring decisions.

To achieve this, I leverage the specifics of the norm in this setting. Employers report that they expect to come under social sanction from their family should the family learn that they chose to hire someone else for a job that a family member wanted. However, they report being much less likely to face familial sanction if they hire a nonfamily employee because they have no other choice (for example, because no one in the family wants the job). I use this fact to offer some employers the ability to tell others that they had "no choice" but to hire someone. I then measure how employers' knowledge that they have this excuse changes whom they choose to hire.

In all three experiments, I vary what information is signaled to others about why a new employee was hired in the business. In the control condition, when employers choose to hire an employee, they know that others will learn that they selected that employee for the position. In the treatment condition, by contrast, employers instead are offered the ability to signal to others that the person who was hired may not have been the employee whom they would have selected themselves. To ensure that the treatment does not involve deception, I sometimes offer employers in both conditions the subsidy for their selected employee and sometimes do not.[11]

Treatment Implementation – Urban Field Experiment

The experiment broadly follows the protocol used by De Mel et al. (2019) to offer employers a subsidy to hire an additional employee into their business for 3 months.[12] Employers are told that they will receive a subsidy for either a local jobseeker or a person of their choice. They are then told that during the period of the subsidy, they will be asked to display one of the following posters in their business:

- **Control Poster:** The poster states that the employer received a subsidy for his business

[11] In cases where employers in the control group receive a subsidy for someone whom they did not select, their signal is adjusted to state that they were offered a subsidy by our project for the selected employee.

[12] To ensure there are no "coercive" hires, it is stipulated that the subsidies not be used for household members or children under 18. A listing of household members is taken before the subsidy is explained, and the ID of the employee is checked before she starts working under the implemented subsidies.

and that he was involved in selecting the employee.[13]

- **Plausible Deniability Poster:** The poster states the same information as the control poster, but in describing the involvement of the employer in selecting the employee, it states that in many cases business owners receive subsidies for employees chosen by the program rather than for their own first-choice hire.[14]

Enumerators explain to businesses in the treatment group how the poster can be used to justify not hiring a particular person.[15] Enumerators then leave and return several days later to ask whom the employer identified to hire for the position. After this, employers probabilistically receive a subsidy for either their chosen hire or a local jobseeker.

Treatment Implementation – Urban Mechanism Experiment

The protocol for this experiment is the same as that above except for the following deviations. During the baseline survey, a listing of the employer's relatives is taken. At the end of the baseline survey, employers are told that they might receive a subsidy to hire either a family or a nonfamily employee. They are asked to identify a candidate hire of each type before a second visit. To ensure that the family employee is related and the nonfamily employee is not, the employer is told that if the subsidy is implemented, the identity of the hire will be checked against the initial list of relatives taken at baseline.

At the second visit, employers who have identified a family and a nonfamily candidate for hire are told that they will be allocated the subsidy for the relative, for the nonrelative, or for their choice out of the two. Before choosing whom to hire in each condition, employers are told that they will be assigned to one of two conditions, as before.

- **Control Poster:** The poster states that the employer received a subsidy for his business and that he was involved in selecting the hired employee.[16]

- **Plausible Deniability Poster:** The poster states the same information as the control poster, except that, when describing the involvement of the employer in the hiring decision, it states that, in many cases, business owners receive subsidies for employees chosen by the program rather than their own first-choice hire.[17]

Enumerators explain to businesses in the treatment group how the poster could be used to justify their hiring someone and then telling others that the selected hire was not their

[13] In cases where employers receive a randomly assigned subsidy, the poster is adjusted to state this.

[14] This statement is not deceptive, as in many cases, the subsidy is ultimately provided for a randomly assigned employee rather than for the employer's chosen hire.

[15] No mention of family is made during this explanation.

[16] In cases where employers receive a randomly assigned subsidy, the poster is adjusted to state this.

[17] This statement is not deceptive, as in many cases, the subsidy is ultimately provided for a randomly assigned employee rather than for the employer's chosen hire.

choice.[18] Choices are then elicited at different subsidy levels for the family and nonfamily employees, to identify the compensating differential required to induce employers to hire the family employee. Employers are then probabilistically assigned the subsidy for hiring the family candidate, for hiring the nonfamily candidate, or for hiring whichever of the two they prefer (at a randomly determined subsidy level).

Treatment Implementation – Rural Mechanism Experiment

The implementation of this experiment is the same as that of the previous experiment with the following deviations. At the second visit, rural employers are asked to bring the identified family and nonfamily candidate hires to the survey. It is then explained to everyone—the employer and the employees—that the employer might randomly receive the subsidy for one of the two employees or might be able to choose between the two.

The employer is then taken to a private area and asked to choose whom he prefers to hire.

- **Control Choice:** Before making his choice, the employer is told that if he is assigned to the choice group, the employees will be told whom is hired and that the reason the person is being hired is that the employer prefers her.
- **Plausible Deniability Choice:** Before making his choice, the employer is reminded that the employees will not find out whether the person is being hired because she is the employer's choice or because of random assignment of the subsidy.

Choices are then elicited at different subsidy levels for the family and nonfamily employees to obtain the compensating differential required for the employer to hire the family employee. Employers are then assigned the subsidy for the family candidate, for the nonfamily candidate, or for the candidate of their choice (at a randomly determined subsidy level)

Treatment Interpretation

In all three experiments, I make it easier for some employers to tell others later that they had no choice but to hire the person for whom they received the subsidy. I then measure how this plausible deniability, which is designed to reduce social pressure from others, changes their decision of whom to hire.

The experiments offer tradeoffs in how I achieve this. In the urban field experiment, I achieve this in a light-touch way, without mentioning family or relatives. This lets me observe choices under the most naturalistic hiring conditions. However, under this setup, I am unable to quantify the magnitude of any costs associated with the distortion. To achieve this, in the urban field experiment, I ask employers to identify two candidates and elicit their preference

[18] No mention of family is made during this explanation.

between the two, such that I can obtain a compensating differential. This provides more information, at the expense of a (potentially) artificial hiring process that groups the world into two categories. Finally, the rural mechanism experiment enables me to implement the subsidy for all employers and therefore measure the returns to hiring. However, the specifics of this setting mean that the variation I create in plausible deniability is potentially artificial and heightened relative to natural hiring conditions since, in real life, employers conceivably are able to make hiring decisions without those decisions immediately being disclosed to their kinship network.

Screening and Selection

Urban Field Experiment We conducted a census of all markets in Katete. [19] During the screening survey, businesses were screened on i) whether the person managing the business was the owner or was able to make hiring decisions for the business independently, ii) whether he was willing to hire an additional employee for his business if he received a subsidy of approximately 50% of the wage, iii) whether the individual had relatives of working age nearby whom he believed would be willing to work for him, iv) if he would be willing to hire a local jobseeker whom we had identified as suitable for a job in this location.

Urban Mechanism Experiment The protocol for this experiment is the same as that above except that 1) because we were less worried about informational spillovers, we used a smaller distance between businesses and 2) we did not screen on willingness to hire a local jobseeker since this was not required in this experiment.

Rural Mechanism Experiment In this experiment, villages were selected based on their proximity to Chipata town. Enumerators then conducted a random walk was in the villages to select households. After visiting each household, employers were screened on i) whether they had any maize to shell, ii) whether they were willing and planned to hire someone to shell their maize later in the year and iii) whether they had adult relatives over 18 years of age in the same village whom they would be able to hire to do the shelling.

Subsidy Details and Implementation

Urban Field and Mechanism Experiments In these experiments, the subsidy that the business owner receives can be used to hire any employee outside the owner's immediate family.[20] Businesses are told that they can set the terms of the hire (wage, working hours) as

[19] Surveyors used a random walk methodology, starting at a random point in the market and moving in a random direction. They were then told to conduct interviews at every X shops, where X depended on the size of the market.

[20] This restriction is imposed to stop coercive hires.

they like except that the employee must work full-time to enable enumerator audits. Business owners are told during the experiment that the subsidy that they will receive is designed to cover approximately 60% of the wage of contracting a new employee for three months.[21] Businesses are also told that they will be randomly audited and that payment of the subsidy will be conditional on successful audits. Finally, businesses are told that the employees' identity of will be checked against their identification cards on the first day of the subsidized employment.

On the date that the subsidized employment begins, our enumerator team visits the business that receives the subsidy and verifies the identity of the employee hired by the business using her national registration card (NRC). After the employee's identity is verified, the employers are told that the subsidy will be paid out at the end of each month, for 3 months, conditional on successful audits of the business. Our enumerator team then makes unannounced visits to the business on average 15 times over the course of the subsidy period and verifies that the employee is working. Finally, after 6 weeks, each employee is interviewed privately and asked what the employer has been paying her in the preceding months. Employers in the urban context who are extended subsidies are visited on average twice each week, and the presence of the employee is verified—any business failing multiple audits is disqualified from receipt of further payments.

Following the experiment, the subsidy is implemented with a 7% probability in both urban samples.

Rural Mechanism Experiment In this experiment, the subsidy that the employer receives can be used to hire any employee outside the owner's immediate family.[22] Businesses are told that they can set the terms of the hire (wage, working hours) as they like except that the employee must work full-time to enable enumerator audits. At the time of being offered the subsidy, rural employers are told that they will receive a subsidy that will cover 60% of the average half-day's wage. On both of the employment days covered by the subsidy, our enumerator team visits the employer and employee. They attend early in the day to ensure that no shelling has already been done and observe whether the employee has arrived. They then stay in the village during the course of the day. At the end of the half-day, the subsidy is paid to the employer conditional on the employee's working the full half-day.

Following the experiment, the subsidy is implemented with 100% probability.

[21] This subsidy level was determined to be enough to induce hiring and is similar to the subsidy amount used in similar studies (Alfonsi et al., 2020; De Mel et al., 2019).

[22] This restriction is imposed to stop coercive hires.

Spillovers and Manipulation

One of our initial concerns was that informational spillovers from the treatment to the control group, or the atypical character of subsidy provision for the hire of an employee, might lead businesses in the control group to try to copy the treatment group and claim to others that they had no choice but to hire the individual for whom they were offered the subsidy. To try to alleviate this concern, we included the following design aspects as part of the protocol. First, we did not survey neighboring businesses, skipping businesses when sampling, to try to minimize informational spillovers. This gap was largest in the urban field experiment, where the plausible deniability poster was introduced on the first visit and therefore there was the scope for informational spillovers to occur was greatest. Second, we tried to complete as many of experiments as possible in the same village/market on the same day so that news about the protocol could not spread. Third, we encouraged businesses in the control group to display the posters we gave them in their businesses and told them that we would remind them to do this in the future: Knowing this, businesses might have expected that others would learn about their hiring involvement in the future. Fourth, we made sure the posters had an official aspect and were hard to replicate (e.g., they featured specific logos and lamination). Finally, on every poster, we included the phone numbers of our field managers and told businesses that, if people called these phone numbers, the field managers could explain to them the subsidy conditions for particular businesses. We also mentioned that we would keep copies of the poster on file for use in future promotion campaigns.

Randomization

In each experiment, there are two randomizations. The first determines whether the employer is assigned to the control or the plausible deniability condition. The second randomization determines for whom the employer receives the subsidy. Finally, in the urban field and mechanism experiments, a third randomization determines whether the subsidy is implemented.

The randomization in the urban mechanism experiment is stratified by market, gender and whether the business has any noncasual employees to improve statistical power. Simple individual-level randomization is conducted in the rural mechanism and urban field experiments.

Outcome Measures

Urban Field Experiment The core outcome measure in this experiment is whether the employer chooses to hire a family employee. To understand the determinants of hiring choices, we also ask for information regarding why the employer chose to hire a particular person.

Urban Mechanism Experiment In addition to the above outcome measure, we measure the employer's hiring preference at different subsidy levels to obtain the compensating differential required for him to hire the family employee.

Rural Mechanism Experiment In addition to the above outcome measures, we measure hiring outcomes in this sample. Specifically, at the end of each day, we measure the total output of maize shelled, the quality of this maize, and the wage paid to the employee.

4 Data and Empirical Strategy

Data sources

We rely on several sources of data for analysis. Our core measure of hiring choices is elicited by enumerators during the experiment. Whether an employer takes up a subsidy after the fact is validated by unannounced enumerator visits to employer's businesses, on average twice per week.

To measure the returns to hiring in the rural sample, enumerators also observed the output of shelling each day of the contract. Enumerators arrived at the villages and monitored that employees were present and working - subsidies were only paid out conditional on this. At the end of the workday, enumerators measured the total quantity of maize shelled, and also rated the quality, using the scale of Bold et al. To validate the quality measure, enumerators also purchased some maize from a subsample of employers, and sold it at nearby markets.

Empirical Strategy

The primary empirical specification used to measure the effect of social pressure on hiring choices is:

$$y_i = \alpha_i + \beta \text{Plausible Deniability}_i + \text{Strata}_i + \epsilon_i$$

where y_i is the primary outcome of interest - either the choice of hiring a family employee or the compensating differential required to hire a family employee, and $Strata_i$ are stratification variables in the Urban Mechanism experiment. The primary coefficient of interest is β - the effect of being offered more plausible deniability on these outcomes.

