Working Paper Series 763
(ISSN 2788-0443)

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CERGE-EI
Prague, September 2023

ISBN 978-80-7343-570-7 (Univerzita Karlova, Centrum pro ekonomický výzkum a doktorské studium)
ISBN 978-80-7344-693-2 (Národohospodářský ústav AV ČR, v. v. i.)

Social Ties at Work and Effort Choice: Experimental Evidence from Tanzania*

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December 2022

Abstract

Many firms hire workers via social networks. Whether workers who are socially connected to their employers exert more effort on the job is an unsettled debate. We address this question through a novel experiment with small-business owners in Tanzania. Participants are paired with a worker who conducts a real-effort task, and receive a payoff that depends on the worker's effort. Some business owners are randomly paired with workers they are socially connected with, while others are paired with strangers. With a design that is sufficiently powered to detect economically meaningful effects, we find that being socially connected to one's employer does not affect workers' effort.

Keywords: firms, hiring, productivity, social ties, kinship networks.

JEL Codes: O17, M51, L2.

*We are grateful to Marcel Fafchamps, Jessica Goldberg, Jeremy Magruder, Nikolas Mittag, Juan Felipe Riaño, Leonard Wantchekon, participants in the CERGE-EI Empirical Micro lunch meeting, and in the GDRP II Research Workshop in Dar es Salaam for valuable comments. The project was conducted with financial support from the Danish Embassy in Dar es Salaam. The trial was pre-registered in the American Economic Association registry for randomized controlled trials (ID: AEARCTR-0007205).

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1 Introduction

Many workers find jobs through social networks, a fact that has been documented both in high-income countries (Granovetter, 1995; Munshi, 2003; Ioannides and Datcher Loury, 2004; Kramarz and Skans, 2014) and in low-income ones (Wahba and Zenou, 2005; Munshi and Rosenzweig, 2006; Beaman and Magruder, 2012; Witte, 2018). However, whether firms obtain *more productive* workers through this practice is still an open question. In general, hiring workers through social networks can affect worker productivity in at least two ways. First, it can help firms to screen workers when skills are hard to observe and available signals are noisy. Such screening activity has been documented in a variety of settings (Montgomery, 1991; Munshi, 2003; Dustmann et al., 2016). Second, being socially connected to the employer may affect workers' effort choice conditional on their underlying skills and characteristics. This second effect may be either positive, e.g., through altruism or increased pressure (Kugler, 2003), or negative, if such ties reduce the risk that workers will be fired for low effort (Ponzo and Scoppa, 2010).[1]

We focus on the second channel, on which the existing empirical evidence is mixed. Heath (2018) and Dhillon et al. (2021) find that social ties affect worker effort on the job in Bangladesh and India, respectively. On the other hand, Pallais and Sands (2016) find evidence of social ties improving screening but no evidence of social ties affecting workers' productivity conditional on their type. Similarly, Burks et al. (2015) find that productivity differences among referred factory workers do not end when the referrer leaves the factory, suggesting selection effects but limited effects of social connections on effort conditional

[1] An older literature has shown that workers who find jobs via social contacts stay longer on that job (Datcher, 1982; Simon and Warner, 1992; Loury, 2006).

on type.[2] These studies, however, focus on social ties *among workers*. We focus on social ties *between workers and employers*. This constitutes a more direct test of the hypothesis of interest and allows us to abstract from additional complexities, such as career concerns (Gibbons and Murphy, 1992) or the structure of payment schemes (Bandiera et al., 2009), which could affect workers' choice of whom to refer. Such complexities may have contributed to the diverging results found in the above-mentioned papers.

We present the results of a novel lab experiment conducted with actual employers and workers in Tanzania, which cleanly tests whether working for an employer with whom a worker shares social ties affects the effort choice of the worker. To our knowledge, this is the first study that experimentally investigates this mechanism. We find no evidence that working for a socially-connected employer affects workers' effort. Our experiment is powered to detect differences in output between working for socially connected vs non-connected employers of 3.8 percent or more (or 2.3 percent, once we control for workers' baseline productivity). The estimates we obtain are smaller than half of the minimum detectable effects. We also find no significant heterogeneity along important dimensions, such as gender or the degree of connectedness between workers and employers.

We conducted our experiment with 313 real-life small and micro-entrepreneurs, sampled from multiple markets and business hubs across Dar es Salaam, the capital city of Tanzania. Each entrepreneur was asked to bring along one further person, who could

[2] A broader literature has studied whether working alongside friends and acquaintances affects workers' effort, regardless of how they were hired, finding negative effects (Park, 2019), small and statistically insignificant effects (Brune et al., 2022), effects that depend on relative abilities of the friends (Bandiera et al., 2010), or peer effects, in that temporarily higher effort of friends increases own effort (Falk and Ichino (2006); Mas and Moretti (2009), see Herbst and Mas (2015) for a review.

be one of their existing employees or a person they would consider hiring if necessary (i.e., minding the business if the owner is absent). Each of these additional persons was matched with one of the entrepreneurs and had to conduct a real-effort task, which consisted of sorting beans by colour. For each gram of sorted beans both the worker and the matched entrepreneur received a monetary payment at the end of the experiment.

Crucially, one-half of the entrepreneur-worker pairs that arrived together at the lab sessions were randomly selected to remain matched with each other for the real-effort task, while among the other half the workers and entrepreneurs were randomly re-matched to each other. Given that the pay-offs for both entrepreneurs and workers depend solely on the effort of the worker, and given that workers and entrepreneurs have in expectations the same characteristics in the two groups, we can cleanly isolate the effect of working for a socially connected entrepreneur on workers' effort. However, as mentioned above, we find that workers who were randomly matched with an unknown entrepreneur did not sort different amounts of beans than those matched with the entrepreneur who brought them to the sessions. Both types of workers respond to monetary incentives in the same way and we find no evidence that socially connected workers feel any additional incentive to perform (nor, conversely, an incentive to shirk).

We also implemented a variation of the basic design with a sub-sample of our entrepreneur-worker pairs to test whether fear of reprisal by socially connected employers is a mechanism driving worker effort choice. In this variation, we (truthfully) inform the workers that their identity will not be revealed to the entrepreneur they are matched with, regardless of whether it is the one who brought them to the sessions or a stranger. This

turns off the possibility of reprisal by the entrepreneur. However, in line with our headline results, we do not find that such a mechanism plays an important role.

