Social Learning among Urban Manufacturing Firms: Energy-Efficient Motors in Bangladesh*

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Abstract

Knowledge spillovers among firms are widely viewed as a key driver of agglomeration and growth, but are difficult to estimate cleanly. We randomly allocated an energy-efficient motor — a "servo" motor — among leather-goods firms in Dhaka, Bangladesh, and tracked adoption, information flows, beliefs about energy savings, and other variables. We use the difference between actual exposure and expected exposure (from simulated randomization draws) to identify the effect of exposure. We find a robust positive effect of exposure to treated neighbors within a small geographic area (500 meters in our baseline specification) on information flows and adoption. A marginal value of public funds (MVPF) calculation taking learning spillovers into account yields a significantly larger value than one considering only treated firms and suggests that adoption subsidies would be a cost-effective policy intervention.

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1 Introduction

Knowledge spillovers among firms have long played a key role in economists' thinking about agglomeration and growth. One of the main reasons firms locate near one another, in Alfred Marshall's famous view, is because in such clusters "[t]he mysteries of the trade become no mysteries; but are as it were in the air" (Marshall, 1890).[1] Knowledge spillovers are the core mechanism generating increasing returns at the sector level in the classic growth models of Romer (1986, 1990) and Lucas (1988).[2] They are also one of the most commonly cited rationales for industrial policy (Harrison and Rodríguez-Clare, 2010; Juhász et al., 2023). Such spillovers are especially salient in the adoption of energy-efficient technologies, given the widely held goal of reducing global emissions.

Knowledge spillovers are also difficult to estimate empirically, particularly among urban manufacturing firms. There are several related challenges. One is the familiar issue that correlated unobservables may lead firms to make similar technology decisions even in the absence of spillovers, well known since the work of Manski (1993). A second challenge is measurement: datasets on manufacturing firms rarely contain direct information on the technologies being used, and researchers have often had to rely on residual-based measures such as total factor productivity (TFP), which are subject to varying interpretations. A third challenge is that a common approach to estimating spillovers — a saturation design with varying intensity of treatment across a large number of independent clusters — is unlikely to be feasible among manufacturing firms, both because few industries have a sufficient number of distinct clusters of firms using similar technology and because information is likely to flow across clusters.

In this paper, we present the results from an experiment among leather-goods producers in Dhaka, Bangladesh, which allows us to estimate knowledge spillovers cleanly. We randomly allocated energy-efficient motors — called "servo" motors — for stitching machines. The motors reduce electricity use by 70-75% and are perceived by managers and workers to be attractive on other dimensions as well. The servo motors can be swapped seamlessly for traditional "clutch" motors on existing machines and require no other changes in the production process — a fact that many producers were unaware of before our intervention. Our research design allows us to address the three challenges

[1] On the role of knowledge spillovers in generating agglomeration, see also, for instance, Jacobs (1969), Glaeser et al. (1992), Henderson et al. (1995), Duranton and Puga (2004), Ellison et al. (2010), and Davis and Dingel (2019).

[2] For more on the role of such spillovers in growth, see for instance Grossman and Helpman (1991), Aghion and Howitt (1998), Black and Henderson (1999), Hausmann and Rodrik (2003), Duranton (2007), Acemoglu (2009), and Stiglitz and Greenwald (2014).

highlighted in the previous paragraph. First, the experiment generated clearly exogenous variation in exposure to the technology. Second, we observe directly whether firms adopted the motors, as well as information flows between firms and several other relevant variables. Third, applying recent methodological advances on estimation in the presence of interference across units, we are able to estimate both direct treatment effects and spillovers in a single connected set of firms.

The experiment had three arms. In the first, which we refer to as the "information only" arm and label T1, we provided a video about servo motors, explaining the cost savings and showing how to replace a clutch motor with a servo motor on an existing machine. In the second, more intensive, arm, which we refer to as the "information and installation" arm and label T2, we provided the video and also installed a servo motor on one machine and electricity meters on two machines, one with the new servo motor and one with a clutch motor. In this arm, we also gave reports to firms showing the electricity usage per hour of the two machines with meters. A third arm, labeled C, served as control. As explained below, we implemented the treatments in a somewhat non-standard way, by varying the underlying price distributions in a Becker, DeGroot and Marschak (1964, hereafter BDM) procedure to elicit firms' willingness-to-pay for a new motor. To be included in the randomization sample, firms had to have at least two stitching machines with clutch motors and to pay for their electricity on a per-unit basis. Using these criteria, there were 505 firms in the randomization sample, which we divided equally among the three arms.[3]

To estimate the direct effects and spillover effects of the treatments simultaneously, we apply the "re-centering" approach of Borusyak and Hull (2023). We construct a measure of expected exposure to an intensively treated (T2) firm, by simulating a large number of counterfactual treatment assignments and taking the mean exposure across simulation draws. We use variation in actual exposure conditional on expected exposure to identify the effect of exposure. Controlling for expected exposure absorbs differences in unobservable characteristics between firms that are more central in networks, for instance because they are located near many other firms, and firms that are less central.

Our baseline specification is a regression of an indicator for adoption of a new servo motor on treatment indicators, exposure within a very local area, and expected exposure, as well as neighborhood and stratum effects. In our preferred specification, exposure is

[3]We implemented two cross-cutting interventions, with incentivized belief elicitation for some firms in all three arms and electricity meters on clutch motors for some T1 firms. These interventions had little effect and we pool firms in the three main arms in our baseline results, with caveats we discuss below.

defined as having a T2 firm within a walking distance of 500 meters. The key finding is a robust positive impact of very local exposure on servo motor adoption: a firm exposed to at least one T2 firm within 500 meters, controlling for expected exposure within that distance, was 16-19 percentage points more likely to adopt at least one new servo motor than a firm without such exposure. This pattern is robust to the distance used to define "local." When we allow for different levels of local exposure — 1-2 firms, 3-5 firms, 6-8 firms or more than 8 T2 firms within 500 meters — the results are monotonically increasing in the intensity of exposure. We also find direct impacts of the two treatments, T1 and T2, on adoption. The effect of T2 on the first new servo motor adoption is mechanical but we also find a significant, non-mechanical effect of 9 percentage points on what we refer to as "intensive adoption" — adoption of two or more servo motors. We also find a positive impact of 9-10 percentage points of T1, the information-only treatment, on adoption. A striking finding is that having a T2 firm nearby had a larger impact than directly receiving information about servo motors in video form.

The rich data we have collected shed light on the mechanisms through which very local exposure has an effect on adoption. We directly observe information sharing: firms locally exposed to a T2 firm were more likely to have spoken to a T2 firm about servo motors and to have seen an electricity-meter report from a T2 firm. We also find that the exposure effects were driven by exposure to T2 firms producing similar products (i.e. shoe producers were mainly influenced by shoe producers, and bag producers by bag producers). At the same time, while we see direct effects of our experimental interventions on beliefs, willingness-to-pay, and firm-level outcomes such as operating costs, we do not see impacts of local exposure on these dimensions. A possible interpretation is that local exposure persuaded firms to adopt the new motors without leading them to fully update their understanding of the energy savings.

We also explore spillovers through channels other than geographic proximity. We define network exposure as having at least one T2 firm within a firm's communication network (the set of business contacts reported by the firm). Although we find that network exposure led to firms discussing servo motors with T2 firms, we find little impact on adoption, beliefs, or willingness-to-pay. This pattern is consistent with the idea that "seeing is believing" — i.e., that before adopting the motors, managers want to see them in operation, which is easier to do at neighboring firms. We also examine the possibility of what have been called "shared supplier spillovers" (Kee, 2015), which we define as sharing an input supplier or a repair technician with at least one T2 firm, but we find little evidence of spillovers on adoption through this channel. We also construct a measure of whether an owner or manager attends the same mosque as the owner or manager of a T2 firm, which we expect to capture incidental face-to-face contact, but we find little evidence of impact through this channel.

Overall, we consider the results to be strong evidence of local learning externalities from adoption of servo motors in our setting. Given these externalities, a natural question is to what extent governments or concerned non-governmental organizations (NGOs) should be willing to subsidize their adoption. If we focus solely on the reduction of emissions from the energy savings of servo motors, using current estimates of the Social Cost of Carbon (SCC) but ignoring the knowledge spillovers, then subsidizing servo motor adoption is not a particularly cost-effective way to reduce carbon emissions. But if we take both the climate externality and the learning externality into account, then such subsidies are revealed to be much more attractive. To quantify the benefits, we calculate the Marginal Value of Public Funds (MVPF) following the approach of Hendren and Sprung-Keyser (2020) and in particular the extension of Hahn et al. (2024), who show how to incorporate learning effects into MVPF calculations. Taking into account both emissions externalities and learning externalities, we arrive at an MVPF of 5.14. Comparing MVPFs for 96 environmental policies studied over the past 25 years, Hahn et al. (2024) consider an MVPF greater than 5 to be large. While an MVPF of 5.14 is not the largest among the interventions they consider, it appears that subsidizing servo motors legitimately deserves attention as an environmental policy intervention. More generally, a key implication is that calculations about environmental policies should take learning externalities as well as emissions externalities into account.

Beyond the studies cited above, our paper is related to several literatures. It is perhaps most closely related to the literature on local knowledge spillovers within cities. Among advertising agencies in southern Manhattan, Arzaghi and Henderson (2008) find that there is a profit benefit to locating very near other agencies, which decays quickly with distance beyond 250 meters. Using cell-phone records in Silicon Valley, Atkin et al. (2022) find an impact of arguably serendipitous face-to-face meetings on innovation, as evidenced by patent citations across firms. Using Canadian administrative data, Baum-Snow et al. (2024) find spillovers on revenues and productivity from co-location within 75 meters of peer firms. For reviews of the literature on learning within cities, see e.g. Henderson (2007), Rosenthal and Strange (2020), Kerr and Robert-Nicoud (2020), and Duranton and Puga (2020). Other notable studies of social learning by non-agricultural firms, which do not focus on micro-geography within cities, include Jaffe et al. (1993), Irwin and Klenow (1994), Thornton and Thompson (2001), Moretti (2004), Greenstone et al. (2010), Combes et al. (2012), Bloom et al. (2013), Bisztray et al. (2018), Bloom et al. (2019), Alfaro-Ureña et al. (2022), Myers and Lanahan (2022), and Giroud et al. (2024). Relative to this literature, the key advantage of our study is the random variation in exposure to the technology. In the absence of a true experiment, the above studies must make potentially controversial assumptions about exclusion restrictions (for instance in the instrumental-variable approaches of Arzaghi and Henderson (2008) and Atkin et al. (2022)) or other aspects of the distribution of unobserved characteristics in order to draw inferences about the causal effect of exposure to other firms.

There is a small experimental literature on knowledge spillovers among non-agricultural firms. Perhaps the closest paper is Cai and Szeidl (2018), which addresses the Manski (1993) issue by manipulating firms' networks. The authors randomly induced Chinese managers to attend monthly meetings and find large effects on firm performance and flows of information among members of the groups, with evidence of less sharing of "rival" information. Also related are studies by Brooks et al. (2018), Fafchamps and Quinn (2018), Hardy and McCasland (2021), and contemporaneous studies by Houeix (2025), Hajdini et al. (2025), and Gechter and Kala (2025).[4] Relative to these studies, a distinctive aspect of our paper is that we focus on a naturally occurring production technology and how it diffuses in existing networks. The consideration of distance in an urban setting is also a differentiating feature.

There is a large literature on social learning in agriculture in developing countries, reviewed by Magruder (2018) and Suri and Udry (2022). Notable contributions include Bandiera and Rasul (2006), Conley and Udry (2010) and the experimental studies of Hanna et al. (2014), Beaman et al. (2021), BenYishay and Mobarak (2019), Meriggi et al. (2021), Kelley et al. (2025), Duflo et al. (2023), Patel (2024), and Cefalà et al. (2024). We see our study as a useful complement to the existing agricultural literature for two main rea-

[4]Brooks et al. (2018) randomly match inexperienced female micro entrepreneurs with a mentor from the same community and industry to examine the impacts of mentorship on micro enterprise performance. Fafchamps and Quinn (2018) put entrepreneurs together to judge business-plan competitions and find some evidence of spillovers on tax registration and having a bank account but not on management practices or innovation. Hardy and McCasland (2021) randomly allocate training in a new weaving technique and randomly place orders for garments using the technique among small Ghanaian garment producers. Houeix (2025) focuses on diffusion of a digital-payment technology among taxi drivers and owners in Senegal. Hajdini et al. (2025) randomly provide information of official forecasts of GDP growth and find evidence of spillovers through communication between firms. Finally, the recent paper by Gechter and Kala (2025) analyze the effects of randomly varying firms' geographic neighbors but does not focus on knowledge spillovers.

sons. One is that urban manufacturing firms are important in their own right for both economic growth and climate change mitigation. A second is that our setting has features worthy of study that are often absent in agricultural settings: imperfect competition among producers, which may generate an incentive to hide information, and dense concentration of producers in a small geographic area, which make very local spillovers particularly salient.