In the Rural Mechanism experiment, we also use the following specification to measure the returns to hiring family and nonfamily employees:

$$y_i = \alpha_i + \beta \text{Relative Subsidy}_i + \text{Subsidy Day}_i + \epsilon_i$$

where y_i is the primary outcome of interest related to performance in the shelling task, either

output produced, quality of output, or wage paid. $SubsidyDay_i$ is a fixed effect for whether it is the first or second day of the subsidy. The primary coefficient of interest is β which measures the effect of being assigned a subsidy for a family employee rather than a nonfamily employee.[23]

Descriptive statistics

Urban Field and Mechanism Experiment Table 1 and Appendix Table A1 provides descriptive statistics for the eligible samples. In the Urban Mechanism experiment. Around 50% of eligible businesses are in the retail sector, 10% in manufacturing, 10% in Agricultural Processing, while the remainder are in other sectors. The proportions of retail and manufacturing businesses are similar to the proportions found in Ashraf et al. (2019) census of Lusaka, Zambia. In the Urban Field experiment, the proportion of retail businesses is slightly higher and manufacturing lower, probably reflecting the smaller town size.

Employers in the Urban Mechanism are also asked to classify the kind of work that their business does along various dimensions, and approximately half say their job requires a high degree of trust, and half state that their job is one where it is easy to make an excuse for poor performance on the job.

In the Urban Mechanism experiment, 60% of eligible businesses have an employee - a proportion that is slightly higher than Ashraf et al. (2019). However only 40% of these businesses classify their worker as full time, with the majority of the remainder classifying their employee as part time. The average business has 0.8 employees and 0.3 employees who they state are related to them (37.5%). These numbers are somewhat lower than those found in Lusaka, partially because Lusaka has a long right tail of businesses that aren't found in smaller urban centers in Zambia, partially because the largest businesses in Chipata are multinationals that could not be enrolled in the study. The level of hiring is similar in the Urban Field experiment with with businesses having on average 0.5 employees among whom 0.2 are related.

The levels of sharing pressure among these enterprises is high. More than 60% of businesses report facing sanctions when not sharing with their family. In the Urban Field experiment, this rate is almost 90%, perhaps reflecting the fact that this is a smaller town.

Rural Mechanism Experiment Employers in the rural mechanism experiment are often men, and approximately 46 years old. Most are experienced - 80-90% previously hired a worker to shell maize for them. The rates of sharing pressure in this sample are high - with more than half specifically stating that they have felt pressure to provide jobs for their relatives previously, and 80% stating that they would be sanctioned for not giving something to their

[23]This speficiation drops employers who were assigned their choice of who to hire, and therefore measures the effect of random assignment to hiring a family versus nonfamily employee.

family when asked. Employers sometimes also perceive drawbacks of hiring family: 30% state that if their relative worked for a nonfamily employer, they would work harder than if they worked for a family employer.

Sample Selection – Urban Mechanism Experiment

To understand how much of our treatment effects maybe driven by selection of businesses, in the Urban Mechanism Experiment we collected baseline data from ineligible businesses. In total 60% of businesses were eligble. Of the entire sample, 15% had no relatives nearby, 10% of those who did have relatives nearby said they had not relatives nearby they could hire to work for them, 3% said they did not have authority to make hiring decisions, and 25% said they would not hire a worker even at a subsidised wage.

Businesses that are ineligible for the experiment appear smaller and less well managed than eligible businesses: they have fewer assets, lower profits, lower scores on management practices and are likelier to be in retail (see Appendix Table A10). They also do not appear obviously much less likely to have issues with family as employees: more than 60% agree or strongly agree they would face sanctions if they did not share with their family if asked, 70% say their relatives work worse for them than they do for other employers who they are not related to, and only 40% say that the job they are doing requires high degrees of trust.

5 Urban Field Experiment – Family Pressure Changes Hiring Choices

How does alleviation of family pressure change hiring choices? I turn first to the decision of employers in the Urban Field Experiment. As seen in Figure 2 Panel A, increased plausible deniability reduces the likelihood of hiring someone you are related to by 12 p.p., approximately 20%.

Do employees selected with the plausible deniability poster differ along other dimensions from those in the control choice condition? I find some evidence that the employees selected in the Plausible Deniability condition maybe more productive. Appendix Table A4 constructs as outcome variables other attributes of the employee. While men are equally likely to be selected in both conditions (Col. 1), employees selected in the Plausible Deniability treatment have 8% higher expected wages ($p = 0.05$) and are 25% likelier to have more than a primary education ($p < 0.01$). These outcomes suggest that employers maybe able to select more productive employees with Plausible Deniability.

Is this finding consistent with the explanations given by firm owners for their hiring? At the end of each survey, enumerators ask business owners to explain why they hired the individual they hired. I then extract the most common words that are used to describe these

choices, and correlate them with whether the employee is related to the employer. Panel A of Figure A1 shows the relative likelihood of using a particular word to describe hiring a family (rather than nonfamily) employee. The largest difference, by far, is the use of the word "help" to describe hiring of relatives.[24] Consistent with the prior literature, family employees are also likelier to be described as trustworthy, although this impact is relatively small and statistically insignificant. By contrast, nonfamily employees are approximately ten p.p. likelier to be described as "reliable" and "hard"(working). Panel B of the same figure shows how the justifications of who was hired differ with treatment. The primary finding is that with Plausible Deniability, business owners are 15 p.p. less likely to justify a hire as being hired to "help" ($p < 0.01$) and 12 p.p. likelier to justify a hire as hiring because the person is reliable ($p < 0.05$).[25]

Is being related to the owner just a proxy for social proximity, that is, are those who are selected with the Plausible Deniability poster just less socially close to the business owner? Columns 4-9 of Appendix Table A4 provide only week evidence for this: along a range of measures of social proximity to the employer I do not see treatment effects, suggesting that the marginal employee with Plausible Deniability is still socially close to the employer. This suggests that there maybe something particular about family that might drive these effects.

6 Urban Mechanism Experiment – Magnitude and Incidence of Hiring Cost

The Urban Field experiment establishes in a light touch manner that increased plausible deniability for employers changes their hiring decisions away from members of their family. To understand the size of these pressure costs, and who faces them in particular, I turn to the urban mechanism experiment. I first begin by replicating the results of the Urban Field Expeirment. I find similar outcomes in this setting: in the Control Poster group employers choose to hire their relative when offered equivalent subsidies for each employee 63% of the time. By contrast, employers in the Plausible Deniability Poster group hire their relative only 47% of the time ($p < 0.01$). This difference of 16 p.p. is not that much larger than the 12 p.p. in the Urban Field Experiment, suggesting that the additional experimental protocols being used to obtain additional information about hiring are not leading employers to change their decisions drastically.[26]

[24] For example, one business owner stated "He has been asking me for a job for a while I believe this was an opportunity to help".

[25] This suggests that the decision to help the family employee is not driven by pure altruism, as this would not change with the signalling of the decision.

[26] This is potentially surprising for another reason - in the Urban Field experiment one possibility is that the marginal employee in the absence of social pressure is *another relative*. This is ruled out as a part of the design in the Urban mechanism experiment. The similarity of the treatment effects in both experiments suggests that

Consistent with the proposed mechanism that these changes in hiring preferences reflect responses to social pressure, Panel A of Figure 4, show heterogeneity in the treatment effects by whether these populations report receiving sanctions from their kinship network if they do not share with them. There are much stronger effects for employers who report not facing sanctions from not sharing with their family. Employers who face sanctions decrease their hiring of family employees by 20 p.p. (from 72% to 52% p<0.01), while those who do not have sanctions see limited change (from 37% to 34% p=0.69).

Panel C of Figure 4, shows suggestive heterogeneity that this redistributive pressure may have important implications for the type of worker that firms match with. Specifically, I explore treatment effects by a measure of whether the worker will work as hard for the employer as they would for an employer whom they were not related to. [27] The treatment effects suggest that employers are particularly averse to hiring employees who they expect to "slack off" when working for a relative. Employers who perceive their relative working harder or equally hard for them almost always choose them in both treatment conditions (Control poster 92%, Plausible Deniability poster 91% p=0.67). But those who perceive there might be moral hazard hire their relative 40% of the time with the Control poster, but only 9% of the time with the Plausible deniability poster ($p < 0.01$).

How large are the pressure costs that employers face when hiring? I proceed to estimate the average compensating differential in both samples: the amount that employers are willing to forgo in terms of subsidy in order to hire their relative. Panels A of Table 2 show the treatment effect on this measure. In the urban sample - on average employers are willing to pay 40 ZMW to choose to hire their relative without the plausible deniability poster. This reduces by ZMW 38 with treatment ($p < 0.01$). This effect size is meaningful: it corresponds to 7% of the median subsidy offered in the experiment, or 5% of the median wage paid, or 4% of the median firm's monthly profits.

These preferences to not hire some family members suggest that there maybe sizable productivity consequences for some businesses from hiring family. To better understand if this is the case, I turn to the Rural Mechanism Experiment that allows me to quantify the returns to hiring the marginal family and nonfamily candidate.

Heterogeneity

In all 3 experiments, a substantial proportion of the sample are women, who maybe susceptible to intrahousehold pressure in decision-making, as well as social pressures related to family hiring. To understand whether intrahousehold pressures are likely to explain all of the effects, we look at heterogeneity in the effect sizes by gender. We find the effects of pressure are equal

this is not that likely at least in this setting.

[27] In the sample, this is constructed by asking employers directly the question of whether their relatives work as hard for them as other employers.

or larger for men: Table A5 shows that while women reduce the chance of choosing to hire a relative when offered plausible deniability by 10.9 p.p. ($p = 0.04$), men reduce the likelihood of hiring a relative by an additional 10.3 p.p. - though this additional effect is not significant at conventional levels. This finding is consistent with some of the prior literature, which has suggested that such sharing norms may operate particularly upon men (Beekman et al., 2015; Fiala, 2018; Squires, 2018; Boltz et al., 2019).

Does this pressure reflect something particular about the family - or is it a pressure that operates generally among individuals who are socially close? For instance, do individuals face pressure to hire individuals who they are generally socially close to? To test this, I construct measures of social distance between the employer and the potential employees, and construct dummy variables for cases where the employer is plausibly socially closer to the nonrelative than to the relative employee.[28] If there pressure to hire were a function of social distance rather than familial relationships, one might expect the relationship to flip - so that with plausible deniability employers become *more* likely to hire their relative employee. I find no evidence of this across all measures, and cannot reject the null that the coefficient is less than or equal to 0 in all cases. This suggests that the norm may operate specifically within families, rather than generally among socially close individuals.

This norm maybe less strong for employers who have already hired relative employees, who the family may have determined have already fufilled their requirements to the family. Alternately, firms without any employees might face particularly strong norms to redistribute, and maybe choosing a smaller firm size as a result. I do not find any evidence for either of these hypotheses: Appendix Table A5 shows no meaningful heterogeneity by whether an employer already has a relative working for them, or by whether the employer already has an employee working for them.

7 Rural Mechanism Experiment – Returns to Family Hiring

I begin by attempting to replicate the results from the previous sections. In this sample and design, plausible deniability again changes hiring choices: Employers in the Control Choice condition choose their relative to work for them 78% of the time, whereas in the plausible deniability choice condition they choose to hire their relative 58% of the time. This effect is not that different from the Urban Field experiment, suggesting that the change to intensive signalling meaningfully changes treatment effects.

I find similar heterogeneity by the likelihood of social sanctions and of relative moral hazard. Rural employers who perceive that their family will sanction them if they do not give

[28] Specifically, these variables are an indicator variable for cases where the employer attends the same church as the nonrelative employee but not the relative employee, sees more often the nonrelative than the relative, is likelier to reciprocate favors for the nonrelative than relative, and has more common friends with the nonrelative than the relative.

them what the ask for have much stronger treatment effects (sanction treatment effect: Control 83%, Plausible Deniability 56% versus without sanctions Control 59% p<0.01, Plausible Deniability 67%). I also construct a measure of moral hazard for this sample based on how much they expect their relative to shell for them as an employer, as opposed to others.[29] Again I find stronger treatment effects for those who expect their relative to exert less effort for them for them: while employers who do not expect their relative to exert less effort decrease their hiring of their relative from 72% (control) to 60% (Plausible Deniability) those who do expect their relative to exert less effort decrease hiring from 92% (control) to 52% (plausible deniability). This heterogeneity again suggests that employers particularly susceptible to family employee frictions in contracting maybe particularly susceptible to pressure.

7.1 What are the returns to hiring in the family?

In all of the previous results, I find that employers choose not to hire family employees when offered plausible deniability for doing so, which suggests that they perceive they are less productive as employees. In the rural experiment, I am able to measure to measure the returns to hiring relatives versus non-relatives by randomizing assignment to the subsidy for the relative and nonrelative non-contract.

On average - employers assigned to the relative contract appear to receive lower returns from doing so. Table 4 shows the amount of output produced, quality of output, and wages paid, during the subsidy implementation. Taken together - the pattern of all results for the three metrics suggests that relative employees generate less surplus for their employers than nonfamily employees.