Our main contribution is the implementation of a novel experiment that exogenously varies the degree of social connectedness between employers and workers, and allows us to cleanly identify the effect of social ties on workers' effort choice. The existing evidence in this area is mostly based on observational studies. Only Pallais and Sands (2016) conduct an experiment, which, however, did not directly assign workers to socially connected versus unconnected employers. They instead work with a firm that had recently hired a number of new workers referred by existing employees and randomly inform a group of these workers that the person who referred them was being informed about their performance. The treatment, therefore, consisted of making existing connections more "salient". We, instead, create and sever matches experimentally. As already discussed above, another important difference is that we focus on the social connection of the worker to the employer, rather than to other employees. Furthermore, while the design of Pallais and Sands (2016) concentrates on the role of social pressure, our setup allows us to investigate other mechanisms such as altruism (i.e., workers internalising the added utility that accrues to their social contacts from them exerting greater effort, independent of any pressure). In fact, the "anonymous worker" variation in our design mentioned above allows us precisely to disentangle these two mechanisms, by eliminating the potential for social pressure. Finally, although our interest in this paper is in workers' effort, our design could be used to study the effects of

social ties on many other aspects of employment relations, such as hiring, remuneration, or investment decisions by employers, as well as bargaining between employers and workers.[3]

We also contribute to the literature investigating whether hiring within social and kinship networks in low-income countries is efficient or rather a friction constraining growth. The latter may be the case if social norms force firms to hire from kin- or friendship networks.[4] On the other hand, hiring from such networks could be an optimal response to lack of trust in workers from outside the network (Bloom et al., 2012; Bertrand and Schoar, 2006). Caria and Falco (2022) experimentally document such lack of trust in Ghana and show that it can constrain firm growth. We investigate another potential reason for hiring within social networks: connected workers may exert higher effort, all else equal. A sizeable share of employers who participated in our experiment (37 percent) agreed with the view that family members or close friends would work harder as employees. We do not find experimental support for such a hypothesis, consistent with the view that hiring from social networks is not bringing particular benefits to firms in low-income countries.[5]

[3]The design could also be used to study the effects of social ties on cooperation between employees, and not just in employer-employee relationships. For that purpose, one would invite pairs of workers and rematch a randomly selected subset with a stranger.

[4]We are not aware of research that has directly tested this channel. However, research from low-income settings has documented pressure within kinship networks to share income (Boltz et al., 2019; Baland et al., 2011), and providing jobs is an important way to distribute income. Also, Bertrand and Schoar (2006) show that firms in countries with more deeply ingrained "family values" have fewer workers.

[5]Effort choice at work is a canonical example of moral hazard. However, in this paper, we refrain from using that conceptualisation because in classic models of moral hazard effort is not observable by the principal, typically because the relationship between effort and output is stochastic. In the real-effort task we use, the random element between effort and output is likely small, and effort and output correspond closely. Our experiment is thus about the effort choice of workers on the job, all else equal, and less about moral hazard per se.

Finally, our results speak to the larger literature studying constraints to firm growth in low-income countries. The focus of such studies ranges from access to capital (De Mel et al., 2008; Banerjee et al., 2015), to informality (De Mel et al., 2013; De Soto, 1989), lack of business knowledge (Karlan and Valdivia, 2011; Campos et al., 2017), or lack of networks and mentoring opportunities (Fafchamps and Quinn, 2018; Brooks et al., 2018). A more recent literature has placed the spotlight on the misallocation of talent and on the challenge of finding good matches when information frictions are significant (Abebe et al., 2020, 2021; Alfonsi et al., 2020; Hardy and McCasland, 2020). We study whether firms face a trade-off between hiring from a limited pool of socially connected workers, who may not be the best matches in terms of skills but may exert more effort on the job. Disentangling the two mechanisms has so far proved difficult without a suitable experimental design. We bridge this gap by providing evidence from a novel experiment suggesting that such a trade-off is not evident.

The remainder of the paper is structured as follows. Section 2 describes the experimental design and our sample. Section 3 presents the results. Section 4 concludes.

2 Experimental Design

We invited 320 small business owners (henceforth "entrepreneurs") from the city of Dar es Salaam to take part in an experimental session conducted at the University of Dar es Salaam. Almost all of them, 313 in total, attended the experimental sessions. Each entrepreneur was asked to bring one other person of their choice to the lab session (henceforth the "worker"). This should be a person who either works at their business, or whom they "would consider giving a job in their business if an opportunity became available".

2.1 Baseline Setup

The experiment is centred around a real-effort task that takes place in the lab. The task is very simple and requires no particular skill. Starting from a bag with three different kinds of beans, workers need to sort the different types of beans into separate batches. Workers have eight minutes to sort as many beans as they can. For every gram of correctly sorted beans, they are paid two Tanzanian Shillings (TZS), in addition to a fixed amount of 700 TZS (ca 0.30 USD), which is independent of performance. On average, workers sort around 350 grams of beans in 8 minutes and thus earn another 700 TZS. The chosen task has been used in previous experimental work (e.g., Caria and Falco (2022)) and it is very simple by design. The objective is not to replicate a specific real-life job, but rather to provide a clean setting to isolate the role of effort when ability plays a very limited role. In this respect, the chosen task can be considered a stylised representation of basic manual tasks that exist in many industries and require workers to apply steady effort in order to attain a simple objective.

Before the first round of the game starts, participants have a few minutes to acquaint themselves with the task and the materials. Workers then play three consecutive rounds of the game (each lasting 8 minutes). For the purpose of the current analysis, we only use data from the first two rounds.[6] In Round 1, workers are simply asked to perform the task for themselves, without an employer, receiving two TZS for every gram plus the fixed amount of 700 TZS. This allows to obtain a measure of their baseline productivity at the task. In Round 2, workers repeat the task but are now randomly matched to an entrepreneur, who receives

[6]Round 3 was originally designed to address ancillary research questions that are not central to the main hypothesis being investigated. However, due to implementation problems, the data from that round cannot be analysed as pre-specified. It can however be left out without affecting the results and their interpretation from the first two rounds.

four TZS for every gram his or her matched worker produces, while workers continue to receive two TZS for every gram plus the fixed amount of 700 TZS.[7] In the baseline version of the game, the entrepreneur remains completely passive. The work is done by the worker and the entrepreneur has no decisions to make. He or she only affects output to the extent that his or her identity, which is revealed to the worker, affects the worker's effort choice. The design intends to replicate real-world situations in which an entrepreneur hires a worker to conduct a task and profits depend on the effort of the worker, who receives a piece rate. Such a payment scheme is common across a range of sectors and occupations in Tanzania.

