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

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September 15, 2026

Abstract

Family hiring is widespread in low- and middle-income countries. Kinship can make workers valuable in jobs requiring trust, but also weaken effort when family obligations limit discipline. I conduct three wage-subsidy experiments with urban firms and agricultural employers in Zambia to test whether redistributive pressure prevents employers from avoiding kin matches they expect to be costly. Giving employers a credible external explanation for rejecting relatives reduces kin hiring by 21–25% relative to control-group hiring rates and shifts choices toward outsiders employers expect to be more profitable. Subsidy choices reveal an implicit tax on hiring outsiders: deniability reduces the additional compensation required to select an outsider by ZMW 49.4 in urban firms, equivalent to approximately 5% of median monthly firm profits. The hiring response depends on whether kinship makes the relative a desirable match for the job. Machine-learning estimates show substantial variation across jobs, with smaller responses when workers are responsible for money and, more suggestively, when poor performance is harder to excuse. These patterns are consistent with trust and family accountability making some kin matches privately attractive. To assess whether costly kin hires reflect lower worker ability or problems within the employment relationship, I combine randomized worker assignment in agriculture with a common pre-employment performance benchmark. Relatives produce approximately 7% less output than unrelated workers, with no detectable wage reduction, despite similar performance before employment. The penalty is larger where employers anticipate deterioration in the kin match, pointing to relationship-specific incentive costs rather than simply less able workers. Redistributive pressure can thus misallocate workers by preventing employers from acting on anticipated differences in match quality.

*I am thankful for support and advice from Supreet Kaur, Ted Miguel, Ned Augenblick and Frederico Finan. Luisa Cefala, Stefano DellaVigna, Madeline Duhon, Benjamin Faber, Matt Lowe, Jeremy Magruder, Imran Rasul, Nick Tsivanidis, Chris Woodruff and David Wu, as well as audiences at BREAD, UC Davis and SITE 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 hire outside their family networks (Jayachandran, 2021). Family ties can help firms overcome problems of information and trust, making relatives attractive employees (Bertrand and Schoar, 2006; Chandrasekhar et al., 2020). Yet social proximity to employees may also have downsides for employers: it can also weaken discipline by making employers reluctant to penalize poor performance.[1] The productive value of kinship therefore depends on the employment relationship: family ties may make relatives particularly valuable in some jobs, while difficulties imposing discipline make them costly in others.

Does the prevalence of family firms primarily reflect the productive advantages of kinship, or also constraints on employers' ability to hire outside the family? While family hiring is often explained by advantages in information and enforcement (Chandrasekhar et al., 2020), a growing empirical literature highlights the economic consequences of redistributive obligations. Pressure to share with relatives can induce individuals to forgo income (Jakiela and Ozier, 2016), and employers may use jobs rather than cash transfers to provide assistance (Macchi and Stalder, 2023). These findings suggest that family claims may extend to employment, making relatives costly to reject. Such obligations could prevent employers from sorting away from kin matches they expect to be unprofitable. Family hiring may then persist despite anticipated discipline problems, rather than because the productive advantages of kinship outweigh them.

While family pressure may lead firms into costly matches, identifying this channel is difficult because observed hiring choices do not reveal why employers favor relatives. An employer may select a relative because they expect higher profits, because they value helping that person, or because rejecting them would provoke family sanctions: revealed preference alone cannot distinguish these motives. An ideal experiment would vary the social cost of rejecting relatives while holding the available workers and their expected productive returns fixed. A natural approach to identifying effects of social pressure is to vary whether choices are publicly observed or kept private, as in experiments identifying the effects of social pressure on educational and labor-market decisions (Bursztyn and Jensen, 2015; Bursztyn et al., 2017; Breza et al., 2019). Here, keeping hiring decisions private could reduce the social cost of rejecting a relative without changing the available matches. Yet employment decisions are difficult to conceal: workers are visible at the business, and relatives may discover that an employer has hired someone else even when the decision is not publicly announced.

[1] Social relationships can affect both the willingness to impose penalties and the allocation of managerial resources. Chen and Li (2009) find that experimentally induced group identity reduces punishment of misbehavior by an in-group member. Letina et al. (2020) formalize how a supervisor's concern for workers can undermine credible performance incentives. In a workplace setting, Bandiera et al. (2009) show that managers with weak monetary incentives favor socially connected workers, reducing aggregate productivity; performance bonuses redirect managerial effort toward high-ability workers.

In this paper, I address this challenge through three field experiments with urban firms and agricultural employers in Zambia. The experiments provide some employers with a credible external explanation for rejecting relatives, allowing them to deflect responsibility for hiring an outsider without concealing the hire. The agricultural setting also permits randomized worker assignment, allowing me to measure the productive consequences of related employment. I find that employers anticipate benefits from some kin matches and costs from others, but family pressure constrains their ability to act on these assessments. Reducing this pressure shifts hiring toward workers whom employers expect to be more profitable, while randomized assignment reveals lower output from relatives on average. Together, these findings show how family obligations can sustain costly employment relationships that employers would otherwise avoid.

I develop a signaling model to explain how family obligations can sustain privately undesirable employment relationships even when employers anticipate their incentive problems. The model takes job-specific productive benefits of kinship as given and focuses on reputational incentives within the family. Employers care about being perceived as willing to support relatives, while workers care about being perceived as willing to contribute. Concern that dismissal signals an unwillingness to help makes dismissing a relative costly, weakening the threat of job loss. Workers' concern for their own reputation provides a countervailing incentive to exert effort.

Three predictions guide the empirical analysis. First, holding employment values fixed, reducing the social cost of rejecting relatives shifts hiring toward outsiders among employers who privately prefer them. Second, where the employer's reputation with the family makes dismissal costly, family employment can weaken effort incentives. Third, clearer performance evidence and the worker's own reputation with the family can provide a countervailing source of accountability. Together, these forces allow employers to anticipate an effort disadvantage yet hire a relative because refusal is costly, while leaving room for other kin matches to be privately attractive.

Testing this framework requires showing that family pressure changes whom employers hire. It also requires evidence that some workers perform worse for a relative than they would for an unrelated employer, and that employers anticipate this difference. I use three complementary field experiments with urban firms and agricultural employers in Zambia to examine these predictions. Employers make hiring choices under wage-subsidy offers.[2] Before they choose, I randomize how the subsidized hire will be explained publicly. For employer-chosen hires, the control message states that the employer received a subsidy and selected the worker. In the plausible deniability condition, the message instead explains that some employers received subsidies *conditional on hiring a particular employee*.[3] This gives

[2]The urban subsidy protocol follows De Mel et al. (2019).

[3]The messages are conveyed through posters in the urban experiments and verbally in the agricultural

employers a credible explanation for rejecting a relative: they can attribute the selection of an outsider to the program rather than their own choice.[4] The first urban experiment tests whether this changes worker selection. The second measures how deniability changes the compensation required to choose an outsider and whether hiring moves closer to employers' expected-profit rankings. In the agricultural experiment, I replicate the hiring comparison and independently randomize which candidate works, allowing me to measure output, quality, and wages. I also measure both candidates' performance on the same task before employment, to examine whether relatives already perform worse or whether a gap emerges when they work for a family employer. Similar performance before employment but lower output when working for a relative would suggest a match-specific effect, rather than simply the selection of less productive workers.

Across the three experiments, I document four main findings. First, at equal subsidies, plausible deniability reduces relative selection by 12.1 percentage points in the first urban experiment ($p = .054$), 15.2 points in the second ($p < .001$), and 19.7 points in the agricultural experiment ($p < .001$). These represent reductions of 21–25% relative to the control groups. To measure the monetary value of this constraint, I use choices across subsidy offers to estimate the compensating differential required to select an outsider. This differential reflects both the employer's valuation of the available matches and the social cost of rejecting kin. Its reduction under plausible deniability prices the component of the kinship tax removed by shifting responsibility for the decision.[5] The estimated mean tax removed is ZMW 49.4 in the urban mechanism experiment and ZMW 9.8 in the agricultural experiment ($p < .001$ in both). Family obligations therefore raise the private advantage an outsider must offer before an employer is willing to reject a relative.

Second, this tax prevents employers from acting on anticipated differences in profitability. Among urban mechanism employers who expect the outsider to generate higher profits, outsider selection rises from 58% under public attribution to 92% under plausible deniability; the adjusted treatment effect is 32.8 percentage points ($p < .001$). The hiring response is also particularly pronounced among employers who report family sanctions and expect a relative to work less hard for them than for an unrelated employer. Importantly, this pattern persists among employers who regard the relative as at least as productive as the outsider when working elsewhere. The relevant disadvantage is therefore not simply that employers consider relatives to be generally inferior workers: they anticipate problems specific to em-

experiment.

[4] To avoid deception, the program genuinely includes take-it-or-leave-it subsidy offers to hire someone other than the employer's preferred candidate. When an employer receives such an offer, the public message is adjusted in both groups to reflect the actual restriction. Plausible deniability therefore comes from uncertainty about which offers were restricted, rather than false statements about the program.

[5] This interpretation requires that attribution leave the private value of employing each candidate unchanged. The estimate measures the reduction in refusal costs, not their full level or a welfare loss. Switching thresholds are measured using the intervals defined by the subsidy schedule.

ploying them within the family. Reducing responsibility for the decision brings hiring closer to employers' expected-profit rankings without providing new information about the candidates. The constraint is not merely knowing which match is preferable, but being able to act on that knowledge.

Third, not all family employment reflects a constraint: kinship can also make a worker privately attractive. I investigate this distinction using regularized machine learning and repeated sample splitting to discover heterogeneity in hiring responses and evaluate it in held-out observations (Chernozhukov et al., 2025; Bryan et al., 2024). Even among employers reporting family pressure, the estimated reduction in relative hiring is approximately 30 percentage points in the most responsive third, compared with only 5 points in the least responsive third (split-aggregated contrast: $p = .091$). This pattern is consistent with some kin matches remaining desirable when the cost of rejecting relatives falls. Such differences could reflect employer or worker characteristics, or the jobs in which relatives would be employed. I use classification analysis to characterize the learned groups, followed by joint interaction regressions to distinguish these explanations. The evidence points toward job context: the less responsive groups more often involve money responsibility, customer-facing work, and high trust requirements. Money responsibility remains associated with a smaller hiring response after accounting for general preferences for kin, perceived worker ability elsewhere, and other job characteristics (Holm-adjusted $p = .031$). More suggestively, the response is also smaller where poor performance is harder to excuse ($p = .027$).[6] One possible explanation is that workers themselves benefit from plausible deniability: poor performance may be attributed to circumstances rather than an unwillingness to work. In the model, more informative performance signals shared by employers and families strengthen both disciplinary incentives and workers' concern for their reputation, raising effort. Job context may therefore affect the desirability of relatives not only through the productive value of trust, but also through whether workers can evade accountability for poor performance.[7]

Fourth, I use the agricultural experiment to measure the productive returns to hiring relatives and investigate whether any disadvantage reflects worker ability or the employment relationship. Randomly assigning the relative rather than the nonrelative reduces average output by approximately 7% ($p = .049$) and increases the number of production defects ($p = .042$), without a detectable reduction in wages ($p = .983$). The two candidates nevertheless produce virtually identical output in the common pre-employment task ($p = .990$), with no difference in input-adjusted defects ($p = .862$). Moreover, controlling for both candidates'

[6] The excuse-difficulty result weakens when all contextual interactions are included simultaneously ($p = .101$).

[7] Money responsibility could operate partly through this channel if financial discrepancies provide clearer evidence of conduct and are harder to excuse. This is a possible interpretation, not an identified mechanism: the evidence does not distinguish stronger accountability from a greater productive value of trusting a worker with money.

baseline performance leaves the employment output penalty largely unchanged. The gap thus emerges during employment rather than in measured baseline productivity, pointing to a relationship-specific disadvantage rather than simply less productive workers.[8] Linked data on naturally occurring agricultural contracts provide complementary evidence. Using an AKM-style specification with worker and employer fixed effects (Abowd et al., 1999), I compare related and unrelated employment net of persistent additive differences across workers and employers. The output estimate again points to a penalty—approximately 5.4% ($p = .259$)—alongside worse reported quality, although these estimates are imprecise. The evidence therefore supports a productive cost of related employment, rather than viewing the allocation of jobs solely as a transfer of earnings.

Why does redistributive pressure affect whom employers hire rather than simply how they distribute income? If cash transfers were an adequate substitute for employment, employers could hire the most productive worker and support relatives separately. Qualitative interviews and survey responses suggest why this separation may be difficult. First, employers often believe that their families do not share their assessments of kin productivity. In the urban mechanism experiment, 48% of employers expect a relative to exert less effort for them than for an unrelated employer, but believe their family does not anticipate this deterioration. Refusal may therefore be interpreted as unwillingness to help rather than a response to an unattractive match. Second, employment may provide a more visible and credible commitment to support than discretionary cash transfers. A job makes assistance observable and may make its withdrawal socially costly. Cash transfers, by contrast, can depend on business income that relatives cannot observe and promises that must be sustained over time. Employment may consequently satisfy family claims in ways that a promise of future transfers does not. These considerations suggest why employers may be unable to separate the productive allocation of jobs from their obligations to support relatives.

This paper contributes to the economics of informal redistribution and the allocation of productive resources. Social networks provide insurance, but the obligations associated with these arrangements can also constrain economic choices (Townsend, 1994; Munshi and Rosenzweig, 2016). Existing work documents consequences for investment and labor supply (Jakiela and Ozier, 2016; Squires, 2018; Carranza et al., 2022). Alby et al. (2020) models inefficient family hiring as a response to redistributive obligations and provides observational comparisons of locally and foreign-owned firms; I experimentally isolate these obligations from other determinants of hiring and quantify the resulting constraint. I provide experimental evidence on this hiring constraint and measure its monetary value. The contribution goes beyond establishing that employers face claims on business income: these claims can affect which

[8]The baseline task differs from employment in its contract and work environment, so this comparison does not uniquely isolate effort. The output estimate is similar in an alternative design-adjusted specification ($p = .068$). These are average assignment effects, not estimates of the productivity loss specifically among pressure-induced hires.

worker produces that income, preventing employers from acting on anticipated differences in match profitability.

Macchi and Stalder (2023) show that employers and workers prefer redistribution through work to cash, even when the work has no productive value. Employment can therefore serve employers' redistributive objectives without raising profits. I show that family demands can instead conflict with employers' private preferences over whom to hire, imposing a tax on choosing outsiders and displacing matches they expect to be more profitable. I quantify this tax and provide independent experimental evidence that hiring relatives reduces output on a productive task. Comparable performance before employment suggests that these costs need not arise simply because relatives are less able workers. The signaling model explains how the employment relationship itself can generate a disadvantage that employers anticipate but do not avoid: family obligations can weaken the threat of dismissal while making the relative costly to reject. Redistributive pressure can thus constrain both worker selection and discipline within the firm, rather than merely influence the form in which employers choose to provide assistance.

The paper also contributes to understanding search and matching frictions in labor markets. Research on referrals shows that networks can convey information and support incentives, but can also induce choices that conflict with firms' productive interests (Beaman and Magruder, 2012; Pallais and Sands, 2016; Heath, 2018). I identify a constraint that can remain even when the employer has information about the available matches: recognizing a more profitable worker does not eliminate the social cost of rejecting a relative. This distinction between finding an attractive match and being able to implement it offers a complementary perspective on the uneven returns to job-search and matching interventions (McKenzie, 2017; Carranza and McKenzie, 2024). Improving information or reducing search costs may leave this constraint intact. The findings therefore connect labor-market matching to the redistributive obligations embedded in social networks, rather than treating networks solely as a technology for finding and screening workers.

Finally, the paper speaks to why production is organized around families. Research on family firms emphasizes both the contracting advantages of family control and the nonpecuniary value owners attach to family involvement (Burkart et al., 2003; Bertrand and Schoar, 2006). Related evidence shows that the productive consequences of workplace connections depend on managerial incentives (Bandiera et al., 2009). I distinguish these considerations from social constraints on the owner-manager's own decisions: even an employer who bears the profit consequences of a poor match may hire the relative because refusal is costly. At the same time, the framework accommodates productive kin benefits and worker accountability, so family employment need not be uniformly undesirable. The resulting distinction helps explain how valuable kin matches can coexist with anticipated but difficult-to-avoid employment problems. Family ties can help firms overcome some contracting frictions while creating

others within the employment relationship.

The remainder of the paper proceeds as follows. Section 2 describes the setting. Section 3 develops the conceptual framework. Section 4 describes the three experiments, and Section 5 explains the data and empirical strategy. Section 6 presents the results. Section 7 considers alternative explanations, Section 8 examines why redistribution operates through employment, and Section 9 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[9] 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.

3 Conceptual Framework

The model clarifies how family employment can reflect both productive advantages and constraints on worker selection. A relative may be particularly valuable in some jobs, for example because they can be trusted with money or firm assets. At the same time, refusing to hire

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

or subsequently dismissing a relative may damage the employer's reputation with the family. Workers face a countervailing reputational incentive: poor performance can signal that they are unwilling to contribute to the family. The model characterizes when the employer-reputation channel weakens discipline, when worker accountability offsets it, and how employers can knowingly enter an undesirable kin match because rejecting the relative is socially costly.

3.1 Environment and timing

An employer chooses between a named relative, R, and an unrelated worker, N, for a job with characteristics x. The relative provides a potentially productive, job-specific benefit B_x. This benefit is taken as given: the model accommodates productive advantages of kinship but does not microfound them. The employer has a loyalty type $a \in [0, 1]$, where a higher a represents a greater intrinsic willingness to support relatives. The relative has willingness type θ, where a higher θ represents a greater intrinsic willingness to contribute. Willingness is distinct from productive ability. Each actor knows their own type, while the family forms beliefs about both types from observed actions and outcomes.

The game has four stages. First, the employer chooses between R and N. The employer may receive an additional subsidy p for selecting the outsider. The hiring decision occurs under attribution regime $z \in \{P, D\}$, where P denotes public employer responsibility and D denotes plausible deniability. Second, if the relative is employed, the worker chooses effort $e \in \{0, 1\}$. Third, the employer and family observe a common performance signal $s \in \{g, b\}$ satisfying

$$\Pr(g \mid e = 1) = \Pr(b \mid e = 0) = \frac{1 + \rho}{2}, \qquad 0 \leq \rho < 1. \tag{1}$$

A larger ρ makes performance more informative about effort. Finally, the employer chooses whether to retain or dismiss the relative. The family observes the hiring decision, the performance signal, and the retention decision, and updates its beliefs about the employer's loyalty and the worker's willingness to contribute.