Our paper is related to a literature in environmental economics on the adoption of energy-efficient technologies. As Jaffe et al. (2005), Popp et al. (2010), Gerarden et al. (2017) and others have pointed out, adoption of such technologies is subject to dual market failures: both the climate externality (emissions created by electricity generation) and the learning externality (the knowledge spillovers analyzed here) can be expected to lead to underinvestment relative to the social optimum. Understanding these dual market failures and how they interact is crucial for the design of effective policies both to mitigate climate change and to stimulate growth. Notable studies of the adoption of environmentally-friendly technologies include Anderson and Newell (2004), Allcott and Rogers (2014), Newell and Siikamäki (2014, 2015), Allcott and Taubinsky (2015), Ryan (2018), Adhvaryu et al. (2020), Berkouwer and Dean (2022) and Chowdhury et al. (2025). Much of this work has focused on households, rather than firms. We are aware of three experiments on adoption of environmentally friendly technologies by firms — Atkin et al. (2017) on adoption of waste-reducing cutting dies by soccer-ball producers in Pakistan, Brooks et al. (2025) on improved practices for zigzag brick kilns in Bangladesh, and Garg et al. (2025) on workplace air purifiers in ready-made garment manufacturers, also in Bangladesh — and none of these focuses on knowledge spillovers.[5]

Our paper is also related to studies estimating spillover effects in other contexts. Duflo et al. (2008) highlight three broad approaches: the saturation designs mentioned above (see e.g. Duflo and Saez (2003), Crépon et al. (2013), Baird et al. (2018)); experimental manipulation of networks, as in Fafchamps and Quinn (2018) and Cai and Szeidl (2018); and using the variation generated by randomization controlling for expected exposure, as in Miguel and Kremer (2004). Our study falls in the third category; the Borusyak and Hull (2023) re-centering approach can be understood as an analogue of the Miguel and Kremer (2004) approach of controlling for the density of schools nearby in their clas-

[5]Summing up the literature on energy-efficient technology adoption as of 2017, Gerarden et al. (2017, p. 1490) write, "We are not aware of any direct empirical evidence of [learning-by-doing] in energy efficiency, and none about the extent of any learning spillovers." Since then, the closest project focusing on spillovers we are aware of is a contemporaneous study by Banares-Sanchez et al. (2024), which estimates the effects of Chinese subsidies for the solar industry in a synthetic difference-in-differences approach.

sic de-worming study.

Finally, our paper is related to a number of recent papers on technology adoption in which social learning arguably does not play a central role. Several papers consider the role of strategic complementarities for technologies with benefits that increase with the number of adopters, such as mobile phones (Björkegren, 2019) and digital payments (Alvarez et al., 2023; Higgins, 2024; Crouzet et al., 2023). A related literature explores the reasons why technology adoption may be slow for non-agricultural firms, which may include the misalignment of incentives within firms (Atkin et al., 2017), the need to reorganize production (Juhász et al., 2024), and switching barriers due to sunk costs (Hornbeck et al., 2024). These features are not present in our setting and their absence may explain the relatively fast adoption of servo motors we observe.

The next section describes our setting and Section 3 describes the experiment. Section 4 discusses our econometric strategy. Section 5 presents the results. Section 6 considers the social benefit of subsidies to servo motor adoption. Section 7 concludes.

2 Setting

2.1 Dhaka Leather-Goods Sector

The two main categories of goods produced in the leather-goods sector in Dhaka are bags (and accessories with similar features such as wallets and belts) and footwear. (We included footwear firms in the sample even if they did not use leather in production, because the production process is generally similar to the process for leather footwear.) Within the bags/accessories and footwear categories, product varieties can be loosely sub-categorized as basic (e.g., carry bags, wallets, sandals, slippers), fashion-oriented (e.g., sling bags, vanity bags, oxford shoes, derby shoes), or related to sports and other specific purposes (e.g., travel bags, sports bags, running shoes, water sports shoes). A small number of firms produce clothing.

The firms in our sample are largely located in geographically dense clusters. We focus on the Dhaka division (a rough analogue to Metropolitan Statistical Area in the Bangladeshi context), which comprises Dhaka district and Kishoreganj district (which contains a cluster of firms in *Bhairab* sub-district), pictured in Figure 1A. The majority of firms in Dhaka district belong to one of three dense industrial clusters, *Kamrangirchar*, *Hazaribagh*, and *Bangshal*,[6] as illustrated by Figure 1B. Figure 2 illustrates the remark-

[6]These areas are officially referred to as *thanas* under the Dhaka City Corporation but function like sub-

able geographical proximity of firms in the *Bangshal* area of old Dhaka, where factories are often located on different floors within the same building.

Rents for factory space differ markedly across Dhaka: in *Bangshal*, firms pay on average 58 Bangladeshi Taka (BDT) (or 0.58 U.S. Dollars (USD)) per square foot per month;[7] in *Kamrangirchar*, 44 BDT (0.44 USD); in *Hazaribagh*, 29 BDT (0.29 USD); and elsewhere in Dhaka, 24 BDT (0.24 USD) on average.[8] A natural question is why firms are willing to pay so much higher rents in *Bangshal*, given that the distances to lower-rent sites are not large. For example, the distance from the center of the *Kamrangirchar* cluster to the center of the *Bangshal* cluster is approximately 1.7 miles as the crow flies.

Firms in the sector are predominantly small and medium-sized: median employment in our sample is 6 workers, and employment at the 75th percentile is 12 workers. They predominantly serve the domestic market, selling an average of 48% of their output (by value) directly to domestic retailers and 38% to intermediaries serving the domestic market; the remaining 14% is exported.[9] The concentration of sales within domestic channels (86% on average) suggests that these firms often compete with each other in the same market.

2.2 Electricity Generation in Bangladesh

Bangladesh relies heavily on fossil fuels for electricity generation. In 2021, only about 1% of the electricity on Bangladesh's grid was generated from renewable energy sources such as hydro, solar, and wind. Natural gas accounted for 73.1% and the corresponding shares of oil and coal were 20.1% and 5.5% (IEA, 2023). Unreliable electricity supply also leads to the extensive use of diesel-fueled backup power generators (Tong and Zhang, 2015).

The consumption of electricity is effectively subsidized in Bangladesh. In the 2022-2023 fiscal year, the cost of generating electricity for the Bangladesh Power Development Board (BPDB), the state-owned electricity utility, was 11.3 BDT (0.11 USD) per kilowatt-hour (kWh). The BPDB also acquired electricity from independent producers (which constituted 63% of the total electricity supply) at 14.6 BDT per kWh. The average price paid by firms in our sample was 12.5 BDT per kWh, above the BPDB's own marginal cost

districts (*upazilas*).

[7]The exchange rate over our study period was approximately 100 BDT/USD and for convenience we use this exchange rate throughout.

[8]These figures are from a phone survey we conducted post-endline with 80 firms randomly selected from four clusters: *Bangshal*, *Hazaribagh*, *Kamrangirchar*, and rest of Dhaka City (20 firms per cluster). We excluded Kishoreganj district from the phone survey.

[9]This information comes from the phone survey described in footnote 8.

of electricity but below the independent producer price. The BPDB also incurs substantial fixed costs which are covered by the government. In the 2022-23 fiscal year, the government provided a subsidy of more than 395.35 billion BDT. In short, the price that would allow BPDB to break even was well above the 12.5 BDT per kWh paid by firms in our sample. In 2023, under pressure from the IMF, the government raised electricity prices at the retail level in three consecutive months — January, February and March — by about 5% each month (Byron, 2024), but prices remained well below average production cost.

The social cost of producing electricity is even higher, given carbon emissions. Despite the fact that natural gas generates less carbon emissions than coal, the power generation sector is still a major source of growing emissions in Bangladesh. In 2022, electricity production accounted for 58.1% of the country's carbon emissions (IEA, 2023). The industrial sector accounted for 42.5% of the total final consumption of electricity in the country (IEA, 2023).

2.3 Servo vs. Clutch Motors

Figure 3 shows photos of a clutch motor and a servo motor. Traditional clutch motors work by magnetic induction: an electrical current passes through an outer chamber and creates a rotating magnetic field which leads a rotor to spin, which (when the clutch is engaged) powers the movement of the needle. It takes time to generate the rotating magnetic field; the motor needs to "warm up." As a result, machine operators typically leave the motor on even when the needle is not moving. It is estimated that the needle moves only about one-third of the time a worker is sitting at the machine. Hence a significant share of the energy use of a clutch motor is from idling. In contrast, servo motors have a powerful magnet built in. There is no "warm up" period; the motor requires electricity only when the needle is moving.

Using data collected in our experiment (described in more detail below), we calculate that a machine with a clutch motor consumes 0.064 kWh per hour of use, while a machine with a servo motor consumes only 0.019 kWh per hour. This represents a 71% reduction in electricity consumption — close to the 75-80% reduction that producers of servo motors claim in their marketing materials. The local market cost of the new servo motor used in our experiment is 4,600 BDT (46 USD). Given a mean electricity price of 12.5 BDT/kWh and assuming that a stitching machine is used for 160 hours per month, we calculate that switching from a clutch to a servo motor reduces electricity costs by 90

BDT (0.90 USD) per month. This implies a payback period of 51 months. While this implies a very respectable return on investment of 23.4% per year (12*.9/46), the payback period is longer than the rule-of-thumb cutoffs that many firms, even in the U.S., use to make technology decisions (Anderson and Newell, 2004).

Servo motors can be switched seamlessly for clutch motors on existing machines. The switching process involves simply unscrewing the bolts holding the existing clutch motor from the stitching table, attaching the servo motor, and adjusting the belt connecting the motor to the stitching component. The process takes approximately 20 minutes.

Servo motors are not a new technology, but they have not fully diffused in the Dhaka leather-goods sector. They are standard equipment on most new sewing machines, but many older machines with clutch motors are still in use. Awareness that servo motors can be easily substituted for clutch motors was not universal at the beginning of our intervention: in our baseline survey (described below), 45% of firms reported that they were unaware that servo motors could be swapped for clutch motors on existing machines. The share of firms with a servo motor was low: at baseline, 16% of firms reported that they had a servo motor in their factory and servo motors made up approximately 12% of motors in use.

Although energy savings are the main advantage of servo motors, we also received positive reports about several other features. Table 1 presents managers' opinions and their reports of employees' opinions, using information from our endline survey (described below). Among managers who perceived a difference between servo and clutch motors, the ratings of servo motors were 4.5 or higher on a 5-point scale for speed, noise, stitching quality, comfort of the operator, and amount of heat generated. Among all managers who had used a servo motor, the average perception of durability/reliability of the servo motors was 4.04 and average overall satisfaction was 4.87. We asked managers whether they had spoken to their employees about the servo motors and the opinions that they expressed. The average employee overall opinion of servo motors was 4.76 on a 5-point scale. In this sense, the current setting differs from the Pakistani soccer-ball cluster studied by Atkin et al. (2017), where employees were resistant to a new technology. As noted above, this may explain in part why we have seen more adoption in this context.

3 Experiment

3.1 Sample

We conducted two listing exercises to find firms suitable for our experiment: a phone survey and a door-to-door survey. The phone survey was conducted in two phases between May and August 2021. (We had initially planned to do only a door-to-door listing exercise but had to start with a phone survey due to COVID lockdowns.) We used multiple datasets to get a list of firm phone numbers for the phone listing exercise.[10] We also conducted snowball sampling from this list. Once the COVID situation improved, we conducted a door-to-door listing survey in September-October 2021.

To be included in our sample, we required leather goods and footwear firms (i) to have at least two stitching machines with clutch motors, (ii) to have a separate electricity meter, and (iii) to pay an electricity bill on a per-unit basis.[11] There were 505 eligible firms that agreed to participate in the BDM and belief-elicitation exercises (described below) and were included in our randomization sample. Of these, 473 continued to participate until the endline.[12]

We conducted the baseline survey in April 2022. We conducted a BDM exercise to elicit willingness-to-pay (WTP) for servo motors and an elicitation of beliefs about electricity consumption by servo and clutch motors (described in more detail below) in a second visit in Oct.-Nov. 2022; we call this the "initial BDM visit." We conducted a midline survey in April 2023 and an endline survey in Oct. 2023; both visits included BDM and belief-elicitation exercises. In most cases, the respondent was the owner (who was also the factory manager). In cases where the owner was not involved in decision-making about technology, we interviewed the production manager. Hereafter we will refer to respondents as "managers."

3.2 Treatment Arms

There were three main treatment arms: information only (T1), information and installation (T2), and Control (C). Assignment to the arms was carried out through the BDM

[10]The datasets are: the 2013 Bangladesh Economic Census 2013, the 2013 and 2019 Bangladesh Business Directories, and member lists from the Leather Goods and Footwear Manufacturers and Exporters Association of Bangladesh, Sports Shoes Manufacturers and Export Association, Bangladesh Paduka Prostutkarok Samity [Bangladesh Footwear Manufacturers' Association], Bhairab Footwear Factory Owners' Association, and the Bangladesh Finished Leather, Leather Goods and Footwear Exporters' Association.