Half of the entrepreneur-worker pairs that arrived at the sessions were randomly chosen to go through the game together. Among the remaining pairs, the workers and entrepreneurs were randomly re-matched. Given that entrepreneurs were sampled from many different locations across Dar es Salaam, a city of more than six million inhabitants, it is highly unlikely that entrepreneurs and workers in re-matched pairs knew each other. We refer to workers matched with the entrepreneur who brought them to the sessions as being "connected workers", and to the others as "non-connected workers". This is the central element in our experimental design, as it allows us to identify the impact of being randomly assigned to work for an entrepreneur with whom one shares social ties versus one with whom the worker does not share ties, all else equal.

[7] The 700 TZS were added to the per-round pay-off of workers in order to be able to pay a higher per-gram piece rate to entrepreneurs than workers while keeping the total pay-offs for workers and entrepreneurs roughly equivalent (for equity considerations). The higher piece rate was meant to make the effect of workers' effort on entrepreneurs' payoff more salient to workers. Both entrepreneurs and workers also received a 3,000 TZS show-up fee for participating in the lab experiment (around 1.30 USD).

2.2 Anonymous Worker Variation

In a subset of the experimental sessions, we *hide the identity of the worker from the entrepreneur* to whom the worker was matched. In this variation, while workers are told whom they are matched with, the entrepreneur does not know whether s/he has been matched with the worker they brought to the sessions or with another randomly selected worker. The workers are informed that their matched entrepreneur is not informed about their identity. This allows us to test whether reciprocity plays a role in workers' effort choice. We use the concept of reciprocity as broadly capturing behaviour that is motivated by the objective of reaping the benefits of future rewards from the other party (or, conversely, to avoid punishment from the other party). By not informing entrepreneurs about the identity of their matched workers, we can turn off this channel since entrepreneurs cannot possibly reciprocate or retaliate against workers if they do not know their identity.[8] If in this variation we observe an impact of being socially connected with the employer on effort, this effect has to be caused by other mechanisms than reciprocity, for example altruism (i.e., the worker internalising the utility of the employer).[9]

[8]Entrepreneurs may still assume that they are matched with their connected worker with a certain probability given the set-up of the lab sessions, and may decide that it is optimal to punish the worker they brought along to the session anyway in case of low output. However, even if the probability of punishment for low output may not be zero when the worker's identity is experimentally concealed, it can nevertheless be assumed to be lower than when it is not concealed.

[9]We implemented two further variations of the design. In one variation, entrepreneurs could *tip* their matched worker after observing their output. In the other one, entrepreneurs were asked, before the task, to choose how to split the combined piece rate of 6 TZS between themselves and the worker. These variations, which address separate research questions, did not reject their respective null hypotheses and do not affect the conclusions in this paper. They were originally included to study secondary research questions, on whether

2.3 Sampling

We conducted the experiment with a sample of 313 entrepreneurs who attended the lab sessions out of an initial 320 who accepted our invitation. Each entrepreneur brought along one worker, as described above. The sampling of the entrepreneurs was conducted using a random walk methodology in selected retail and service areas of Dar es Salaam, as well as in a number of manufacturing clusters. From a randomly selected starting point, e.g. a prominent intersection, the research assistants started walking in a randomly drawn direction and invited the owner (or manager, if the owner was not present) of every 6th business to take part in the experiment. Upon arriving at another intersection or road fork, a randomly selected direction was drawn to continue the random walk. The experiment was implemented in 32 sessions with 10 "entrepreneurs" and 10 "workers" each. Within each session, half of the entrepreneur-worker pairs were randomly chosen for re-matching. The entrepreneurs and workers in those pairs were randomly re-matched with other workers and entrepreneurs within the same session.[10] Table 1 shows basic summary statistics for the entrepreneurs that participated in the sessions, as well as for the workers they brought along. As shown in Column 1 of that table, 58.5 percent of the entrepreneurs are female, they are on average 40.2 years old, have on average 2.1 children, went to school for 8.5 years, and 51 percent of them are Muslim while the rest are Christian. Workers are 60 percent female.

social ties affect the remuneration decisions of entrepreneurs. For completeness, we show these additional results in Appendix A.

[10]We aimed to have 10 entrepreneurs and their workers in each session. In some sessions, not all invited pairs turned up, resulting in six sessions with 9 instead of 10 workers, and one with 8 workers. Finally, one session had 11 pairs. In sessions with odd numbers of pairs, the pool of pairs to be rematched contained one extra pair than the pool of pairs that stayed together.

They are, on average, 33.8 years old, they have 1.9 children, attended school for 8.3 years, and are 54 percent Muslim. Looking at the gender configurations of the pairs that came to the sessions, 160 out of the 313 pairs were female entrepreneurs with a female worker, 100 were male entrepreneurs with a male worker, 30 were male entrepreneurs with a female worker, and 23 were female entrepreneurs with a male worker. Finally, entrepreneurs have on average 0.95 (full-time) employees in their real-life businesses, with eight entrepreneurs having more than three (the maximum being seven employees).

2.4 Experimental Balance

Looking at the entrepreneurs randomly chosen to play the game with the workers they brought, versus the remaining ones who were matched with a stranger worker, Column 2 of Table 1 shows that the two groups are balanced. Only one of seventeen observable characteristics at our disposal shows a statistically significant difference at the 1 percent level (a second one only shows significance at the 10 percent level). Looking at the workers, we find a difference between the two groups in only one variable, and the difference is only significant at the 10 percent level (Column 4 of Table 1). We also reject the hypothesis that all the variables are jointly different between the two groups, for both entrepreneurs and workers, using F-tests.

2.5 Baseline evidence on hiring through social connections in the study population

Hiring from kinship or close friendship networks is common among the entrepreneurs that participated in our study: 65 percent referred to the worker they brought along as a

"family member" or "friend", while 67 of workers reported the same about the employer who brought them along. 37 percent of the entrepreneurs agreed with the statement that socially connected workers exert more effort, while 52 percent disagreed (the rest neither agreed nor disagreed). 35 percent agreed that family members are less likely to "steal" from the business, and 47% agreed that if one spends salary on a worker, it is best to spend it on a family member.[11] On the worker side, 40 percent agreed that an employer with whom one shares social ties will put less pressure on one to work hard, 39 percent agreed that employers with whom one shares ties are less likely to cheat on wage payments, and 26 percent agreed that such employers will pay more for the same work. Meanwhile, only 10 percent of employers state that they would pay a friend or family member more for the same work. Overall, this descriptive evidence indicates that sharing a connection with one's employer is perceived as an important determinant of workers' productivity by a sizeable share of respondents but not necessarily by the majority. This suggests that the effect may be mixed. Our experimental evidence will largely confirm these preliminary findings.