3.2 Worker selection

Let the employer's current profit from employing the relative be

$$\pi_R(e; x) = v_1 + B_x - w_1 + (q_1 - b)e, \tag{2}$$

where w_1 is the wage, b is an effort-contingent payment, and q_1 is the productive return to effort. The employer anticipates the relative's effort and the possibility of subsequent dismissal when making the hiring decision. Write the employer's expected value from hiring the relative

as

$$\mathcal{V}_R(a;x) = \mathbb{E}[\pi_R(e;x)] + \eta a + \delta \mathbb{E}_s \left[\max\{V_K(a,s), V_F(a,s)\}\right], \tag{3}$$

where ηa captures the employer's direct value from initially supporting the relative, and V_K and V_F are the continuation values from keeping and firing the relative.

The corresponding value from selecting the outsider is

$$\mathcal{V}_N(p,z) = \pi_N + p + \delta \Pi_N - C_z. \tag{4}$$

Here C_z is the social cost of refusing the relative. Plausible deniability reduces the extent to which the employer can be held personally responsible for this decision, so

$$0 \leq C_D < C_P. \tag{5}$$

The appendix provides an attribution foundation in which C_z depends on the family's posterior probability that the employer was responsible for selecting the outsider.[10]

Define the outsider's private advantage before the additional subsidy and the initial refusal cost as

$$v_x(a) = \pi_N + \delta \Pi_N - \mathcal{V}_R(a;x). \tag{6}$$

This is a private-utility comparison rather than a comparison of technological productivity alone: it includes the employer's genuine concern for the relative and anticipated post-hiring social consequences. The employer hires the relative if and only if

$$v_x(a) + p \leq C_z. \tag{7}$$

This condition distinguishes productive and constrained family employment. When $v_x(a) < 0$, the employer prefers the relative even in the absence of pressure. When

$$0 < v_x(a) \leq C_z, \tag{8}$$

the employer would privately prefer the outsider but hires the relative because refusal is costly. Reducing the refusal cost from C_P to C_D therefore changes hiring among employers satisfying

$$C_D < v_x(a) \leq C_P. \tag{9}$$

Holding anticipated employment values fixed, the effect of plausible deniability on relative

[10] The same loyalty type a affects the employer's willingness to hire and retain the relative, so hiring changes the family's prior about employer loyalty at the dismissal stage. The contemporaneous sanction for refusing the relative is summarized by C_z rather than being derived entirely from that posterior.

hiring is consequently

$$\Delta_H = -\Pr\{C_D < v_x(a) \leq C_P\} \leq 0. \tag{10}$$

The subsidy experiment also gives the hiring wedge a monetary interpretation. Let p_z^* denote the additional outsider subsidy that makes an employer indifferent between the candidates. Equation (7) implies

$$p_z^* = C_z - v_x(a), \qquad p_P^* - p_D^* = C_P - C_D. \tag{11}$$

Thus, under the assumption that plausible deniability does not itself change anticipated employment returns, the change in the reservation premium measures the component of the hiring wedge generated by public responsibility for rejecting the relative. It does not identify the total level of family pressure or, by itself, a welfare loss.

3.3 Employment incentives

After the relative is hired, family reputation affects both sides of the employment relationship. Let μ_{Ks} and μ_{Fs} denote the family's posterior expectation of employer loyalty following retention and dismissal. The employer's continuation payoffs can be written as

$$V_K(a, s) = \Pi_s + a + \omega\lambda\mu_{Ks}, \tag{12}$$

$$V_F(a, s) = \Pi_N + \omega\lambda\mu_{Fs} - \omega J(z_s), \tag{13}$$

where Π_s is expected continuation profit from the relative, Π_N is the value of replacement, and ω measures the employer's exposure to the family. The term $\lambda\mu$ captures the value of being regarded as loyal. The term $J(z_s)$ captures conflict generated by dismissing the relative, where

$$z_s = \Pr(e = 0 \mid R, s)$$

is the posterior probability that the worker did not exert effort. I assume that J is decreasing: evidence that the worker shirked makes dismissal easier to justify, although it need not eliminate its social cost.

The worker values continued employment and also cares about being regarded as willing to contribute. Ignoring terms that do not depend on effort, the worker's expected utility is

$$U_W(e, \theta) = (b + \theta - k)e + \delta R(\theta)\Pr(K \mid e) + \chi\mathbb{E}[m_{fs} \mid e], \tag{14}$$

where k is the cost of effort, $R(\theta)$ is the value of continued employment, and

$$m_{fs} = \mathbb{E}[\theta/k \mid R, s, f]$$

is the family's posterior assessment of the worker's willingness to contribute. The parameter χ measures how much the worker values this reputation.

In equilibrium, workers follow a cutoff strategy, exerting effort when $\theta \geq k - t$. Let

$$d(t) = \Pr(K \mid e = 1) - \Pr(K \mid e = 0),$$
$$\Gamma(t) = \mathbb{E}[m_{fs} \mid e = 1] - \mathbb{E}[m_{fs} \mid e = 0].$$

The worker exerts effort when

$$b + \theta - k + \delta R(\theta) d(t) + \chi \Gamma(t) \geq 0. \tag{15}$$

The two final terms capture distinct incentives. The employment term rewards effort when better performance increases the probability of retention. The reputation term rewards effort when it causes the family to regard the worker as more willing to contribute.

Consider an interior equilibrium in which a good report does not lead to dismissal but a bad report sometimes does. Under the regularity conditions stated in the appendix, greater employer exposure to the family makes dismissal more costly, reduces the employment consequence of poor performance, and lowers effort. A stronger worker concern for family reputation has the opposite effect. More precise performance evidence observed by both the employer and family also raises effort: it improves the employer's ability to identify low continuation returns, makes dismissal easier to justify, and increases the reputational return to worker effort. Family employment therefore need not reduce effort. Its effect depends on whether protection from employer discipline or accountability to the family is stronger.

3.4 Job context

The task-specific benefit B_x raises the employer's private return from the relative but does not enter the worker's effort condition in Equation (15). A relative can therefore be privately desirable in a job requiring trust even if kinship provides some protection from dismissal. Conversely, where productive benefits and worker accountability are weak, the employer may anticipate low effort and nevertheless hire the relative because C_z is large.

Job context also affects the observed hiring response. Writing the outsider advantage as $v_x(a) = v^0(a) - B_x$, plausible deniability changes hiring among employers satisfying

$$B_x + C_D < v^0(a) \leq B_x + C_P. \tag{16}$$

A productive kin benefit can move an employer into or out of this interval. Consequently, a small hiring response in a particular class of jobs does not, by itself, prove that pressure is absent or that every relative is desirable. It indicates that fewer employers lie on the margin

affected by responsibility for rejecting kin.

3.5 Predictions

The model does not predict that family employment is uniformly inefficient. Some relatives are privately attractive because they provide productive benefits or face strong accountability from the family. The model instead identifies conditions under which social pressure prevents employers from avoiding relatives they would prefer not to hire, and conditions under which the employment relationship subsequently reduces effort.

Prediction 1: Reducing responsibility for rejecting a relative changes worker selection. Suppose plausible deniability lowers the social cost of refusing the relative, so that $C_D < C_P$. Holding anticipated employment values fixed, relative hiring must weakly decline:

$$\Delta_H = -\Pr\{C_D < v_x(a) \leq C_P\} \leq 0. \tag{17}$$

The response comes from employers who privately prefer the outsider but select the relative when they can be held responsible for rejecting them. It should therefore be concentrated among employers for whom the outsider has a larger expected private return. Moreover, the additional outsider subsidy required to change the employer's choice satisfies

$$p_P^* - p_D^* = C_P - C_D. \tag{18}$$

The subsidy schedules therefore provide a monetary measure of the change in the hiring wedge generated by public responsibility.

Prediction 2: Employer reputation with the family can reduce relative-worker effort. Once a relative is employed, dismissing them can damage the employer's reputation for loyalty and generate conflict with the family. This protection weakens the relationship between performance and continued employment. In an interior equilibrium in which bad performance sometimes leads to dismissal, greater employer exposure to the family reduces the fraction of relative workers exerting effort:

$$\frac{\partial t}{\partial \omega} < 0. \tag{19}$$

When this protection channel dominates the worker's concern for their own reputation, a worker performs worse for a relative than they would in an otherwise comparable unrelated employment relationship. Forward-looking employers anticipate this effect. The model therefore predicts relationship-specific deterioration rather than necessarily lower underlying worker ability.

Prediction 3: Worker accountability and shared evidence can offset dismissal protection. Workers may also care about being perceived by the family as willing to contribute. Greater concern for this reputation increases effort:

$$\frac{\partial t}{\partial \chi} > 0. \tag{20}$$

More precise performance evidence observed by both the employer and family also increases effort under the conditions characterized in the appendix:

$$\frac{\partial t}{\partial \rho} > 0. \tag{21}$$

Clearer evidence raises the worker's reputational return to effort and makes poor performance easier for the employer to act upon. Family matches may therefore remain privately attractive where performance is difficult for workers to excuse or where kinship provides job-specific benefits such as trust over money or assets.

This final prediction concerns the incentives created by shared evidence, not employer-only monitoring. It also does not imply that the binary effect of plausible deniability must vary monotonically with any one job characteristic. Productive benefits, worker accountability, refusal costs, and the distribution of employers around the hiring margin can all affect the observed hiring response.

The three experiments directly test Prediction 1. The randomized rural worker assignment, the pre-employment productivity comparison, and employers' forecasts provide evidence consistent with Prediction 2. The heterogeneous hiring responses across jobs, particularly the suggestive relationship with the ease of excusing poor performance, provide evidence consistent with Prediction 3. Because job characteristics are not randomly assigned, these heterogeneous responses help interpret the mechanism but do not separately identify χ, ρ, or the productive kin benefit B_x.

4 Experiment

Common Experimental Structure

All three experiments subsidize a real employment relationship and vary whether the employer can be held personally responsible for selecting an outsider. The treatment is designed around the reported norm in this setting. Employers expect sanctions when a relative wants a job and the employer chooses someone else, but substantially less sanction when the employer is unable to make that choice. The experiments therefore change the explanation available for rejecting a relative rather than concealing who is hired.

Employers are randomly assigned to one of two attribution conditions. In the Public

Control condition, the public explanation states that the employer received a subsidy and was involved in selecting the worker. In the Plausible Deniability condition, the explanation additionally states that some employers received subsidies conditional on hiring a particular worker. An employer who selects an outsider can therefore point to a genuine program restriction rather than defend the decision as their unconstrained choice. The treatment does not provide information about either candidate's productivity and does not conceal the identity of the person hired.

The design does not use deception. The program actually includes take-it-or-leave-it offers under which the subsidy is available only for a particular worker. When such an offer is implemented, the public explanation is adjusted in both experimental conditions to describe the true restriction. Plausible deniability comes from uncertainty about which employers faced restricted offers, not from a false statement about any employer's subsidy.

Figure 2 summarizes the three experiments and their sample flows.

Urban Field Experiment

The Urban Field experiment follows the wage-subsidy protocol of De Mel et al. (2019). Firms are offered a subsidy covering part of the cost of a full-time employee for three months. Before identifying the worker they would hire, firms are assigned either the Control Poster or the Plausible Deniability Poster. The posters are displayed at the business if the subsidy is implemented. No reference to family is made when the treatment is explained. The primary outcome is whether the worker selected by the employer is related to the employer. Of 254 firms randomized, hiring choices are observed for 252; 14 firms are assigned subsidy implementation.

Urban Mechanism Experiment

The Urban Mechanism experiment fixes the candidate set. Employers first identify one relative and one nonrelative whom they would be willing to hire. At the second visit, employers are assigned a poster condition and choose between the two named candidates under a sequence of candidate-specific subsidy offers. The equal-subsidy choice provides the hiring outcome used across experiments. The full sequence identifies the interval containing the additional outsider subsidy required to change the employer's choice. Among 684 eligible employers, 619 complete the equal-subsidy choice and 617 provide complete, monotone subsidy schedules. Forty-eight firms are assigned subsidy implementation.

After choices are elicited, the program determines whether the employer receives a subsidy for the relative, for the nonrelative, or for the candidate selected under one randomly chosen subsidy pair. This randomization supplies the genuine restricted offers referenced by the posters while keeping the employer's elicited choices consequential.

Rural Mechanism Experiment

The Rural Mechanism experiment applies the same logic to short agricultural contracts. Each employer identifies one relative and one nonrelative willing to perform maize shelling. Both candidates attend the experimental visit and are told that the employer may receive a subsidy conditional on hiring either candidate or may receive the candidate chosen by the employer. The attribution condition is communicated verbally rather than through a business poster. Employers then make the equal-subsidy choice and complete the subsidy schedule.

All 322 employers subsequently enter a separate assignment stage. The program assigns 135 employers the relative, 160 the nonrelative, and implements the elicited choice for 27. The first two arms form the randomized productivity comparison. This second randomization is distinct from the attribution treatment: the attribution treatment changes whom the employer wants to hire, while worker assignment determines which candidate actually works. The design therefore separates pressure at the hiring margin from the productive consequences of related employment.

Treatment Interpretation

The treatment changes responsibility for worker selection. It does not make the hire private, change the candidates available to the employer, or reveal new information about their ability. Differences in hiring therefore identify the effect of relaxing the social constraint on rejecting a relative. The Urban Field experiment provides a less structured hiring decision; the Urban Mechanism experiment measures the monetary hiring wedge and expected-profit sorting for named candidates; and the Rural Mechanism experiment adds independent worker assignment and direct measurement of output, quality, and wages.

Screening and Candidate Selection

The urban samples are drawn from market censuses. Eligible respondents own the business or can make hiring decisions, are willing to add a worker under a partial wage subsidy, and can identify an adult relative living nearby. The Urban Field experiment additionally requires willingness to hire a local jobseeker identified by the project. The Urban Mechanism experiment instead asks each employer to nominate one relative and one nonrelative whom they would be willing to hire. The rural sample is drawn from villages near Chipata. Eligible employers have maize to shell, expect to hire for shelling, and can identify an adult relative in the village. Relative candidates cannot be members of the employer's household; this restriction limits the risk of coercive hiring.

Subsidy Implementation

In the urban experiments, the subsidy covers approximately 60% of the cost of a full-time worker for three months, a magnitude comparable to other hiring-subsidy experiments (Alfonsi et al., 2020; De Mel et al., 2019). Employers set the wage and other employment terms. If the subsidy is implemented, enumerators verify the worker's identity and make unannounced visits throughout the subsidy period. Firms receive about 15 audits on average, and continued payment requires the worker to be present. Approximately six weeks after employment begins, the worker is interviewed privately about wages paid. The implementation lottery selects about 7% of the urban employers.

In the rural experiment, the subsidy covers approximately 60% of the prevailing half-day wage for two days of maize shelling and is implemented for every employer. Enumerators arrive before work begins, verify that no maize has already been shelled, and return at the end of the work period to record attendance, output, quality, and wages. Subsidy payment requires the assigned worker to complete the contracted work period.

Randomization, Spillovers, and Outcomes

The attribution treatment is randomized at the employer level in every experiment. The Urban Mechanism randomization is stratified by market, owner gender, and whether the firm has a noncasual employee; the Urban Field and Rural Mechanism experiments use simple individual randomization. In the mechanism experiments, a separate lottery determines whether the subsidy is restricted to the relative, restricted to the nonrelative, or implements the employer's choice. The urban implementation lotteries separately determine whether a subsidy is funded. Appendix Tables 4, 5, and 6 report balance.

Several protocol features limit spillovers and preserve the credibility of the public explanation. The sampling procedure leaves space between surveyed businesses, and interviews within a market or village are grouped closely in time. The urban posters use official project logos, are laminated, list field-manager telephone numbers, and are kept on file so that their contents can be verified. Control firms are encouraged to display their posters, making it less plausible that the treatment works simply by drawing more attention to the hiring decision.

The primary outcome in all three experiments is whether the employer selects the relative. The Urban and Rural Mechanism experiments also elicit choices across candidate-specific subsidy amounts, which identify the outsider reservation premium. Pre-choice beliefs record which worker the employer expects to be more profitable and whether the relative is expected to work differently for a family employer. The rural experiment adds realized attendance, output, output per hour, defects, and wages. Both rural candidates also complete the same researcher-paid shelling task before employment assignment.

5 Data and Empirical Strategy

The analysis combines employers' hiring choices, choices over candidate-specific subsidy amounts, pre-choice beliefs about the two candidates, coded descriptions of the proposed job, and realized employment outcomes. The hiring outcome is an indicator for selecting the related worker. In the Urban and Rural Mechanism experiments, the subsidy schedules additionally identify the interval containing the outsider reservation premium. I code the schedule using interval midpoints and the observed endpoints for censored choices, restricting the analysis to complete, monotone schedules.

The main hiring specification is estimated separately by experiment:

$$H_i = \alpha + \beta D_i + X_i'\gamma + \varepsilon_i, \tag{22}$$

where H_i indicates that employer i selects the relative and D_i indicates assignment to Plausible Deniability. In the Urban Field experiment, X_i contains location controls and industry fixed effects, and standard errors are clustered by employer. In the Urban Mechanism experiment, X_i contains the employee-count randomization stratum, owner gender, and location controls. The Rural Mechanism estimate is unadjusted. The same specifications are used for the outsider reservation premium and the indicator that this premium is positive. The coefficient β measures the effect of reducing employer responsibility for rejecting the relative; interpreting the premium difference entirely as a change in refusal costs additionally requires the continuation-value exclusion described in Section 3.

For the rural productivity analysis, I restrict attention to employers randomly assigned either the relative or the outsider, excluding the arm in which the employer's choice was implemented. I estimate employer-level specifications of the form

$$Y_i = \alpha + \tau R_i + \delta_{s(i)} + X_i'\gamma + \varepsilon_i, \tag{23}$$

where R_i indicates assignment of the related worker and $\delta_{s(i)}$ denotes subsidy-amount fixed effects. The preferred specification also controls for prior worker-employer experience, perceived sharing sanctions, the employer's stock of unshelled maize, and prior maize-shelling hiring. Average daily output is measured at the employer level and codes non-arrival as zero. Output per hour, quality, and wages are conditional on worker arrival. Standard errors are clustered by employer. I compare the two candidates' separately incentivized baseline performance within employer and report specifications that additionally control for both candidates' baseline output and quality.