[11]For requirements (ii) and (iii), it was sufficient to have a "sub-meter" installed by their landlord and pay the latter on a per unit basis. Firms that paid their landlords a fixed amount for electricity did not meet our eligibility conditions.

[12]With the exception of one refusal to respond at midline, all attrition from the randomization sample was due to firm closures.

procedure as described in Sections 3.3 and 3.5 below. Here we first describe the content of the information provided.

3.2.1 Information-Only Arm (T1)

Firms in T1 were provided detailed information about servo motors in a 5-minute video shown on a tablet. The video showed servo motors in operation in firms similar to those in our sample. It explained that servo motors use 75% less electricity than clutch motors and walked through a calculation of electricity costs for machines with servo and clutch motors. The video also explained that clutch motors can be easily replaced by servo motors in about 15-20 minutes using ordinary tools and showed a step-by-step demonstration of the replacement procedure.

As noted above (footnote 3), we subdivided this treatment arm into two subgroups. In the first subgroup (labelled T1a), we only showed the informational video. In the second subgroup (labelled T1b), we showed the video and installed an electricity meter on one stitching machine with a clutch motor. The meter displayed and stored electricity usage data on the specific machine on which it was installed. We collected these data every month. Between January and June 2023, we provided firms with monthly reports about electricity usage on the metered machine (described in greater detail below). The intent behind the T1b treatment was to examine whether making electricity consumption by stitching machines more salient — while not explicitly comparing it across clutch and servo motors — has an impact on beliefs and adoption.

3.2.2 Information and Installation Arm (T2)

Firms in T2 were given a more intensive information intervention. Through the BDM procedure described below, we replaced a clutch motor with a servo motor on one stitching machine. We asked T2 firms to show us their two most-used clutch motor machines and randomly picked one of those machines for servo motor installation. We also installed electricity meters on both machines, the machine with the new servo motor and the other machine (not selected for the servo motor).

We provided monthly electricity-usage reports to T2 firms based on monthly meter readings between January and June 2023. For each of the two machines with meters, the reports showed (i) the past month's total electricity consumption, (ii) total hours of usage, and (iii) average electricity consumption per hour.[13] The recipient firm could easily

[13]Because the meters did not record the exact fraction of an hour for which the machine was in use, we

compare electricity usage on its clutch and servo machines by looking at the report. Figure 4 presents an example. (Each T1b report contained the same summary statistics, but only for the recipient firm's meter-equipped clutch motor machine, corresponding to the blue bars in Figure 4.)

3.3 Eliciting Willingness-to-Pay

We elicited managers' willingness-to-pay for a servo motor through a Becker-DeGroot-Marschak (BDM) procedure along lines that have been widely used elsewhere (see e.g. Berry et al. (2020)). Respondents were first provided with a receipt for BDT 10,000 (USD 100) and informed that they would receive this "incentive amount" in their mobile money account once they fully completed the exercise. They were also informed that if they purchased a servo motor in the exercise, we would reduce the incentive amount by the price of the servo motor.

We employed an ascending-auction format: we started by stating a very low price and asking if the respondent would purchase the item at that price; if the respondent said yes, we raised the price in small increments until he or she said no. We then presented the respondent with a bag, explained that it contained chits with random prices, and asked them to pick a chit from the bag. If the price drawn from the bag was equal to or lower than the respondent's stated maximum price, then the respondent received the servo motor at the price displayed on the chit, and we reduced the incentive payment by that price. If the respondent's maximum price was lower than the price on the chit, then he or she did not receive the servo motor and received the full 10,000 BDT incentive payment. The distributions of prices in the bags of chits varied by treatment group, as described in Section 3.5. When conducting the exercise, we informed respondents about the support of the distribution, which was between 0 and 10,000 BDT, but we did not state the distribution itself.[14] An advantage of implementing the randomization in this way is that it tended to minimize Hawthorne-type effects, since managers were unaware of which treatment arm they were in. Further details are in Appendix A.1.

measure total hours as total number of hours with positive electricity usage. This almost certainly overstates the true number of usage hours.

[14]In a related BDM exercise, Burchardi et al. (2021) vary whether the distribution is stated or unstated to respondents and find that bids are unaffected.

3.4 Eliciting Beliefs about Electricity Usage

In the belief-elicitation exercise, we asked managers to imagine a firm similar to theirs and to state their beliefs about the electricity usage of a typical stitching machine on an average day with an eight-hour shift and normal scheduled breaks (a) with a clutch motor and (b) with the clutch motor replaced by a servo motor. On a tablet, we divided the range of possible answer values into ten bins and asked respondents to allocate 10 matchsticks among those bins. We explained that each matchstick represented a 1 in 10 chance, and conducted two practice rounds using other questions. Procedures of this type have been extensively used in field studies (Delavande et al., 2011).

To understand whether incentives improve respondents' answers, we implemented a cross-cutting randomization, with half of our sample in each treatment group participating in an incentivized belief-elicitation exercise and half in a non-incentivized one. In the incentivized exercise, the reward for how close the response was to the true answer was determined by the Quadratic Scoring Rule (QSR), a commonly used method in experimental economics (Schotter and Trevino, 2014; Schlag et al., 2015). Respondents were informed that the reward would be disbursed after the endline survey.

The belief-elicitation exercise was conducted using a tailor-made tablet-based application. Figure 5 presents a screenshot of the app interface for the incentivized group. On this app, respondents could allocate matchsticks to the bins using a drag-and-drop feature. Once all 10 matchsticks were allocated, a table appeared showing the 10 potential reward amounts from that allocation (with each potential amount corresponding to one of the 10 bins being correct). This table updated as respondents' changed their allocations, allowing them to see in real time how their potential rewards varied with their allocations. More details are in Appendix A.2.

3.5 Randomization Procedure

We embedded the randomization into the baseline BDM procedure by varying the distribution from which the BDM price was drawn. For the control arm, the distribution had almost all prices near the maximum of the support of the distribution (10,000 BDT). As a result, the BDM price was almost always above the stated baseline WTP, and almost no control firm received the servo motor. The distribution for T1 was similar, ensuring that almost no T1 firm purchased the servo motor. For the T2 arm, the distribution had almost all prices at or very near zero. As a result, almost all T2 firms received the servo motor. At midline and endline, we no longer sought to give out servo motors at a low price, so we used the distribution with high prices for all treatment groups (i.e. T2 as well as T1 and C).[15]

We randomized the treatment within strata defined by product type and firm size. The product categories we used were (i) only shoes, (ii) only bags, or (iii) mixed, and the firm size groups were defined by the number of stitching machines in operation: (i) 2 machines, (ii) 3-5 machines, or (iii) 6 or more machines. In total, there were 9 strata.

3.6 Balance Tests

Table 2 compares the characteristics of the control and treatment arms in the baseline sample of 505 firms and reports t-tests comparing the means of each treatment group to the control group. None of the tests are significant at the 10% level, suggesting that the randomization succeeded in achieving balance. Appendix Table A.1 repeats the balance test for the set of 473 non-attriters, using information from the baseline survey. None of the t-tests are significant at the 10% level, except for the one comparing the respondent's years of education between control (mean of 5.96 years) and T1 (mean of 6.65 years), which has a p-value of 0.10. This suggests that non-random attrition was not a major issue.

4 Econometric Strategy

Our goal is to estimate the direct effect of our T1 and T2 interventions along with the spillover effect of being exposed to a treated firm. The challenge in estimating exposure is that firms that are systematically more exposed to treated firms, for instance because they are located in denser neighborhoods or are more central in communication networks, may differ in unobservable ways from those that are less exposed, which could generate omitted variable bias. To remove this bias, we follow Borusyak and Hull (2023) in constructing a measure of *expected* exposure to treated firms. We can think of observed exposure as one realization of a shock assignment process and simulate the shock assignment process many times (1,000 times in our implementation) to generate counterfactual values of exposure. The average of these draws is a firm-level measure of expected exposure. Using this measure, Borusyak and Hull (2023) suggest two related pro-

[15] At baseline, 3 control firms (out of 169), 3 T1 firms (out of 176, 2 from T1a and 1 from T1b), and 158 T2 firms (out of 160) received a servo motor from us. At midline, 2 control firms, 1 T1 firm (from T1a), and 4 T2 firms received one. At endline, 3 control firms, 2 T1 firms (both from T1a) and 6 T2 firms received one.

cedures. One is a control-function approach, which includes the expected exposure as a control variable in a regression of an outcome on firms' treatment assignments and actual, observed exposure. The second is to use "re-centered" exposure, the difference between the observed and expected exposure, as an instrument for observed exposure. In either case, it is the difference between actual and expected exposure that identifies the coefficient on exposure. We primarily use the control-function approach; results using using re-centered exposure as an instrument are very similar.

In our baseline specifications, we assume a simple parametric form for exposure: an indicator for whether a firm has a T2 firm within a certain walking distance. We estimate ANCOVA regressions of the form:

$$\begin{aligned} y_{ijs} &= \beta_0 + \beta_1(T1_i) + \beta_2(T2_i) + \beta_3 Exposure^d_{T2,i} \\ &\quad + \beta_4(\mathbb{E}[Exposure^d_{T2,i}]) + \lambda_j + \upsilon_s + y^{baseline}_{ijs} + \varepsilon_{ijs} \end{aligned} \tag{1}$$

where y_{ijs} is an outcome for a firm i at endline, in stratum j, located in sub-district (upazila) s. $T2_i$ and $T1_i$ are 0/1 indicators of the corresponding treatment groups. We include strata (λ_j) and subdistrict (υ_s) fixed effects in all our specifications. $Exposure^d_{T2,i}$ is an indicator for whether a firm is exposed to a T2 firm within walking distance d (500 meters in our baseline specification). $\mathbb{E}[Exposure^d_{T2,i}]$ is the average value of $Exposure^d_{T2,i}$ from 1000 randomization simulations. To increase precision, we also include baseline values of the outcome variable, $y^{baseline}_{ijs}$; ANCOVA specifications of this type have been found to increase statistical power over specifications with firm fixed effects (McKenzie, 2012).

We are primarily interested in the coefficients β_1, β_2, and β_3. Coefficients β_1 and β_2 capture the direct effects of treatments T1 and T2. The coefficient β_3 estimates the effect of exposure to at least one T2 firm within a walking distance of d meters. For statistical inference, we present two sets of standard errors. The first, reported in parentheses below, are Conley (1999, 2010) standard errors, which allow flexibly for spatial correlation. In square brackets, we also report standard heteroskedasticity-robust standard errors. Following Borusyak and Hull (2023), for our main results we also use the shock counterfactuals to conduct randomization inference and provide p-values for the coefficient on exposure.[16]

As equation 1 is written, the spillover effect, β_3 is restricted to be the same for firms

[16] We provide details of the randomization inference procedure in Appendix B.

across groups (T1, T2, control). However, since T2 firms were intensively treated, the spillovers on them from other T2 firms are likely to differ from the spillovers on firms that were not given a servo motor (T1 and C). To isolate spillovers on the latter group, we also estimate an alternate version of equation 1 in which we drop T2 firms from the sample.

As mentioned above, we implemented two cross-randomizations. Firms within the T1 group were subdivided into T1a (informational video only) and T1b (informational video and an electricity meter on a clutch motor machine); see Section 3.2.1. Also, in all groups, half of the firms participated in an incentivized belief elicitation; see Section 3.4 and Appendix C.2. Neither cross-randomization is crucial to our experiment. In presenting our results, we pool T1a and T1b firms and incentivized and non-incentivized firms. (The results considering the effects separately, discussed in Appendix C.1.2, are very similar.) The practice of pooling in this way, while common, has been called into question by Muralidharan et al. (forthcoming). As they make clear, our estimates should be interpreted as weighted averages of effects conditional on the cross-cutting treatments, rather than as estimates of unconditional average treatment effects.

5 Results

5.1 Simple Comparisons by Treatment Group

We begin by presenting simple comparisons of our main outcome variables across survey rounds and treatment groups (T1, T2, C). These comparisons should not be interpreted as causal, since they do not address the issue of interference across units from spillovers, but they are useful to illustrate salient patterns in the data. Figure 6 plots servo motor usage across groups at baseline, midline, and endline. We include two measures of usage: whether a firm used at least one servo motor on a stitching machine and whether a firm used more than one servo motor on stitching machines. The increase in the fraction of T2 firms that used at least one servo motor is mechanical; T2 firms effectively received one servo motor during the intervention. But the fraction of T2 firms using two or more servo motors is not mechanical and also increased. We see an increases in both measures of usage for T1 firms, and, perhaps most surprisingly, for control firms.