3 Results

3.1 Average Effects

We begin by showing basic comparisons of the output in grams of beans sorted by "connected workers" relative to the "non-connected" ones. Column 1 of Table 2 shows that non-connected workers successfully sorted 356 grams of beans, on average, while connected workers sorted six grams less. The difference is small and not statistically significant. In

[11] 27 percent agreed with both of the first two statements, and 20 percent with all three.

Table 1: Summary Statistics & Balance

Variable	(1) Entrep. matched w. own Worker (Mean)	(2) Entrep. matched w. own - unknwn W. (Diff.)	(3) Workers matched w. own Entrep. (Mean)	(4) Worker matched w. own - unknwn Entrep. (Diff.)
Female	0.608	0.045	0.634	0.053
	(0.490)	(0.056)	(0.483)	(0.055)
Age	39.654	-1.140	33.529	-0.464
	(11.504)	(1.287)	(9.712)	(1.190)
Live Dar E.S.	15.497	-1.366	13.484	-0.616
	(13.561)	(1.672)	(12.069)	(1.429)
Head Household	0.654	-0.009	0.562	-0.032
	(0.477)	(0.054)	(0.498)	(0.056)
Married	0.660	0.066	0.536	0.055
	(0.475)	(0.055)	(0.526)	(0.059)
Nbr. Children	2.033	-0.130	1.843	-0.038
	(1.664)	(0.197)	(1.544)	(0.195)
Head Household	0.654	-0.009	0.562	-0.032
	(0.477)	(0.054)	(0.498)	(0.056)
Household Size	2.922	0.009	2.732	0.095
	(1.848)	(0.203)	(2.033)	(0.204)
Years Schooling	8.451	-0.030	8.612	0.593
	(2.900)	(0.301)	(2.555)	(0.306)*
Pay taxes Y/N	0.562	-0.025	0.458	0.026
	(0.498)	(0.056)	(0.500)	(0.056)
Working Y/N	0.131	0.049	0.386	-0.027
	(0.338)	(0.035)	(0.488)	(0.056)
Income	323.105	72.105	272.847	86.681
	(782.351)	(221.501)	(767.175)	(97.111)
Musl.(1) or Christ.(0)	0.431	-0.162	0.533	-0.011
	(0.497)	(0.056)***	(0.501)	(0.057)
Literate Y/N	0.935	0.060	0.902	0.046
	(0.248)	(0.033)*	(0.298)	(0.037)
Soc.Conn. to Partner	0.654	0.004	0.660	0.023
	(0.477)	(0.054)	(0.475)	(0.054)
Actual Worker for Boss	0.680	0.017	0.680	0.017
	(0.468)	(0.053)	(0.468)	(0.053)
Nbr. Full Time Employees	0.915	-0.060		
	(1.057)	(0.115)		
Output Training Round			326.484	-10.573
			(64.815)	(7.104)
Observations	153	313	153	313

Notes: The table summarises key observable characteristics of the 313 entrepreneurs that participated in the sessions (Columns 1-2), and the 313 workers they brought along (Columns 3-4). Columns 1 and 3 show the average values of the observed variables for entrepreneurs and workers who were randomly selected to remain matched with the worker or entrepreneur with whom they arrived at the sessions, respectively. Columns 2 and 4 show differences in these variables, for entrepreneurs and workers, respectively, between those randomly selected to remain matched with the person they came to the sessions with and those who were re-matched with a stranger. An F-test of whether all variables jointly predict treatment allocation produces a p-value of 25 percent for entrepreneurs and 53 percent for workers. * $p<.1$; ** $p<.05$; *** $p<.01$

percentage terms, this is a difference of 1.8 percent and our minimum detectable effect size

(at the 5 percent level) is 3.8 percent. Therefore, if the effect of being socially connected to one's employer were economically meaningful, we would have been able to detect it.[12]

The effect size we estimate shrinks even further when we control for the "baseline" output of the worker in round 1, the practice round in which the worker had no employer and produced only for his/her own gain (Column 2 of Table 2). The estimated effect of being socially connected with the boss is now a positive 2.25 grams, or 0.6 percent of the output produced by non-connected workers. The minimum detectable effect in this specification is a 2.3 percent difference (at the 5 percent level). Thus, again, we can conclude that if working with a connected boss had economically meaningful effects, we would have detected them.

So far, we have assumed that all workers have social ties to the entrepreneurs who brought them to the session. This need not be the case. Some workers could be employees the entrepreneur hired from outside any social or kinship network, and with whom s/he shares a purely professional relationship. The existence of such worker-entrepreneur pairs among the random sub-sample of "connected" pairs could bias the estimated effect of social ties downwards. Since we asked workers and entrepreneurs about the nature of their relationship with the person with whom they arrived, we can test for this possibility. As already mentioned above, among both entrepreneurs and workers, about 65 percent state that the person they came with is an (extended) family member or friend, while the rest describe the person as a "colleague", "boss", or similar. In Column 3 of Table 2, we replicate the results from the previous column on the sample of 203 workers who report that their

[12]It is important to note that this is not an artefact of the task generating little variation in productivity across subjects. In fact, variation in the output measure is substantial, with workers at the 75th percentile of the output distribution producing 26 percent more than workers at the 25th percentile (the 90th/10th percentile ratio being 65 percent). Appendix B shows a histogram of output in the task, which broadly follows a Normal distribution.