Column 1 shows that the output produced by Relatives on average produce 6.7% less output during the course of the contract ($p = 0.05$). Perhaps more importantly, the maize that is produced is of lower quality - while 88% of the maize shelled by non-relatives shows no defects as measured by the scale of Bold et al. (2022), only 82% of the maize shelled by relative employees shows no defects ($p = 0.06$). This quality measurement does not reflect relatives receiving lower quality maize to shell than non-relatives, as the measure adjusts for the quality of maize received by the employee and only measures new defects created by the employee.

To allow the contract to be as naturalistic as possible, employers were allowed to decide all aspects of the contract, subject to the contract involving the employee working for half of a day. Therefore - low output and quality maybe preferable by employers if it is accompanied by a lower wage bill. I find no evidence, however, of relatives, commanding lower wages. Column

[29] In the rural sample, employers are asked to forecast which of the employees - the family employee or nonfamily employee, would be able to shell more maize in a baseline shelling task designed to elicit productivity. The employers are then asked which employee would likely shell more maize for them. Employers facing a negative match specific effect are those whom forecast better performance of the relative on the baseline task as compared to the own employer task.

3 shows that the average wage paid is 0.3 ZMW more to family employees, and insignificant at conventional levels ($p = 0.84$).

This difference in performance might reflect differences in baseline abilities of the employees, or might instead reflect differences in performance when matched with the employer. I first explore whether this appears to be driven by a difference in employee selection. Appendix Table A7 shows whether there are noticeable differences in observable characteristics of family and nonfamily employees. In general, there are few differences, and adding them to the regressions on maize output does not meaningfully change the results, suggesting that the returns are not mediated through these observable characteristics.

The effects, however, maybe mediated through of unobserved heterogeneity. To understand whether this is the case, I conduct an additional shelling task for employees in which they complete an incentivized shelling task in which they receive a piece rate for how much maize they shell. Appendix Figure 5 shows the CDF of output produced by family and nonfamily employees under this contract. The CDF suggests there is no average difference performance, with regression analysis confirming no statistically significant difference in productivity at baseline. This evidence, suggesting similar performance at baseline, suggests that the effect runs primarily not through lower ability of such hires, but instead potentially a negative match specific effect between employees and employers.

8 Alternative Explanations

The previous sections suggest that the prevalence of hiring relatives as employees may partially reflect redistributive norms that are perceived as a cost by firms, with potentially important productivity implications for the businesses. The next section discusses alternate explanations for the results.

Noisy Decisions/Low Stakes

This project uses choices in the hiring experiment to infer employer's beliefs about productivity differences in employees as well as the signalling costs of the employer's choice. Doing so relies on businesses making considered choices, despite their choices only being implemented probabilistically. However, business owners might have learnt that their was a chance they would not receive a subsidy and not made their decisions carefully, or have assumed that they could receive a subsidy without hiring an individual. While it is not clear why noisy choices, or choices that seek to maximize revenues would lead to the treatment effects observed in this study, we do several checks to understand how prevalent such issues might be.

We see limited evidence that the hiring decisions in the experiment were not considered. Treatment effects for the subsample receiving the subsidy look similar to the overall sample

(see Appendix Figure A5). We also see choices translate into hiring for the vast majority of this subsample: all but one employer hire the employee randomly assigned or chosen by them at the end of the experiment, and all but two complete the subsidy period in the urban experiment. . These employers also receive this subsidy conditional only on successful audits of their business. Businesses were audited on average 15 times over the course of the subsidy, unannounced times and in 98% of cases the employee was found working at the business.

An alternative possibility is that employers in the urban experiment may have manipulated the subsidies in order to keep a portion of the cash themselves, or to reduce the amount of the wage paid by themselves so much so as to incur almost no cost from hiring the individual[30]. While the subsidies were formulated to cover 70% of the wage on average, similar to the levels provided in De Mel et al. (2019); Alfonsi et al. (2020), it is possible that the employer may have changed their behavior after the fact. Again, while it is not clear why this would generate the observed patterns of results, it would make the experiment potentially a lower stakes decision.

To check for this possibility, our surveyors conducted private interviews with hired employees approximately 2 months after the subsidy was initiated to ask them how much they had been paid by the employer in the previous months. We see limited evidence for the manipulation of wages: in 83% of cases workers reported being paid what the employer had told us they would pay the worker, in 10% of cases they reported being paid less and 5% of cases they reported being paid more.

Taken together, this evidence suggest that businesses made considered and high-stakes decisions in the experiment, that had considerable consequences for spending of the business owner themselves.

Employee Selection

The treatment effects in the experiment may reflect the effect of redistributive pressure on hiring decisions in the experiment, they may not carry forward to real hiring decisions if the candidates selected for the experiment do not reflect the marginal relative and nonrelative candidates for actual jobs. In particular, the treatment effects found in the experiment may overstate the importance of redistributive pressure on hiring decision if the relative selected for the experiment were worse than the marginal relative candidate (because, for example, the employer thought that the experiment could be a cheap way to hire this employee, or because this employee heard about the experiment and demanded a job which would have not occured in the absence of the experiment), or if the nonrelative selected were better than the marginal nonrelative candidate (for example, if employers searched harder for this candidate because they knew there was a likelihood that they were going to be randomly assigned a subsidy for

[30]This behavior was impossible in the rural experiment as the subsidy was paid directly to the employer and the payment to the worker was observed at the end of each day

this individual, or because they were asked to search for a nonrelative and therefore incurred a search cost that they would not normally have incurred). More subtly, the the marginal nonrelative might be expected to be better than outside of the experiment because non-relatives maybe expected to perform better within the experiment (for example, because they believe non-relatives wont be likely to steal while partaking in the experiment).

We do several supplemental data collection exercises to test for these possibilities. First, at the end of the experiment, we ask all employers to rank the relative they selected for the experiment relative to the other relatives that they could have selected for the job, and ask them to explain why they chose that individual. Two-thirds of employers stated that this worker was the best available among their relatives, and that this was why the individual was selected, and 24% said the relative was the second best available. Only in approximately 10% of cases, did the employer state that the relative was lower ranked, and hired for some other reason (for instance, because the relative that they did want to hire had taken up another job and was not available).

Second, we conduct follow up surveys with approximately 90% of businesses that do not receive the subsidy approximately 3 months following the experiment, and ask them if they have hired any employees since the experiment ended, and if so who they hired. We find that within this period 21% of firms hired any employee to work into their business - among these 15.7% hired at least one relative permanent employee and 6.1% hired at least one nonrelative permanent employee. In both cases we find a high likelihood of hiring the individual brought to our experiment: approximately 54.6% of relatives and 54.0% of non-relatives hired for a permanent position are the individual invited to the experiment - suggesting that in many cases this was an individual seriously considered by the employer for hiring even outside of the experiment.

Finally, a necessary design feature of the experiment, following De Mel et al. (2019) was to not allow businesses to hire employers to hire members of their household for jobs, in order to limit the possibility that employers might coerce members of their families to work for them unwillingly. However, this makes it possible that all of the treatment effects maybe driven by employers for whom the marginal candidate outside the experiment would have been a member of their household. We find this is not the case: during the survey we ask employers if they would have identified a different relative candidate if they were allowed to hire a member of their household and 25% of employers say yes. However, heterogeneity in treatment effects by this variables suggest that this does not account for the treatment effects: Appendix figure A4 shows that among the 75% of employers who stated they did not have a desired replacement in their household there is still a sizable treatment effect of the poster.

An additional possibility is that the nonrelative employee selected in the experiment would not be the marginal nonrelative candidate - for instance, because the search costs of finding this nonrelative were particularly high and search effort was made only because the job would

be subsidised.

Several pieces of evidence suggest this is not the case. First - in the urban experiment we see high levels of hiring the nonrelative in the experiment, particularly for casual (short term jobs after the experiment) - of all non-relatives hired for such jobs, the nonrelative selected for the experiment is hired 57% of the time.[31] Second, in general in the urban experiment it seems like the nonrelative selected for the experiment is a person that the firm owner knew well, and was unlikely to have searched very hard for. The average nonrelative employee selected was known by the business owner for 6 years, in 85% of cases is someone that the firm owner said they see weekly or multiple times per week, in 30% of cases was someone who had previously worked for the employer. Therefore, it seems unlikely that the employer chose an individual that would be difficult or implausible for them to find outside of the experiment. Finally, in the rural experiment, we specifically measure the search costs associated with finding both the relative and nonrelative employee. We find the difference in search time for the nonrelative to be 3 minutes larger than that for relatives - making it implausible that this constitutes a meaningful barrier to hiring non-relatives in this context.

Finally, one could argue that the interaction of the experiment and the identity of the employee makes hiring non-relatives more desirable than it would be naturally. For example, employers might believe that non-relatives may be less likely to steal from them knowing they are part of the experiment, or they believe that the fact that the nonrelative knows that the business is receiving the subsidy will make the nonrelative likelier to work hard/not quit for another job. While these concerns are plausible, they might equally apply to family hires. Moreover, the fact that nonfamily hires are hired outside of the experiment (see Appendix Figures A8 and A7) suggests that these employees are not undesirable outside of the experiment.

Welfare and Signalling Benefits

This paper suggests that redistributive pressure imposes a cost on businesses by distorting their hiring decisions, and that my experiment measures a signalling cost associated with employers choosing to hire a nonfamily employee. An alternative view of the experiment is that employers derive a signalling benefit from choosing their relative (for instance, by allowing the employers to signal to others how desirable the employee is). In these alternate stories, my experiment by introducing noise in the mapping from choices to outcomes, might reduce signalling benefits that employers value.

To test for this, I allowed respondents in the rural experiment, prior to the making their choice, to decide if they preferred their choice to be made with or without plausible deniability, with the choice implemented for a small portion of the sample to make the choice incentive

[31] Since we do not have a pure control, however, this could be driven by firms overcoming search costs as part of the experiment

compatible. I find evidence that individuals prefer their choices to be made privately and perceive signalling their choices as a cost. Almost 90% of respondents prefer for their choice to be made privately.

Magnitude of Signalling Cost

One concern about the experimental design is that the level of social pressure in the control conditions maybe larger than normal. For example, knowledge of the experiment may lead family members to seek out the employer and ask questions of them that they would not normally. Moreover, particularly in the rural experiment, the control condition may appear unnatural as employers may not expect to have their hiring choices immediately amplified to the network, or have the nonfamily employee be passed over so saliently. While these concerns do not undermine the test for the existence of pressure, they matter for interpreting the magnitudes, which maybe overstated because of these design features.

Three reasons suggest that this may not be a large cause for concern. First, the design of the Control Poster condition in the urban experiment removes many potentially "unnatural" features of the hiring decision arising in the rural experiment, and treatment effects are still large in this case.

Second, motivating evidence regarding the norm suggests that family is likely to learn that an individual is hired, and that ex-post excuses (eg. "You weren't available at the time") are unlikely to be accepted by the family. Therefore, while the experiment might heighten the signalling of the opportunity to others, this is unlikely to be a major driver of the pressure felt.

Finally, the literature on social pressure has generally found stronger effects when individuals are approached by others with a request (Lazear et al., 2012; DellaVigna et al., 2012). While our experiment abstracts away from requests for jobs these are common in our context, approximately 40% of employers report hiring a worker because of the worker approaching the employer requesting a job. If social pressure is particularly concentrated among such requests, our experiment may underestimate the prevalence of pressure to hire in the population.

Mechanism for Signalling Cost

This paper interprets the change in hiring patterns arising from observability of hiring choices as reflecting a cost anticipated by employers arising from the fact that they deviate from social expectations that they choose to hire their relative. However, alternative interpretations of the signalling cost are possible. For example, individuals might believe that if other employers observe their decision to not hire their relative, they will revise downwards their beliefs about the employees type, making it less likely that that individual is employed elsewhere in the

future.[32] If this signalling to others mechanism is so strong, then the effect maybe driven by altruism, rather than social pressure.

While we cannot entirely rule out alternative signalling stories, several pieces of evidence suggest this is not the primary reason for the behavioral response to choice signalling. First, this heightened signalling to others is most pronounced in the Rural Mechanism Experiment - but I find similar effects in all three experiments. Second, the fact that 90% of employers choose not to signal hiring when they have the choice suggests that there is limited upside to the employee of having it signalled to others that they were chosen by the employer - but if there is a range of abilities of family employees one might expect there is a positive signalling benefit for some family employees. Finally qualitative evidence suggests that the primary mechanisms that employers mention for choosing to hire their relative have to do with sanctions and disagreements with their family, rather than humiliation for the potential employee. Taken together, this suggests that these results reflect employers abiding by social expectations, rather than a deep concern of signalling negative news about their relative.

External Validity - Business Selection

In all experiments, businesses are screened out as part of the experimental protocol. This might make the employers that our experiment deals with particularly unrepresentative of businesses as a whole.

Appendix Table A10 shows observable characteristics of eligible and ineligible businesses in the Urban Mechanism experiment, where we collected detailed information on the characteristics of ineligible businesses. While these businesses differ along observables, they do not appear noticeably less likely to report pressure to hire: ineligible businesses are likelier to report that they would face sanctions if they do not provide for their family when asked, and report high levels of hiring of relatives, and stating that relatives are more susceptible to moral hazard. Taken together, this provides suggestive evidence that this sample is not less likely to face pressure to hire.