Figure 7 presents comparisons across survey rounds for beliefs about electricity usage and willingness-to-pay for a servo motor. In Figure 7, we see that beliefs about servo motor electricity usage (the group average of mean beliefs from the distributions reported in the belief elicitation) decline over time for all groups, with a greater decrease for T2 firms. The willingness-to-pay increases from baseline to endline across groups. Given that there is a market for servo motors and that the one we offered in the experiment could be easily resold, one would expect the willingness-to-pay of informed firms to converge to the market price, and indeed that is what we observe. Fairly quickly, willingness-to-pay for all three groups converged to a value just a bit below what we found to be the market price (4600 BDT/46 USD). Again, caution is warranted in interpreting these simple comparisons; we now turn to specifications that allow us to separate spillovers from direct treatment effects.

5.2 Treatment and Local Spillover Effects

In this section, motivated by the literature on literature on knowledge flows within cities (Arzaghi and Henderson, 2008; Atkin et al., 2022; Baum-Snow et al., 2024), we focus on exposure within narrow geographic areas. We consider other forms of exposure in Section 5.3 below. We consider regressions of the form of equation 1, dropping T2 firms in some specifications to focus on spillover effects on T1 and C firms. While we report both Conley (1999, 2010) standard errors (in parentheses) and heteroskedasticity-robust standard errors (in square brackets), our discussion of statistical significance is based on the former.

5.2.1 Adoption of Servo Motors

In Table 3, we consider the effect of exposure to any T2 firm within a walking distance of 500 meters on adoption of servo motors. We consider two measures of adoption: an indicator for whether a firm purchased one or more servo motors (columns 1 and 3) and an indicator for whether a firm purchased two or more servo motors (columns 2 and 4), which we refer to as “intensive adoption.” (We include the motor “purchased” from us in the BDM exercise, if there was one, in these measures.)

In columns 1 and 2, we include T2 firms in the sample. In column 1, we see that firms in the T1 group were 9 percentage points more likely than the control group to have adopted a servo motor by the endline. The fact that the coefficient for T2 firms, which near-automatically received a servo motor from us, is less than one, reflects the fact that some control group firms adopted. The coefficient on exposure indicates that exposure to a T2 firm within 500 m increased the likelihood of adopting a servo motor by 15.8 percentage points. In column 2, we consider the intensive adoption outcome. This outcome is not mechanical for T2 firms; it only takes the value 1 if firms purchased a servo motor beyond the one they received from us. We find that T2 firms were 9.1 percentage points more likely to adopt intensively than the control group, and T1 firms were 5.7 percentage points more likely. Exposure to a T2 firm increased the likelihood of adopting more than one servo motor by 10.5 percentage points for all firms across both treated and control groups.

In columns 3 and 4, we remove T2 firms from the sample. In addition to the fact that one might expect spillovers onto T2 firms to be different from those onto T1 or C firms, one might be concerned that the comparison of T2 to T1 and C firms for intensive adoption in column 2 is not apples-to-apples, since T2 firms only had to purchase one servo motor on their own to be considered intensive adopters, while T1 and control firms had to purchase two. For this reason, the estimates excluding T2 firms in columns 3 and 4 are our preferred estimates. In column 3, we find that T1 firms were 9.9 percentage points more likely than control firms to adopt at least one servo motor, and that exposure to T2 firms in a 500 m radius increased the likelihood of adoption for T1 and control firms by 19.3 percentage points. In column 4, when we look at intensive adoption, we find that T1 firms were 4.8 percentage points more likely to adopt more than one servo motor and that exposure to T2 firms in a 500 m radius increased the likelihood of adoption for T1 and control firms by 10.2 percentage points.

The spillover effects in Table 3 are robust to varying the distance used when defining exposure. In Figure 8, we show the coefficient on exposure at different distances ($d \in \{250$ m, 500 m, 750 m, 1000 m$\}$), using the specification from column 3 of Table 3, where we exclude T2 firms. There is some evidence of decay with distances greater than 500 m, but the decay is not very rapid.

We also examine how the strength of spillover effects varied with the number of T2 firms to which a given firm was exposed in the local area. To do so, we define four measures of exposure based on four bins of the number of T2 neighbors within 500 m: 1-2 firms, 3-5 firms, 6-8 firms, or 9 or more firms. We calculate four expected exposure measures accordingly. The results are in Table 4, which is organized similarly to Table 3. We generally find that the effects of local exposure were positive and monotonically increasing in the number of T2 neighbors, especially when excluding T2 firms from the sample. Although the estimates for intensive adoption in column 2 for the full sample are neither statistically significant nor monotonic, we observe a monotonic pattern for intensive adoption when we exclude T2 firms in column 4 (as is arguably appropriate when focusing on spillovers).

In Appendix C.1, we probe the robustness of the results on adoption in various ways. We report results using the Borusyak and Hull (2023) re-centered IV approach in Appendix C.1.1 and results separating the T1a and T1b groups in Appendix C.1.2. We also present specifications controlling flexibly for neighborhood effects rather than expected exposure in Appendix C.1.3. The results of these explorations are generally similar to the findings reported above. We examine heterogeneity in the adoption responses of firms by various manager and firm characteristics in Appendix C.1.4. Although no overarching narrative about heterogeneity by firm or manager characteristics emerges, there are some interesting patterns. Less-experienced managers were more likely to be influenced by the T1 (video) treatment, while more-experienced managers were more likely to be influenced by neighbors. Lower scorers on a Raven's cognitive test were more likely to be influenced by neighbors than higher scorers.

Overall, we interpret the findings as strong evidence of positive spillovers on adoption at a very local level. Perhaps the most striking finding is that local exposure to a T2 neighbor had a larger effect, on average, than directly receiving information about the servo motors in video form: 19.3 percentage points vs. 9.9 percentage points in our preferred baseline specification (column 3 of Table 3).

5.2.2 Exposure to Producers of Common Products

Next, we explore how being exposed to a neighbor T2 firm that produced a similar or different product affected adoption. In the interest of simplicity, we consider separately two subsets of firms: firms that produce shoes and firms that produce bags.[17] For firms that produce shoes, we define two exposure measures: exposure to T2 firms that produce shoes and exposure to T2 firms that do not produce shoes, along with their corresponding expected exposure measures. We construct similar measures for bag producers: exposure to bag producers and to non-bag producers, and the corresponding expected exposure measures. Table 5 presents the results. For shoe producers, in columns 1 (full sample) and 3 (excluding T2), we observe that exposure to a T2 firm producing any shoes had a large and statistically significant positive effect. In contrast, exposure to a T2 firm that did not produce shoes was statistically insignificant. We observe the same pattern of

[17] We consider a firm to be a shoe producer if it produces at least one shoe and to be a bag producer if it produces at least one bag. There is overlap in these subsets, since some firms produce both.

exposure among bag producers, i.e., they were affected in a statistically significant way by exposure to T2 firms that produces bags, and but not by exposure to T2 firms that did not produce bags. Taken together, the results indicate that it was exposure to T2 firms producing similar products that drove the local exposure effects.

5.2.3 Information Flows

We construct two proxies for information flows between firms. The first is derived from the question, "Did other firms show you their electricity use report card or discuss it with you? Please indicate all the firms that you spoke to." From this question, we construct a 0/1 indicator of whether the respondent was shown a report by a T2 firm. Our second proxy is an indicator for whether the respondent discussed servo motors with a T2 firm.[18] Table 6 reports specifications with these two variables as outcomes. The table is organized similarly to Table 3, including T2 firms in columns 1-2 and excluding them in columns 3-4. Local exposure strongly affected both measures of information flows between firms. In columns 1 and 3, we find that local exposure to a T2 firm increased the likelihood that a firm was shown a meter report card by 2.9 and 1.9 percentage points. In columns 2 and 4, we find that local exposure to T2 firms increased the likelihood that a firm discussed servo motors with a T2 firm by 15.5 and 22.4 percentage points. Figure A.2 plots the exposure coefficients, similar to the column 3 and 4 coefficients of Table 6, for exposure defined at different distances. In short, there is clear evidence that geographic proximity led to direct information flows between firms about the new motors.

5.2.4 Beliefs about Servo Electricity Usage and Willingness-to-pay

In Table 7, we present results for managers' beliefs about electricity consumed (kWh/day) by servo motors and managers' willingness-to-pay for a servo motor. The table is organized similarly to Table 3 but with the mean of each manager's belief distribution as the outcome in columns 1 and 3, and the maximum BDM bid as the outcome in columns 2 and 4. In column 1, we find that T2 firms revised their beliefs downwards significantly by 0.08 kWh/day relative to control firms. Although the coefficients on T1 are negative in both columns 1 and 3 (when excluding T2 from the sample), they are not statistically significant at conventional levels. Notably, there were no significant effects of exposure

[18] We elicited this information in two steps. We first asked respondents which other firms they had spoken to about business, and then asked if they had discussed servo motors with the firm. We coded the variable as one if the manager reported a conversation about servo motors with a T2 firm and zero otherwise (independently of whether firms said they had seen or discuss the electricity report card with a T2 firm).

within 500 m on beliefs.[19] Figure A.3 plots the exposure coefficients (similar to the column 3 coefficient of Table 7) for exposure at different distances (as in Figure 8). There is some evidence of a marginally significant spillover effect of beliefs within a very local area (250 m walking distance). But, overall, there is not very strong evidence of spillover effects on beliefs, despite the robust spillover effects on adoption. It appears that managers may have been convinced to adopt the servo motors by positive signals from neighbors without fully understanding their benefits.

For willingness-to-pay, we find modest positive direct effects of T1 and T2. In column 2, we find that the willingness-to-pay of T1 and T2 firms was 267 BDT and 259 BDT higher, respectively, than that of control firms. In column 4, when we exclude T2 from the sample, T1 firms had a 260 BDT higher willingness-to-pay than control firms. As with beliefs, however, we do not find any statistically significant effect of exposure to T2 firms nearby (in either column 2 or 4). Figure A.3 plots the exposure coefficients similar to the column 4 coefficient of Table 7, for exposure defined at different distances (similar to Figure 8); there is little evidence of a spillover effect on willingness-to-pay at any of distance. As suggested above, the results are consistent with the idea that positive signals from neighbors led managers to adopt servo motors without deepening their understanding of the costs and benefits of doing so.

5.2.5 Costs, Sales, Employment

In Table 8, we consider firm-level cost and performance variables. We look at two measures of firm-level costs: log operating costs per worker (columns 1 and 5) and log electricity costs per worker (columns 2 and 6). In calculating these variables, we use costs in the month prior to the survey and divide by the number of workers employed at baseline to avoid incorporating endogenous employment responses to the intervention. As rough proxies for firm performance, we look at sales and employment. For sales, because some firms had zero sales in the month prior to a survey round, we take the inverse hyperbolic sine transformation (columns 3 and 7). For employment, we consider the log of the total number of employees, including paid and unpaid employees (columns 4 and 8).

Generally, we find mixed evidence of direct effects on these variables and no evidence of spillover effects. There is evidence of negative effects of T2 on costs of operations per worker and electricity costs per worker and less robust but still suggestive evidence of

[19] As mentioned above, we conducted a cross-cutting randomization in which some respondents received incentives for accuracy in their reports in the belief elicitation. We discuss results for incentivized and non-incentivized respondents in Appendix C.2.

an effect of T1 on the same variables. There is little evidence of direct effects on sales or employment. Nor is there evidence of spillover effects on any of the firm-level outcomes we consider. In part, this is likely due to the fact that many factors contribute to the firm-level outcomes, and the signal from the treatment effects may be hard to pick up amid the noise from these other factors. The non-results highlight the advantage of having detailed, direct observations of firm technology use.

5.3 Other Channels of Spillovers

In this section, we consider three other channels through which spillovers may occur: firms' communication networks, shared suppliers, and incidental face-to-face contact.

To measure managers' communication networks, we asked managers in the baseline and midline surveys to list the people they would ask for advice about a new technology. We then matched the listed firms to the directory of firms in our sample. We take the listed firms that are in our randomization sample as the firm's communication network. A challenge in using this network information is that once we restrict to firms in our randomization sample, the managers' communication networks are very sparse. To mitigate this problem, we pool network information from the baseline and midline surveys. While this approach runs the risk that midline communication networks might already have reflected endogenous responses to our intervention, it appears to yield more reliable estimates of the effect of communication-network exposure. We define network exposure as taking the value one if a firm in a manager's communication network received the T2 treatment and zero otherwise.

To measure firms' shared-supplier exposure, we use endline survey responses about which repair shop and motor technician firms used and which suppliers they purchased machinery, equipment, and other inputs from. We define shared-supplier exposure as taking the value of one if a T2 firm also visited the same supplier, repair shop, or motor technician.

To measure likely face-to-face contact, we use an endline question which mosque managers go to most often to offer prayers. We construct a 0/1 indicator of whether the firm manager frequented the same mosque that a T2 firm also frequented.[20]

[20] Although we do not see minute-by-minute location data and cannot be certain that managers meet face-to-face, we note that prayers happen at set times during the day, and if two managers attend the same mosque, it is highly likely that they are often there at the same time. This measure provides information similar to the smartphone geolocation information that is the state of the art in the literature (see e.g. Atkin et al. (2022)).