To investigate heterogeneity in the hiring response, I combine pre-treatment owner and firm characteristics, direct measures of the candidate relationship, and coded job content. Job titles and open-ended task descriptions are classified using an auditable dictionary fixed with-

out reference to outcomes. The preferred representation reduces 42 inputs to 27 dimensions without using treatment outcomes. An elastic-net model predicts treatment-effect heterogeneity using repeated sample splitting: preprocessing, dimension reduction, and estimation are repeated in each training sample, and GATES are estimated in the held-out sample. I use the learned groups to motivate a smaller set of joint regressions comparing employer and worker type with job context. These comparisons characterize heterogeneity; because the job characteristics are not randomly assigned, they do not identify causal effects of money responsibility or the ease of excusing poor performance.

Figure 1 reports the descriptive facts that motivate the analysis. Related employment is common, but employers anticipate substantial variation in how relatives perform and in how easily they can be disciplined. Appendix Tables 4, 5, and 6 report experimental balance, while Appendix Table 13 compares eligible and ineligible Urban Mechanism firms.

6 Results

The results proceed in four steps. I first test whether reducing responsibility for rejecting a relative changes worker selection. I then use the subsidy schedules to measure the size of the resulting hiring wedge. Third, I estimate how the response varies across employers, workers, and jobs. Finally, I use randomized worker assignment to measure the productive consequences of family employment and distinguish lower worker ability from a disadvantage specific to the kin match.

6.1 Family pressure changes whom employers hire

Figure 3 and Panel A of Table 1 report the effect of Plausible Deniability on relative hiring at equal subsidies. The treatment reduces relative hiring in all three experiments. In the Urban Field experiment, relative hiring falls by 12.1 percentage points ($p = .054$, $N = 252$) from a control mean of 58.2%. In the Urban Mechanism experiment, it falls by 15.2 points ($p < .001$, $N = 619$) from a control mean of 62.7%. In the Rural Mechanism experiment, it falls by 19.7 points ($p < .001$, $N = 322$) from a control mean of 78.0%. These estimates correspond to reductions of 20.9%, 24.2%, and 25.3% relative to their control means. The common result is that employers choose fewer relatives when they have a credible external explanation for selecting an outsider.

The treatment changes responsibility for the hiring decision without providing new information about either candidate. The hiring response therefore cannot be explained by employers learning that outsiders are more productive. Instead, it shows that some employers select a relative when the decision can be attributed to them, but select an outsider when the program can plausibly be responsible. This is the first prediction of the model: lowering the

social cost of refusing a relative changes the allocation of workers.

Figure 4 examines whether these changes occur where both components of the proposed constraint are present. I classify employers by whether they report family sanctions for refusing assistance and whether they expect the relative to perform worse for them than for another employer. Among employers reporting both high pressure and an expected bad match, Plausible Deniability reduces relative hiring by 37.6 percentage points ($p < .001$). The corresponding effect is 5.3 points among high-pressure employers who expect a good kin match ($p = .120$) and 12.9 points among low-pressure employers who expect a bad match ($p = .024$). The high-pressure, bad-match effect differs from these adjacent cells by 32.3 and 24.7 points, respectively ($p < .001$ and $p = .002$). Pressure is therefore most consequential when employers expect the relative to be a poor employee for them.

The treatment also moves choices toward employers' stated profitability rankings. Figure 5 separates employers according to which candidate they expect to generate greater profits. Among employers who expect the outsider to be more profitable, 57.7% select the outsider under public attribution, compared with 92.1% under Plausible Deniability. The adjusted treatment effect is 32.8 percentage points ($p < .001$). The corresponding response is 6.8 points among employers who expect the relative to be more profitable. Expected profits need not capture every component of the employer's private utility, but the result establishes that pressure can prevent employers from implementing their own ranking of the available matches.

6.2 The monetary value of the hiring constraint

The equal-subsidy choices establish that pressure changes worker selection. The subsidy schedules measure how much compensation employers require before selecting the outsider. For each employer, I identify the outsider reservation premium: the additional subsidy at which the employer switches from the relative to the outsider. This premium combines the employer's valuation of the two matches with the social cost of rejecting kin. Random assignment to Plausible Deniability holds the candidates fixed while changing responsibility for refusing the relative. The difference between conditions therefore measures the component of the hiring wedge generated by public attribution.

Panel B of Table 1 reports the estimates. In the Urban Mechanism experiment, Plausible Deniability lowers the mean outsider reservation premium by ZMW 49.4 ($p < .001$, $N = 617$). This is approximately 4.9% of median monthly firm profits. It also reduces the share of employers requiring a positive premium by 14.8 percentage points ($p < .001$). In the Rural Mechanism experiment, the corresponding reduction is ZMW 9.8 ($p < .001$, $N = 318$), and the share requiring a positive premium falls by 20.0 points ($p < .001$). Figure 6 displays the underlying subsidy schedules.

These estimates give the hiring constraint a price-equivalent interpretation. Public re-

sponsibility acts as an implicit tax on hiring an outsider: the outsider must offer a sufficiently large private advantage to compensate the employer for rejecting the relative. The experiment identifies the portion of this wedge removed by Plausible Deniability. It does not identify the full level of family pressure or convert the wedge mechanically into a welfare loss.

6.3 Heterogeneity in the desirability of kin matches

The average hiring effect combines employers for whom the relative is privately desirable with employers who select the relative because refusal is costly. I use a data-driven procedure to examine this heterogeneity without choosing subgroups based on the realized treatment effects. I first harmonize job titles and employers' descriptions of the work, code the tasks performed in each job, and combine these measures with pre-treatment employer, worker, and match characteristics. The preferred specification reduces 42 inputs to 27 dimensions without using the outcome. I then estimate treatment effects using repeated sample splitting: each split re-estimates preprocessing and the prediction model in a training sample and evaluates treatment effects in held-out observations. Figure 7 reports GATES estimates across terciles of the predicted response (Chernozhukov et al., 2025; Bryan et al., 2024).

The point estimates show economically meaningful variation even among the 443 employers who report family pressure. Plausible Deniability reduces relative hiring by approximately 30.0 percentage points in the most responsive tercile, 21.6 points in the middle tercile, and 5.2 points in the least responsive tercile. The split-aggregated difference between the extreme terciles is 26.0 points ($p = .091$). The BLP and omnibus tests are less precise ($p = .114$ and $p = .272$), so I use the exercise to characterize the sources of heterogeneity rather than to claim that the distribution of treatment effects is known exactly. A small response also does not by itself identify a desirable relative: both match values and refusal costs determine whether an employer lies near the switching margin.

I next use the learned groups to discipline a smaller set of type and job-context comparisons. Table 2 reports the clearest contextual patterns in the pressure-reporting sample. The response to Plausible Deniability is 47.0 percentage points smaller in jobs where the worker is responsible for money, after accounting for general preferences for kin, perceived worker ability outside the family match, job trust, and the remaining employer and job characteristics (Holm-adjusted $p = .031$). This is consistent with relatives providing a productive advantage where misuse of money or assets is costly.

The scope for explaining poor performance provides a second, more suggestive pattern. A one-standard-deviation increase in the difficulty of excusing poor performance is associated with a 9.3-point smaller response in the specification adding the information measures ($p = .027$). The estimate falls to 7.2 points when all twelve treatment-characteristic interactions enter jointly ($p = .101$). One interpretation follows the worker-accountability channel in the

model: when poor performance is difficult to explain away, the family can infer more from it and hold the worker responsible. This evidence is not a randomized test of job context, and direct employer monitoring has the opposite sign. The stronger conclusion is that kinship does not have a constant value across jobs. Trust can make some relatives valuable, while the ability to excuse poor performance can weaken accountability in others.

6.4 The productive consequences of family employment

The hiring experiments show that some employers prefer to avoid a relative, but they do not by themselves measure the return to doing so. In the Rural Mechanism experiment, I separately randomize which candidate receives the subsidized contract. Among the 295 employers assigned either the relative or the outsider, 135 are assigned the relative and 160 the outsider. This comparison holds the employer's hiring choice fixed and identifies the effect of assigning a related rather than unrelated worker to the employer.

Table 3 reports the results. Assigning the relative reduces average daily output by 26.78 kilograms ($p = .049$), or 6.9% of the nonrelative mean. Output per hour falls by 5.04 kilograms ($p = .030$). Relative assignment also increases the number of defect types introduced during production by 0.08 ($p = .042$), although the 4.94-point reduction in defect-free output is less precise ($p = .136$). These losses are not offset by lower pay: the wage difference is ZMW 0.03 ($p = .983$). The observed burden therefore appears in lower output and quality rather than a reduction in the relative's wage.

The remaining question is whether this difference reflects worker type or the employment relationship. Before assignment, both candidates complete the same incentivized maize-shelling task for the research team. Figure 8 shows nearly identical baseline distributions. Relatives produce 63.01 kilograms and nonrelatives 63.03 kilograms on average; the difference is -0.01 kilograms ($p = .990$). The share producing no input-adjusted defects is also similar, 88.5% among relatives and 88.8% among nonrelatives ($p = .862$). Specifications controlling for both candidates' baseline performance produce a similar employment penalty. The output gap therefore emerges when the worker is employed by the relative rather than in measured productive capacity before employment.

Employers anticipate where this deterioration will occur. Table 4 compares the randomized relative-assignment effect according to employers' pre-assignment forecasts. Relative assignment reduces output per hour by approximately 2.6% when the employer does not expect the relative to work more slowly for them ($p = .455$), compared with 15.4% when the employer anticipates slower work ($p = .002$). The formal difference between these effects is 13.2 percentage points ($p = .035$). Employers are therefore not simply surprised by lower relative performance. They anticipate which kin relationships are likely to deteriorate, yet the hiring experiments show that pressure can prevent them from avoiding those matches.

Appendix Table 3 examines naturally occurring maize-shelling contracts using worker and employer fixed effects. Related workers produce approximately 5.4% less output, although the estimate is imprecise ($p = .259$), and several measures of quality and contract enforcement also point in an adverse direction. These comparisons are observational and do not reproduce the experimental assignment. They provide secondary evidence that the randomized result is not unique to the subsidized contracts.

7 Alternative Explanations

This section considers four threats to the interpretation of the results: that employers did not treat the choices as consequential, that the nominated workers were selected for the experiment rather than for actual jobs, that Plausible Deniability changed a valued signaling benefit rather than a social cost, and that the experimental samples are unusual.

Consequential Choices

The hiring choices were implemented probabilistically, but the implemented choices generated real employment relationships. Among employers selected to receive an urban subsidy, all but one hired the worker assigned or chosen through the experiment, and all but two completed the subsidy period. Firms were audited about 15 times on average, and the worker was present in 98% of visits. Appendix Figure A6 also shows that the hiring response among employers selected for implementation is similar to the response in the full sample.

The urban subsidy could still become a low-stakes choice if employers retained it by reducing the worker's pay. I examine this directly using private worker interviews conducted approximately two months after employment began. In 83% of cases, workers report receiving the wage the employer had stated; 10% report less and 5% report more. The rural design leaves less scope for this behavior because enumerators observe the worker and the payment at the end of each work period. The evidence is inconsistent with employers treating the subsidy choices as costless statements of preference.

Candidate Selection

The mechanism experiments require employers to nominate one relative and one nonrelative. The estimates would be less informative about ordinary hiring if employers supplied artificial candidates for the experiment. Several checks argue against this interpretation. Two-thirds of employers describe the nominated relative as their best available relative for the job, and another 24% rank that person second. In follow-up surveys of urban firms not selected for a subsidy, 21% hire a worker within three months. Among those hires, 15.7% of firms hire a relative and 6.1% hire a nonrelative. Roughly 55% of the new relative and nonrelative

permanent hires are the workers nominated for the experiment. Appendix Figures A9 and A8 report the follow-up patterns.

The nominated outsiders are also people the employers already know. Employers have known them for six years on average; 85% report seeing them at least weekly, and 30% have employed them before. In the rural sample, locating the outsider takes only three minutes longer on average than locating the relative. The response therefore does not appear to depend on the program conducting an otherwise prohibitive search for an outsider.

The protocol excludes members of the employer's household to reduce the risk of coercive hiring. One-quarter of employers say that they would have nominated a different relative if household members were eligible. Appendix Figure A5 shows that the treatment effect remains sizable among the other three-quarters, for whom the restriction did not exclude a preferred household candidate.

What Does Attribution Change?

Plausible Deniability may remove a valued benefit from publicly choosing a relative rather than reduce a penalty for rejecting one. A separate incentive-compatible choice in the rural experiment weighs against this interpretation: almost 90% of employers prefer not to have their hiring choice disclosed. The qualitative interviews also emphasize family sanctions and disagreement about the relative's likely effort, rather than a benefit from advertising the relative's ability. These facts support the interpretation that personal attribution is costly to the employer.

A related possibility is that rejecting a relative reveals negative information about the worker to other employers. I cannot rule out this channel completely. It is less natural in the Urban Field experiment, where the poster concerns the program's hiring restriction rather than a direct announcement that a named relative was rejected, yet the treatment produces a similar hiring response. More generally, the design establishes that attribution changes worker selection. The evidence on reported sanctions, expected match deterioration, and family beliefs indicates that the relevant audience is the family and that the central concern is the employer's willingness to help.

The control condition could heighten scrutiny relative to an ordinary hiring decision. This concern affects the magnitude rather than the existence of the hiring response. The Urban Field design does not place a relative and outsider side by side, yet produces a similar proportional reduction in kin hiring. Moreover, requests for work are common outside the experiment: about 40% of employers report having hired a worker after that person approached them for a job. Because social pressure is often strongest following a direct request (Lazear et al., 2012; DellaVigna et al., 2012), removing requests from the protocol can also attenuate the pressure employers ordinarily face.

Sample Selection

Every experiment screens employers for the ability to use a hiring subsidy and identify eligible workers. Appendix Table 13 compares eligible and ineligible firms in the Urban Mechanism sampling frame. Ineligible firms are smaller and less well managed, but they do not report weaker family pressure. More than 60% expect sanctions for refusing a family request, and they report both high relative employment and substantial concern about relative-worker effort. The experiment should therefore not be read as representative of every firm, but the screening procedure does not appear to select the sample toward unusually severe family pressure.

8 What Generates Pressure to Hire?

Why does redistributive pressure operate through jobs rather than cash? If an employer could hire the most profitable worker and compensate relatives through transfers, family claims need not distort worker selection. The interviews and survey responses point to two reasons why this separation is difficult.

First, employers and their families often disagree about the relative's productivity in the family firm. Figure 9 classifies employers according to their own forecast of the relative's effort and their report of what the family believes. In the largest disagreement cell, containing 298 of 619 employers, the employer expects the relative to work less hard for them but believes that the family does not expect this deterioration. Plausible Deniability reduces relative hiring by 31.3 percentage points in this group ($p < .001$). When the employer and family agree that the relative will not work less hard, the treatment effect is 4.8 points ($p = .118$). If the family believes that kinship should improve rather than weaken effort, rejecting the relative is difficult to defend as an efficiency choice. It instead appears to reflect an unwillingness to help.

Second, a job can provide a more credible transfer than a promise of cash. Employment creates a visible and recurring flow of wages tied to attendance, with the firm as the payer. Business income is less transparent to relatives, and a promise to make transfers from future cash flow can be renegotiated or abandoned. Employment therefore bundles redistribution with a commitment technology. These features help explain why pressure targets the allocation of jobs even when the employer would prefer to separate production from redistribution.

9 Conclusion

Family employment reflects two forces. Kinship can make workers valuable where trust and accountability matter, but it can also make poor matches difficult to avoid and difficult to

discipline. The experiments separate these forces by changing responsibility for rejecting a relative while holding the available candidates fixed. Plausible Deniability moves hiring away from kin, reduces the monetary premium required to select an outsider, and moves choices toward workers whom employers expect to be more profitable. The heterogeneous response across jobs is consistent with some relatives remaining privately attractive rather than with a uniform cost of family employment.

The rural assignment experiment shows why the constrained choices matter for production. Relatives produce approximately 7% less output at essentially unchanged wages, despite performing similarly to outsiders before employment. The penalty is largest where employers anticipated that the relative would work more slowly for them. The evidence therefore points to a disadvantage generated within the employment relationship rather than simply the selection of less able relatives.

The central implication is that information about workers is not sufficient for efficient matching when employers face social costs from acting on it. Family networks may help firms find trustworthy workers and sustain informal contracts, yet the same networks can tax the decision to hire outside the family and weaken discipline after a relative is hired. Redistributive pressure can therefore misallocate workers even when employers correctly anticipate differences in match quality.