9 What Generates Pressure to Hire?

Why should relatives side with employees who take undesirable actions toward their relative employers? Appendix Figure A2 offers some suggestive evidence that this maybe because employers and their family have different beliefs about the performance of the relative as an employee. The figure shows employers beliefs, as well as their beliefs about the beliefs of members of their family, about whether the selected relative employee would work better or worse for them than for another employer who they were not related to. As noted previously

[32] Employers might care about this because of altruism, or because they expect to be able to draw on income from this individual in the future, and therefore would like them to have more income.

there is substantial heterogeneity in the beliefs of business owners in the experiment regarding the performance of their relative in their business. However, in general employers perceive that there is likely to be disagreement between them and their relatives on this point: among business owners who believe that their relative will not work as well for them as they would for someone they are not related to, almost 75% believe that their relatives believe the employee would work harder for them than someone they are not related to. One also finds that a substantial proportion of the treatment effects from the experiment are concentrated in this region of disagreement: although the sample where there is agreement is small, there is no statistically significant impact of the treatment on the choice of who to hire when the employer thinks the relative will work less well for him and believes his relatives believe this as well.

Finally, Appendix Figure A3 presents some additional supplementary evidence that the pressure maybe driven by disagreements between the employer and members of their extended rather than nuclear family. The figure shows treatment effects conditional on the relationship of the employee and the employer, and suggests that in cases where the employee is the child of the employer and therefore a member of their nuclear family, there is no pressure to hire. However, by contrast where the employee is a sibling living outside of the household, or a more distant relative (such as a cousin or uncle) the employer is likelier to avoid hiring the individual when offered the treatment poster.

10 Conclusion

This paper examines whether some employers in LMICs hire family members as employees because of redistributive pressures. In three experiments, I document that when employers know they can make excuses for not hiring someone, they are less likely to select a family employee for a job in their business. I find that employers perceive this redistributive pressure to be costly. Finally, by randomizing subsidies for family and nonfamily employees in one task, I find that there are productivity consequences from the hiring of family employees, who are less productive for firm owners.

These findings have important implications for how we think about labor market functioning in LMICs. While labor market frictions have been posited to be a constraint on firm growth, active labor market policies have generally been found to have muted effects (McKenzie, 2017). Similarly to Macchi and Stalder (2023), this paper suggests that one explanation for this might be that work may have a redistributive role. In contrast to Macchi and Stalder (2023), however, I suggest that the decision to give a job might result partly from family negotiations and pressure rather than from employers' own preferences.

Many important questions remain. One is how formal policies with a redistributive function, such as social insurance schemes, might interact with informal redistributive mechanisms such as the one discussed here. Another important question is whether there are alternate

transfer mechanisms for business owners to alleviate hiring pressure and what they entail.

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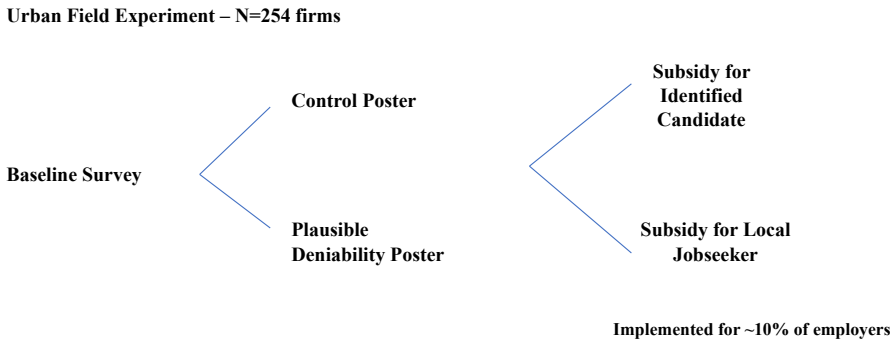
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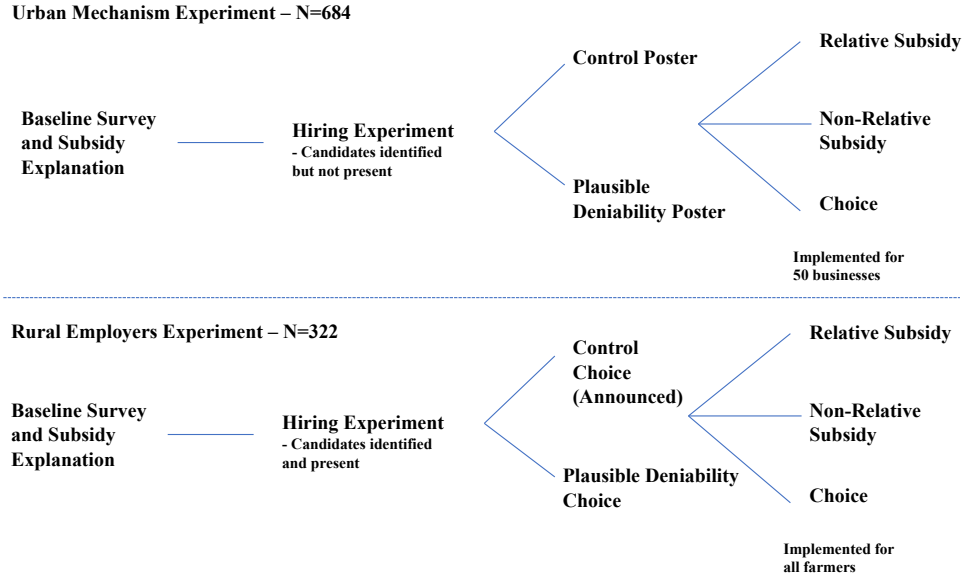
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11 Figures

Figure 1: Overview of Field Experiment Designs



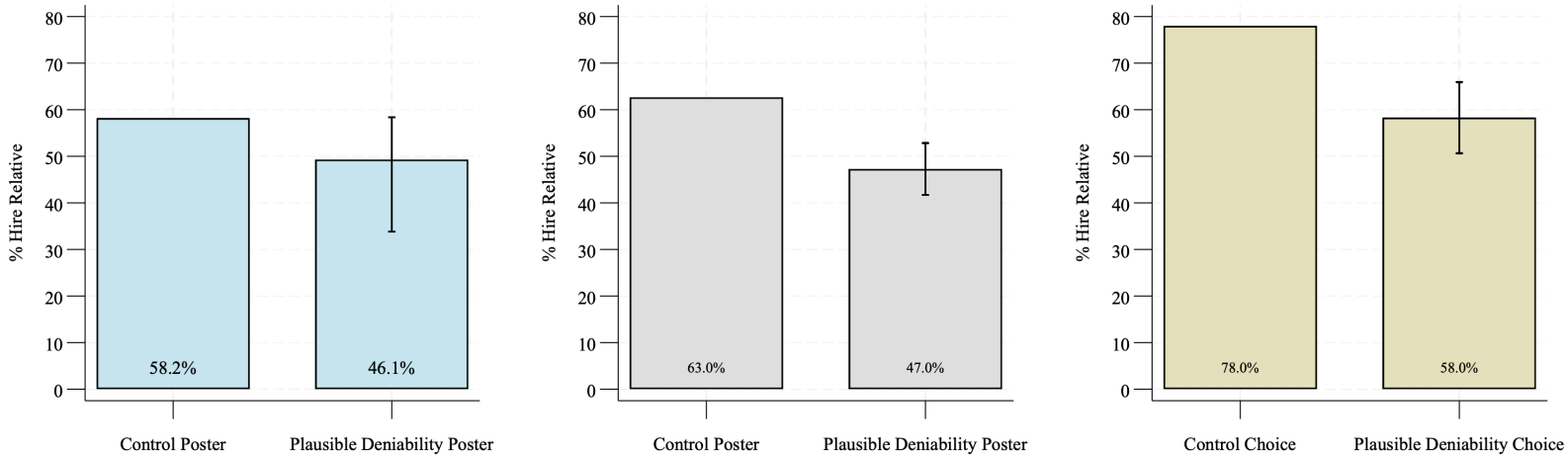
(a) Experiment Design - Urban Field Experiment



(b) Experiment Design - Urban and Rural Mechanism Experiments

Notes: These figures show the field experiment designs. Panel A shows the experiment design of the Urban Field Experiment. Panel B shows the experiment design of the Urban Mechanism and Rural Mechanism Experiments.

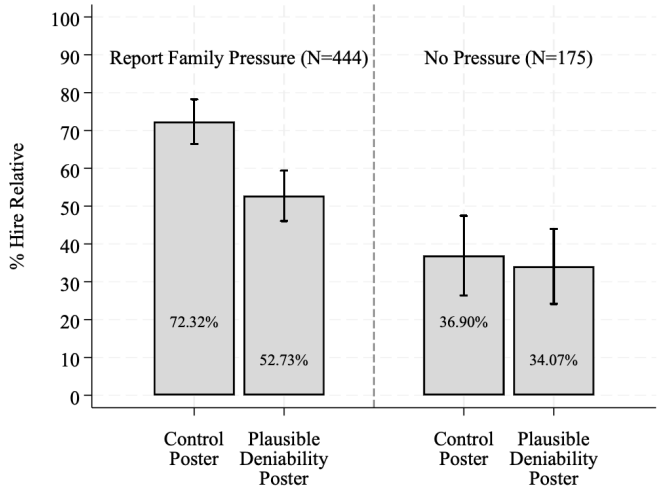
Figure 2: All Field Experiments: Treatment effects on hiring a family employee



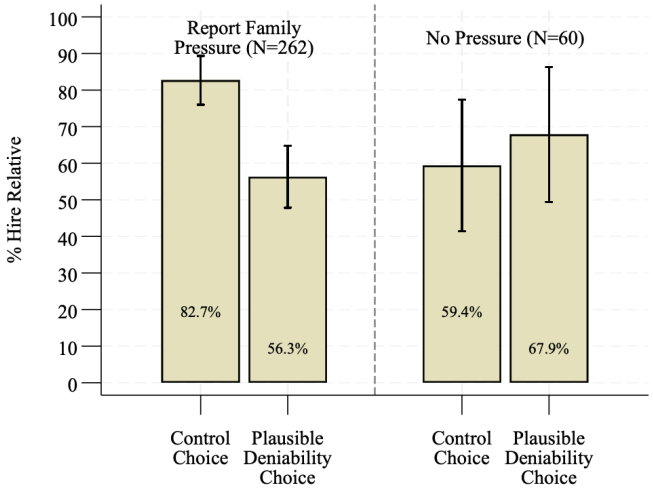
(a) Urban Field Experiment (b) Urban Mechanism Experiment (c) Rural Mechanism Experiment

Notes: These figures show the proportion of employers who choose to hire a family employee in each experiment, across treatment conditions. Panel A shows the proportion of employers selecting a relative to hire with the subsidy in the Control conditions, as well as the proportion of employers selecting to hire a relative in the plausible deniability condition, using the coefficient of a regression of likelihood of hiring a relative on an indicator for Plausible Deniability, location and industry fixed effects. Panel B shows the proportion of employers selecting a relative to hire when offered the same subsidy for a family and nonfamily employee in the Control and Plausible Deniability Poster conditions. Panel C shows the proportion of employers selecting a relative to hire when offered the same subsidy for a family and nonfamily employee in the Control and Plausible Deniability Choice conditions. Robust standard errors are shown.

Figure 3: Treatment Effect Heterogeneity in Urban and Rural Mechanism Experiments - By Stated Family Pressure



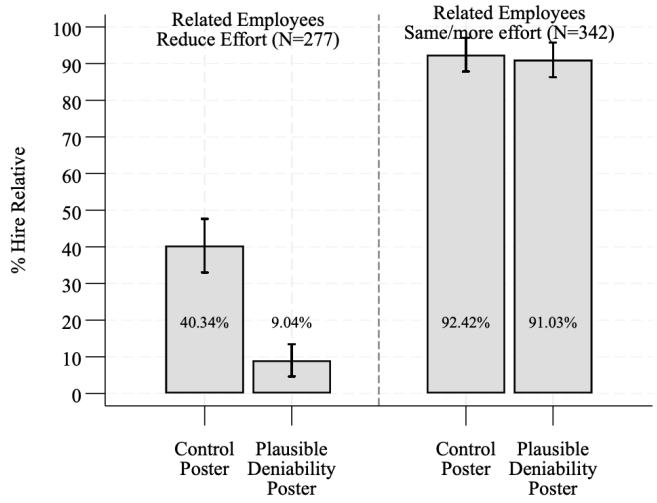
(a) Threat of Sanctions – Urban Mechanism Experiment



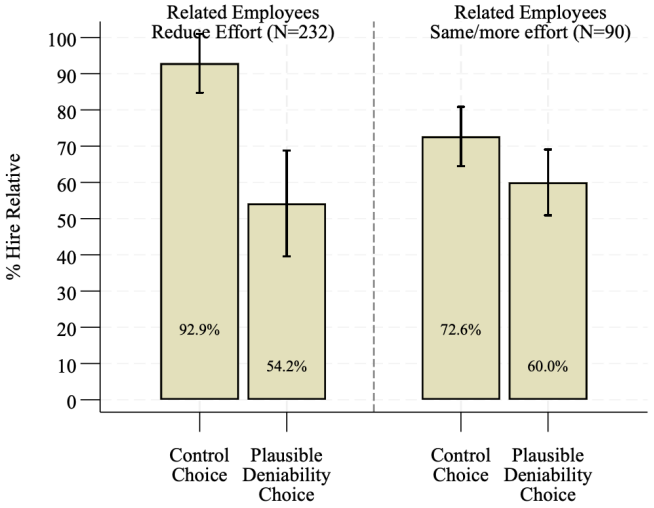
(b) Threat of Sanctions – Rural Experiment

Notes: These figures show the proportion of employers who choose to hire a family employee in each experiment, across treatment conditions, conditional on reporting redistributive pressure from their family. In the rural mechanism experiment, a respondent is coded as facing pressure if they agree or strongly agree that "If my relative learned that I chose my nonrelative and not him for the job, I would face negative consequences from others". In the Urban Mechanism Experiment, employers who are described as facing redistributive pressure are those who agree or strongly agree that "If your relative expects something from you, and you don't help them, there can be strong consequences for you". Robust standard errors are shown.