We estimate specifications of the form in equation 1 with these three exposure measures and the corresponding expected exposure measures. As we are primarily interested in spillovers in this section, we exclude T2 firms from the sample in all regressions. Table 9 reports the results for adoption. In contrast to the results for local exposure in Table 3, we find no statistically significant spillover effects through communication networks, shared suppliers, or shared mosques. Note that if managers endogenously sought out information about servo motors by contacting managers directly (for the communication network) or by interacting with technicians and suppliers known to serve T2 firms (for the shared-spillover network), that would tend to generate a positive bias in our estimates of exposure on adoption. In this sense, our estimates of the exposure coefficients in Table 9 can be considered to be upper bounds of the true effect.

Table 10 reports the results for information flows between firms and includes the same outcome variables as Table 6. The table is organized similarly to Table 9 and excludes T2 firms from the sample. There is some evidence of information flows through communication and shared-supplier links, but not through shared-mosque links. In column 2, we find that communication-network exposure increased the likelihood that firms discussed servo motors a T2 firm. In column 3, we find that shared-supplier exposure led to an increased likelihood of seeing an electricity-meter report of a T2 firm. Table 11 reports results for beliefs and willingness-to-pay. Although there are some marginally significant coefficients, we interpret the results as suggesting that there is little robust evidence of spillover effects on beliefs or willingness-to-pay through these channels.

Overall, the message we take away from this analysis is that these other forms of exposure are substantially weaker than exposure to neighbors within very local areas. The evidence is consistent with the idea that "seeing is believing" — that just talking about the new technology is not sufficient. Although we were not able to track managers' visits to other firms, it seems quite possible that local exposure allowed them to see a servo motor in action and that this played an important role in spurring adoption.

6 Social Benefits of the Intervention

Given the environmental and learning externalities we have documented, it is natural to ask whether governments should subsidize the adoption of servo motors. In this section, we use our estimates to calculate the benefits that would be realized by government subsidies for a clutch-to-servo motor replacements, similar to our T2 intervention. This calculation includes the private benefits to firms from switching to energy-saving servo motors, the environmental benefits associated with the reduction in their electricity use, and the knowledge spillovers to other firms. We quantify these benefits in terms of the Marginal Value of Public Funds (MVPF), following the approach of Hendren and Sprung-Keyser (2020) and more specifically of Hahn et al. (2024), who show how to incorporate learning externalities into MVPF calculations, applying their method to more than 90 environmental policies from the past 25 years. The MVPF of a policy change is the ratio of the total willingness-to-pay (WTP) for the policy change to its net cost to the government. We calculate the MVPF that would be obtained for our intervention if the subsidized servo motors had been paid for by the government, rather than by our funders.

The MVPF calculation is summarized in Table 12. Here we walk through the calculation step by step. Following Hahn et al. (2024), our MVPF estimates are based on an annual discount rate of 2%.[21] We also assume that new servo motors have a life of 10 years.[22]

In the first step, summarized in the first row of the table, we estimate the number of firms that benefit per subsidized servo motor. They consist of three types of servo motor adopters: "adopter T2 firms" (i.e., those T2 firms that were induced to adopt a servo motor by the intervention); "inframarginal T2 firms" (i.e., those T2 firms that would have adopted a servo motor anyway, regardless of the intervention); and "spillover adopters" (i.e., those T1 and C firms that adopted a servo motor due to spillovers from T2 firms).

The first row of Table 12 presents our estimates of the number of adopters per subsidized motor, based on our preferred specification for estimating adoption impacts. Our estimate of inframarginal T2 adopters per subsidized motor, 0.16, is the predicted control group adoption rate after accounting for spillovers, from the specification in column 3 of Table 3. The number of adopter T2 firms per subsidized motor is 0.84 (i.e., one minus the number of inframarginal T2 adopters per subsidized motor).

To calculate the number of spillover adopters per subsidized motor, we conduct a simulation. In each simulation round, we randomly seed one-third of firms with a fully subsidized servo motor.[23] Next, using our location data, we identify those non-seeded

[21]We choose this rate for consistency with Hahn et al. (2024). US environmental studies typically use rates of 2-3 percent (e.g., Fowlie et al. (2018)). By contrast, the Government of Bangladesh, in its cost-benefit analysis of public investments (Government of Bangladesh, 2024), uses a considerably more conservative rate of 12 percent per annum.

[22]While we cannot verify this assumption in our setting, given the recency of clutch-to-servo-motor conversions, it is consistent with a servo motor lifespan of 20,000 to 30,000 hours, as generally claimed by specialist retailers, and our assumption that stitching machines are used for 160 hours per month in our setting.

[23]The strength of spillovers in general is sensitive to the share of units that are treated; see, for example,

firms that are "exposed" to at least one seeded firm (i.e., located within 500 m of a seeded firm). Then, we simulate spillover adoption among the exposed firms by applying the estimated spillover adoption probability.[24] This yields the number of first-order spillover adopters. We repeat this step for second-order spillovers. Finally, we take averages across 1000 such simulations. The mean number of first- and second-order spillover adopters across the simulations 0.59 per subsidized motor.[25] This is the estimate of number of spillover adopters that appears in the first row of Table 12.

The second step in our MVPF calculation, summarized in the second row of Table 12, is to estimate the private willingness-to-pay (WTP) of each type of adopter. The private willingness-to-pay of an adopter T2 firm is the Present Discounted Value (PDV) of the electricity cost savings from the servo motor. This equals USD 98.95, based on our estimate that switching from a clutch to a servo motor reduces electricity costs by USD 0.90 USD per month, or USD 10.80 per annum (Section 2.3).[26] Because the servo motor purchased by a spillover adopter is not subsidized, the private WTP of a spillover adopter is the PDV of the electricity cost savings from a clutch-to-servo motor switch (USD 98.95) minus the one-time servo motor cost of USD 46, i.e. USD 52.95. The private willingness-to-pay of an inframarginal T2 firm is the subsidy amount, or the market value of the subsidized servo motor, which equals USD 46.

The third step in our MVPF calculation is an assessment of the environmental benefits of servo motor adoption, as summarized in the third row of Table 12. We focus on the benefits of the CO2 emissions abatement associated with reduced electricity use. As discussed in Section 2.3, we estimate that clutch- and servo-motor stitching machines consume, respectively, 123 kWh and 36 kWh of electricity per year. Using the estimated Operating Margin Grid Emission Factor for Bangladesh (U.N. FCC, 2021), which is 528 grams of CO2 emissions per kWh, this amounts to 64.9 kg of annual CO2 emissions for one clutch-motor machine versus 19.3 kg for one servo-motor machine. This suggests an annual reduction of CO2 emissions by 45.7 kg (0.046 metric tons) by replacing one clutch motor with a servo motor. According to the most recent EPA estimates of the so-

Faridani and Niehaus (2024). A treatment share of one-third is a natural baseline for the simulation exercise because it corresponds to the T2 arm's sample share in our actual intervention.

[24] Specifically, the spillover adoption outcome of each exposed firm depends on a draw of a random binomial variable with a success probability of 0.19 (the exposure coefficient from Table 3, column 3).

[25] There is no inherent reason to stop at second-order spillovers. However, we chose this approach because our simulations show that on average, 54% of firms have servo motors after the second spillover round (T2 plus first- and second-order spillover adopters), which matches the observed endline adoption rate of 55.8%, a natural focal point.

[26] The PDV is calculated using the following formula: $PDV = \sum_{t=1}^{10} \frac{10.80}{(1.02)^t}$. As specified earlier, this formula uses a 2% per annum discount rate and an assumed servo motor lifespan of 10 years.

cial cost of carbon, 1 metric ton of CO2 emissions in 2020 would cost society USD 193 in present discounted value terms due to its contribution to global warming (EPA, 2023).[27] Using this number, the environmental value of reducing CO2 emissions by 0.046 metric tons is USD 8.74. Given our assumptions about the discount rate and the lifespan of a servo motor, this annual environmental benefit has a PDV of USD 80.26. This is the "social WTP" associated with each adopter T2 firm and spillover adopter. Inframarginal T2 firms do not generate environmental benefits, since their electricity usage is unaffected by the intervention.

Summing up, the total private WTP and environmental value per subsidized motor equals USD 236.26 (Table 12, fourth row). Dividing this amount by the net cost of the policy change (the price of the servo motor, USD 46), we arrive at an MVPF of 5.14.[28] This number is in the upper range of the 90 plus MVPF estimates calculated in the Hahn et al. (2024) meta-analysis. Hahn et al. (2024) consider MVPFs above 5 to be high in their sample of studies. There are interventions with higher MVPFs — notably, the intervention to promote energy efficient cookstove adoption studied in Berkouwer and Dean (2022) is estimated to have an MVPF above 300 — but the MVPF we calculate would certainly put subsidies for servo motors in the range of interventions that deserve serious consideration. We note that 5.14 is a conservative estimate of the MVPF because we limited spillovers to the second order. For example, including spillovers up to the fifth and seventh order would raise the MVPF estimate to 6.67 and 7.27, respectively.

Two limitations of our MVPF estimate are important to note. First, it does not include all the potential environmental benefits from servo motor adoption, which include the abatement of greenhouse gases other than CO2; reduced particulate matter pollution from power plants (Barrows et al., 2019; Deryugina et al., 2019); and heat and noise reduction from quieter servo motors, which have the potential to raise workplace productivity (Adhvaryu et al., 2020; Dean, 2024). We did not include these in our calculation because we were unable to find quantified estimates for Bangladesh's electricity sector. In this sense, too, our MVPF estimate may be an underestimate of the true MVPF of servo motor subsidies. Second, our estimate does not account for potential rebound effects on electricity usage. In a field experiment with Indian manufacturing plants, Ryan (2018) finds that firms responded to greater energy efficiency (brought about by randomly al-

[27]The EPA presents social costs of greenhouse gases in 2020 for three near-term Ramsey discount rates: 2.5%, 2% and 1.5% per annum (U.S. EPA, EPA). Following Hahn et al. (2024) and others in the environmental economics literature, the USD 193 estimate we use corresponds to a near-term Ramsey discount rate of 2%.

[28]We ignore administrative costs but expect them to be modest based on our experience.

located energy consulting) by expanding output and increasing energy use. Such effects would tend to cause our MVPF estimate to be an overestimate of the true MVPF. Given data constraints, we must leave the task of improving the MVPF calculation on these dimensions to future work.

With these caveats, the clear message that emerges from this calculation is that it is important to take into account learning spillovers when evaluating interventions to promote adoption of energy-efficient technologies. As shown in Table 12, had we ignored learning spillovers, the MVPF from our intervention would be 3.43. Thus, 33% of our total MVPF is accounted for by knowledge spillovers. Ignoring the spillovers, subsidies for adoption of servo motors would not stand out as a particularly cost-effective intervention. But taking the spillovers into account, it becomes clear that such subsidies are at the higher end of the historical distribution of environmental policies.

It is also worth noting that there is high potential to scale up subsidies for servo motors. Using data from the World Bank Firm Adoption of Technology survey, we estimate that approximately 76,000 stitching machines with clutch motors were being used in Bangladesh's ready-made garments (RMG) and leather goods sectors in 2019.[29] Our estimated private and environmental benefits of servo motor adoption suggest that the total PDV of switching all of these clutch motors to servo motors would be approximately USD 10.12 million.[30] Although data on clutch motor usage is not readily available for other countries, we expect similar potential for scale-up in several other countries at similar levels of industrial development.

7 Conclusion

This paper has presented one of the first experimental studies of knowledge spillovers among firms, and to our knowledge the first about a naturally occurring production technology among urban manufacturing firms. Using the Borusyak and Hull (2023) recentering approach to estimate direct and spillover effects simultaneously, we find direct adoption effects of both the information-only (T1) and information-and-installation (T2) treatments and, perhaps more importantly, robustly positive, very local effects of exposure to T2 firms. Having at least one T2 firm within 500 m had a greater positive effect

[29] This estimate is derived from the 2019 World Bank Firm Level Adoption of Technology survey (Gu et al., 2021), which collected data on the number of stitching machines and their motor types among firms in the RMG and leather goods industries.

[30] We arrive at this number by multiplying the number of clutch motors with the private plus environmental PDV of a servo motor net of its cost: 76,000*(98.95+80.26-46).

on adoption than receiving an informational video about the technology. We find little evidence of spillovers through firms' communication networks, shared suppliers, or incidental face-to-face contact.

These findings contribute to the empirical understanding of the microfoundations for agglomeration economies. The idea that knowledge spillovers are an important reason for geographic clustering has had influential adherents at least since Marshall (1890), but convincing empirical demonstrations have been elusive. As noted above, rents in the dense *Bangshal* neighborhood in Old Dhaka are significantly higher than in other industrial areas within the city (*Hazaribagh*, *Kamrangirchar*). It is natural to ask why firms would be willing to pay these higher rents. Our study suggests one answer: they are more likely to be exposed to knowledge spillovers there. There may be other reasons for local clustering, but our evidence indicates that knowledge flows are a salient one.