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10 Figures

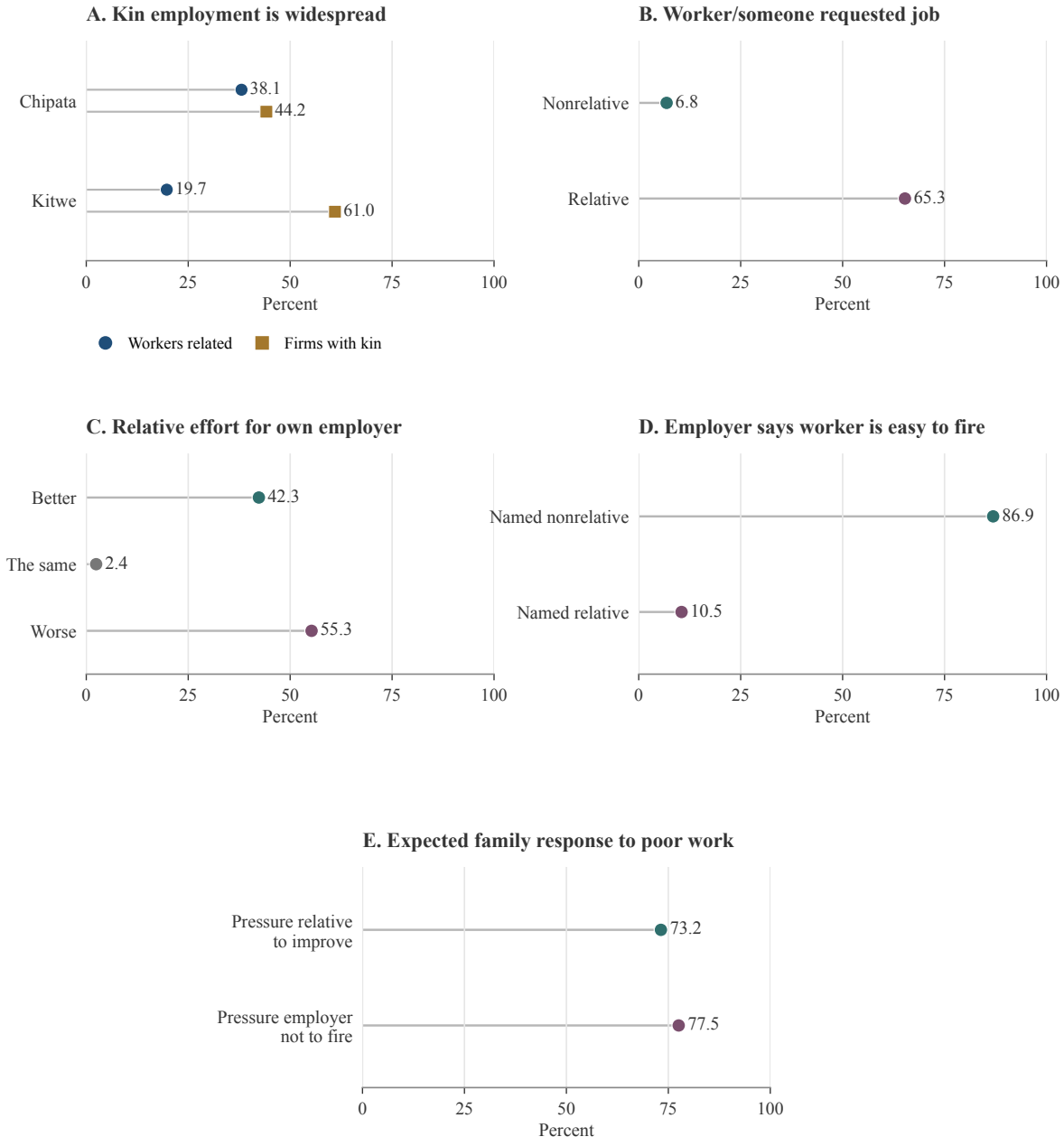


Figure 1: The Dual Role of Kinship in Employment

Notes: All panels report descriptive means rather than causal estimates. Panel A reports the prevalence of related employment in Chipata and Kitwe. Panel B compares whether related and unrelated workers obtained employment after they or someone else asked the employer for a job. Panels C–E use the 619 Urban Mechanism employers and report expected relative-worker effort, the ease of firing the named relative and nonrelative, and anticipated family pressure on the worker and employer. The two city samples have different sampling frames.

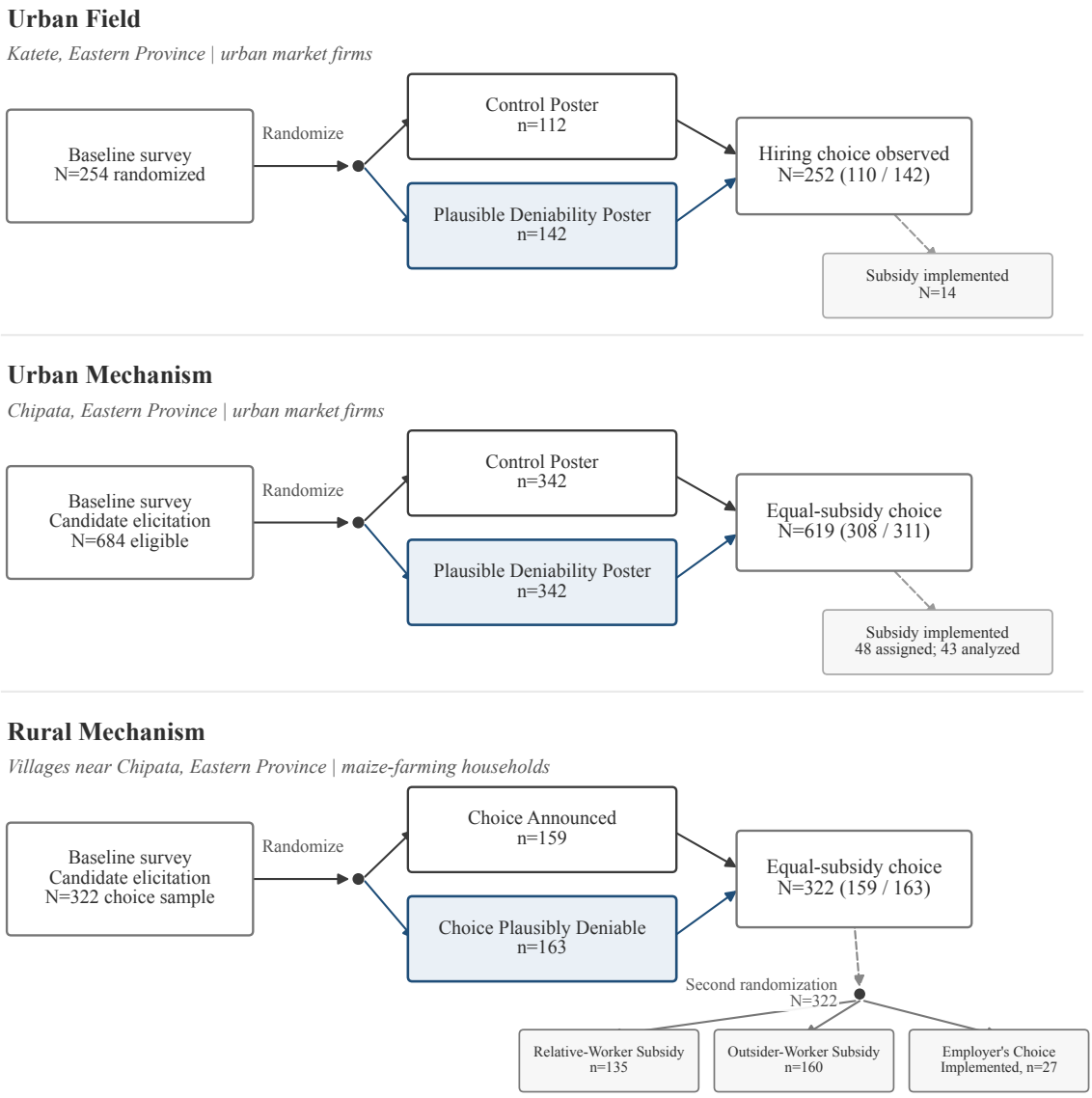


Figure 2: Experimental Designs and Sample Flow

Notes: The figure summarizes the three field experiments and reports observed sample counts. The Urban Field equal-subsidy comparison uses 252 firms. The Urban Mechanism equal-subsidy comparison uses 619 firms, of whom 617 have complete, monotone subsidy schedules. The Rural Mechanism equal-subsidy comparison uses 322 employers. Its second-stage productivity comparison contains 135 employers assigned the relative and 160 assigned the outsider; 27 additional employers had their elicited choice implemented.

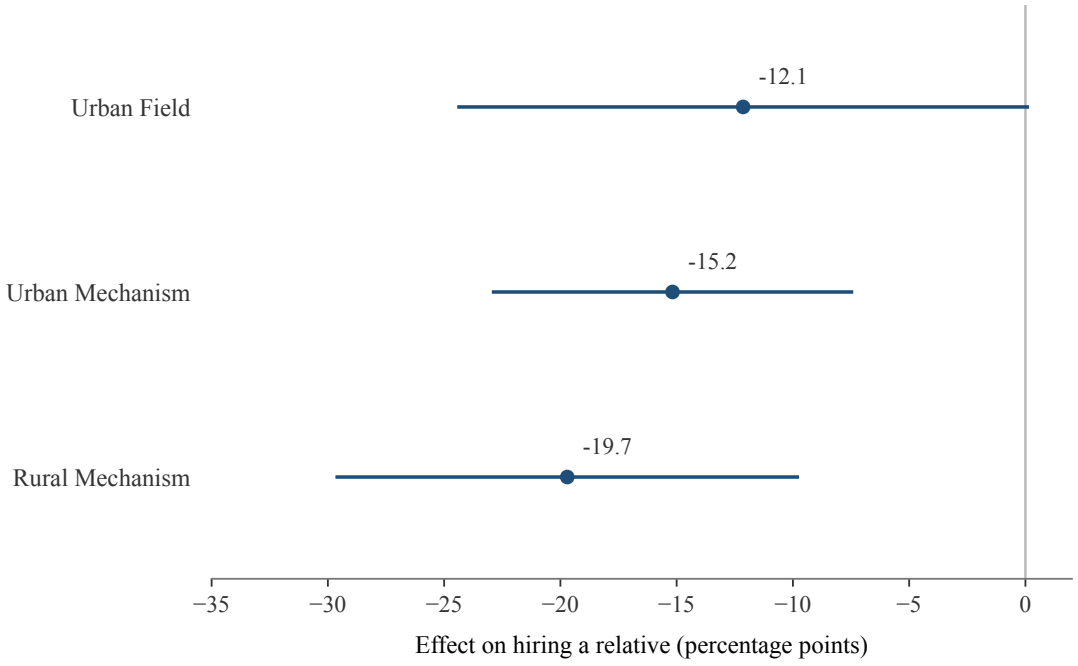


Figure 3: Effects of Plausible Deniability on Relative Hiring

Notes: Points report experiment-specific regression estimates of the effect of Plausible Deniability on an indicator for hiring a related worker; horizontal bars show 95 percent confidence intervals. The Urban Field specification includes location controls and industry fixed effects and clusters standard errors by employer. The Urban Mechanism specification includes the employee-count randomization stratum, owner gender, and location controls. The Rural Mechanism estimate is unadjusted. Control means are 0.582, 0.627, and 0.780, and sample sizes are 252, 619, and 322.

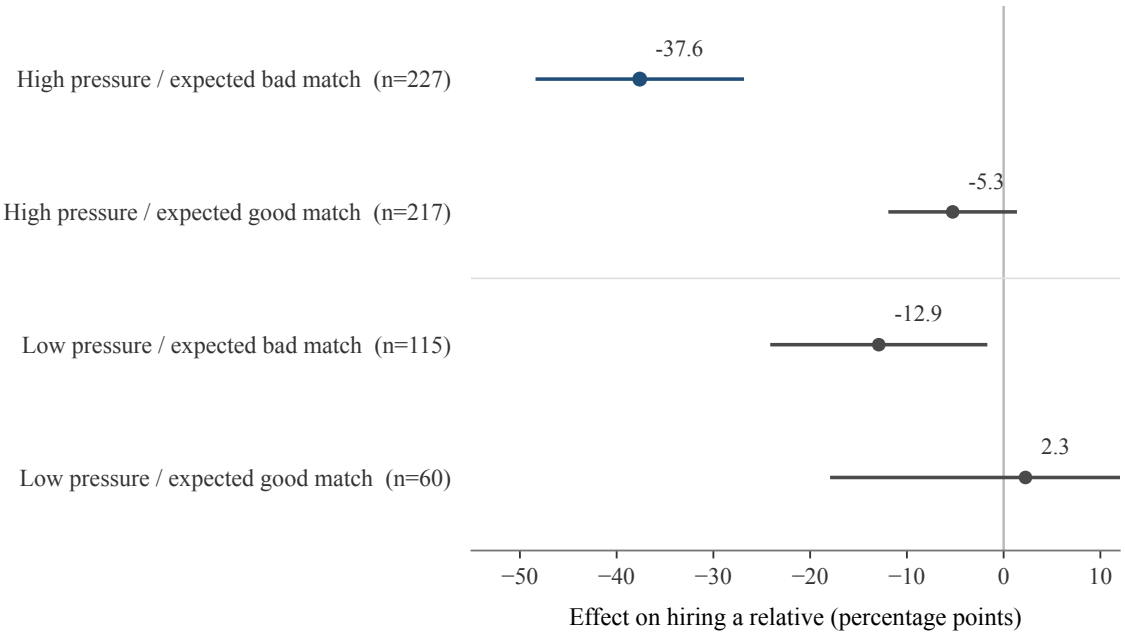


Figure 4: Family Pressure and Expected Match Quality

Notes: Points report effects of Plausible Deniability on relative hiring within four pre-treatment pressure-by-expected-match cells; horizontal bars show heteroskedasticity-robust 95 percent confidence intervals. High pressure means that the employer reports that refusing to help a relative can cause family sanctions. An expected bad match means that the employer expects the relative to work worse for them than for someone else. The sample contains 619 Urban Mechanism employers. The fully interacted regression includes the employee-count randomization stratum, owner gender, and location controls.

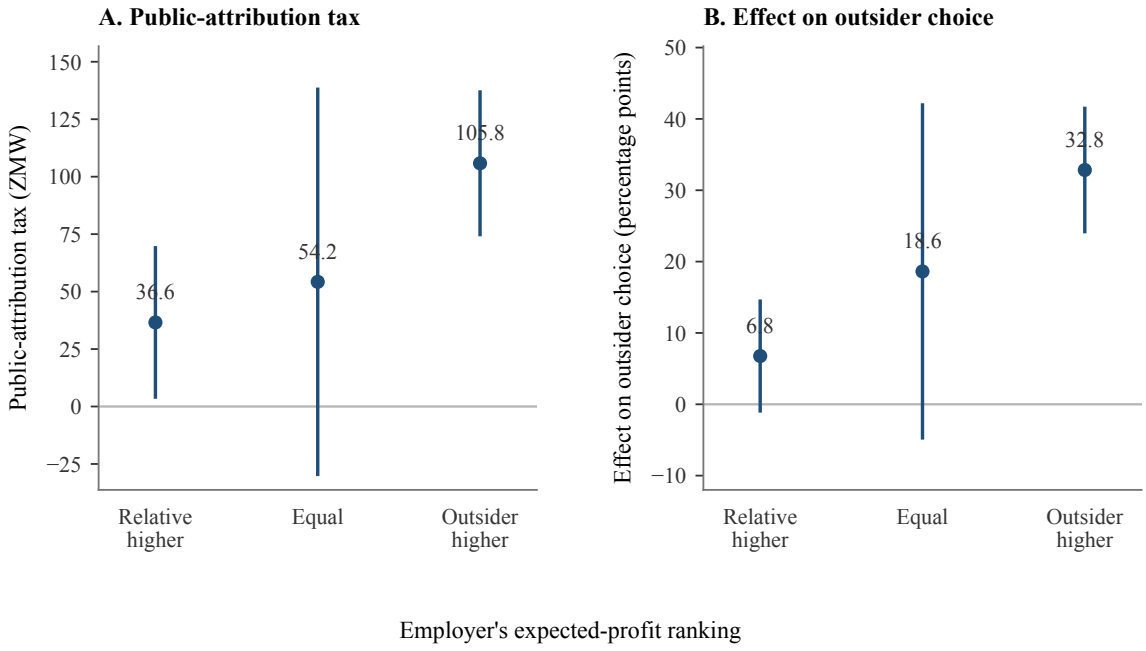


Figure 5: The Hiring Response and Employers' Expected Returns

Notes: The sample contains 617 Urban Mechanism employers with complete, monotone subsidy schedules. Employers are grouped by their pre-choice ranking of the candidates' expected profits. Panel A reports the public-attribution tax, defined as the Control Poster reservation premium minus the Plausible Deniability reservation premium. Panel B reports the effect of Plausible Deniability on outsider hiring. Regressions include the employee-count randomization stratum, owner gender, and location controls. Bars show heteroskedasticity-robust 95 percent confidence intervals. Expected profit is elicited before the hiring choice and is not realized productivity.

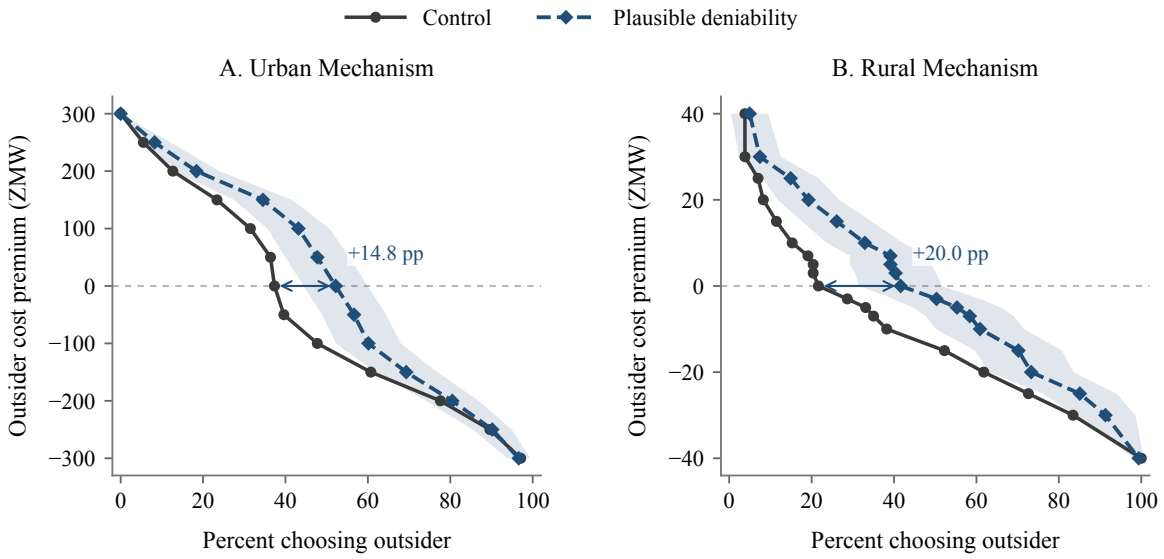


Figure 6: Demand for Outsider Workers

Notes: Each panel plots an inverse demand curve with the share of employers choosing the outsider on the horizontal axis. The vertical axis is the outsider cost premium, defined as the relative-worker subsidy minus the outsider-worker subsidy; the horizontal line marks equal subsidies. The shaded region is the pointwise 95 percent confidence interval for the Plausible Deniability minus Control contrast, anchored at the control curve. The Urban Mechanism estimates use 617 employers with complete, monotone schedules; the Rural Mechanism estimates use 318.