Table 2: Main Results

Dep. Var.: Beans sorted (Grams)	(1)	(2)	(3) Actual Social Tie	(4)	(5) Congruent Sample	(6)
Connected Entrep.	-6.403	2.251	-1.696			-0.091
	(6.763)	(4.129)	(5.322)			(4.251)
Male Worker × Male Entr. × Conn.				-5.903	-4.419	
				(8.068)	(8.084)	
Fem. Worker × Male Entr. × Conn.				1.154	9.707	
				(11.731)	(15.350)	
Male Worker × Fem. Entr. × Conn.				22.013	28.650	
				(14.998)	(17.706)	
Fem. Worker × Fem. Entr. × Conn.				3.739	3.242	
				(6.548)	(7.201)	
Connected Entr. × Unknown Worker						9.697
						(11.550)
Output Basel. Round		0.834***	0.858***	0.828***	0.850***	0.836***
		(0.040)	(0.049)	(0.043)	(0.051)	(0.040)
Control Mean	356.4		358.4			
Observations	313	313	203	313	257	313
Session FE	Yes	Yes	Yes	Yes	Yes	Yes
Gender Comp. FE				Yes	Yes	

Notes: The table shows results from regressing workers' output in the real-effort task, as measured in grams of sorted beans, on the core independent variable of interest ("Connected Entrep."), a dummy for whether the worker was randomly selected to remain matched with the entrepreneur s/he arrived at the experimental session with, as opposed to being randomly re-matched with another (stranger) entrepreneur from the session. The regressions control for session fixed effects. All regressions except for that in column 1 control for the output of the worker in a test round of the same task with no matched entrepreneur yet. Column 3 restricts the sample to workers who report that they have social connections of "kinship" or "friendship" to the entrepreneur they arrived with. Column 4 shows effects within worker-entrepreneur pairs of all four possible gender configurations. Column 5 does the same but restricts the sample of re-matched pairs to those where both workers and entrepreneurs are re-matched with a counterpart of the same gender as the person with whom they arrived at the session ("congruent" pairs, see section 3.2 for more details). Column 6 interacts the core variable of interest with a dummy indicating whether the identity of the worker is withheld from the matched entrepreneur. Robust standard errors in parentheses: * $p<.1$; ** $p<.05$; *** $p<.01$

relationship to the person who brought them along is one of friendship or kinship. The results remain qualitatively unchanged, with the treatment coefficient in this sub-sample being less than half a percent of the average control-group output.

3.2 Effects by Gender

An important dimension of heterogeneity for the analysis at hand is the gender composition of the worker-employer pairs. Although gender gaps in the labour market are the subject of a vast literature, including recent work on gender gaps in the attitudes of entrepreneurs towards workers (Caria and Falco, 2022), we are not aware of any experimental evidence on the effect of being socially connected to one's boss on the productivity of male and female workers.

In Column 4 of Table 2, we break down the main coefficient from Column 2 into separate estimates for the four sub-samples of all possible gender compositions of the worker-entrepreneur pairs (male-male, male-female, female-male, female-female). We already discussed in the previous section that 51 percent of the pairs who arrived at the sessions were female-female, and 32 percent of them were male-male. In Column 4, we compare connected pairs with randomly re-matched pairs *of the same employer-worker gender configuration*. For none of the four configurations is the effect of being connected significant, and except for the smallest of the four groups (male worker, female entrepreneur), the effect size is always lower than two percent.

The above comparisons within gender-configuration subgroups are, however, subject to a source of potential bias. On the one hand, for instance, male workers who are randomly chosen to be re-matched with another male entrepreneur are in expectation representative of *all* male workers in the sample. On the other hand, male workers who arrived with a male entrepreneur are not necessarily representative of all male workers in the

sample (i.e., they may differ from male workers brought along by a female entrepreneur).[13] A simple way to deal with this problem is to restrict the sample for this analysis to what we term *congruent* worker-entrepreneur pairs. That is, from among the randomly re-matched pairs, we only include in the analysis those for which the (random) gender configuration of the pair is the same as the configuration of the pairs in which the entrepreneur and worker arrived at the sessions. For example, a pair of a male worker randomly re-matched to a male entrepreneur is only included in the analysis if both the worker and the entrepreneur came to the session in male-male pairings. This overcomes the aforementioned bias.

When we restrict the sample to such "congruent" pairs in Column 5 of Table 2, the results do not change qualitatively, especially not for the two largest groups of male-male and female-female pairs.[14] This suggest that the potential bias discussed above is not severe. Overall, we conclude that working for a connected boss has no significant impact irrespective of the gender mix of the pair.[15]

[13]Clearly, this also applies to those, among them, who are randomly selected to remain with their connected entrepreneur for the experiment. Hence, when we compare this group to workers who are re-matched, we may introduce bias into our estimates.

[14]The minimal detectable effects size in the congruent sample in Column 5 is 5.0% of baseline output for male-male pairs, 3.8% for female-female pairs, 8.8% for female workers paired with male entrepreneurs, and 9.6% for male workers paired with female entrepreneurs.

[15]We also do not find significant heterogeneity in a simpler specification where we interact the main treatment indicator with a dummy for female employers. The heterogeneous results by gender also remain the same when we replicate them on the sub-sample of workers who report that their relationship to the person who brought them along is one of friendship or kinship, as it was done in Column 3 of Table 2.

3.3 Reciprocity: Anonymous Workers

As discussed above, connected workers may choose to work harder for fear of retaliation, e.g. the boss may shun them in other settings (outside work) in which the worker and the entrepreneur interact. To test for this mechanism, in 8 out of 32 sessions we hide the identity of the worker to their matched entrepreneur, so that the entrepreneur does not know if the worker s/he is matched with is the worker whom s/he brought along and is therefore unlikely to retaliate for low effort.

We find that hiding the identity of the worker and shutting down the channel of retaliation does not change our conclusions. When we interact the dummy for a "connected worker" with a dummy indicating that the game was played in a session in which workers' identities were concealed, the interaction effect is small and not statistically significant (Column 6 of Table 2).[16]

3.4 Further Tests: Riskiness of Non-connected Workers

Socially connected workers may not on average produce more or less than workers without social ties to their employers, but may have less variability in their output. Thus, risk-averse employers may for that reason still prefer to hire socially connected workers. In our baseline survey, 40 percent of entrepreneurs agree with the statement that it is more difficult to predict the performance of a potential hire that is not family or friend (while 51 percent disagree, with the rest not having a clear view).

[16]The minimum detectable effect for the interaction term is 6.2 percent of average output in the control group.

When we test this hypothesis, we do not find evidence of lower output variance among connected workers (Table 3). We test this by first obtaining the residuals from a regression of output on session fixed effects and a dummy equal to one if the worker is connected to the boss. We then regress the *absolute value* of these residuals, as a measure of the deviation in the output of the worker from the levels that could be expected from him or her on average, on session fixed effects and the same dummy for being a connected worker. There is virtually no difference in these deviation measures between connected and non-connected workers (Column 1, Table 3). There is also no difference if we use the squared values of the residuals instead, which gives larger weight to larger deviations (Column 2). If anything, we find for male workers working for female bosses, that connected male workers deviate on average more from the expected output, based on both absolute and squared deviations (Columns 3 and 4). Overall, we find no evidence supporting the hypothesis that employing workers with whom an employer shares no social ties is riskier in terms of output uncertainty.