Our findings have potentially important implications for debates in environmental economics about how to mitigate climate change. A marginal value of public funds (MVPF) calculation which takes into account learning spillovers as well as emissions reductions by treated firms yields a value of 5.14, which compares well to the broad set of environmental policies that Hahn et al. (2024) consider. Notably, 33% of our MVPF is due to learning spillovers. This result underscores the importance of considering learning spillovers when designing policies to encourage green technology adoption. It also suggests that subsidies for servo motors would be a promising policy intervention.

One important issue that our study does not speak to the optimal way to inject information about new technologies into existing networks. In the interests of simplicity, we assigned equal probabilities to T1, T2 and Control, without attention to network structure. Work by Banerjee et al. (2013), Beaman et al. (2021) and others in other settings suggests that adoption and overall welfare could be increased by more judicious choices of which firms to subsidize. This is a promising topic for future work.

Another still-open question is what fraction of the tendency of firms to cluster geographically can be explained by spillovers of production knowledge. Marshall (1890) emphasized pooling of labor with specialized skills and concentration of consumer search (as recently emphasized by Nakajima and Teshima (2018) and Vitali (2024)) in addition to knowledge spillovers.[31] We have focused on documenting that knowledge spillovers exist and are quantitatively important, but it would be useful to use these and similar

[31]Another reason why small firms often cluster together is because proximity enables them to mechanize more efficiently (through machine rental markets) by effectively increasing their scale of operations (Bassi et al., 2022).

estimates to evaluate their magnitude relative to these other drivers, revisiting an issue examined by Ellison et al. (2010) among others. This is another good topic for future research.

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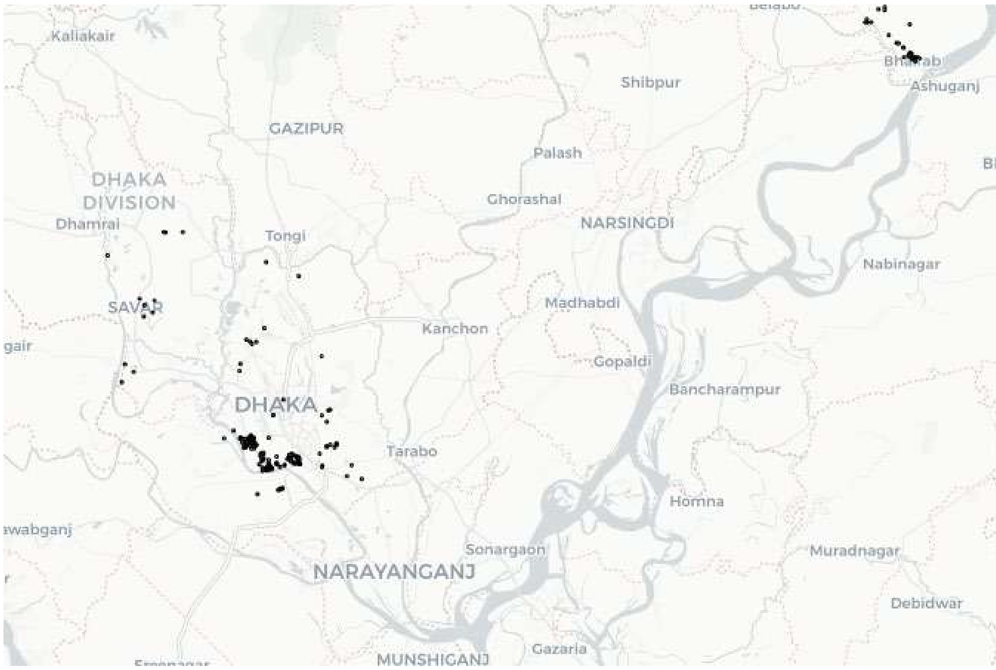
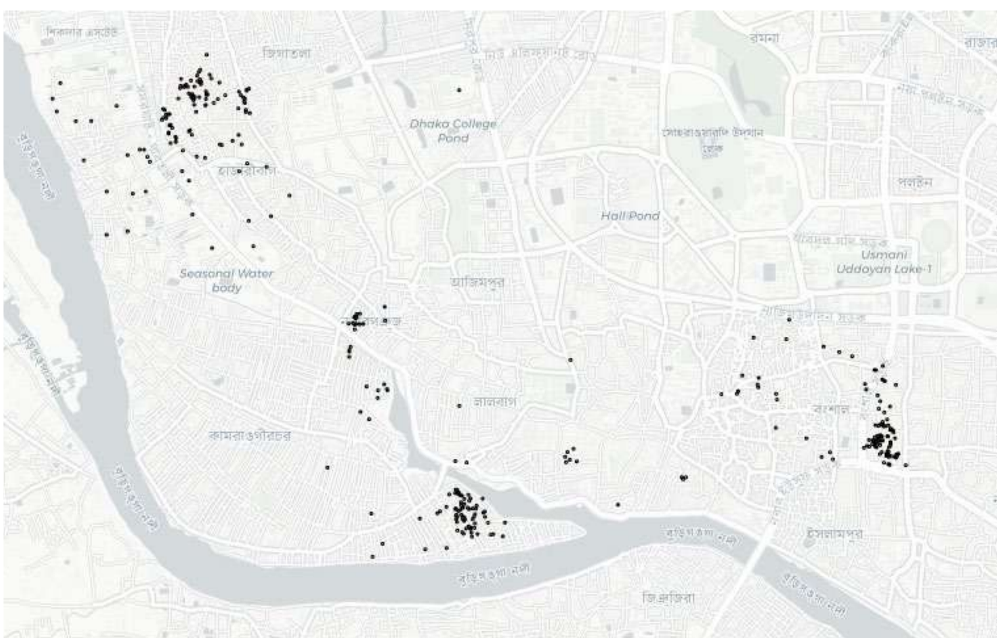
Figure 1. Maps of Dhaka, Bangladesh

A. Dhaka Division

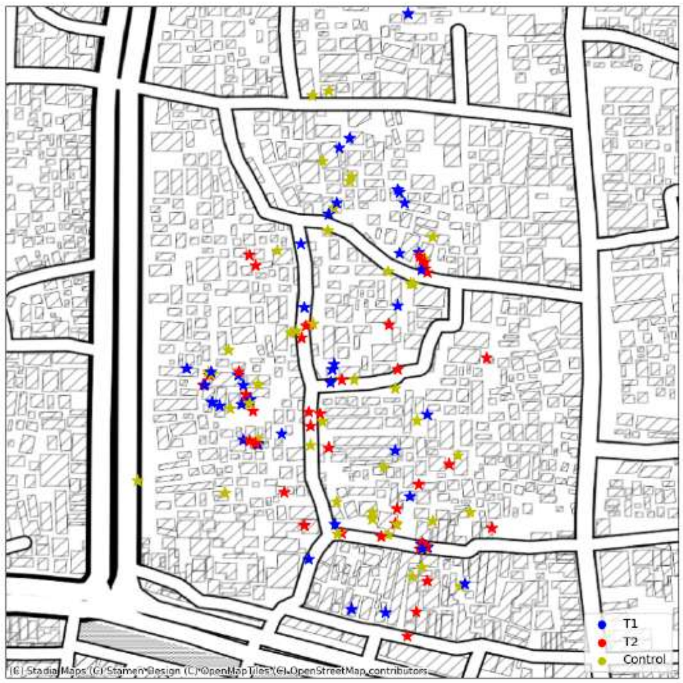


B. City of Dhaka



Notes: Map A shows the Dhaka Division, the administrative unit covering greater Dhaka, including the city of Dhaka (center-left) and an outlying cluster in Bhairab (at the top right). Map B shows Dhaka district, with three main clusters: Hazaribagh in the top left, Kamrangirchar in the bottom center, and Bangshal in the bottom right. In both maps, each dot is a firm in our sample.

Figure 2. Bangshal Cluster, Dhaka



Notes: The figure shows a cluster in the Bangshal neighborhood (upazila) in old Dhaka, along Hazi Moin Uddin Road. This cluster appears in the bottom right part of Figure 1B. The treatment groups are marked with different colors. The cross-hatched rectangles are buildings. In many cases, several firms occupy a single building. (The GPS coordinates are measured with some error, which is why some factories appear to be on roads.)

Figure 3. Photos of Servo and Clutch Motors

Clutch motor

Servo motor

Figure 4. Example of Meter-Reading Report for T2 firm

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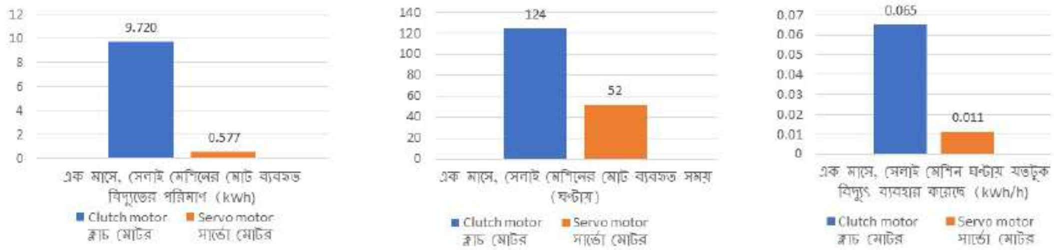
Electricity Consumption - Monthly Report (বিদ্যুৎ ব্যবহার – মাসিক রিপোর্ট)
Date: 22/05/2023–21/06/2023
ID: T-157 Firm: [redacted]

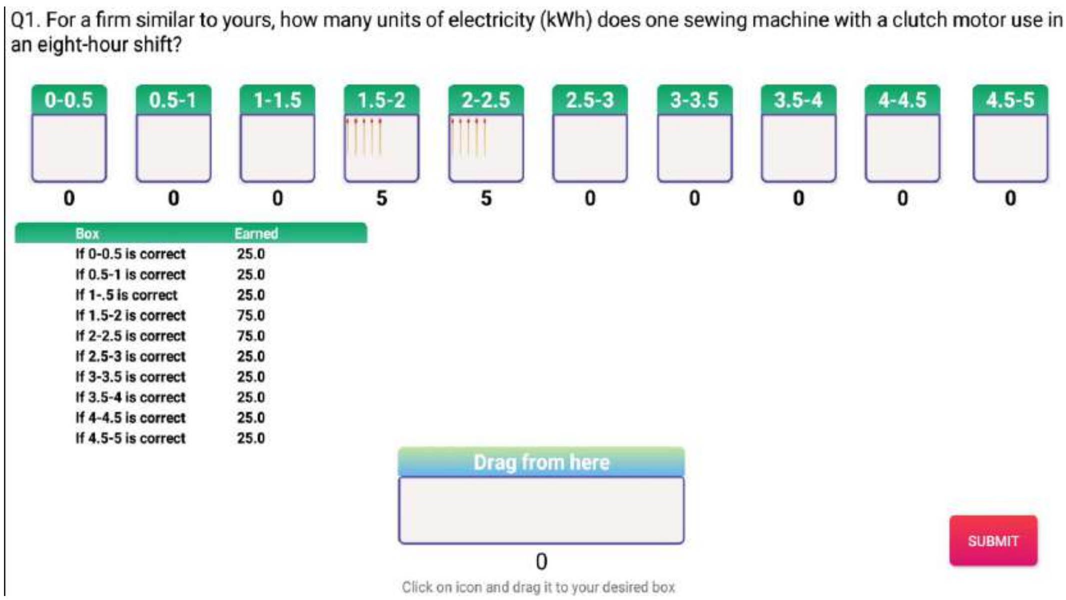
	Clutch motor ক্লাচ মোটর	Servo motor সার্ভো মোটর
Total electricity consumption for sewing machine with meter per month এক মাসে, সেলাই মেশিনের মোট ব্যবহৃত বিদ্যুতের পরিমাণ (kWh)	9.720	0.577
Total number of one-hour intervals for that machine in the month এক মাসে, সেলাই মেশিনের মোট ব্যবহৃত সময় (ঘণ্টায়)	124	52
Average energy consumption per one hour interval for that machine in the month এক মাসে, সেলাই মেশিন ঘণ্টায় যতটুকু বিদ্যুৎ ব্যবহার করেছে (kWh/h)	0.065	0.011



দ্রষ্টব্য: মেশিনটি প্রতি ঘণ্টায় যতটুকু সময়ের জন্য ব্যবহৃত হোক না কেন, তাকে আমরা এক ঘন্টা ব্যবহারের হিসাবে বিবেচনা করছি

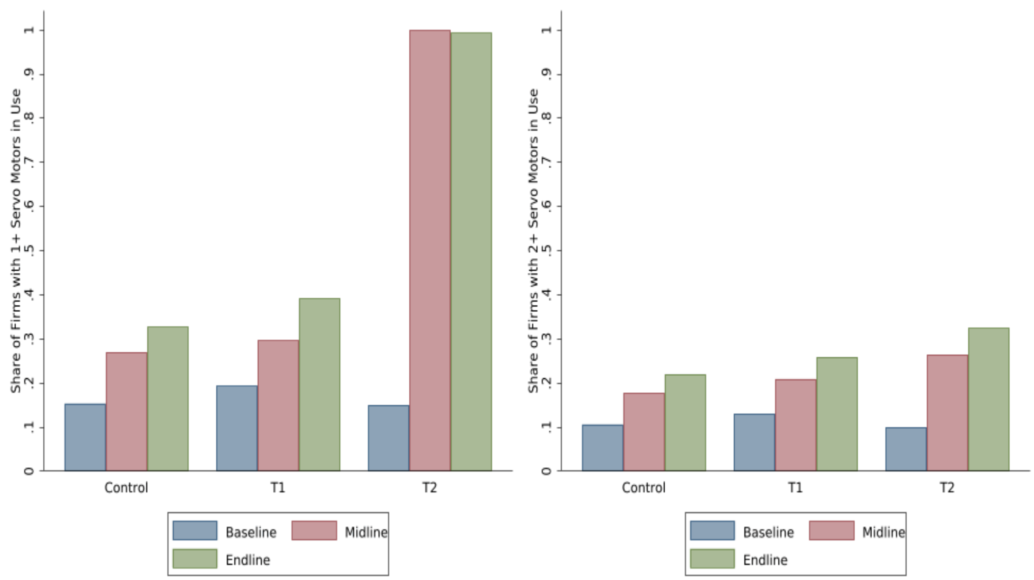
Notes: This is an example of a monthly meter-reading report presented to a T2 firm. The report presents the total electricity consumption, the total number of one-hour intervals in which the machine was running, and the implied mean electricity consumption per hour in the previous month, for the two machines in the firm in which we installed meters. One of them was a clutch motor machine, and the other was a machine in which we replaced the clutch motor with a servo motor.