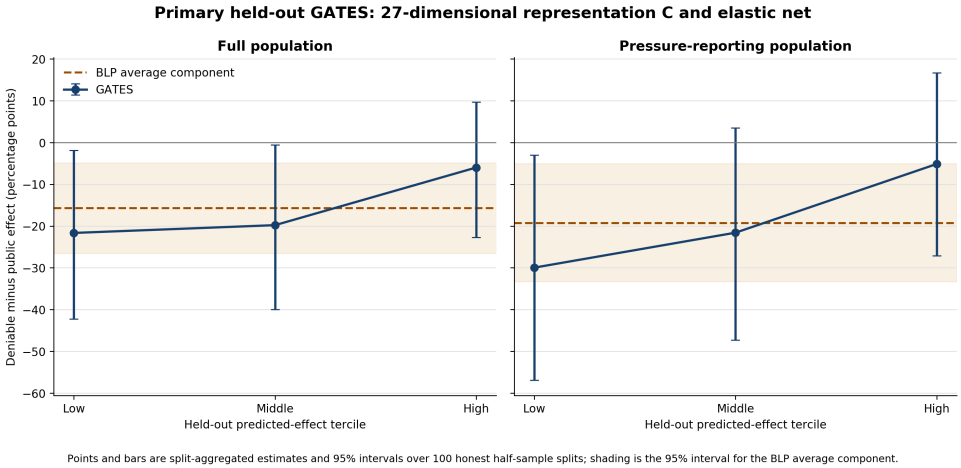


Figure 7: Data-Driven Heterogeneity in the Effect of Plausible Deniability

Notes: The figure reports GATES estimates for the 443 Urban Mechanism employers who report family pressure. Employers are grouped into terciles using treatment effects predicted out of sample. The preferred representation reduces 42 pre-treatment employer, worker, match, and job inputs to 27 dimensions without using outcomes. Preprocessing and the prediction model are re-estimated within each training sample over 100 repeated sample splits. Bars show split-adjusted 95 percent confidence intervals. The dashed line and shaded region report the average component and its 95 percent confidence interval.

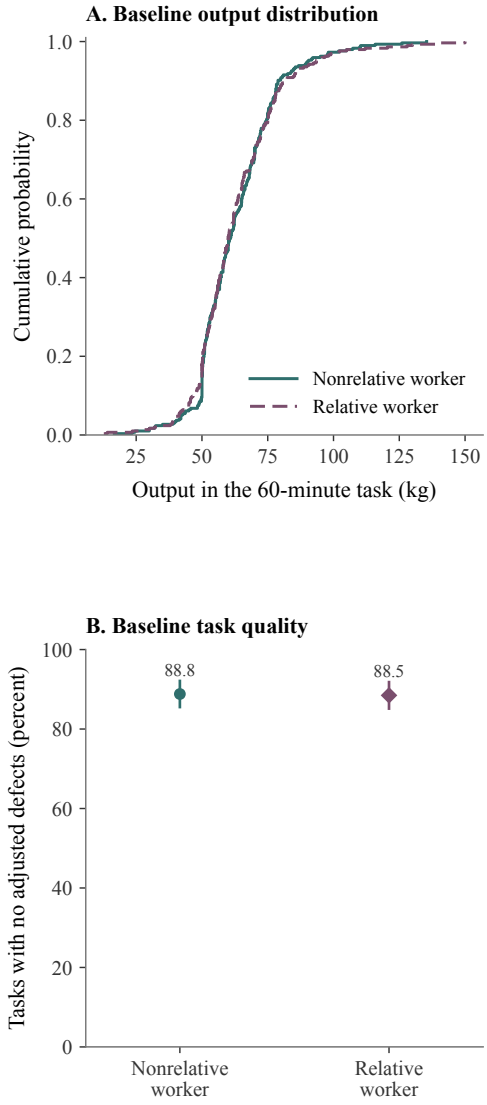


Figure 8: Baseline Productivity of Relative and Nonrelative Workers

Notes: The figure compares the two workers named by each of 295 Rural Mechanism employers in a researcher-paid, 60-minute baseline task conducted before employment assignment. Panel A plots empirical cumulative distribution functions of output. Panel B reports the share of tasks with no newly introduced defects; vertical bars show 95 percent confidence intervals with standard errors clustered by employer. The sample contains 590 candidate-task observations. Mean output is 63.03 kg for nonrelatives and 63.01 kg for relatives ($p = .990$). Adjusted defect-free shares are 88.81 and 88.47 percent ($p = .862$).

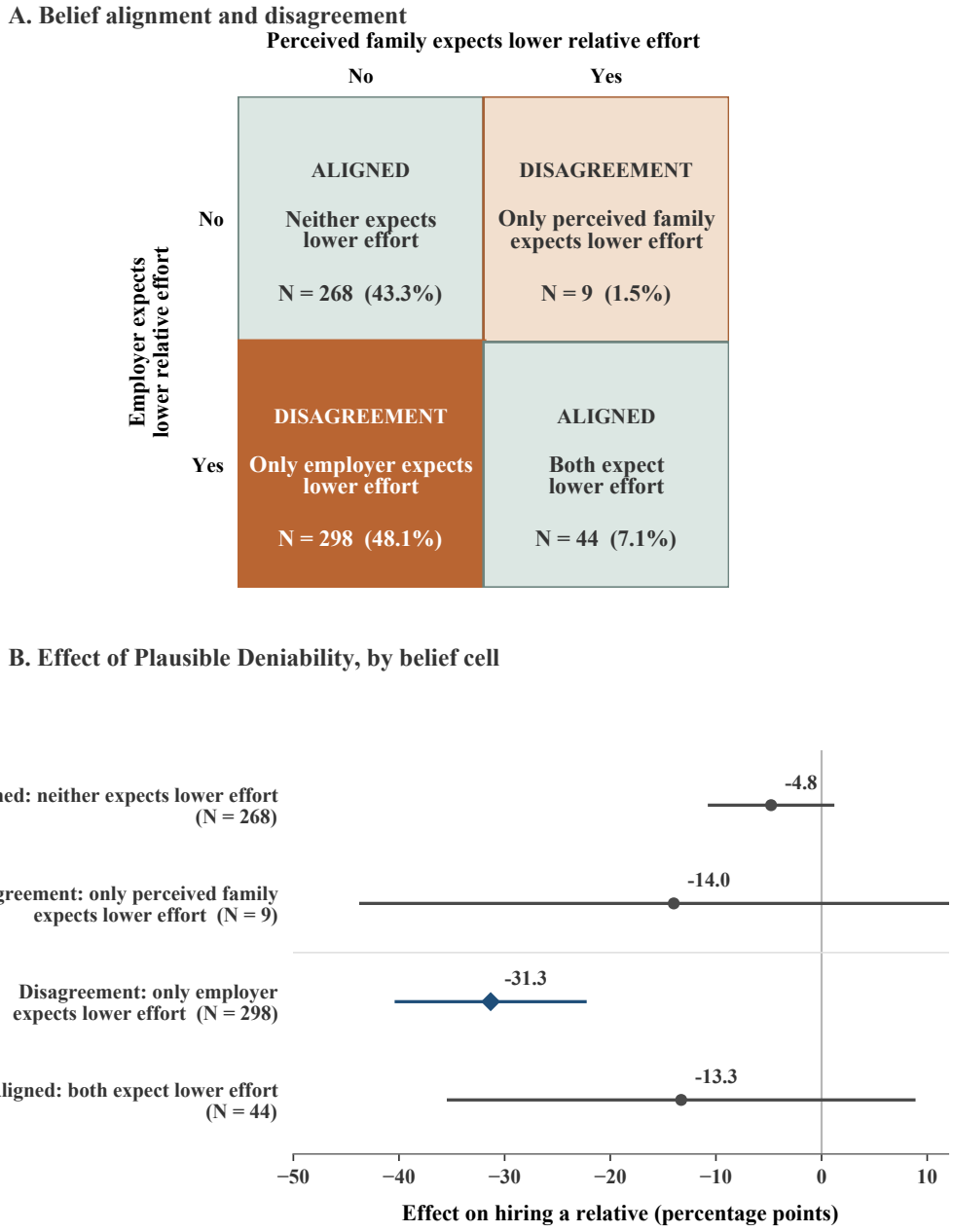


Figure 9: Belief Disagreement and the Effect of Plausible Deniability

Notes: Panel A divides the 619 Urban Mechanism employers according to whether the employer expects the relative to work worse for them and whether the employer believes the family shares that expectation. No family member was interviewed. Panel B reports effects of Plausible Deniability on relative hiring within these belief cells. Regressions include the employee-count randomization stratum, owner gender, and location controls. Bars show heteroskedasticity-robust 95 percent confidence intervals. The perceived-family-only cell contains nine employers and is shown for completeness rather than as precise evidence.

11 Tables

Table 1: Plausible Deniability, Relative Hiring, and the Public-Attribution Tax

Panel A: Effects on hiring a related worker at equal subsidies

	Outcome: related worker hired		
	Urban Field	Urban Mechanism	Rural Mechanism
	(1)	(2)	(3)
Plausible Deniability − Public Control	-0.121	-0.152	-0.197
	(0.063)	(0.040)	(0.051)
	[0.054]	[< 0.001]	[< 0.001]
Public-Control mean	0.582	0.627	0.780
Reduction / Public-Control mean	20.9%	24.2%	25.3%
Observations	252	619	322
Industry fixed effects	Yes	No	No
Location controls	Yes	Yes	No
Stratification controls	No	Yes	No

Panel B: Public-attribution tax

	Mean tax (ZMW)		Employers with positive tax (pp)	
	Urban Mechanism	Rural Mechanism	Urban Mechanism	Rural Mechanism
	(4)	(5)	(6)	(7)
Public Control − Plausible Deniability	49.4	9.8	14.8	20.0
	(14.9)	(2.3)	(4.0)	(5.1)
	[< 0.001]	[< 0.001]	[< 0.001]	[< 0.001]
Public-Control mean	44.7	11.2	62.7	78.3
Observations	617	318	617	318
Design controls	Yes	No	Yes	No

Notes: Panel A reports experiment-specific effects of randomized assignment to Plausible Deniability on an indicator for hiring the related worker when the two workers receive equal subsidies. The omitted condition is the public-attribution condition, labeled Public Control. The Urban Field sample contains 252 firms with hiring choices observed by 5 September 2023 (110 Public Control and 142 Plausible Deniability); the specification includes location controls and industry fixed effects and clusters standard errors by employer. The Urban Mechanism sample contains 619 eligible employers who completed the equal-subsidy choice (308 Public Control and 311 Plausible Deniability); the specification includes the employee-count randomization stratum, owner gender, and location controls. The Rural Mechanism sample contains 322 employers after the documented exclusion of surveys administered by surveyors 5 and 6 (159 Choice Announced/Public Control and 163 Choice Plausibly Deniable); the specification has no additional controls. "Reduction / Public-Control mean" is the magnitude of the treatment coefficient divided by the displayed reference mean and is descriptive, not a separate estimand.

Panel B uses complete, monotone subsidy-choice schedules to measure the public-attribution tax. A positive reservation premium is the additional outsider subsidy an employer requires before choosing the outsider; the mean tax is the Public-Control reservation premium minus the Plausible-Deniability reservation premium. Columns (6) and (7) report the corresponding difference in the share of employers with a positive reservation premium, in percentage points. Reservation premia use interval midpoints and observed range endpoints for censoring. The Urban schedule sample is 617 (308 Public Control and 309 Plausible Deniability), excluding two of the 619 equal-subsidy observations because their schedules are incomplete or nonmonotone. The Rural schedule sample is 318 (157 Public Control and 161 Plausible Deniability), excluding four of the 322 equal-subsidy observations for the same reason. Urban estimates include the controls listed above; Rural estimates are unadjusted. Standard errors are heteroskedasticity robust unless otherwise stated, two-sided p-values are in brackets, and no significance stars are shown.

Table 2: Job Context and Heterogeneity in the Hiring Response

	Total N (control N)	Control-group kin choice	Interaction (pp) Core + excuses	Interaction (pp) All controls
Money responsibility: no	107 (65)	75.4%	–	–
Yes (versus no)	336 (158)	70.9%	46.9	47.0
Raw p-value			[0.001]	[0.003]
Ease of excusing poor performance: easy	253 (131)	72.5%		
Not easy, but possible	134 (68)	72.1%		
Very difficult	56 (24)	70.8%		
Harder to excuse, interaction per SD			9.3	7.2
Raw p-value			[0.027]	[0.101]
Observations			443	
Employer and design controls			Yes	

Notes: The sample contains the 443 Urban Mechanism employers who report family pressure and have complete data for the job-context regressions. The outcome is an indicator for choosing the related worker at equal subsidies. The interaction coefficients report changes in the effect of Plausible Deniability on relative hiring; positive coefficients indicate less movement away from kin. They are not treatment effects within the displayed subgroups. Group N includes both treatment arms. Control N is the denominator for the unadjusted control-group kin-choice mean. Excuse difficulty retains the original three-level scale and enters the interaction standardized by its full-sample standard deviation. The Core + excuses specification includes general kin preference, perceived worker ability outside the family match, job trust, excuse difficulty, external verifiability, all moderator main effects, employee-count strata, owner gender, and area controls. The All controls specification includes all twelve treatment-characteristic interactions. Inference uses HC2 standard errors. The Holm-adjusted p-value for money responsibility in the saturated model is 0.031. The corresponding Holm-adjusted p-values for excuse difficulty are 0.165 and 0.807. Job characteristics are not randomly assigned.

Table 3: Productive Consequences of Randomly Assigning a Relative

	Quantity		Quality		Wage
	Average daily output (kg) (1)	Output rate (kg/hour) (2)	Adjusted defect count (3)	Adjusted defect-free (percentage points) (4)	Wage paid (ZMW) (5)
Assigned Relative Worker	-26.78	-5.04	0.08	-4.94	0.03
	(13.52)	(2.32)	(0.04)	(3.31)	(1.60)
	[0.049]	[0.030]	[0.042]	[0.136]	[0.983]
Nonrelative mean	387.65	79.26	0.13	88.22	38.88
Employer observations	295	290	290	290	290
Subsidy-amount fixed effects	Yes	Yes	Yes	Yes	Yes
Employer controls	Yes	Yes	Yes	Yes	Yes

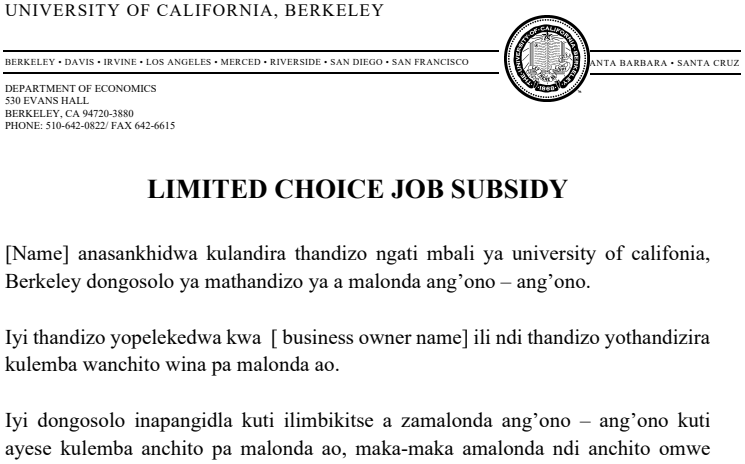
Notes: The sample contains the 295 Rural Mechanism employers randomly assigned either the related worker or the nonrelative worker after the equal-subsidy hiring choice; 135 were assigned the related worker and 160 the nonrelative worker. Each column reports the coefficient on Assigned Relative Worker from the preferred employer-pooled specification. Average daily output is the employer-level mean of kilograms produced on the randomized workdays and retains a no-show as zero. Output rate is total valid output divided by total valid work time. Adjusted defect count records the number of defect types newly introduced during production, and adjusted defect-free is an indicator, reported in percentage points, for introducing no defect types. Output rate, quality, and wage are conditional on worker arrival and retain 290 employers. Specifications include subsidy-amount fixed effects, prior worker-employer experience, perceived sharing sanctions, current unshelled maize, and prior maize-shelling hiring. The nonrelative mean is the mean under assignment to the nonrelative worker. Standard errors, clustered at the employer level, are in parentheses; two-sided p-values are in brackets. Models that additionally control for both candidates' baseline performance are similar and are reserved for the appendix. No significance stars are shown.

Table 4: Employer Forecasts and Relationship-Specific Deterioration

	Forecasts of realized output		Forecasted relationship-specific deterioration			
	Nonrelative assigned (1)	Relative assigned (2)	Not expected slower (3)	Expected slower (4)	Subgroup difference (5)	Ordered forecast (6)
Named output forecast	23.53	8.88				
	(8.29)	(6.30)				
	[0.005]	[0.161]				
Assigned Relative Worker			-2.62	-15.42		
			(3.55)	(5.43)		
			[0.455]	[0.002]		
Forecast interaction or difference					-13.15	-6.57
					(6.64)	(3.35)
					[0.035]	[0.043]
Outcome mean	387.65	363.12	77.85	82.04	79.26	79.26
Observations	150	124	290	290	290	290
Employer-level clusters	150	124	290	290	290	290

Notes: Columns (1) and (2) report the association between the employer's pre-assignment named-candidate output forecast, measured in predicted bags, and the assigned worker's realized average daily output. The regressions are estimated separately by randomized assignment arm and control for the assigned candidate's baseline output, adjusted defects, husking status, worker characteristics, employer characteristics, and design fixed effects. The randomized arms contain 160 nonrelative assignments and 135 relative assignments; complete controls retain 150 and 124 employers, respectively. The larger point estimate in column (1) than in column (2) is descriptive; this table does not report a formal cross-arm test of the two forecast slopes. Columns (3)–(6) use the 290 employers with valid pooled output rates to test whether the pre-assignment belief that the relative would work more slowly predicts relationship-specific deterioration. Columns (3) and (4) report proportional effects of Assigned Relative Worker within the two direct-forecast subgroups. Column (5) reports their formal difference. Column (6) replaces the binary forecast with the ordered response and reports the interaction for a one-step movement toward expecting the relative to work more slowly. These specifications control symmetrically for both named output forecasts, forecast confidence, both candidates' baseline output and adjusted defects, employer controls, and design fixed effects. Coefficients in columns (3)–(6) are approximate percentage changes, $100[\exp(\widehat{\beta})-1]$; their displayed standard errors equal 100 times the log-coefficient standard error, while bracketed p-values are exact tests in log points. Standard errors are clustered at the employer level. The corresponding lower-quality forecast result is not included because it does not bear directly on the output-slacking claim and remains in the audit appendix. No significance stars are shown.

A Appendix Figures



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DEPARTMENT OF ECONOMICS
530 EVANS HALL
BERKELEY, CA 94720-3880
PHONE: 510-642-0822/ FAX 642-6615

LIMITED CHOICE JOB SUBSIDY

[Name] anasankhidwa kulandira thandizo ngati mbali ya university of califonia, Berkeley dongosolo ya mathandizo ya a malonda ang'ono – ang'ono.