Figure 4: Treatment Effect Heterogeneity in Urban and Rural Mechanism Experiments - By Employer's expectation that family worker works less hard for family employer



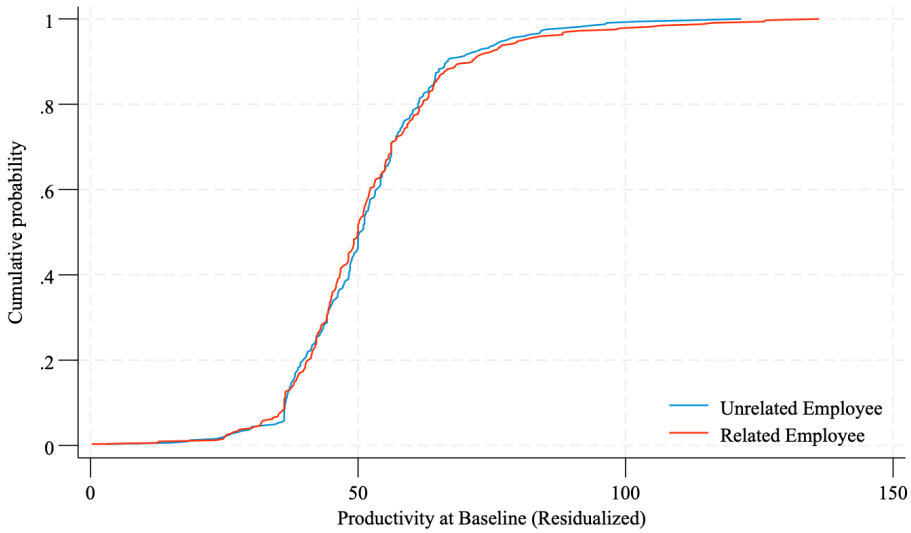
(a) Related Employee Moral Hazard – Urban Mechanism Experiment



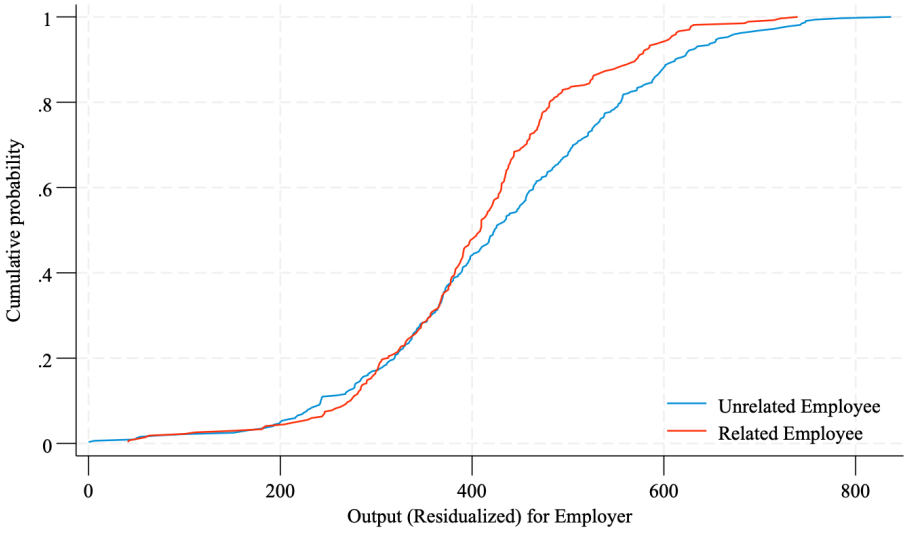
(b) Related Employee Moral Hazard – Rural Mechanism Experiment

Notes: These figures show the proportion of employers who choose to hire a family employee in each experiment, across treatment conditions, conditional on reporting that they expect their family employee to work less hard for them than they do for a nonfamily employer. In the rural mechanism experiment, a respondent is coded as expecting their relative to work less hard for them based on their expectations about which employee would produce more output when doing maize shelling - for themselves for a piece rate versus when working for the employer. In the Urban Mechanism Experiment, employers who expect family employee moral hazard are those who state that family employees work less hard for a family employer, than a nonfamily employer. Robust standard errors are shown.

Figure 5: Rural Mechanism Experiment - Family and Nonfamily Employee Output



(a) Family and Nonfamily Employee Productivity



(b) Family and Nonfamily Employee Output for Employer

Notes: These figures show employee output as measured by a piece-rate task at baseline, and when working for the employer. The task at baseline is residualized for task fixed effects, and renormalized to start at 0. The task at for the employer is residualized by subsidy day, subsidy amount and baseline controls, and then renomralized to start at 0. The blue line shows output for nonfamily employees, the red line for family employees. Output is measured as kilograms of shelled maize.

Table 1: Urban Mechanism Experiment: Balance table among micro-enterprises in subsidy experiment

Variable	Control Mean	Treatment Mean	P-value Δ
Demographics			
Age	38.9	38.4	0.54
	(10.0)	(10.2)	
Gender	0.4	0.4	0.64
	(0.5)	(0.5)	
Household Size	5.6	5.4	0.21
	(2.2)	(2.2)	
Savings			
Digit Span	5.8	5.9	0.14
	(1.2)	(1.1)	
Ravens	6.0	6.3	0.11
	(2.8)	(2.9)	
Agree Family Sanction for not sharing	0.7	0.7	0.58
	(0.4)	(0.5)	
Business Characteristics			
Manufacturing	0.1	0.1	0.37
	(0.3)	(0.3)	
Retail	0.5	0.6	0.25
	(0.5)	(0.5)	
Ag*related*	0.1	0.1	0.67
	(0.3)	(0.3)	
Profits	1735.9	1738.3	0.99
	(2665.2)	(3074.5)	
Total Own Asset Value	11480.6	10691.7	0.80
	(41386.3)	(36440.2)	
Business Registered	0.5	0.5	0.16
	(0.5)	(0.5)	
Total Employees			
Total Rel Emp	0.3	0.3	0.43
	(0.6)	(0.6)	
Management score	8.2	7.9	0.24
	(3.1)	(3.2)	
Job/Employee			
Profit Difference	-53.8	-14.0	0.06
	(262.6)	(258.2)	
Moral Haz Rels	0.6	0.5	0.35
	(0.5)	(0.5)	
Job Easy Excuse	0.6	0.5	0.15
	(0.5)	(0.5)	
Job requires trust	0.5	0.5	0.77
	(0.5)	(0.5)	
Observations	308	311	

Notes: This table shows the balance between micro-enterprise owners in the Control poster and PLausible Deniability treatments in the Urban Mechanism Experiment. Variables were collected at baseline and corresponds to the entire sample. In Column (1) the mean and standard deviation are displayed for the variable of interest among the control group. In column (2) the same information is displayed for the treatment group. Finally, in Column (3) the p-value for a t-test of equality of means is displayed. The p-values of the difference in means test come from robust standard errors.

Table 2: Urban and Rural Mechanism Experiment – Pressure cost estimation

Panel A: Urban Mechanism Experiment	Compensating Differential ZMW	Compensating Differential ZMW
	(1)	(2)
Plausible	-38.21	-38.21
Deniability Poster	(15.29)	(15.08)
	[0.01]	[0.01]
Control mean	39	39
Obs	619	619
Regression	OLS	Tobit
Stratification Controls	Yes	Yes
Panel B: Rural Mechanism Experiment		
	(1)	(2)
Plausible	-8.36	-8.36
Deniability Choice	(2.08)	(2.07)
	[0.00]	[0.00]
Control mean	9	9
Obs	322	322
Regression	OLS	Tobit

Notes: This table shows treatment effects on the compensating differential required to hire a family employee across treatment conditions. Column 1 uses OLS, column 2 uses a Tobit specification. All regressions for the Urban Mechanism Experiment control for stratification variables: gender, having non-casual employees and market location. Robust standard errors in parentheses.

Table 3: Urban Mechansim Experiment – Treatment effects on hiring choices, heterogeneity by employer and job characteristics

	Hired Relative			
	(1)	(2)	(3)	(4)
Plausible Deniability	-0.02	-0.05	-0.04	-0.19
Poster	(0.07)	(0.04)	(0.06)	(0.06)
	[0.80]	[0.19]	[0.45]	[0.00]
X	-0.18	-0.24	-0.21	0.07
	(0.06)	(0.04)	(0.06)	(0.06)
	[0.00]	[0.00]	[0.20]	[0.24]
Plausible Deniability	-0.18	-0.24	-0.21	0.07
Poster x [X]	(0.09)	(0.06)	(0.08)	(0.08)
	[0.04]	[0.00]	[0.01]	[0.36]
Heterogeneity Variable	Sanctions Sharing	Moral Hazard	Excuse	Trust
Stratification Variables	Yes	Yes	Yes	Yes
p- Plausible + Plausible x [X]=0	0.00	0.00	0.00	0.04
Control mean	0.37	0.92	0.67	0.60
Obs	619	619	619	619

Notes: This table shows treatment effects on the microenterprise owners' choice of who to hire and expected profitability of hiring when offered the Plausible Deniability Poster, interacted with dimensions of heterogeneity. The outcome in all columns is an indicator variable for whether the microenterprise owner chose to hire their relative when offered equal subsidies. The dimension of heterogeneity in Column 1 is whether the business owner agrees or strongly agrees that they would face sanctions if their family asked for something and they did not give it. The dimension of heterogeneity in Column 2 is an indicator variable equalling one if the business owner agrees that if the employee did not perform well on the job it would be easy to make an excuse. The dimension of heterogeneity in Column 3 is an indicator variable equalling one if the business owner states that the job is one where it would be easy for the worker to make an excuse for poor performance. The dimension of heterogeneity in Column 4 is an indicator variable equalling one if the business owner states that family employees work for family employers less hard than they would for nonfamily employers. All regressions control for stratification variables: gender, having non-casual employees and market location. Robust standard errors in parentheses.

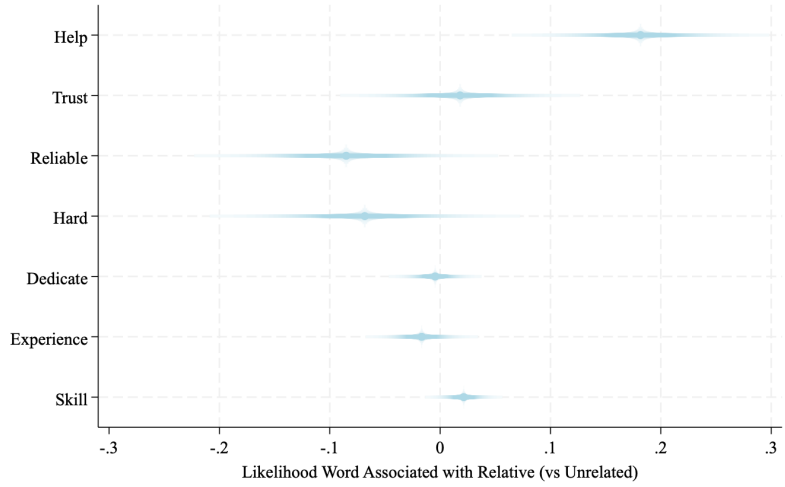
Table 4: Rural Mechanism Experiment – Random assignment to (relative or nonrelative) subsidy and employee's performance

	Output (kg) (1)	Output (kg) (2)	Wage (ZMW) (3)	Quality (4)
Relative Subsidy	-27.7	-26.4	0.3	-0.06
	(13.9)	(13.1)	(1.6)	(0.03)
	[0.05]	[0.04]	[0.84]	[0.06]
Control Mean	389	389	39	0.88
N	588	588	576	582
Subsidy Day FE	Yes	Yes	Yes	Yes
Subsidy Amount FE	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes

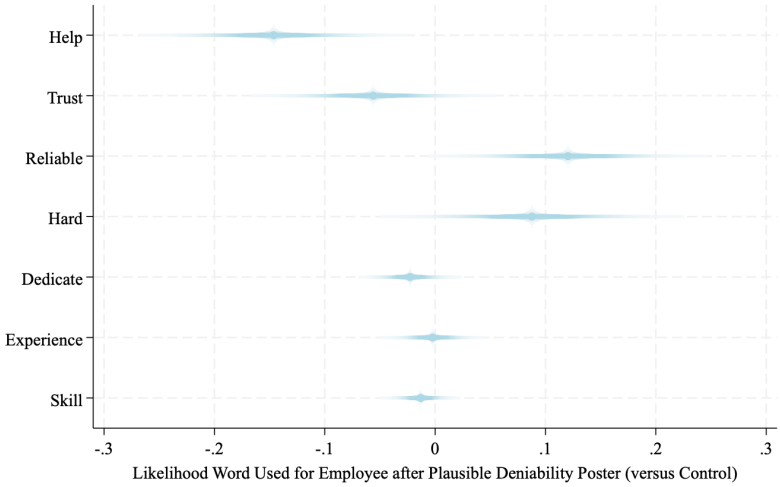
Notes: This table shows treatment effects on employee's output quantity, quality and wages, when employers in the Rural Mechanism experiment are assigned a subsidy either for a family or nonfamily employee. The outcome in columns 1 and 2 is the employer output (maize shelled in kg) over the course of the half day contract. The outcome in column 3 is the wage paid in local currency per day. The outcome in column 4 is an indicator variable for the finished output showing quality defects. Standard errors clustered at the household level in parentheses.