4 Conclusions

An active literature is interested in whether workers hired via social networks perform better. These studies usually find that such workers are better selected, but it remains unclear whether workers also exert different levels of effort at work, conditional on their type (Pallais and Sands, 2016; Burks et al., 2015; Heath, 2018; Dhillon et al., 2021). We address this question using a novel experiment conducted with pairs of real-world "workers" and "entrepreneurs" in Tanzania. Our design allows us to exogenously "break" social ties in these pairs, allowing us to isolate cleanly the effect of social connections on the effort choice of workers.

Table 3: Risk: Deviations of Output

	(1)	(2)	(3)	(4)
Dependent Variable:	Deviation	Deviation^2	Deviation	Deviation^2
Sample:			Congruent	Congruent
Connected Entrep.	-0.003	118.672		
	(3.787)	(489.077)		
Male Worker × Male Entr. × Conn.			-0.644	-332.480
			(7.737)	(1,075.895)
Fem. Worker × Male Entr. × Conn.			0.873	-377.928
			(20.976)	(3,099.220)
Male Worker × Fem. Entr. × Conn.			27.417*	3,672.571*
			(15.860)	(1,978.857)
Fem. Worker × Fem. Entr. × Conn.			-7.984	-777.690
			(6.091)	(742.214)
Observations	313	313	257	257
Output Basel. Round	Yes	Yes	Yes	Yes
Session FE	Yes	Yes	Yes	Yes

Notes: Column 1 shows results from regressing the absolute deviation of workers' output from the average output of all workers in the real effort task (after residualising the output using session and treatment fixed effects) on the core independent variable of interest ("Connected Entrep."), a dummy for whether the worker was randomly selected to remain matched with the entrepreneur s/he arrived with, as opposed to being randomly re-matched with another entrepreneur from the same session. The regressions control for session fixed effects. Column 2 does the same, except for using the squared deviation. Columns 3 and 4 replicate the previous two specifications, but, as in column 5 of Table 2, estimate the effects separately for entrepreneur-worker pairs of all four possible gender configurations. They do so on the sample of "congruent" entrepreneur-worker pairs (see notes of Table 2 and section 3.2 in the main text for further details). Robust standard errors in parentheses: * $p<.1$; ** $p<.05$; *** $p<.01$

Despite being powered to detect differences of as little as 2.3 percent of output between socially connected and unconnected workers, we find no significant effect of being socially connected with one's boss on workers' effort. Our point estimates are typically less than half of the minimum detectable effects. This pattern does not vary with the gender composition of the entrepreneur-worker pairs. Our evidence is in line with related work

by Pallais and Sands (2016) and Burks et al. (2015), who do not find experimental nor quasi-experimental evidence that social ties *among workers* affect effort. We show that social ties *between workers and employers* do not have a significant impact either.

The result that workers do not exert different levels of effort if this benefits employers with whom they are socially connected remains puzzling. Prima facie, this may be due to the fact that workers either do not incorporate the pay-off to their socially connected employers into their utility, or do not fear retaliation. In one variation of the design, we did not reveal the identity of the workers to the bosses to shut down the latter channel, but found no difference in the results. This indicates that the overall null-result is unlikely to derive from these two mechanisms working in opposite directions and cancelling each other out.

One explanation for why employees may fail to internalise the added utility to their connected employers is that these entrepreneurs could have higher income, so the marginal utility of added pay-offs for them from more effort by the worker may be too small to significantly affect the utility of the worker. In our baseline survey, entrepreneurs report, on average, 70 percent higher income than workers (190 vs 113 US$ annually, 215 vs 155 US$ among men). If workers perceive employers as significantly richer than themselves, they may regard the gains from the game as unimportant to them. In Appendix Table 4, we replicate the core regression from Table 2, Column 2, but interacting the treatment variable (i.e., being socially connected to one's boss) with another dummy indicating whether the entrepreneur with whom the worker arrived to the sessions reported above-median income relative to all entrepreneurs in the sample. If the lack of an effect were due to the higher

income of entrepreneurs, we would expect a negative coefficient on this interaction. We do not find evidence supporting this hypothesis.[17]

Finally, the muted role of the retaliation channel could be due to the fact that retaliation by socially-connected employers may be difficult if other obligations are embedded in the social networks that connect them. A rich literature has explored the importance, multi-dimensionality, and constraining effects of social networks as basic insurance mechanism in Tanzania (De Weerdt and Dercon, 2006; De Weerdt and Fafchamps, 2011) and in other low-income settings (Fafchamps and Gubert, 2007a,b; Munshi and Rosenzweig, 2016). Hiring workers from social or kinship networks may entail other valuable rewards for an entrepreneur, such as support by the worker (or his/her family) in case of negative economic shocks to the entrepreneur. These parallel concerns may limit the ability of the entrepreneur to punish low effort in the game. In other words, the entrepreneur may hire socially connected workers not because they exert more effort at work, but due to possible benefits elsewhere, or because they may be better selected as discussed in the introduction. We plan to follow up on these questions in future research.

[17]The same is true when focusing on male-male worker-entrepreneur matches or female-female matches only (Columns 2 and 3, Table 4).

Table 4: Heterogeneity by Income

Dep. Var.: Beans sorted (Grams)	(1)	(2) Male-Male Congruent	(3) Fem-Fem Congruent
Connected Entrep.	-3.307	-6.621	-9.694
	(5.686)	(12.346)	(7.921)
Connected Entrep. × Orig. Boss above Inc.	12.325	15.422	25.145
	(9.440)	(19.765)	(15.971)
Orig. Boss above Inc.	-6.395	-7.723	-10.377
	(6.460)	(18.249)	(11.242)
Output Basel. Round	0.829***	0.921***	0.781***
	(0.042)	(0.108)	(0.075)
Control Mean	355		
Observations	301	74	132
Output Basel. Round	Yes	Yes	Yes
Session FE	Yes	Yes	Yes

Notes: The table shows results from regressing workers' output in the real-effort task, as measured in grams of sorted beans, on the core independent variable of interest ("Connected Entrep."), a dummy for whether the worker was randomly selected to remain matched with the entrepreneur s/he arrived to the session with, as opposed to being randomly re-matched with another entrepreneur from the same session. The regressions control for session fixed effects. Column 1 replicates column 2 of Table 2, but it interacts the core treatment indicator "Connected Entrep." with a dummy variable indicating whether the entrepreneur who brought the worker to the session has above-median earnings relative to all entrepreneurs in the sample. Column 2 replicates the regression in column 1 on the sample of male entrepreneurs matched with male workers only, and Column 3 uses the sample of female entrepreneurs matched with female workers only. In both Columns 2 and 3, the sample is restricted to "congruent" pairs (see notes of Table 2 and section 3.2 in the main text for further details). Robust standard errors in parentheses: * $p<.1$; ** $p<.05$; *** $p<.01$

Appendix A

In this section, we report results from two supplementary variations of the basic experimental design, each implemented in a set of eight out of the 32 sessions. These variations focus on the use of financial incentives and do not constitute the focus of this paper. They also do not affect the conclusions discussed in the article, but we report the results for completeness and because they were pre-specified in the pre-analysis plan.