Figure 5. Screenshot from Belief-Elicitation Tablet App



Notes: This figure presents a screenshot from our tailor-made belief-elicitation app for tablets. The respondent dragged-and-dropped matchsticks into bins. The table below the bins (present only for incentivized subgroup) reports the potential reward from this allocation depending on which bin is correct. In this screenshot, the respondent has made an allocation but not locked it in. This table was updated in real time as respondents changed their allocations.

Figure 6. Simple Comparisons: Usage of Servo Motors

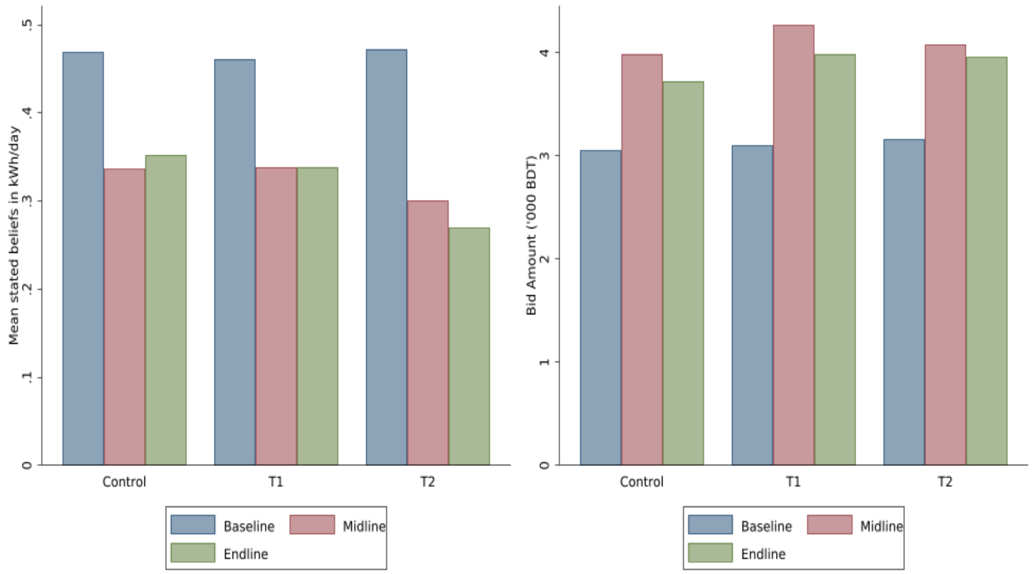


A. Usage of 1+ servo motors (0/1)

B. Usage of 2+ servo motors (0/1)

Notes: The usage of servo motors reflects whether the firm reported using at least one (panel A) or at least two (panel B) machines with a servo motor at the time of the survey round.

Figure 7. Simple Comparisons: Beliefs & Willingness-to-Pay

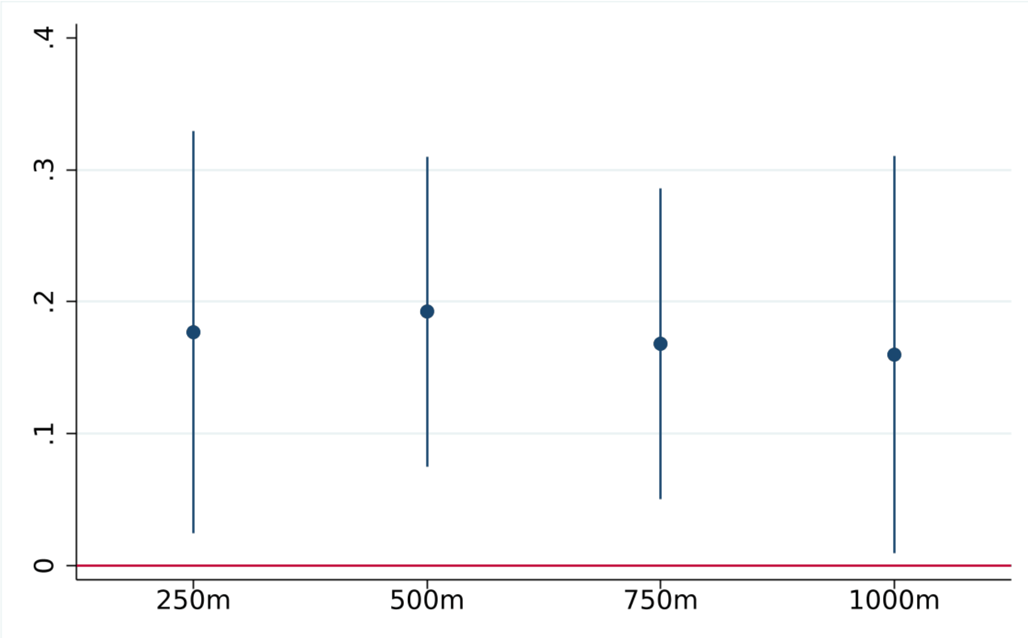


A. Beliefs about servo electricity usage

B. BDM Bid (000s BDT)

Notes: Beliefs are group averages of means of distributions reported by individual managers for kWh used by a machine with a servo motor for an eight-hour shift with regular breaks; see Section 3.4. Willingness-to-pay for servo motor is the maximum bid from the Becker et al. (1964, BDM) procedure described in Section 3.3, averaged across respondents within treatment groups.

Figure 8. Spillovers in Servo Adoption, by Distance



Notes: The figure plots the estimates and 90% confidence intervals on the exposure coefficient from equation 1 in text, at various distances, where the dependent variable is a 0/1 indicator for having purchased 1+ servo motors between baseline and endline and we exclude T2 firms from the sample (similar to column 3 of Table 3). The standard errors use the spatial correction from Conley (1999).

Table 1. Opinions about Servo vs. Clutch Motors

A. Manager Opinions, Conditional on Perceiving Differences

	Share of managers who perceived difference	Mean score (5 pt. scale) conditional on perceiving difference
Speed	55.18	4.74
Noise	60.04	4.89
Stitching	33.83	4.51
Comfort	56.45	4.76
Heat	57.29	4.77

B. Manager Opinions, Conditional on Having Worked with Servo Motor

	Mean score (5 pt. scale) cond. on owning or working w/ servos
Durability	4.04
Overall Satisfaction	4.87

C. Employee Opinions, Reported by Managers

	Share of managers who discussed servos w/ employee	Mean employee opinion (5 pt. scale)
Employee Opinion	29.18	4.76

Notes: Responses are from our endline survey. In panels A and B, respondents are 285 managers who reported owning or having worked with a servo motor. In panel A, respondents were asked first if they perceived a difference between servo and clutch motors on the indicated dimension (first column) and, if so, were asked to rate servo motors vis-a-vis clutch motors on a 5-point scale (1=much worse, 2=a little bit worse, 3=about the same, 4=a little bit better, 5=much better). In panel B, managers were asked their overall satisfaction with the servo motor on a 5-point scale (1=very dissatisfied, 2=somewhat dissatisfied, 3=indifferent, 4=somewhat satisfied, 5=very satisfied) and their perception of the reliability and durability of the motor on a 5-point scale (1=much less reliable/durable, 2=somewhat less reliable durable, 3=about the same, 4=somewhat more reliable/durable, 5=much more reliable/durable). In panel C, of the 473 respondents, 138 reported having spoken to employees about servo motors. The second column presents managers' average report about employees' opinion on 5-point scale (1=strongly negative, 2=weakly negative, 3=indifferent, 4=weakly positive, 5=strongly positive).

Table 2. Balance at Baseline

	Control (1)	T1 (2)	T2 (3)	P-value, C=T1 (4)	P-value, C=T2 (5)
A. Number of Machines					
Num. machines (total)	5.93	6.12	6.62	(0.65)	(0.59)
Num. machines (non-motorized)	0.25	0.16	0.16	(0.18)	(0.16)
Num. machines (w/ ext. motor)	0.18	0.19	0.24	(0.86)	(0.51)
Num. machines (w/ clutch motor)	4.99	4.81	5.39	(0.72)	(0.83)
Num. machines (w/ servo motor)	0.68	0.83	0.80	(0.56)	(0.92)
Firm has Servo at Baseline (0/1)	0.15	0.19	0.15	(0.25)	(0.63)
B. Beliefs					
Beliefs, elec usage (kwh/day) for clutch motor	0.66	0.67	0.70	(0.79)	(0.22)
Beliefs, elec usage (kwh/day) for servo motor	0.47	0.46	0.47	(0.79)	(0.79)
BDM bid ('000 BDT)	3.05	3.10	3.16	(0.73)	(0.60)
Beliefs, price of electricity (BDT/kwh)	11.17	10.94	10.96	(0.24)	(0.31)
Knows servo can swap for clutch (0/1)	0.59	0.53	0.54	(0.24)	(0.26)
C. Firm Characteristics					
Num. employees (paid and unpaid)	10.66	10.39	12.17	(0.88)	(0.52)
Production costs, last month ('000,000 BDT)	0.47	0.39	0.50	(0.36)	(0.95)
Profits, last month ('000 BDT)	30.95	26.15	37.63	(0.66)	(0.58)
Sales, last month ('000,000 BDT)	0.52	0.41	0.54	(0.25)	(0.93)
Electricity costs, last month ('000 BDT)	6.27	4.67	5.05	(0.34)	(0.33)
Direct exports, as % of sales	2.03	1.36	1.56	(0.45)	(0.52)
Exporter, direct (0/1)	0.05	0.05	0.03	(0.96)	(0.16)
D. Respondent Characteristics					
Respondent is male (0/1)	0.96	0.98	0.96	(0.18)	(0.91)
Respondent's years of education	6.09	6.58	6.52	(0.24)	(0.46)
Respondent's age	35.88	36.23	35.34	(0.77)	(0.47)
Respondent's experience	15.47	15.68	14.94	(0.83)	(0.40)
Attrited by Endline (0/1)	0.06	0.06	0.07	(0.93)	(0.56)
Number of Firms	169	176	160		

Notes: Data are from 505 respondents in the randomization sample at baseline.

Table 3. Treatment & Local Spillover Effects on Adoption

	Including T2		Excluding T2	
	Purchased 1+ servo motors	Purchased 2+ servo motors	Purchased 1+ servo motors	Purchased 2+ servo motors
	(1)	(2)	(3)	(4)
T1	0.090	0.057	0.099	0.048
	(0.035)**	(0.018)***	(0.033)***	(0.018)***
	[0.041]**	[0.034]*	[0.042]**	[0.034]
T2	0.809	0.091		
	(0.079)***	(0.025)***		
	[0.032]***	[0.039]**		
Exposure (500m)	0.158	0.105	0.193	0.102
	(0.049)***	(0.049)**	(0.071)***	(0.057)*
	[0.062]**	[0.062]*	[0.078]**	[0.066]
Exp. Exposure (500m)	-0.010	0.029	0.001	0.016
	(0.088)	(0.093)	(0.118)	(0.093)
	[0.098]	[0.090]	[0.133]	[0.102]
Observations	473	473	325	325
Upazila FE	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y
Rand. Inf. p-value (Exposure)	[0.018]	[0.090]	[0.024]	[0.066]

Notes: Estimates are of equation 1 in text. Data are from endline survey. Exposure (500 m) is a 0/1 indicator for having one or more T2 firm within a walking distance of 500 m. Exp. Exposure (500 m) is mean of Exposure from 1,000 counterfactual treatment assignment draws. Dependent variables are 0/1 indicators for having purchased or received (from us) 1+ or 2+ servo motors between baseline and endline. (Means of the dependent variables at baseline are zero.) The standard errors in parentheses use the spatial correction from Conley (1999). The standard errors in square brackets are heteroskedasticity-robust (without the correction for spatial correlation). *p <0.10; **p <0.05; ***p <0.01. The randomization inference p-values for the coefficient on the realized exposure term are based on the test statistic recommended by Borusyak and Hull (2023) (the sample covariance of the difference between Exposure and Exp. Exposure).