A Appendix Figures

Figure A1: Urban Field Experiment – Relative likelihood of using certain words for justifying the hiring choice



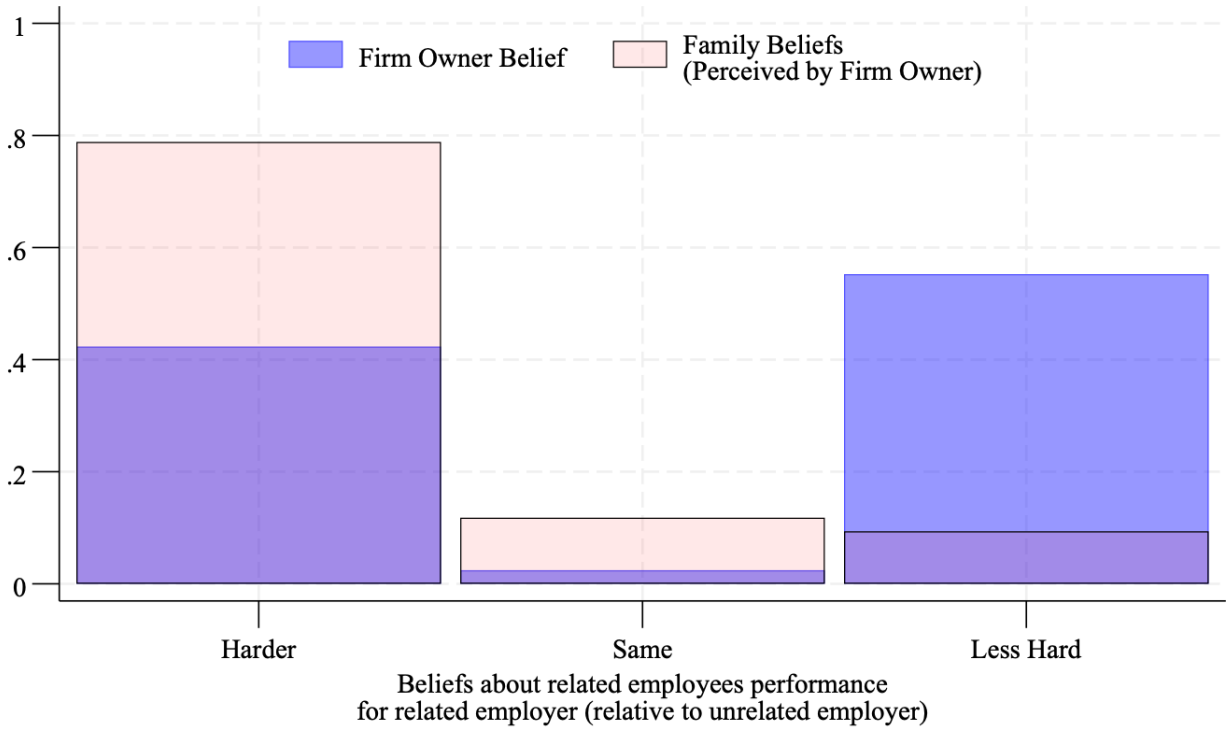
(a) By whether the employee is related



(b) By Plausible Deniability Treatment

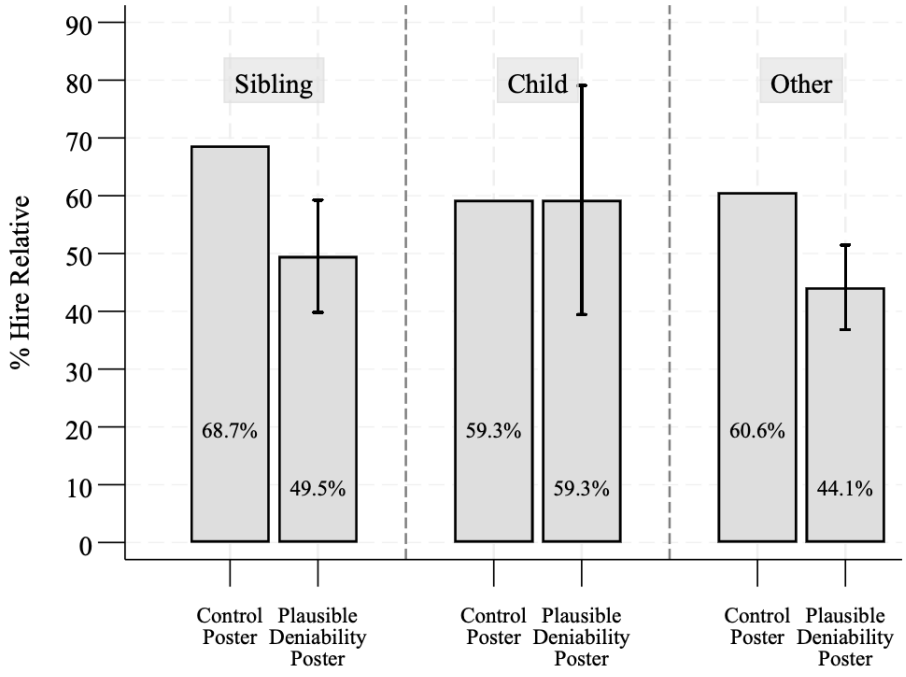
Notes: These figures show the likelihood of using particular words to rationalize hiring choices in the Urban Field Experiment. The coefficients in Panel A are regression coefficients from regressing the word in question on a dummy variable for whether the employee selected for the job was related to the employer, or not. Therefore it measures the increased likelihood that a particular word is used to justify hiring a relative rather than a nonrelative. Panel B shows regression coefficients from regressing the word in question on a dummy variable for whether the employer was assigned to the Plausible Deniability poster, or not. Therefore it measures the increased likelihood that a particular word is used to justify hiring an employee in the plausible deniability condtion. All regressions also control for location and industry fixed effects. Robust standard errors are shown.

Figure A2: Urban Mechanism Experiment - Disagreement between Employer and Family Regarding Work Effort of Employee



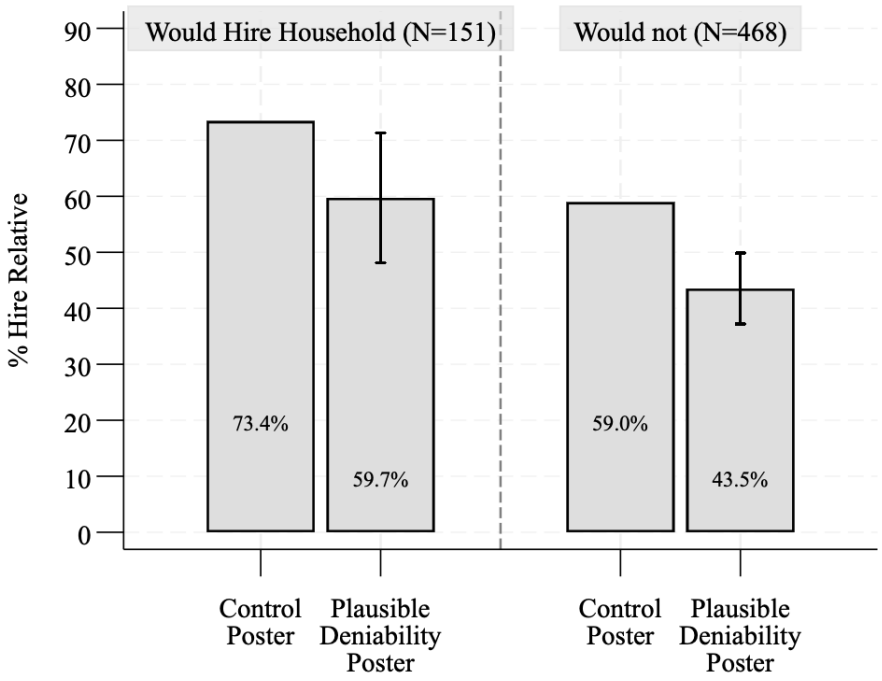
Notes: This figure shows the belief of the the employer, and the second order beliefs of the employer about their family's beliefs, of the family employee's work effort. Each bar displays the proportion who perceive that the family employee will work harder, the same, or less hard for their family employer than other nonfamily employers. The blue bars show the beliefs of the firm owner. The red bars show the beliefs of the firm owner of their family's beliefs.

Figure A3: Urban Mechanism Experiment - Treatment Effect heterogeneity conditional on relationship of Employee to Employer



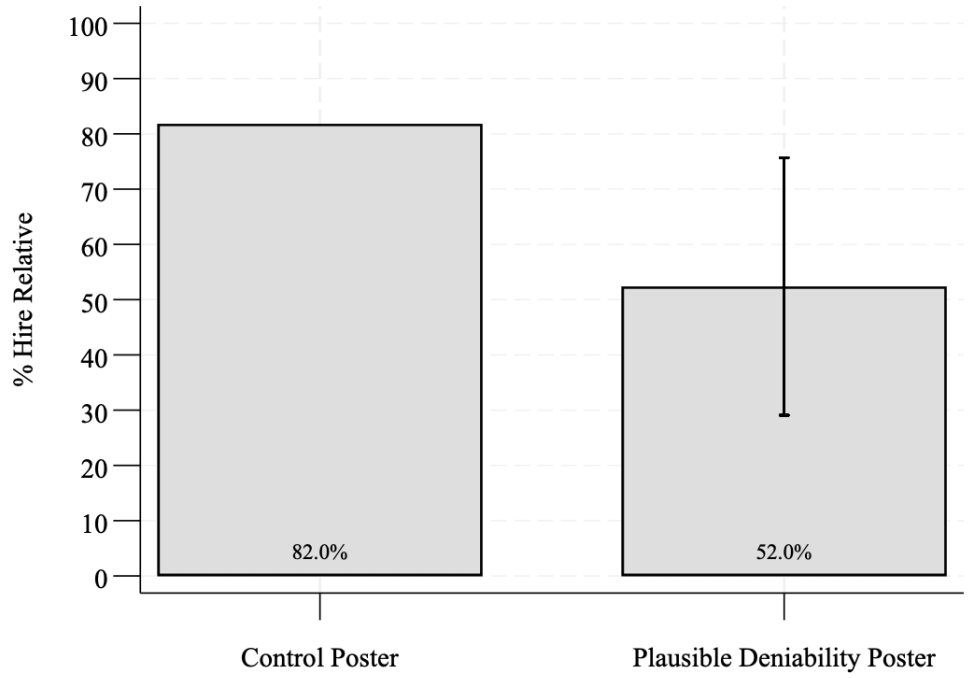
Notes: This figure shows the likelihood of a relative being selected in the Urban Mechanism experiment, conditional on the relationship between the employer and the family employee selected.

Figure A4: Urban Mechanism Experiment - Treatment Effect heterogeneity conditional on employer stating that the marginal relative hire is a member of their household



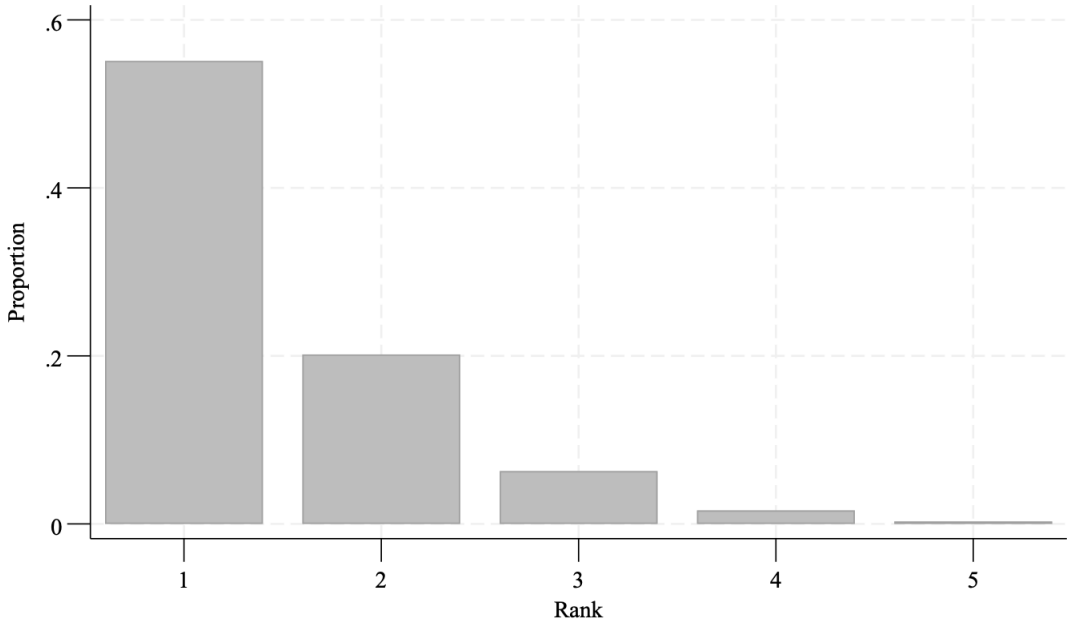
Notes: This figure shows the likelihood of a relative being selected in the Urban Mechanism experiment, conditional on the employer stating at baseline that the maringal relative hire was, or was not a member of their household.