Supplementary variation: ***Entrepreneurs can tip***

In the first variation, matched entrepreneurs and workers went through the same experimental protocol as in the basic version of the game, except that bosses had the opportunity to "tip" their matched worker after the worker's output was revealed. The tip would be subtracted from the pay-off the entrepreneur received based on the output of the worker. The employer could choose any amount for the tip and may decide not to tip at all. Importantly, workers in these sessions were informed about the possibility of being tipped before they started the real-effort task. The intention of this variation was to test whether connected workers feel that the amount of the tip they would receive is less dependent on effort relative to non-connected workers. In other words, connected workers may feel entitled to receiving a tip from a connected boss no matter the effort. This may lead their effort to be less elastic to the presence of the tip, indicating that entrepreneurs may find it more costly to motivate connected workers through ex-post rewards. This hypothesis forms part of further work we are planning to undertake.

In Table A.1, Column 1, we first study whether connected vs unconnected entrepreneurs set different tip levels *unconditionally* (i.e., independently of workers' effort). We

find that entrepreneurs set insignificantly lower tips for connected workers. This result does not change when controlling for the level and squared level of the output of the worker (Column 2), which is not surprising, given that the output levels do not differ, on average, between connected and unconnected workers. We also do not find significant differences in tips across the four possible gender configurations (male-male, male-female, female-male, female-female), as shown in Column 3. Finally, Column 4 shows that the effect of being matched to a connected entrepreneur on workers' output does not differ in the sessions in which bosses had the opportunity to tip.

Supplementary Variation: ***Entrepreneurs set the piece rate***

The second supplementary manipulation offers an additional test of whether entrepreneurs use monetary incentives differently when matched with strangers vs connected workers. The game is as in the baseline version with employers and workers knowing each other's identity. However, the piece rate for the worker is not pre-determined; instead, the combined piece rate for the worker and the entrepreneur of 6 TZS is offered to the boss, who has to decide how to split it between the worker and him- or herself. Thus, any amount given to the worker is subtracted from the amount the boss receives, as in the "tips" variation above. The crucial difference with the tips manipulation is that here the piece rate is set ex-ante, while the tip is decided ex-post. Our objective is to test whether bosses use higher or lower monetary incentives when matched to socially connected workers, and how this affects output (relative to being matched to a stranger). For example, socially connected workers may have a higher intrinsic motivation to put effort into the work, which could allow the entrepreneur to set a lower piece rate, keeping more of the revenue received from the

Table A.1: Opportunity to Tip for Bosses

	(1)	(2)	(3)	(4)
Dependent Variable:	Tip	Tip	Tip	Beans sorted
Sample:			Congruent	
Connected Entrep.	-44.820	-39.085		5.822
	(55.717)	(56.829)		(5.004)
Connected Entrep. × Tips				-13.593
				(8.667)
Male Worker × Male Entr. × Conn.			18.890	
			(110.358)	
Fem. Worker × Male Entr. × Conn.			-6.111	
			(199.331)	
Male Worker × Fem. Entr. × Conn.			143.836	
			(241.500)	
Fem. Worker × Fem. Entr. × Conn.			1.386	
			(90.190)	
Output		2.478	6.617*	
		(2.785)	(3.939)	
$Output^2$		-0.003	-0.009	
		(0.004)	(0.006)	
Control Mean	623.9			
Observations	81	81	65	313
Output Basel. Round	-	-	-	Yes
Gender Comp. × Tips				
Session FE	Yes	Yes	Yes	Yes

Notes: The first column tests whether entrepreneurs who were randomly selected to remain matched with the worker they brought to the session ("Connected Entrep.") chose a different tip level (after the output of the worker was revealed) than entrepreneurs re-matched with other workers from the same session. The sample is from eight sessions (out of 32) in which entrepreneurs could give tips. Column 2 controls for the level and square of the output produced by the matched worker. Column 3 tests for different tip levels among worker-entrepreneur pairs of different gender configurations (male-male, male-female, female-male, female-female). It thereby restricts the sample further to "gender-congruent" pairs (see notes of Table 2 and section 3.2 in the main text for further details). Column 4 tests if the effect of being matched with a connected entrepreneur on workers' output is different in the sessions where entrepreneurs could tip. All the specifications control for session fixed effects. Robust standard errors in parentheses: * $p<.1$; ** $p<.05$; *** $p<.01$

output for themselves. Alternatively, the effort of connected workers may be more elastic to the piece rate. In that case, setting a higher piece rate for a connected worker may be optimal.

As shown in Column 1 of Table A.2, entrepreneurs randomly chosen to remain matched with their connected worker do not offer a statistically higher piece rate than entrepreneurs matched with another random worker. This fits with our baseline survey, in which only nine percent of entrepreneurs stated they would set a lower piece rate for socially connected workers, while six percent would set a higher rate, with the remaining 85 percent not adjusting the rate they would offer. Entrepreneurs matched with random workers set on average a piece rate of 1.938 TZS, very close to the exogenously set rate of 2 TZS in the sessions in which the entrepreneurs do not chose the piece rate. Entrepreneurs matched with their connected worker pay an insignificantly higher piece rate of 2.128 TZS. The difference is slightly larger among male workers working for male entrepreneurs, where entrepreneurs pay a 0.889 TZS higher piece rate to connected workers, with a p-value for this difference of 0.14 (Column 2). On the other hand, in the smaller sub-group of male entrepreneurs who brought female workers, these entrepreneurs seem to reduce the piece rate for female workers if they share social ties with them, with the effect significant at the 10 percent level.