Iyi thandizo yopelekedwa kwa [business owner name] ili ndi thandizo yothandizira kulemba wanchito wina pa malonda ao.

Iyi dongosolo inapangidla kuti ilimbikitse a zamalonda ang'ono – ang'ono kuti ayese kulemba anchito pa malonda ao, maka-maka amalonda ndi anchito omwe nthawi zambiri sangayese kulemba chocho.

A zamalonda ambiri omwe analandwa thandizo iyi analandira thandizo ya wanchito wasankhidwa ndi dongosolo (program), osasankhidwa ndi iwo olemba nchito. Cholinga cimodzi ca thandizo iyi chinali kuti tikope eni a zamalonda kuti ayese kulemba anchito omwe sangalembe nthawi zambiri.

Coode dziwani – malonda osiyana –siyana omwe atengako mbali mu dongosolo (program) iyi chipetsidwa mathandizo ndi ma dongosolo a pangano osiyana – siyana. Madongosolo a pangano ya thandizo omwe amalonda aliyense alandisa ndi yolongoledwa pa certificate yao. Musayelekeze kuti a zamalonda onse analandira madongosolo a pangano yolingana.

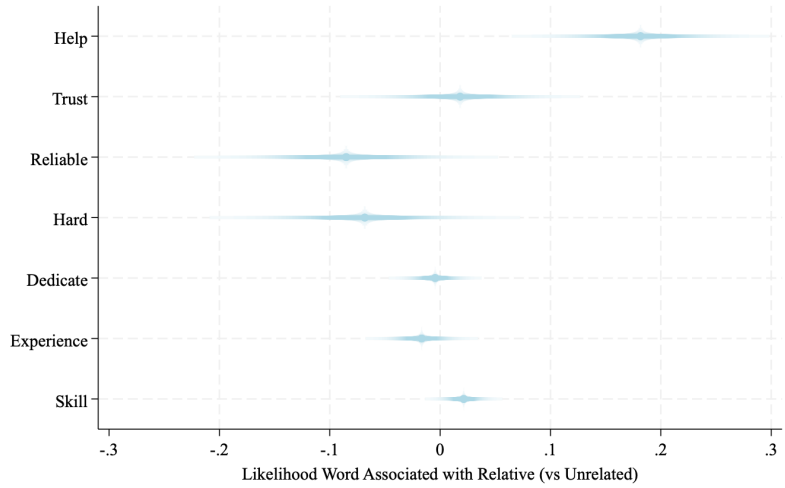
Ngati muli ndi mafunso pa ya mathadizo awa khalani omasuka kulankhula ndi ma program managers:

Sarah Tembo 0979122987
William Phiri 0979119715

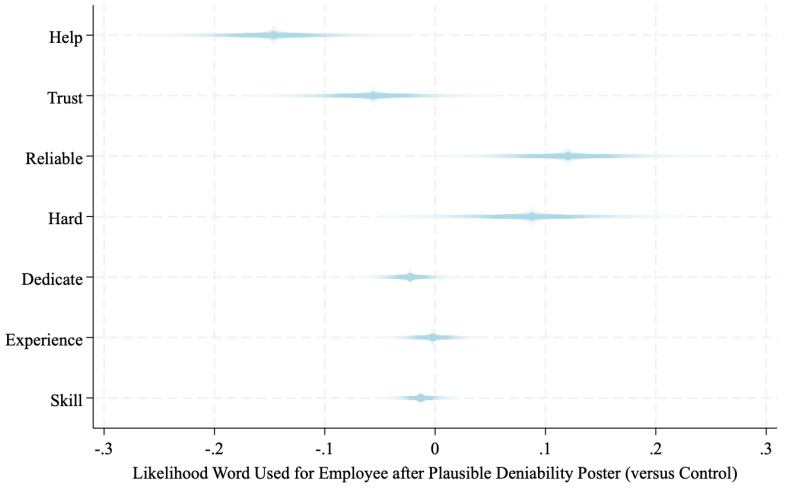
Figure A1: Plausible Deniability Poster

Notes: The figure reproduces the English translation of the poster used in the Plausible Deniability condition in the urban experiments. The poster states that some employers received subsidies conditional on hiring a particular worker. Employers receiving an actually restricted offer received a poster describing that restriction, so the design did not make false statements about any employer's subsidy.

Figure A2: Urban Field Experiment – Relative likelihood of using certain words for justifying the hiring choice



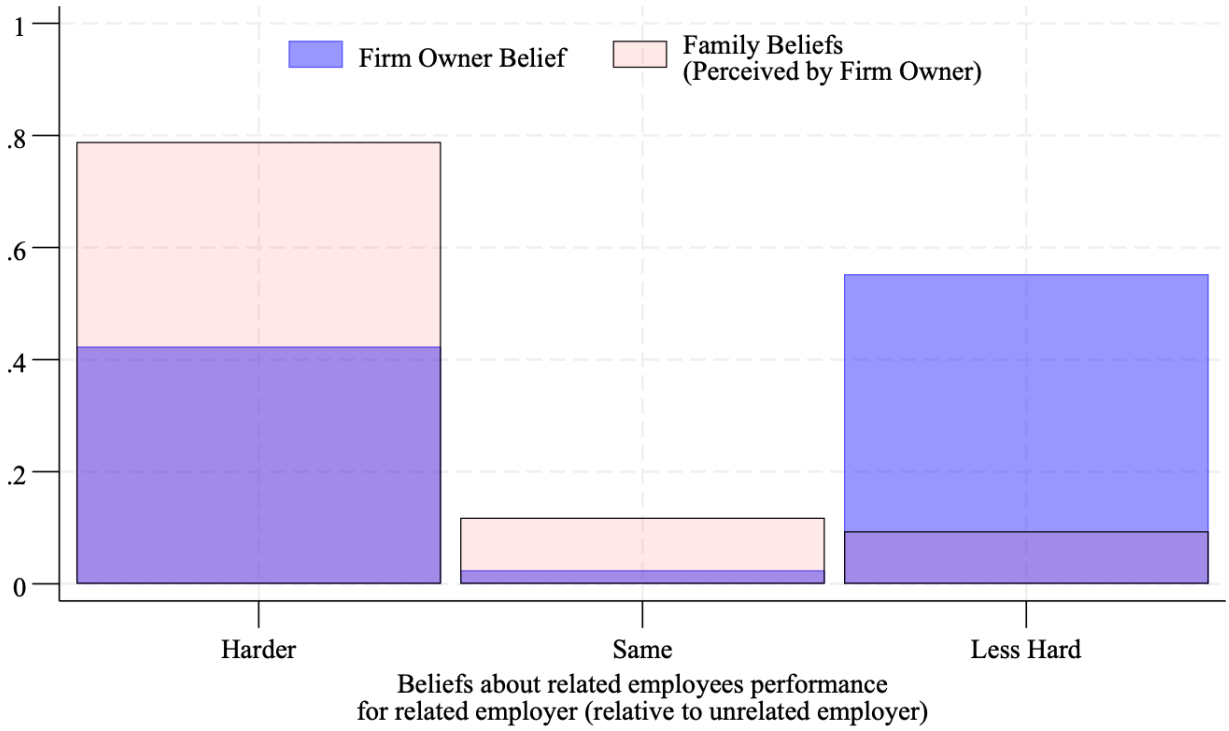
(a) By whether the employee is related



(b) By Plausible Deniability Treatment

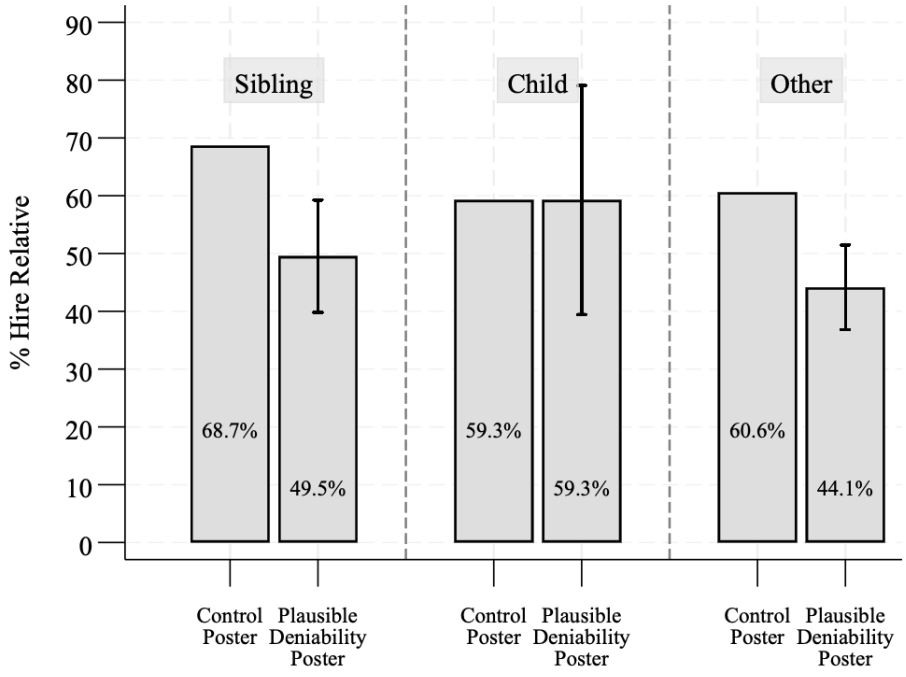
Notes: The figure reports differences in the words employers use to explain their Urban Field hiring choices. Panel A compares employers who select a relative with those who select a nonrelative. Panel B compares employers assigned the Plausible Deniability Poster with those assigned the Control Poster. All regressions include location controls and industry fixed effects. Bars show robust 95 percent confidence intervals.

Figure A3: Urban Mechanism Experiment - Disagreement between Employer and Family Regarding Work Effort of Employee



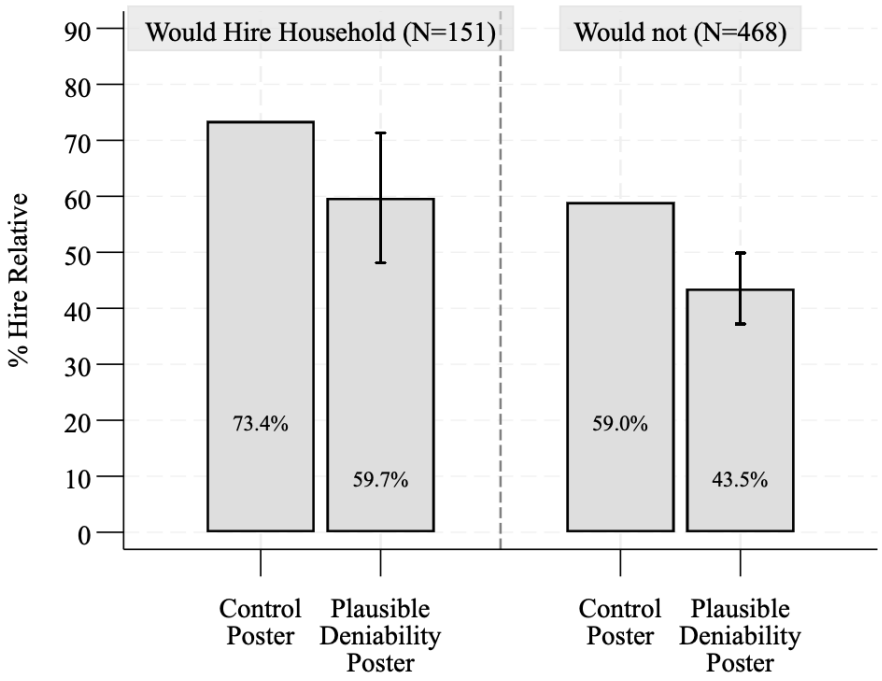
Notes: This figure shows the belief of the the employer, and the second order beliefs of the employer about their family's beliefs, of the family employee's work effort. Each bar displays the proportion who perceive that the family employee will work harder, the same, or less hard for their family employer 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 A4: Urban Mechanism Experiment - Treatment Effect heterogeneity conditional on relationship of Employee to Employer



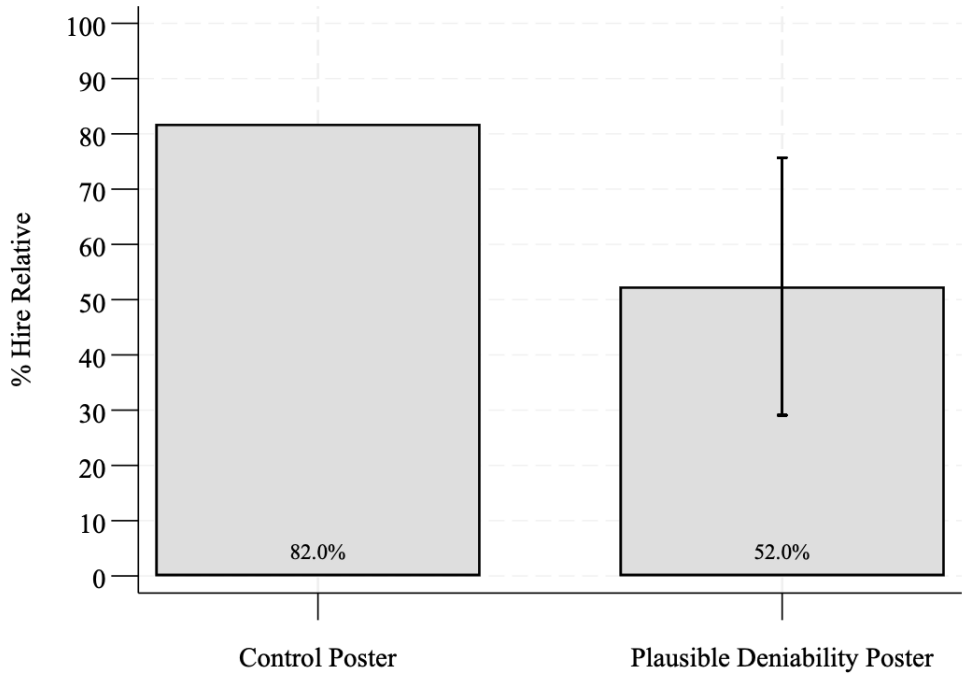
Notes: This figure shows the likelihood of a relative being selected in the Urban Mechanism experiment, conditional on the relationship between the employer and the family employee selected.

Figure A5: Urban Mechanism Experiment - Treatment Effect heterogeneity conditional on employer stating that the marginal relative hire is a member of their household



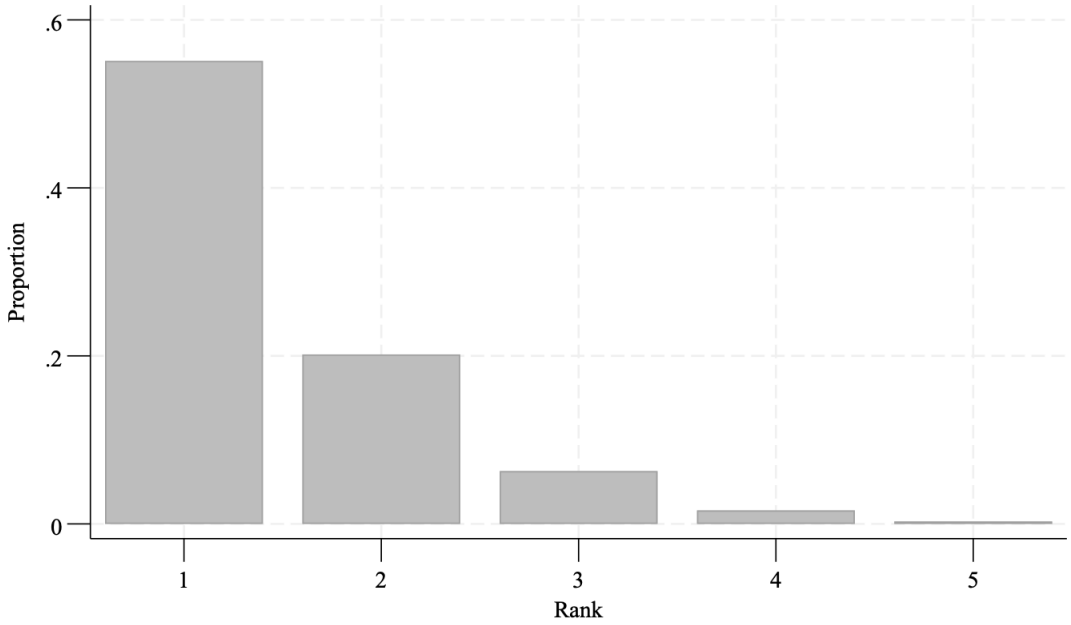
Notes: The figure reports the effect of Plausible Deniability on relative selection in the Urban Mechanism experiment, separately by whether the employer states that the marginal relative hire would have been a member of the household.

Figure A6: Urban Mechanism Experiment - Treatment Effect for those randomly selected to receive subsidy



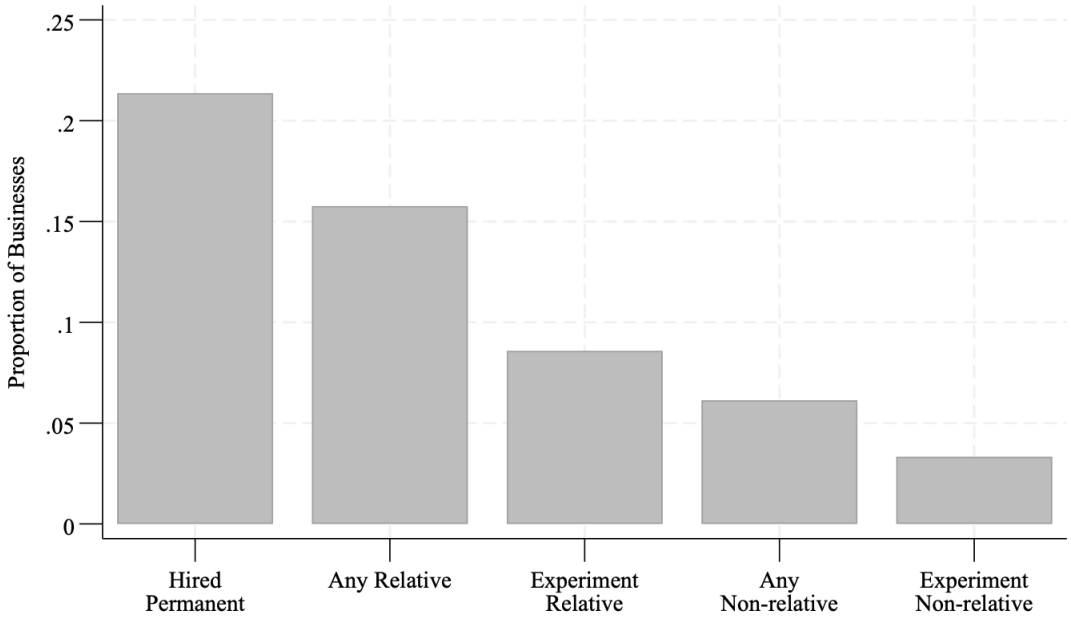
Notes: The figure reports relative selection by attribution condition among Urban Mechanism employers randomly selected to receive a subsidy.