Figure A5: Urban Mechanism Experiment - Treatment Effect for those randomly selected to receive subsidy



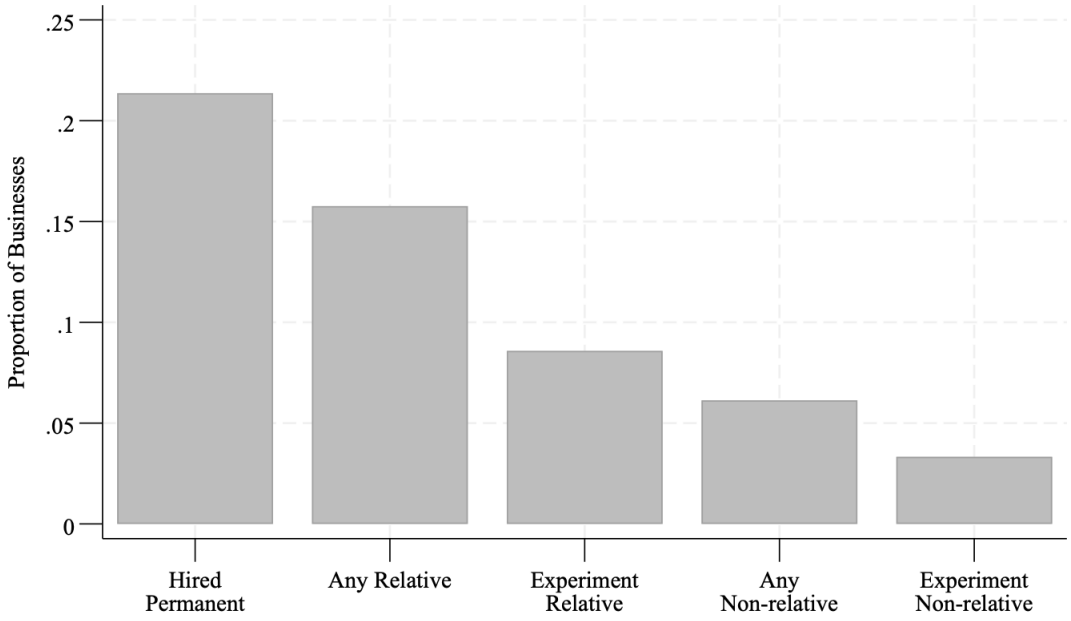
Notes: This figure shows the likelihood of selecting a family employee in the Urban Mechanism experiment across treatment conditions only conditional on the sample of employers randomly selected to receive the subsidy.

Figure A6: Urban Mechanism Experiment - Employer Ranking of Selected Relative among Relative Options



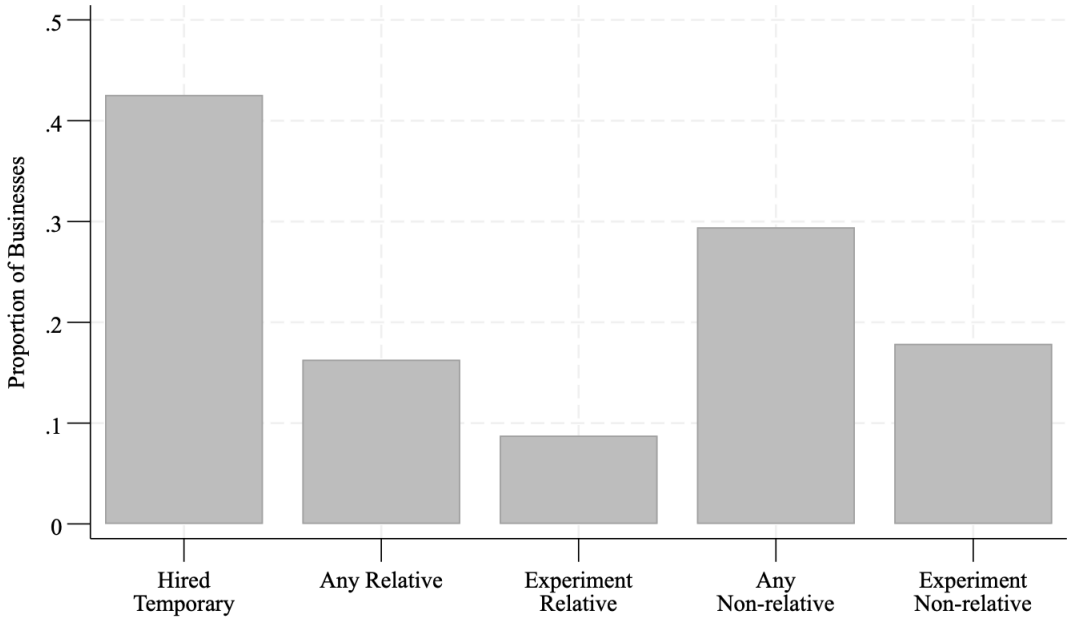
Notes: These figures show how employers ranked the relative selected for the experiment among the relatives they listed at baseline as a prospective employee. 1 indicates best available. Data was collected for employers not assigned the subsidy following the experiment.

Figure A7: Urban Mechanism Experiment - Identity of employees hired for employers not given subsidy following experiment - Permanent jobs



Notes: These figures show which permanent employees, if any, employers in the experiment hired after the experiment. Each bar is an indicator variable equal to one if the employer hired that category of employee. Data was collected for employers not assigned the subsidy following the experiment.

Figure A8: Urban Mechanism Experiment - Identity of employees hired for employers not given subsidy following experiment - Casual jobs



Notes: These figures show which casual employees, if any, employers in the experiment hired after the experiment. Each bar is an indicator variable equal to one if the employer hired that category of employee. Data was collected for employers not assigned the subsidy following the experiment.

B Appendix Tables

Table A1: Balance - Urban Field Experiment

Variable	Control Mean	Treatment Mean	P-value Δ
Demographics			
Age	41.2	40.2	0.47
	(9.9)	(10.5)	
Gender	0.5	0.6	0.54
	(0.5)	(0.5)	
Household Size	4.9	4.9	0.86
	(1.7)	(4.0)	
Savings			
Digit Span	5.6	5.6	0.83
	(0.7)	(0.9)	
Ravens	5.0	4.7	0.47
	(3.0)	(2.8)	
Agree Family Sanction for not sharing	0.9	0.8	0.20
	(0.3)	(0.4)	
Business Characteristics			
Manufacturing	0.2	0.1	0.21
	(0.4)	(0.3)	
Retail	0.7	0.7	0.50
	(0.5)	(0.5)	
Ag*related*	0.1	0.1	0.44
	(0.3)	(0.3)	
Profits	1656.7	1761.6	0.65
	(1711.8)	(1929.2)	
Total Own Asset Value	16911.3	13537.5	0.34
	(30704.3)	(24528.7)	
Business Registered	0.5	0.5	0.89
	(0.5)	(0.5)	
Total Employees	0.5	0.5	0.60
	(0.7)	(0.6)	
Total Rel Emp	0.2	0.2	0.84
	(0.4)	(0.4)	
Management score	7.1	6.7	0.15
	(2.2)	(2.1)	
Job requires trust	0.3	0.4	0.60
	(0.5)	(0.5)	
Observations	112	142	

Notes: This table shows balance for employers in the Urban Field Experiment. Column 1 shows mean and standard deviations for Employers in the Control Poster condition, Column 2 for the treatment poster condition. P-values for a test of equality are in column 3. Robust standard errors in parentheses.

Table A2: Balance - Rural Experiment Plausible Deniability

Variable	Control Mean	Treatment Mean	P-value Δ
Demographics			
Age	47.7	46.5	0.43
	(13.6)	(14.2)	
Primary	0.2	0.2	0.50
	(0.4)	(0.4)	
Secondary	0.6	0.6	0.39
	(0.5)	(0.5)	
Household Size	6.3	6.1	0.38
	(2.4)	(2.4)	
Married	0.8	0.8	0.97
	(0.4)	(0.4)	
Gender	0.7	0.6	0.33
	(0.5)	(0.5)	
Savings	1284.1	1010.7	0.27
	(2469.6)	(1992.0)	
Farm/Shelling			
Land (acres)	5.7	5.7	0.92
	(2.8)	(2.9)	
Farm Earnings	18234.9	15068.0	0.08
	(16992.9)	(15362.2)	
Owns Business			
Current Maize	46.4	40.9	0.07
	(33.6)	(17.3)	
Hired Shell Ever	0.9	0.8	0.48
	(0.3)	(0.4)	
Pressure/Relative Beliefs			
Report Hiring Pressure	0.6	0.5	0.59
	(0.5)	(0.5)	
Report Sanctions	0.8	0.8	0.50
	(0.4)	(0.4)	
Shelling MH	0.3	0.3	0.55
	(0.5)	(0.4)	
Rel shell$_{m}ore_{b}aseline$	0.5	0.5	0.27
	(0.5)	(0.5)	
Rel better quality baseline	0.5	0.5	0.99
	(0.5)	(0.5)	
Observations	163	159	

Notes: This table shows balance in the Shelling sample wrt Plausible Deniability. Robust standard errors in parentheses.

Table A3: Balance - Rural Experiment Subsidy Assigment

Variable	Control Mean	Treatment Mean	P-value Δ
Demographics			
Age	47.7	46.6	0.50
	(14.2)	(13.9)	
Primary	0.2	0.2	0.52
	(0.4)	(0.4)	
Secondary	0.6	0.6	0.68
	(0.5)	(0.5)	
Household Size	6.4	6.2	0.40
	(2.4)	(2.3)	
Married	0.8	0.8	0.48
	(0.4)	(0.4)	
Gender	0.7	0.7	0.78
	(0.5)	(0.5)	
Savings	1267.1	1094.2	0.53
	(2540.3)	(2041.4)	
Farm/Shelling			
Land (acres)	5.8	5.8	0.93
	(3.0)	(2.8)	
Farm Earnings	16317.9	17382.8	0.58
	(15531.9)	(17633.6)	
Owns Business			
Current Maize	45.0	43.4	0.61
	(26.2)	(29.2)	
Hired Shell Ever	0.9	0.8	0.74
	(0.4)	(0.4)	
Pressure/Relative Beliefs			
Report Hiring Pressure	0.5	0.6	0.28
	(0.5)	(0.5)	
Report Sanctions	0.8	0.9	0.03
	(0.4)	(0.4)	
Shelling MH	0.3	0.3	0.99
	(0.4)	(0.4)	
Rel shell$_{m}ore_{b}aseline$0.5	0.6	0.59	
	(0.5)	(0.5)	
Rel better quality baseline 0.5	0.4	0.25	
	(0.5)	(0.5)	
Observations	135	160	

Notes: This table shows balance in the Shelling sample wrt Subsidy assignment. Robust standard errors in parentheses.

Table A4: Urban Field Experiment - Treatment Effects on Other Traits of Employee

	Demographics/Wage			Social Proximity					
	Male	Wage	Post Primary	Hired Previously	See Often	Fin Assistance	Years known	Share Friends	Social Connections
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Plausible	0.01	51.67	0.19	-0.04	-0.01	-0.09	-1.18	0.00	-0.06
Deniability Poster$_i$	(0.06)	(25.67)	(0.06)	(0.04)	(0.04)	(0.06)	(0.99)	(0.05)	(0.06)
	[0.85]	[0.05]	[0.00]	[0.35]	[0.82]	[0.13]	[0.23]	[0.98]	[0.36]
Control mean	0.58	626.82	0.60	0.11	0.91	0.3	11.7	0.19	0.19
Obs	251	251	251	251	251	251	251	251	251
Area FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the effect of having the Plausible Deniability poster in the Urban Field experiment on the type of employee selected, along different margins. The outcome in column 1 is whether the employee selected was male. The outcome in column 2 is the expected wage of the employee. The outcome in column 3 is whether the employee had more than primary education. The outcome in column 4 is whether the employee had previously been hired by the employer. The outcome in column 5 is whether the employee was someone the employer sees often. The outcome in column 6 is whether the employee is someone the employer offers financial assistance to. The outcome in column 7 is the number of years the employee has been know by the employer. The outcome variable in column 8 is an indicator variable for whether the employer and employee share many friends. The outcome variable in column 9 is an indicator variable for whether the employer and employee have many social connections. Robust standard errors in parentheses.