However, the ability of entrepreneurs to set piece rates for workers does not change the effect of being matched to one's socially connected employer on worker's effort, either unconditionally (Table A.2, Column 3) nor controlling for the endogenous piece rate set by the entrepreneur (Column 4).

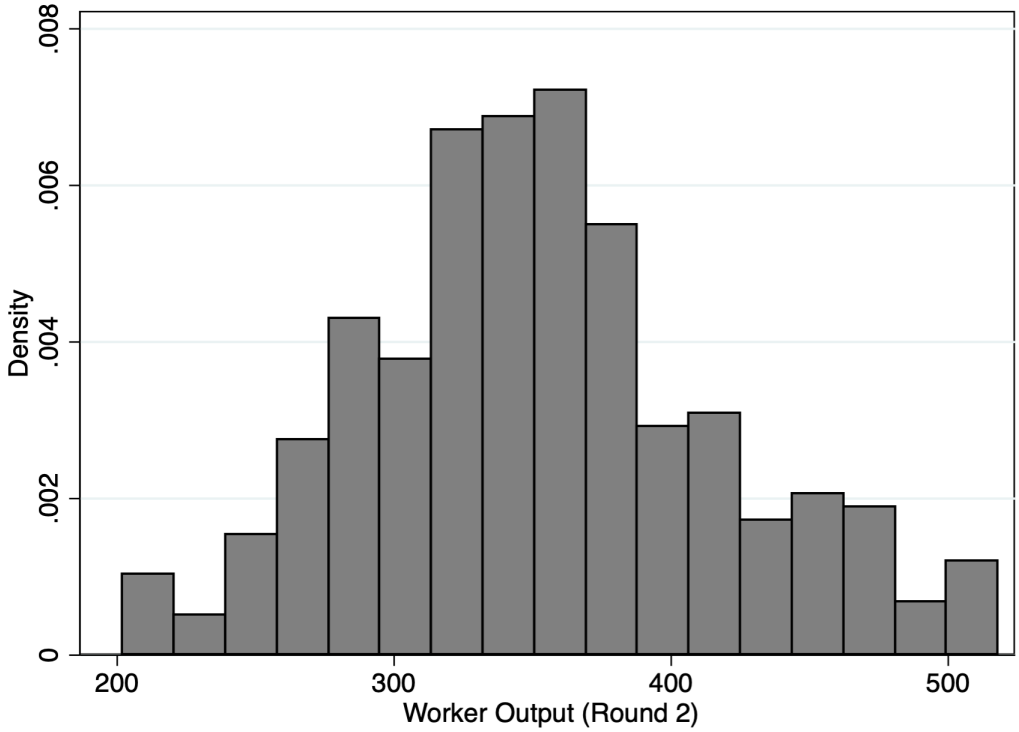
Table A.2: Bosses set Piece Rates

	(1)	(2)	(3)	(4)
Dependent Variable:	Rate	Rate	Beans sorted	
Sample:		Congruent		
Connected Entrep.	0.190		2.513	2.524
	(0.225)		(4.966)	(4.985)
Connected Entrep. × Endog. Rate			-1.103	0.548
			(8.663)	(8.427)
Male Worker × Male Entr. × Conn.		0.889		
		(0.586)		
Fem. Worker × Male Entr. × Conn.		-1.129*		
		(0.639)		
Male Worker × Fem. Entr. × Conn.		1.028		
		(0.901)		
Fem. Worker × Fem. Entr. × Conn.		0.096		
		(0.324)		
Endog. Rate				-15.157
				(12.391)
Endog. Rate^2				1.554
				(2.497)
Control Mean	1.938			
Observations	76	62	313	313
Output Basel. Round	-	-	Yes	Yes
Session FE	Yes	Yes	Yes	Yes
Gender Comp. FE		Yes		

Notes: The first two columns test whether entrepreneurs who were randomly selected to remain matched with the worker they brought to the session ("Connected Entrep.") choose a different piece rate to be paid to the worker for every gram of correctly sorted beans compared to entrepreneurs re-matched with workers unknown to them. The sample is from eight sessions (out of 32) in which entrepreneurs set the piece rate. Column 2 tests for this effect separately within worker-entrepreneur pairs of all four possible gender configurations (using "congruent" pairs only - see table notes of Table 2 and section 3.2 in the main text for more details). Column 3 tests whether the effect of working for a socially connected entrepreneur on workers' output is different when the entrepreneur can set the piece rate. Column 4 replicates the previous column, but controls for the endogenous piece rate set by the entrepreneur (by inserting both a linear and a quadratic term). All the specifications control for session fixed effects. Robust standard errors in parentheses: * $p<.1$; ** $p<.05$; *** $p<.01$

Appendix B

Figure B.1: Histogram of Output
(Grams of Beans Sorted in Round 2)



Notes: Histogram of output in Round 2, when workers were subject to the main experimental variation discussed in the paper. Output is measured in grams of correctly sorted beans by colour.

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Abstrakt

Mnoho firem najímá pracovníky pomoci sociálních sítí. Zda pracovníci, kteří jsou sociálně propojeni se svými zaměstnavateli, vyvíjejí v práci větší úsilí, je otevřená otázka. Tuto otázku řešíme prostřednictvím nového experimentu s majiteli malých podniků v Tanzanii. Účastníci jsou spárováni s pracovníkem, který provádí úkol skutečného úsilí, a dostávají odměnu, která závisí na úsilí pracovníka. Někteří majitelé firem jsou náhodně spárováni s pracovníky, se kterými jsou sociálně spojeni, zatímco jiní jsou spárováni s cizími lidmi. Pomoci designu, který je dostatečně schopný detekovat ekonomicky smysluplné efekty, zjišťujeme, že sociální spojení se zaměstnavatelem neovlivňuje úsilí pracovníků.

Working Paper Series
ISSN 2788-0443

Individual researchers, as well as the on-line version of the CERGE-EI Working Papers (including their dissemination) were supported from institutional support RVO 67985998 from Economics Institute of the CAS, v. v. i.

Specific research support and/or other grants the researchers/publications benefited from are acknowledged at the beginning of the Paper.

Published by
Charles University, Center for Economic Research and Graduate Education (CERGE)
and
Economics Institute of the CAS, v. v. i. (EI)
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Editor: Byeongju Jeong

The paper is available online at https://www.cerge-ei.cz/working-papers/.

ISBN 978-80-7343-570-7 (Univerzita Karlova, Centrum pro ekonomický výzkum a doktorské studium)
ISBN 978-80-7344-693-2 (Národohospodářský ústav AV ČR, v. v. i.)