Figure A7: Urban Mechanism Experiment - Employer Ranking of Selected Relative among Relative Options



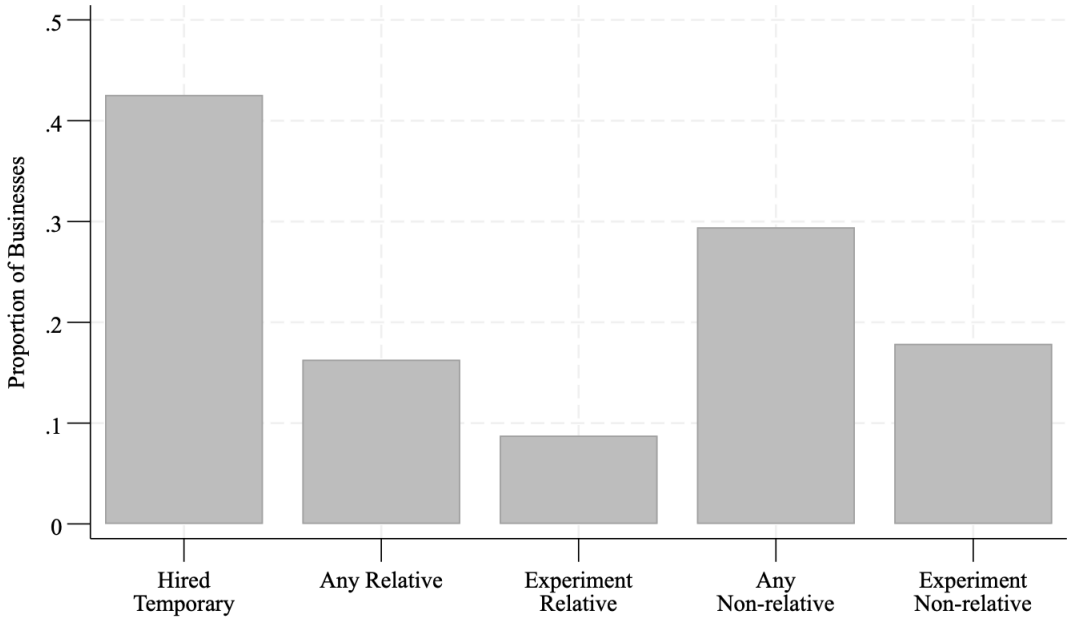
Notes: These figures show how employers ranked the relative selected for the experiment among the relatives they listed at baseline as a prospective employee. 1 indicates best available. Data was collected for employers not assigned the subsidy following the experiment.

Figure A8: 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 A9: 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: Plausible Deniability, Family Pressure, and Expected Match Quality

	Effect within pressure-by-match cells				Targeted difference	
	High pressure Bad match (1)	High pressure Good match (2)	Low pressure Bad match (3)	Low pressure Good match (4)	Focal minus Low pressure / Bad match (5)	Focal minus High pressure / Good match (6)
Plausible Deniability	-0.376	-0.053	-0.129	0.023		
	(0.055)	(0.034)	(0.057)	(0.103)		
	[< 0.001]	[0.120]	[0.024]	[0.826]		
Targeted difference					-0.247	-0.323
					(0.078)	(0.065)
					[0.002]	[< 0.001]
Control mean	0.517	0.953	0.172	0.808		
Cell observations	227	217	115	60		
Regression observations	619					
Joint test of columns (5)–(6)	$F(2,595) = 12.466$; $p < 0.001$					
Controls	Employee-count stratum, owner gender, and location					

Notes: The sample is the 619 eligible employers in the Urban Mechanism experiment in Chipata who completed the equal-subsidy hiring choice. The outcome is an indicator for choosing the related worker. Plausible Deniability is an indicator for assignment to the Plausible Deniability condition; the omitted condition is the Control Poster. High pressure means that the employer reports that refusing to help a relative can cause sharing sanctions, and an expected bad match means that the employer expects the related worker to perform worse for them than for someone else. Columns (1)–(4) report deniability-minus-control effects from a single fully interacted pressure-by-match regression. Columns (5) and (6) compare the focal high-pressure/bad-match effect with the indicated adjacent cell effect. The control mean and cell observations correspond to each pressure-by-match cell; all coefficients use the full regression sample of 619. All specifications include the employee-count randomization stratum, owner gender, and location controls. Heteroskedasticity-robust standard errors are in parentheses, and two-sided p-values are in brackets. The Holm-adjusted p-values for columns (5) and (6) are 0.002 and < 0.001, respectively. No significance stars are shown.

Table 2: Belief Disagreement, Defensibility, and Relative Hiring

Panel A: Effects across pre-choice belief cells

	Outcome: relative hired			
	Neither expects lower effort (1)	Perceived family only expects lower effort (2)	Employer only expects lower effort (3)	Both expect lower effort (4)
Plausible Deniability	-0.048	-0.140	-0.313	-0.133
	(0.031)	(0.152)	(0.046)	(0.113)
	[0.118]	[0.357]	[< 0.001]	[0.240]
Public-Control mean	0.953	0.200	0.429	0.200
Cell observations	268	9	298	44
Regression observations	619			
Controls	Employee-count stratum, owner gender, and location			

Panel B: Effects by public defensibility

	Outcome: relative hired		
	Easy to excuse (less verifiable) (5)	Not easy to excuse (more verifiable) (6)	Difference (5) − (6) (7)
Plausible Deniability	-0.250	-0.045	-0.206
	(0.052)	(0.059)	(0.079)
	[< 0.001]	[0.446]	[0.010]
Public-Control mean	0.595	0.675	–
Subgroup observations	354	265	–
Regression observations	619	619	619
Controls	Employee-count stratum, owner gender, and location		

Panel C: Disagreement and defensibility in one model

	Outcome: relative hired (8)
Deniability × employer-only disagreement	-0.246
	(0.069)
	[< 0.001]
Deniability × not easy to excuse	0.002
	(0.072)
	[0.980]
Observations	619
Joint test of both interactions	$F = 6.816$; $p = 0.0012$
Controls	Yes

Table A2 (continued): Notes

Notes: The sample contains the 619 eligible employers in the Urban Mechanism experiment in Chipata who completed the equal-subsidy hiring choice (308 assigned the public Control Poster and 311 assigned the Plausible Deniability Poster). The outcome is an indicator for choosing the related worker. Coefficients in Panels A and B are deniability-minus-control effects from fully interacted OLS regressions. "Employer expects lower effort" means that the employer expects the relative to work worse for them than for someone else. "Perceived family expects lower effort" refers to the employer's report that relatives believe the worker will work less hard for the employer than for someone else; no family member was interviewed.

The four Panel A cells contain 268, 9, 298, and 44 employers and exhaust the regression sample. Because column (2) contains only nine employers (five public and four deniable), it should be interpreted as a disclosed small-cell estimate rather than precise evidence. Ease of excuse is a pre-treatment employer-reported proxy for whether worse relative productivity would be publicly defensible, not a randomized verification treatment. Column (7) is the effect in the easy-to-excuse group minus the effect in the not-easy-to-excuse group. Panel C includes both treatment interactions in one regression. Every regression includes the employee-count randomization stratum, owner gender, and location controls. Heteroskedasticity-robust standard errors are in parentheses and two-sided p-values are in brackets. No significance stars are shown.

Table 3: Relatedness and Outcomes in Naturalistic Village Employment Contracts

Panel A: Work performance

	Bags shelled per day (1)	Quality rated excellent (pp) (2)	Quality rated okay or poor (pp) (3)
Related worker	-0.281	-7.6	4.0
	(0.248)	(11.8)	(3.2)
	[0.259]	[0.521]	[0.218]
Nonrelative mean	5.233	80.1	0.7
Observations	683	684	684
Related-worker observations	132	132	132
Employer / worker clusters	286 / 216	286 / 216	286 / 216
Worker fixed effects	Yes	Yes	Yes
Employer fixed effects	Yes	Yes	Yes
Crop fixed effects	Yes	Yes	Yes

Panel B: Compensation and contract enforcement

	All-in compensation per bag (ZMW) (4)	Contract condition violated (pp) (5)	Identifiable employer discipline (pp) (6)	Worker was fired (pp) (7)
Related worker	0.200	6.4	5.4	0.3
	(0.451)	(4.9)	(5.2)	(2.6)
	[0.658]	[0.194]	[0.308]	[0.895]
Nonrelative mean	16.482	1.8	2.2	0.7
Observations	683	684	684	684
Related-worker observations	132	132	132	132
Employer / worker clusters	286 / 216	286 / 216	286 / 216	286 / 216
Worker fixed effects	Yes	Yes	Yes	Yes
Employer fixed effects	Yes	Yes	Yes	Yes
Crop fixed effects	Yes	Yes	Yes	Yes

Table A3 (continued): Notes

Notes: The data are retrospective reports about maize-shelling contracts made by 773 employers in 24 villages near Chipata. The underlying panel contains 1,572 contracts with 780 workers: 1,225 contracts with nonrelatives and 347 with relatives. Each column reports the coefficient on an indicator that the worker is related to the employer from an OLS model with worker, employer, and crop fixed effects. These are conditional associations within the linked employer-worker network, not randomized treatment effects. Binary-outcome coefficients and means are reported in percentage points. Bags per day and all-in compensation per bag are winsorized at the first and ninety-ninth percentiles. All-in compensation includes the reported value of cash and other payments, such as in-kind gifts or lunches.

Quality is employer reported. The full sample contains 1,232 contracts rated excellent, 306 rated good, 21 rated okay, and 13 rated poor. Columns (2) and (3) are therefore not complements: contracts rated good are zero in both outcomes. A contract-condition violation is the employer's report that the worker violated at least one contract condition. The instrument then asks whether the problem was lateness, leaving early, poor-quality work, absence, theft or suspected theft, little effort, or another issue. Of 46 reported violations, issue detail is recorded for 32: 24 mention poor quality, 9 lateness, 7 little effort, 2 theft or suspected theft, and 1 absence; multiple responses are allowed. Issue detail is missing for 14 violations, including 10 of 19 involving relatives and 4 of 27 involving nonrelatives, so issue-specific regressions are not reported.

The instrument separately asks whether the employer sanctioned the worker and, if so, records firing, contacting the village chief, contacting family or relatives, deducting salary, or another sanction. Column (6) codes an identifiable disciplinary action: a structured action or an open-text response that clearly describes speaking to or warning the worker. It excludes seven relative-worker records whose "other sanction" text instead describes coming late, laziness, or not working hard; all seven also report no contract-condition violation. In the full panel, 60 contracts have an identifiable disciplinary action and 26 involve firing. The unrestricted any-sanction response, which includes those seven ambiguous records, is reserved for the appendix. These outcomes can reflect worker conduct, employer reporting, or employer responses and should not be interpreted as causal measures of disciplinary treatment.

Reghdfe's recursive singleton pruning leaves the connected identifying samples shown in the table: 683 observations for quantity and compensation and 684 for the five fully observed outcomes. These samples contain 132 related-worker observations and 551 or 552 nonrelative observations. The identified samples contain 23 identifiable disciplinary actions (12 nonrelative and 11 related) and 8 firings (4 and 4). Inference uses two-way employer-by-worker CV3max cluster-jackknife standard errors. The employer, worker, and intersection cluster counts are 286, 216, and 675 or 676; the reference distribution has 215 degrees of freedom, and no cluster deletion failed. Standard errors are in parentheses, two-sided p-values are in brackets, and no significance stars are shown. Nonrelative means are descriptive means within the identified estimation sample.

Table 4: 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)	
Agriculture-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 5: 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)	
Relative shells more at baseline	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: The table reports balance in the Rural Mechanism sample with respect to assignment to Plausible Deniability. Robust standard errors are in parentheses.

Table 6: 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)	
Relative shells more at baseline	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: The table reports balance in the Rural Mechanism sample with respect to worker assignment. Robust standard errors are in parentheses.

Table 7: 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	(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 8: 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: The outcome in every column is an indicator that the Urban Mechanism employer selects the relative at equal subsidies. Each column interacts Plausible Deniability with the baseline characteristic listed in the table: owner gender, Raven score, management score, an indicator for any permanent employee, the number of related employees, or total employment. All regressions include the employee-count randomization stratum, owner gender, and location controls. Robust standard errors are in parentheses and p-values are in brackets.

Table 9: 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]
Heterogeneity variable	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 × heterogeneity variable	(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: The outcome in every column is an indicator that the Urban Mechanism employer selects the relative at equal subsidies. Each column interacts Plausible Deniability with the pre-treatment social-distance measure listed in the table: attending the same church, seeing the relative more often, reciprocal favors, or having more friends in common. All regressions include the employee-count randomization stratum, owner gender, and location controls. Robust standard errors are in parentheses and p-values are in brackets.

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 10: 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 11: 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 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 va total profits plus one, logged. Standard errors clustered at the employer level in parentheses.

Table 12: 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 13: 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)	
Agriculture-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.

C Conceptual Framework: Details and Proofs

This appendix provides the full environment, characterizes equilibrium behavior, and establishes the results summarized in Section 3. The selection results compare attribution regimes holding anticipated continuation values fixed. The effort results concern an assigned-employment benchmark and an interior regular equilibrium. These restrictions allow the

experiments to be mapped to distinct theoretical objects without claiming that every result follows from an unrestricted selected-employment equilibrium.

C.1 Worker continuation value

If retained after the initial task, the worker receives a final employment opportunity. The worker receives wage w, obtains an effort-contingent payment β, and incurs effort cost k. The worker's terminal effort choice is

$$e_2(\theta) = \mathbf{1}\{\theta \geq k - \beta\}. \tag{24}$$

Relative to dismissal and outside payoff u, the value of retention is

$$R(\theta) = w - u + (\beta + \theta - k)_+. \tag{25}$$

I assume $R(\theta) > 0$. The terminal opportunity represents any future return to remaining in good standing with the employer, including repeat work or continued access to employment. The protection mechanism requires such a continuation value: if workers expect no employment consequence from current performance, making dismissal more difficult cannot reduce effort through the employment channel.

C.2 Worker posteriors

Workers follow a cutoff strategy,

$$e(\theta) = \mathbf{1}\{\theta \geq k - t\}, \tag{26}$$

where a larger t corresponds to a larger share of workers exerting effort. Define

$$\begin{aligned} S(t) &= 1 - F(k - t), \\ P &= S(t), \\ A &= S(\min\{t, \beta\}), \\ B &= S(\beta), \\ U &= \mathbb{E}[\theta/k], \\ V &= \mathbb{E}\left[\frac{\theta}{k}\mathbf{1}\{\theta \geq k - t\}\right]. \end{aligned}$$

The probabilities of good and bad reports are

$$\ell_g = \frac{1 - \rho}{2} + \rho P, \qquad \ell_b = 1 - \ell_g. \tag{27}$$

Bayes' rule gives the employer's posterior probability that the relative will exert effort in the terminal opportunity:

$$Q_g = \frac{(1-\rho)B/2 + \rho A}{\ell_g}, \tag{28}$$

$$Q_b = \frac{(1+\rho)B/2 - \rho A}{\ell_b}. \tag{29}$$

The posterior probabilities that the worker did not exert effort on the initial task are

$$z_g = \frac{(1-\rho)(1-P)}{2\ell_g}, \tag{30}$$

$$z_b = \frac{(1+\rho)(1-P)}{2\ell_b}. \tag{31}$$

Finally, the family's posterior assessments of worker willingness are

$$m_g = \frac{(1-\rho)U/2 + \rho V}{\ell_g}, \tag{32}$$

$$m_b = \frac{(1+\rho)U/2 - \rho V}{\ell_b}. \tag{33}$$

A good report predicts both higher continuation productivity and greater willingness to contribute:

$$Q_g \geq Q_b, \qquad m_g \geq m_b, \qquad z_g \leq z_b.$$

The reputational return to effort is

$$\Gamma(t) = \rho(m_g - m_b). \tag{34}$$

For an interior effort cutoff,

$$\Gamma(t) = \frac{4\rho^2 \operatorname{Cov}\left(\theta/k, \mathbf{1}\{\theta \geq k - t\}\right)}{1 - \rho^2(2P-1)^2} > 0. \tag{35}$$

Thus worker reputation is generated endogenously by the fact that more willing workers are more likely to exert effort. It is not an exogenous reward attached to observed output.

C.3 Employer beliefs and dismissal

Suppose employers follow a hiring cutoff, choosing the relative when $a \geq h$. Under the uniform prior,

$$a \mid R \sim U[h, 1]. \tag{36}$$

Hiring the relative therefore changes the family's prior about employer loyalty before any retention decision is observed.

Let

$$\Pi_s = v_2 - w + (q_2 - \beta)Q_s \tag{37}$$

denote expected continuation profit from retaining the relative after report s, and define the productive gain from replacement as

$$D_s = \Pi_N - \Pi_s. \tag{38}$$

Suppose an employer with type a dismisses the relative after report s when $a \leq \tau_s$. Conditional on hiring, the family's posterior expectations of employer loyalty are

$$\mu_{Fs} = \frac{h + \tau_s}{2}, \tag{39}$$

$$\mu_{Ks} = \frac{1 + \tau_s}{2}. \tag{40}$$

The reputational loss from dismissal is therefore

$$\mu_{Ks} - \mu_{Fs} = \frac{1 - h}{2}. \tag{41}$$

Comparing the continuation payoffs in Equations (12) and (13), the employer dismisses the relative when

$$a \leq D_s - \omega K_s(h), \tag{42}$$

where

$$K_s(h) = \lambda \frac{1 - h}{2} + J(z_s) > 0. \tag{43}$$

The equilibrium dismissal cutoff is consequently

$$\tau_s(h, t) = \left[D_s - \omega K_s(h)\right]_h^1, \tag{44}$$

where

$$[y]_h^1 = \min\{1, \max\{h, y\}\}.$$

Absent the additional social consequences of dismissal, the employer would dismiss whenever $a \leq D_s$. The set of employers retained in a privately undesirable continuation match is therefore

$$\tau_s < a \leq \min\{D_s, 1\}. \tag{45}$$

This is the model's post-hiring lock-in region.