Table A5: Urban Mechanism Experiment - Treatment effects on hiring choices - Additional heterogeneity

	Hired Relative					
	(1)	(2)	(3)	(4)	(5)	(6)
Plausible Deniability	-0.11	-0.15	-0.16	-0.16	-0.15	-0.16
Poster	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)
	[0.04]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
X	-0.10	-0.01	0.04	0.02	-0.01	0.01
	(0.06)	(0.03)	(0.03)	(0.06)	(0.04)	(0.03)
	[0.80]	[0.80]	[0.06]	[0.66]	[0.77]	[0.52]
Plausible Deniability	-0.10	-0.01	0.04	0.02	-0.01	0.01
Poster x [X]	(0.08)	(0.04)	(0.04)	(0.09)	(0.06)	(0.04)
	[0.20]	[0.79]	[0.30]	[0.86]	[0.84]	[0.80]
Heterogeneity Variable	Male	Ravens	Mgmt Score	Binary Perm Emp	Rel Emp	Emp
p-Treatment + Treatment x [X]=0	0.00	0.00	0.05	0.06	0.00	0.00
Stratification Variables	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.63	0.63	0.63	0.63	0.63	0.63
Obs	619	619	619	619	619	619

Notes: This table shows treatment effects on microenterprise owners choice of who to hire and expected profitability of hiring when offered the Plausible Deniability Poster, interacted with dimensions of heterogeneity. The outcome in all columns is an indicator variable for whether the microenterprise owner chose to hire their relative when offered equal subsidies. The dimension of heterogeneity in Column 1 is whether the business owner agrees or strongly agrees that ... The dimension of heterogeneity in Column 2 is an indicator variable equalling one if the business owner agrees that if the employee did not perform well on the job it would be easy to make an excuse. The dimension of heterogeneity in Column 3 is an indicator variable equalling one if the business owner [trust in job]. The dimension of heterogeneity in Column 4 is an indicator variable equalling one if the business owner [moral hazard]. All regressions control for stratification variables: gender, having non-casual employees and market location. Robust standard errors in parentheses.

Table A6: Urban Mechanism Experiment - Treatment effects on hiring choices - heterogeneity by social distance

	Hired Relative			
	(1)	(2)	(3)	(4)
Plausible Deniability	-0.177	-0.152	-0.162	-0.150
Poster	(0.042)	(0.045)	(0.043)	(0.042)
	[0.000]	[0.001]	[0.000]	[0.000]
X	0.237	-0.007	0.061	-0.024
	(0.090)	(0.068)	(0.078)	(0.090)
	[0.163]	[0.100]	[0.357]	[0.097]
Plausible Deniability	0.237	-0.007	0.061	-0.024
Poster x [X]	(0.127)	(0.096)	(0.115)	(0.122)
	[0.062]	[0.946]	[0.592]	[0.845]
Heterogeneity Variable	Attend same church	See more often	Reciprocate Favor	More common friends
Proportion of Sample	0.11	0.23	0.14	0.12
p-Treatment + Treatment x [X]=0	0.618	0.061	0.340	0.129
Stratification Variables	Yes	Yes	Yes	Yes
Control mean	0.640	0.653	0.641	0.644
Obs	619	619	619	619

Notes: This table shows treatment effects on microenterprise owners choice of who to hire and expected profitability of hiring when offered the Plausible Deniability Poster, interacted with dimensions of heterogeneity. The outcome in all columns is an indicator variable for whether the microenterprise owner chose to hire their relative when offered equal subsidies. The dimension of heterogeneity in Column 1 is whether the business owner agrees or strongly agrees that ... The dimension of heterogeneity in Column 2 is an indicator variable equalling one if the business owner agrees that if the employee did not perform well on the job it would be easy to make an excuse. The dimension of heterogeneity in Column 3 is an indicator variable equalling one if the business owner [trust in job]. The dimension of heterogeneity in Column 4 is an indicator variable equalling one if the business owner [moral hazard]. All regressions control for stratification variables: gender, having non-casual employees and market location. Robust standard errors in parentheses.

Notes: This table shows the mean and standard deviation of observable characteristics for Family and Non-family employees selected in the rural mechanism experiment. P values for a test of equality are in column 3.

Table A7: Balance - Rural Mechanism Experiment Observable Differences in Employee Characteristics

	Mean Relatives (1)	Mean Non-Relatives (2)	p-difference (3)
Male	0.68	0.72	0.48
	(0.47)	(0.45)	
Age	27.73	31.06	0.00
	(8.82)	(10.64)	
Married	0.65	0.56	0.15
	(0.48)	(0.50)	
HH Size	5.56	5.17	0.56
	(7.68)	(2.28)	
No Education	0.13	0.16	0.37
	(0.33)	(0.37)	
Primary Ed	0.65	0.64	0.89
	(0.48)	(0.48)	
Maize	18.55	20.13	0.50
	(17.99)	(22.57)	
Acres	3.08	3.59	0.09
	(2.26)	(2.72)	
Shell Maize	0.96	0.96	0.98
	(0.19)	(0.19)	
Shell Maize Often	0.64	0.66	0.64
	(0.48)	(0.48)	
Experience	3.78	3.87	0.66
	(1.61)	(1.60)	
N	134	159	

Table A8: Correlation - Pressure and Hiring

Panel A: Employer Controls	Any Rel	Any Non-Rel	Total Rel	Emp	Mgmt Score	Log Asset Value	Log Sales	Log Profit
pressure_share	0.12	-0.04	0.13	0.10	-0.48	-0.67	0.08	-0.02
	(0.030)	(0.034)	(0.038)	(0.060)	(0.066)	(0.225)	(0.078)	(0.098)
	[0.000]	[0.302]	[0.001]	[0.085]	[0.000]	[0.003]	[0.299]	[0.868]
N	936	936	936	936	936	936	936	936
Outcome Mean	0.21	0.36	0.25	0.78	8.52	6.40	7.71	6.55
Ind FE	No	No	No	No	No	No	No	No
Market FE	No	No	No	No	No	No	No	No
Employee Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
Business Controls	No	No	No	No	No	No	No	No
Panel B: Employer Controls & FE	Any Rel	Any Non-Rel	Total Rel	Emp	Mgmt Score	Log Asset Value	Log Sales	Log Profit
Family Sanctions	0.13	-0.05	0.16	0.14	-1.46	-0.61	0.10	-0.00
	(0.031)	(0.033)	(0.041)	(0.057)	(0.212)	(0.208)	(0.075)	(0.097)
	[0.000]	[0.143]	[0.000]	[0.012]	[0.000]	[0.003]	[0.166]	[0.961]
N	931	931	931	931	931	931	931	931
Outcome Mean	0.21	0.36	0.25	0.78	8.52	6.40	7.71	6.55
Ind FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Market FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employee Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Business Controls	No	No	No	No	No	No	No	No
Panel C: Employer and Business Controls & FE	Any Rel	Any Non-Rel	Total Rel	Emp	Mgmt Score			
pressure_share	0.13	-0.05	0.17	0.16	-1.40			
	(0.031)	(0.033)	(0.041)	(0.056)	(0.207)			
	[0.000]	[0.135]	[0.000]	[0.005]	[0.000]			
N	931	931	931	931	931			
Outcome Mean	0.21	0.36	0.25	0.78	8.52			
Ind FE	Yes	Yes	Yes	Yes	Yes			
Market FE	Yes	Yes	Yes	Yes	Yes			
Employee Controls	Yes	Yes	Yes	Yes	Yes			
Business Controls	Yes	Yes	Yes	Yes	Yes			

Notes: This table shows the correlation between an employer reporting they face sanctions if they do not share with their fa and firm outcomes, from all eligible and ineligible firms in the Urban Mechanism Experiment. Any Rel is an indicator variable e to one if the Employer has any relative hired in their business. Any Non-Rel is an indicator variable equal to one if the Employe any nonrelative hired in their business. Total rel is the total number of relatives the Employer has hired in their business. Em the total number of employees the employer has hired in their business. Management score is the an index of management prac Log asset value is the monetary value of assets plus one, logged. Log sales value is total sales plus one, logged. Log profits val total profits plus one, logged. Standard errors clustered at the employer level in parentheses.

Table A9: Correlation - Moral Hazard and Firm Outcomes

	Any Rel	Any Non-Rel	Total Rel	Emp	Mgmt Score	Log Asset Value	Log Sales	Log Profit
Panel A: Employer Controls								
Relatives Perform Worse	0.00	0.04	-0.01	0.07	0.04	0.14	0.10	0.03
	(0.032)	(0.032)	(0.040)	(0.057)	(0.067)	(0.224)	(0.074)	(0.095)
	[0.925]	[0.174]	[0.889]	[0.206]	[0.564]	[0.537]	[0.166]	[0.760]
N	936	936	936	936	936	936	936	936
Outcome Mean	0.26	0.31	0.32	0.78	7.68	5.95	7.70	6.54
Ind FE	No	No	No	No	No	No	No	No
Market FE	No	No	No	No	No	No	No	No
Employee Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Business Controls	No	No	No	No	No	No	No	No
	Any Rel	Any Non-Rel	Total Rel	Emp	Mgmt Score	Log Asset Value	Log Sales	Log Profit
Panel B: Employer Controls & FE								
Relatives Perform Worse	0.01	0.03	0.01	0.09	0.02	0.20	0.08	0.02
	(0.031)	(0.032)	(0.040)	(0.055)	(0.068)	(0.217)	(0.071)	(0.094)
	[0.798]	[0.309]	[0.894]	[0.125]	[0.792]	[0.369]	[0.262]	[0.802]
N	931	931	931	931	931	931	931	931
Outcome Mean	0.26	0.31	0.32	0.78	7.68	5.95	7.70	6.54
Ind FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Market FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employee Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Business Controls	No	No	No	No	No	No	No	No
Panel C: Employer and Business Controls & FE								
	Any Rel	Any Non-Rel	Total Rel	Emp	Mgmt Score			
Relatives Perform Worse	0.01	0.03	0.00	0.07	0.01			
	(0.031)	(0.032)	(0.040)	(0.054)	(0.066)			
	[0.873]	[0.345]	[0.999]	[0.197]	[0.935]			
N	931	931	931	931	931			
Outcome Mean	0.26	0.31	0.32	0.78	7.68			
Ind FE	Yes	Yes	Yes	Yes	Yes			
Market FE	Yes	Yes	Yes	Yes	Yes			
Employee Controls	Yes	Yes	Yes	Yes	Yes			
Business Controls	Yes	Yes	Yes	Yes	Yes			

Notes: This table shows the correlation between an employer reporting that their relatives work less well for them as employees firm outcomes, from all eligible and ineligible firms in the Urban Mechanism Experiment. Any Rel is an indicator variable equ one if the Employer has any relative hired in their business. Any Non-Rel is an indicator variable equal to one if the Employe any nonrelative hired in their business. Total rel is the total number of relatives the Employer has hired in their business. Em the total number of employees the employer has hired in their business. Management score is the an index of management prac Log asset value is the monetary value of assets plus one, logged. Log sales value is total sales plus one, logged. Log profits val total profits plus one, logged. Standard errors clustered at the employer level in parentheses.

Table A10: Characteristics of Eligible and Ineligible Businesses

Variable	Ineligible Mean	Eligible Mean	P-value Δ
Demographics			
Age	38.8	38.9	0.99
	(38.8)	(38.9)	
Gender	0.5	0.4	0.45
	(0.5)	(0.4)	
Household Size	4.6	5.6	0.00
	(4.6)	(5.6)	
Savings			
Digit Span	5.7	5.9	0.13
	(5.7)	(5.9)	
Ravens	5.2	6.1	0.00
	(5.2)	(6.1)	
Agree Family Sanction for not sharing	0.6	0.7	0.05
	(0.6)	(0.7)	
Business Characteristics			
Manufacturing	0.1	0.1	0.00
	(0.1)	(0.1)	
Retail	0.9	0.6	0.00
	(0.9)	(0.6)	
$Ag_related$	0.0	0.1	0.00
	(0.0)	(0.1)	
Profits	753.3	1841.3	0.00
	(753.3)	(1841.3)	
Total Own Asset Value	4141.7	13194.2	0.00
	(4141.7)	(13194.2)	
Business Registered	0.5	0.5	0.16
	(0.5)	(0.5)	
Total Employees	0.8	0.8	0.32
	(0.8)	(0.8)	
Total Rel Emp	0.3	0.3	0.62
	(0.3)	(0.3)	
Management score	6.9	8.1	0.00
	(6.9)	(8.1)	
Job/Employee			
Moral Haz Rels	0.7	0.6	0.00
	(0.7)	(0.6)	
Job requires trust	0.4	0.5	0.03
	(0.4)	(0.5)	
Observations	256	683	

Notes: This table shows the characteristics of eligible and ineligible businesses in the Urban Mechanism experiment. Column 1 shows means and standard deviations for ineligible businesses. Column 2 shows means and standard deviations for eligible businesses. P-values for a test of equality are shown in column 3. Robust standard errors in parentheses.