Because a bad report predicts lower continuation productivity and provides a stronger

justification for dismissal,

$$\tau_b \geq \tau_g.$$

The difference in retention probabilities induced by worker effort is

$$d_h(t) = \rho \frac{\tau_b - \tau_g}{1 - h} \geq 0. \tag{46}$$

C.4 Worker effort equilibrium

Using Equations (25), (34), and (46), the difference between the worker's utility from exerting and not exerting effort is

$$b + \theta - k + \delta R(\theta) d_h(t) + \chi \Gamma(t). \tag{47}$$

This expression is increasing in θ, which justifies the cutoff strategy in Equation (26).

At an interior cutoff, the marginal worker has $\theta = k - t$. Let

$$\mathcal{R}(t) = R(k - t).$$

The equilibrium effort cutoff satisfies

$$t = b + \delta \mathcal{R}(t) d_h(t) + \chi \Gamma(t). \tag{48}$$

An assigned-employment equilibrium sets $h = 0$ and solves Equation (48). A selected-employment equilibrium additionally requires that h be consistent with employers' optimal hiring decisions. The comparative statics below use assigned employment because the rural assignment experiment bypasses employer selection.

C.5 Employer protection and worker accountability

Consider an assigned-employment equilibrium with

$$\tau_g = 0, \qquad 0 < \tau_b < 1. \tag{49}$$

Thus a good report never leads to dismissal, while a bad report sometimes does. Define the feedback denominator

$$M = 1 - \delta \mathcal{R}'(t) d_0(t) - \delta \mathcal{R}(t) \frac{\partial d_0(t)}{\partial t} - \chi \frac{\partial \Gamma(t)}{\partial t}. \tag{50}$$

I assume $M > 0$. This holds, for example, under the local-stability condition stated below.

Proposition A1: Protection and accountability. In the equilibrium described above,

$$\frac{\partial t}{\partial \omega} = -\frac{\delta \mathcal{R}(t) \rho K_b(0)}{M} < 0, \tag{51}$$

$$\frac{\partial t}{\partial \chi} = \frac{\Gamma(t)}{M} > 0. \tag{52}$$

Proof. In the discipline region,

$$d_0(t) = \rho \tau_b.$$

At fixed t, Equation (44) gives

$$\frac{\partial \tau_b}{\partial \omega} = -K_b(0) < 0.$$

Implicitly differentiating Equation (48) with respect to ω gives Equation (51). The reputational return $\Gamma(t)$ is positive at an interior cutoff, so differentiation with respect to χ gives Equation (52). □

Suppose a common increase in family exposure σ strengthens both employer pressure and worker accountability:

$$\omega = \sigma \bar{\omega}, \qquad \chi = \chi_0 + \sigma \bar{\chi}.$$

Then

$$\frac{\partial t}{\partial \sigma} = \frac{\bar{\chi} \Gamma(t) - \delta \mathcal{R}(t) \rho \bar{\omega} K_b(0)}{M}. \tag{53}$$

Family involvement increases effort when the worker-accountability term dominates and reduces effort when the employer-protection term dominates. This explains why the model permits both productive and unproductive family employment.

C.6 Shared performance evidence

Proposition A2: Shared evidence. Under the conditions of Proposition A1, increasing the precision of a performance signal observed by both employer and family raises equilibrium effort:

$$\frac{\partial t}{\partial \rho} = \frac{\delta \mathcal{R}(t) \left[\tau_b + \rho \frac{\partial \tau_b}{\partial \rho} \right] + \chi \frac{\partial \Gamma(t)}{\partial \rho}}{M} > 0. \tag{54}$$

Proof. At a fixed interior effort cutoff, a more precise bad report increases the posterior probability that the worker failed to exert effort:

$$\frac{\partial z_b}{\partial \rho} > 0.$$

It also reduces expected continuation productivity:

$$\frac{\partial Q_b}{\partial \rho} < 0.$$

The productive return to replacing the worker therefore rises. Because $J'(\cdot) \leq 0$, stronger evidence of shirking also reduces the conflict generated by dismissal. Together these imply

$$\frac{\partial \tau_b}{\partial \rho} > 0.$$

A more precise signal also increases the difference between the family's posterior assessments following good and bad performance:

$$\frac{\partial \Gamma(t)}{\partial \rho} > 0.$$

Implicit differentiation of Equation (48) gives Equation (54). □

This result applies to evidence shared with the family. Employer-only monitoring is different. Private information may help the employer identify when replacement is profitable without making dismissal easier to justify to the family. The model therefore makes no unconditional prediction that greater private monitoring raises relative-worker effort.

C.7 Selection and the monetary hiring wedge

Proposition A3: Selection under plausible deniability. Hold anticipated continuation strategies and beliefs fixed across attribution regimes and suppose $C_D < C_P$. Then:

1. The relative is hired if and only if

$$v_x(a) + p \leq C_z.$$

2. The deniable-minus-public effect on relative hiring is

$$-\Pr\{C_D < v_x(a) \leq C_P\}.$$

3. The additional outsider subsidy required for indifference is

$$p_z^* = C_z - v_x(a),$$

so

$$p_P^* - p_D^* = C_P - C_D.$$

Proof. The hiring condition follows directly from comparing Equations (3) and (4). At $p = 0$, employers satisfying

$$v_x(a) \leq C_D$$

choose the relative under either regime, while those satisfying

$$v_x(a) > C_P$$

choose the outsider under either regime. Only employers satisfying

$$C_D < v_x(a) \leq C_P$$

change their choice. Setting the two hiring payoffs equal yields the reservation subsidy. □

The continuation-value restriction is substantive. If attribution also changes anticipated employment values, write the outsider advantage under regime z as $v_z(a)$. The reservation-premium difference then becomes

$$p_P^* - p_D^* = (C_P - C_D) - [v_P(a) - v_D(a)]\,. \tag{55}$$

Interpreting the entire experimental difference as a refusal-cost change therefore requires the second term to be zero.

C.8 An attribution foundation for the refusal cost

The reduced-form refusal cost can be linked to the family's inference about responsibility for selecting the outsider. Let

$$C_z = c_0 + \ell_H A_z, \qquad A_z = \Pr(\text{employer responsible} \mid N, z), \tag{56}$$

where c_0 captures obligations that remain even when the employer is not held responsible, and ℓ_H is the cost of being personally blamed.

Suppose the employer has discretion with probability α_z. When the employer lacks discretion, the program selects the outsider with probability q_z. Let o_z denote the probability that an employer with discretion selects the outsider. Bayes' rule gives

$$A_z = \frac{\alpha_z o_z}{\alpha_z o_z + (1-\alpha_z) q_z} = \frac{o_z}{o_z + \xi_z}, \qquad \xi_z = \frac{(1-\alpha_z) q_z}{\alpha_z}. \tag{57}$$

Plausible deniability increases the relative likelihood that outsider hiring was externally imposed, represented by a larger ξ_z. This lowers the family's posterior that the employer was responsible and therefore lowers C_z.

Because employer choices themselves enter the family's posterior, outsider hiring solves

the fixed-point condition

$$o_z = 1 - G\left(c_0 + \ell_H \frac{o_z}{o_z + \xi_z}\right), \tag{58}$$

where G is the distribution of the outsider's private advantage. Under continuity and positive outsider choice, this equation has a unique solution. Moreover,

$$\frac{\partial o_z}{\partial \xi_z} = \frac{g(C_z)\ell_H o_z/(o_z+\xi_z)^2}{1 + g(C_z)\ell_H \xi_z/(o_z+\xi_z)^2} \geq 0. \tag{59}$$

Thus endogenous updating about employer responsibility does not reverse the prediction that credible external attribution increases outsider hiring.

C.9 Productive kin benefits and heterogeneous responses

Let the outsider's private advantage in the absence of the job-specific kin benefit be v^0. Because B_x enters the employer's current return from the relative,

$$v_x = v^0 - B_x. \tag{60}$$

If G denotes the distribution of v^0, the magnitude of the plausible-deniability effect is

$$-\Delta_H(B_x) = G(B_x + C_P) - G(B_x + C_D). \tag{61}$$

Its derivative is

$$-\frac{\partial \Delta_H(B_x)}{\partial B_x} = g(B_x + C_P) - g(B_x + C_D), \tag{62}$$

which need not have a constant sign. A productive benefit may move an employer into the switching interval or move them beyond it into a region where the relative is privately preferred.

The reservation premium is

$$p_z^* = C_z - v^0 + B_x. \tag{63}$$

Thus B_x raises the level of the outsider premium one-for-one but cancels from its treatment-induced change:

$$p_P^* - p_D^* = C_P - C_D. \tag{64}$$

Under the maintained assumption that B_x is an additive current benefit, it also does not enter the worker's effort condition. A relative trusted with firm assets can therefore provide a productive benefit while still facing weak employment discipline.

These results explain why heterogeneous binary hiring responses cannot, on their own, distinguish productive kin benefits from heterogeneous pressure. The full subsidy schedules provide a potentially sharper distinction between the level of the employer's preference for the relative and the change in the public-attribution wedge.

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CURRENT POSITIONS:
Assistant Professor, Cornell University, Department of Economics

AFFILIATIONS:
Weiss Fund nominated researcher
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Postdoctoral Fellow, Stanford University, King Center on Global Development – 2024-2025

FIELDS OF CONCENTRATION: Development Economics, Behavioral Economics, Labor Economics

EDUCATION:	**DEGREE**	**DATE**	**FIELD**
University of California, Berkeley	PhD	2024	Economics
LSE *Distinction*	M.Sc.	2015	Economics
University of Cambridge *First-Class Honors*	B.A.	2011	Economics

WORKING PAPERS:

"When outsiders become Insiders: Beliefs and the Persistence of Exclusionary Norms" (Forthcoming, AEA Papers and Proceedings
with Luisa Cefala, Franck Irakoze and Pedro Naso

"All in the Family: Kinship Pressure and Firm Worker Matching"

"Under-training by Employers in Informal Labor Markets: Evidence from Burundi"
with Luisa Cefala, Pedro Naso and Michel Ndayikeza

"The Economic Consequences of Knowledge Hoarding"
with Luisa Cefala, Franck Irakoze and Pedro Naso

"Retrieval Failures and Consumption Smoothing: A Field Experiment on Seasonal Poverty"
with Ned Augenblick, Kelsey Jack, Supreet Kaur and Felix Masiye
Revise and Resubmit, *Quarterly Journal of Economics*

SELECTED WORKS IN PROGRESS:

"Measuring Slums from Space" with Michael Gechter, Minas Sifakis and Nick Tsivanidis. Approved for inclusion in the Journal of Economic Perspectives

"Commitment Savings in Farming Populations" with Supreet Kaur, Rajdev Kaur and Gautam Rao

"Freeriding and New Product Adoption" with Luisa Cefala, Grady Kileen and Redempteur Ntawiratsa

"The Welfare Effects of Easier Liquidity Access for Low-Income Workers: Evidence from Earned Wage Access" with Luisa Cefala, Eric Koepcke and Afras Sial

PUBLICATIONS:

Swanson, N., Christensen, G., Littman, R., Birke, D., Miguel, E., Paluck, E.L. and Wang, Z., 2020, May. Research transparency is on the rise in economics. In *AEA Papers and Proceedings* (Vol. 110, pp. 61-65).

Ferguson, J., Littman, R., Christensen, G., Paluck, E.L., Swanson, N., Wang, Z., Miguel, E., Birke, D. and Pezzuto, J.H., 2023. Survey of open science practices and attitudes in the social sciences. Nature communications, 14(1), p.5401.

PROFESSIONAL EXPERIENCE:

RESEARCH:

Research Assistant (with Nick Tsivanidis), Department of Economics, U.C. Berkeley (2021-2022)
Research Assistant (with Supreet Kaur), Department of Economics, U.C. Berkeley (2017-2018)
Research Assistant (with Ted Miguel), Department of Economics, U.C. Berkeley (2017-2018)
Research Assistant (with Nava Ashraf), Department of Economics, LSE (2015-2016)
Research Assistant (with Heather Schofield, Frank Schilbach) Cornell and MIT (2013-2014)

TEACHING:

ECON 4560: Development Economics (Spring 2026)
ECON 4960: Field Experiments in Economics (Spring 2026)
Teaching Assistant, MBA Programs, Haas School of Business (Spring 2020)
Strategic Leadership
Teaching Assistant, Department of Economics, U.C. Berkeley (Fall 2018, Spring 2019)
Introduction to Economics, Intermediate Microeconomics

OTHER:

M&A Analyst, Evercore Partners, 2013-2014
Equity Research Analyst, HSBC, 2011-2012

CONFERENCES AND PRESENTATIONS

2026	ASSA, Midwest International Develpoment Conference (scheduled), SOLE (scheduled)
2025	ASSA, University of Washington, PacDev, CSAE, Cornell, Princeton NLSE, UC Davis, Networks and Development (own and co-author presentation),, BREAD (own and co-author presentation), Firms, Labor Markets, and Development, NBER SI Development (co-author presentation), SITE Missing Markets
2024	CSAE Conference, Networks and Development, BREAD, NBER SI Development, USC, Harvard-MIT, Yale Y-Rise
2023	PacDev, Warwick PhD Conference, All UC Labor, Advances in Field Experiments, IZA/CREST Conference: Labor Market Policy Evaluation, World Bank Dime-KDI, NEUDC
2022	Advances in Field Experiments, STEG Workshop, IPA GPRL, NEUDC
2021	PEP Convening, PacDev, Early-Career Behavioral Economics Conference
2020	Yale Whitebox Conference

FELLOWSHIPS AND AWARDS:

2024	CEGA Doctoral Dissertation Award
2023	Stone Center Fellowship
2023	IRLE Dissertation Fellowship
2022	Global Priorities Fellowship
2021/2022	Rocca Dissertation Fellowship
2009, 2011	Kings Scholar, Gerald Shove Prize

RESEARCH GRANTS:

PI

2024	IGC (~$26,000), PEDL (~$40,000), GDRF (~$27,000), CEGA ($15,000)
2023	IGC (~$25,000), Strandberg Research Grant ($9,300)
2022	SurveyCTO Research Grant Awardee ($2,000), Weiss Fund ($48,120), Weiss Fund ($44,660)
2022	Strandberg Research Grant ($10,000)
2021-23	Psychology and Economics of Poverty Grants (multiple) ($39,000)
2021	ATAI ($8,527), JPal-JOI ($7,310)
2020	PEDL Exploratory Grant (~$40,000), EASST Grant Award ($54,250)
2018-23	CEGA Development Challenge (multiple) ($23,500)

Co-PI

2024	WAM ($1,500,000), JPAL SPI ($250,000), USAID-DIV ($1,500,000)
2022	IGC (~$25,000)
2020	USAID-DIV ($426,146)
2020	MRR ($256,716)
2019	Weiss Foundation ($75,862)

SERVICE

Committees: Cornell Placement Committee (2025)

Referee: *American Economic Journal: Applied Economics; Journal of Development Economics; Journal of the European Economic Association; Journal of Political Economy; Review of Economics and Statistics*

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September 15, 2026

Dear Workshop Selection Committee:

I am writing to apply for the NBER Workshop on Development Economics and Networks for Early-Career Researchers. I am an Assistant Professor of Economics at Cornell University and received my PhD in Economics from the University of California, Berkeley in 2024.

I would present a substantially revised version of my paper "All in the Family: Kinship Pressure and Firm Worker Matching." The revision places greater emphasis on heterogeneity in the effects of family ties on hiring. The accompanying statement provides more detail on the relationship between this paper and my broader research on networks, learning, technology and skill diffusion, and informal redistribution.

If I am shortlisted, Professor Supreet Kaur, Associate Professor of Economics at the University of California, Berkeley, could provide a recommendation. Her email address is supreet@berkeley.edu.

Thank you for considering my application.

Sincerely,

Nicholas Swanson

Motivation and Research Interests

Nicholas Swanson

My research asks why knowledge and productive practices spread slowly and how social relationships shape economic decisions in low-income settings. Both parts of this agenda are fundamentally about networks. I am applying to the workshop because it brings together researchers studying how networks shape learning, diffusion, and enforcement. I would value the opportunity to discuss these questions with scholars working on related problems and to develop relationships that can inform my future research.

Technology and skill diffusion are learning processes embedded in networks. Adopting an existing technology requires local experimentation and learning about heterogeneous returns. The resulting information does not move automatically across social or professional ties: producing it is costly, and the people who generate or transmit useful knowledge may be unable to capture its value. My research studies how these incentives affect whether knowledge is created and shared. In work on under-training in informal labor markets, short employment relationships weaken employers' incentives to create transferable skills, while a labor guarantee restores training. In work on knowledge hoarding, skilled competitors choose whether to transmit a technique, and local competition and social enforcement can sustain non-diffusion. An ongoing project on patents asks how formal protection changes incentives to invest in and diffuse new technologies. Across these projects, network relationships determine who experiments, who shares what they learn, and which skills and technologies spread.

A second strand of my research examines networks as systems of obligation and enforcement. My paper "All in the Family: Kinship Pressure and Firm Worker Matching" studies whether family employment can function as redistribution. In an experiment in Zambia, employers choose between a relative and a non-relative worker, while the experiment randomly provides plausible deniability for not hiring kin. Plausible deniability reduces relative hiring by 11 to 12 percentage points. Related workers are 7 percent less productive, and employers are willing to give up roughly 4 percent of monthly profits to avoid the obligation. These results suggest that kinship ties do more than transmit information or mitigate contracting problems: they can also enforce redistributive claims that distort matching and productivity. The paper has been substantially revised and now places greater emphasis on heterogeneity in the effects of family relationships on hiring. This heterogeneity helps clarify which relationships generate obligations and how the consequences of those obligations vary across employers.

I would especially like feedback at the workshop on this revised paper and on how to connect its heterogeneity results to the broader argument about networks, redistribution, and firm behavior. More generally, I would like to discuss my work on technology and skill diffusion with researchers who think about networks as channels of learning. I also hope to meet like-minded scholars working on social learning, diffusion, kinship, and informal insurance, and to build a network of colleagues with whom I can exchange ideas as these projects develop.