Table 4. Effects on Adoption by Intensity of Local Exposure

	Including T2		Excluding T2	
	Purchased 1+ servo motors (1)	Purchased 2+ servo motors (2)	Purchased 1+ servo motors (3)	Purchased 2+ servo motors (4)
T1	0.092	0.062	0.102	0.050
	(0.032)***	(0.017)***	(0.031)***	(0.018)***
	[0.041]**	[0.034]*	[0.041]**	[0.034]
T2	0.811	0.088		
	(0.076)***	(0.024)***		
	[0.032]***	[0.038]**		
Exposure, 1-2 neighbors (500m) (0/1)	0.152	0.102	0.158	0.080
	(0.059)**	(0.052)*	(0.085)*	(0.057)
	[0.066]**	[0.065]	[0.088]*	[0.071]
Exposure, 3-5 neighbors (500m) (0/1)	0.186	-0.038	0.246	0.191
	(0.100)*	(0.107)	(0.125)**	(0.092)**
	[0.109]*	[0.122]	[0.145]*	[0.121]
Exposure, 6-8 neighbors (500m) (0/1)	0.295	0.038	0.416	0.271
	(0.116)**	(0.135)	(0.174)**	(0.102)***
	[0.173]*	[0.147]	[0.251]*	[0.153]*
Exposure, 9+ neighbors (500m) (0/1)	0.576	0.433	0.824	0.584
	(0.104)***	(0.271)	(0.167)***	(0.087)***
	[0.239]**	[0.264]	[0.349]**	[0.274]**
Observations	473	473	325	325
Exp. Exposure Controls	Y	Y	Y	Y
Upazila FE	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y

Notes: Estimates are of equation 1 in text, but with four separate exposure terms and the corresponding expected exposure terms. "Exposure, a-b ngh. (500 m)" is a 0/1 indicator for the number of T2 firms (within a walking distance of 500m) $\in [a, b]$. To save space, the four expected exposure terms are not reported in the table (but are included in the regressions). Dependent variables are 0/1 indicators for having purchased or received (from us) 1+ or 2+ servo motors between baseline and endline. (Means of the dependent variables at baseline are zero.) The standard errors in parentheses use the spatial correction from Conley (1999). The standard errors in square brackets are heteroskedasticity-robust (without the correction for spatial correlation). *p <0.10; **p <0.05; ***p <0.01.

Table 5. Effects of Exposure to Producers of Common Product

	Including T2		Excluding T2	
	purchased 1+ servo motors			
	(1)	(2)	(3)	(4)
T1	0.192	0.054	0.166	0.066
	(0.054)***	(0.033)*	(0.067)**	(0.033)**
	[0.074]**	[0.055]	[0.076]**	[0.055]
T2	0.859	0.744		
	(0.069)***	(0.101)***		
	[0.054]***	[0.044]***		
Exposure common prod. (500m)	0.175	0.189	0.258	0.185
	(0.067)***	(0.055)***	(0.084)***	(0.071)***
	[0.077]**	[0.095]**	[0.101]**	[0.119]
Exposure no common prod. (500m)	0.063	-0.023	0.141	-0.065
	(0.158)	(0.110)	(0.208)	(0.140)
	[0.198]	[0.100]	[0.235]	[0.130]
Exp. Exposure common (500m)	-0.168	-0.110	-0.213	-0.043
	(0.131)	(0.139)	(0.166)	(0.184)
	[0.160]	[0.139]	[0.219]	[0.180]
Exp. Exposure no common (500m)	0.100	0.112	0.129	0.203
	(0.224)	(0.066)*	(0.289)	(0.091)**
	[0.215]	[0.080]	[0.269]	[0.111]*
Observations	140	322	95	217
Products	Shoes	Bags	Shoes	Bags
Upazila FE	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y

Notes: Estimates are of equation 1 in text. Data are from endline survey. In columns 1 and 3, the sample includes respondents who report at least one product in the "shoes" category. In columns 2 and 4, the sample includes respondents who report at least one product in the "bags" category. Firms may produce both shoes and bags and these subsamples are not mutually exclusive. "Exposure, common prod. (500 m)" is a 0/1 indicator for having one or more T2 firms within walking distance of 500 m that reported at least one product in the same category as the respondent. "Exposure, no common prod. (500 m)" is a 0/1 indicator for having one or more T2 firms within a walking distance of 500 m that reported no product in the same category as the respondent. Exp. Exposure is mean of Exposure from 1,000 counterfactual treatment assignment draws. Dependent variables are 0/1 indicators for having purchased or received (from us) 1+ or 2+ servo motors between baseline and endline. (Means of the dependent variables at baseline are zero.) The standard errors in parentheses use the spatial correction from Conley (1999). The standard errors in square brackets are heteroskedasticity-robust (without the correction for spatial correlation). *p <0.10; **p <0.05; ***p <0.01.

Table 6. Effects on Information Flows

	Including T2		Excluding T2	
	Shown report by T2 firm	Discussed servos w/ T2 firm	Shown report by T2 firm	Discussed servos w/ T2 firm
	(1)	(2)	(3)	(4)
T1	-0.007	0.017	-0.010	0.024
	(0.017)	(0.034)	(0.016)	(0.029)
	[0.018]	[0.046]	[0.018]	[0.046]
T2	0.007	0.056		
	(0.022)	(0.044)		
	[0.020]	[0.050]		
Exposure (500m)	0.029	0.155	0.019	0.224
	(0.013)**	(0.072)**	(0.008)**	(0.063)***
	[0.013]**	[0.058]***	[0.010]**	[0.060]***
Exp. Exposure (500m)	-0.002	-0.018	0.011	-0.141
	(0.013)	(0.115)	(0.012)	(0.094)
	[0.021]	[0.106]	[0.011]	[0.121]
Observations	473	473	325	325
Upazila FE	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y
Rand. Inf. p-value (Exposure)	[0.014]	[0.030]	[0.020]	[0.000]

Notes: Estimates are of equation 1 in text. Data are from endline survey. "Exposure (500 m)" is a 0/1 indicator for having one or more T2 firms within a walking distance of 500 m. "Exp. Exposure (500 m)" is mean of Exposure from 1,000 counterfactual treatment assignment draws. Outcome in columns 1 and 3 is a 0/1 indicator for whether the firm reported having seen an electricity report from a T2 firm. Outcome in columns 2 and 4 is a 0/1 indicator for whether the firm reported discussing servo motors with a T2 firm. The standard errors in parentheses use the spatial correction from Conley (1999). The standard errors in square brackets are heteroskedasticity-robust (without the correction for spatial correlation). *p <0.10; **p <0.05; ***p <0.01. The randomization inference p-values for the coefficient on the realized exposure term are based on the test statistic recommended by Borusyak and Hull (2023) (the sample covariance of the difference between Exposure and Exp. Exposure).

Table 7. Effects on Beliefs and Willingness-to-Pay

	Including T2		Excluding T2	
	beliefs about servo kWh/day	BDM bid	beliefs about servo kWh/day	BDM bid
	(1)	(2)	(3)	(4)
T1	-0.020	0.267	-0.021	0.260
	(0.013)	(0.087)***	(0.014)	(0.091)***
	[0.018]	[0.143]*	[0.018]	[0.142]*
T2	-0.082	0.259		
	(0.011)***	(0.094)***		
	[0.018]***	[0.162]		
Exposure (500m)	-0.013	0.084	-0.026	0.035
	(0.029)	(0.232)	(0.032)	(0.272)
	[0.030]	[0.282]	[0.035]	[0.297]
Exp. Exposure (500m)	-0.073	-0.102	-0.091	0.009
	(0.042)*	(0.418)	(0.048)*	(0.362)
	[0.050]	[0.450]	[0.060]	[0.475]
Dep var at baseline	0.051	0.199	0.057	0.133
	(0.041)	(0.050)***	(0.049)	(0.020)***
	[0.031]	[0.046]***	[0.039]	[0.050]***
Observations	473	473	325	325
Baseline mean	0.466	3.104	0.465	3.071
Upazila FE	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y
Rand. Inf. p-value (Exposure)	[0.575]	[0.781]	[0.344]	[0.929]

Notes: Estimates are of equation 1 in text. Data are from endline survey. Exposure (500 m) is a 0/1 indicator for having one or more T2 firm within a walking distance of 500 m. Exp. Exposure (500 m) is mean of Exposure from 1,000 counterfactual treatment assignment draws. Dependent variable for columns 1 and 3 is the mean of the distribution of beliefs about electricity use of a servo motor. Dependent variable for columns 2 and 4 is a firm's willingness-to-pay for a servo motor in levels (000s of BDT) and logs, elicited using the Becker, DeGroot and Marschak (1964, BDM) procedure. Bids are in thousands of BDT. Exchange rate is approximately 100 BDT/USD. The standard errors in parentheses use the spatial correction from Conley (1999). The standard errors in square brackets are heteroskedasticity-robust (without the correction for spatial correlation). *p <0.10; **p <0.05; ***p <0.01. The randomization inference p-values for the coefficient on the realized exposure term are based on the test statistic recommended by Borusyak and Hull (2023) (the sample covariance of the difference between Exposure and Exp. Exposure).

Table 8. Effects on Operating Costs, Electricity, Sales and Employment

	Including T2				Excluding T2			
	log operating costs per worker	log electricity costs per worker	ihs sales, previous month	log employment	log operating costs per worker	log electricity costs per worker	ihs sales, previous month	log employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T1	-0.165	-0.043	-0.063	-0.023	-0.162	-0.052	-0.056	-0.029
	(0.049)***	(0.042)	(0.096)	(0.060)	(0.045)***	(0.037)	(0.063)	(0.056)
	[0.106]	[0.093]	[0.170]	[0.064]	[0.107]	[0.093]	[0.169]	[0.065]
T2	-0.211	-0.148	-0.255	0.024				
	(0.071)***	(0.079)*	(0.236)	(0.043)				
	[0.106]**	[0.095]	[0.214]	[0.068]				
Exposure (500m)	0.121	0.039	0.043	-0.003	-0.012	0.072	-0.127	0.027
	(0.190)	(0.090)	(0.207)	(0.140)	(0.207)	(0.119)	(0.239)	(0.153)
	[0.177]	[0.140]	[0.212]	[0.131]	[0.200]	[0.159]	[0.234]	[0.136]
Exp. Exposure (500m)	-0.512	-0.466	-0.601	-0.239	-0.353	-0.446	-0.350	-0.383
	(0.303)*	(0.216)**	(0.333)*	(0.194)	(0.330)	(0.271)*	(0.426)	(0.220)*
	[0.292]*	[0.238]*	[0.334]*	[0.201]	[0.335]	[0.279]	[0.376]	[0.223]*
Dep var at baseline	0.454	0.664	0.507	0.675	0.516	0.638	0.518	0.630
	(0.050)***	(0.062)***	(0.058)***	(0.070)***	(0.072)***	(0.073)***	(0.078)***	(0.083)***
	[0.066]***	[0.066]***	[0.068]***	[0.047]***	[0.077]***	[0.084]***	[0.080]***	[0.057]***
Observations	473	473	473	473	325	325	325	325
Upazila FE	Y	Y	Y	Y	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y	Y	Y	Y	Y
Rand. Inf. p-value (Exposure)	[0.464]	[0.767]	[0.797]	[0.981]	[0.965]	[0.563]	[0.492]	[0.809]

Notes: Estimates are of equation 1 in text. Data are from endline survey. Exposure (500 m) is a 0/1 indicator for having one or more T2 firm within a walking distance of 500 m. Exp. Exposure (500 m) is mean of Exposure from 1,000 counterfactual treatment assignment draws. The dependent variable in columns 1 and 5 is log operating costs per worker; in columns 2 and 6 it is log electricity costs per worker. In constructing dependent variable for columns 1-2 and 5-6, we use employment at baseline. The dependent variable in column 3 and 7 is reported sales for the firm in the month prior to endline (the values are inverse-hyperbolic-sine-transformed as some firms report zero sales); in columns 4 and 8 it is the log of the number of employees on a firm's payroll, which includes paid workers both permanent and otherwise, as well as unpaid workers. The standard errors in parentheses use the spatial correction from Conley (1999). The standard errors in square brackets are heteroskedasticity-robust (without the correction for spatial correlation). *p <0.10; **p <0.05; ***p <0.01. *p <0.10; **p <0.05; ***p <0.01. The randomization inference p-values for the coefficient on the realized exposure term are based on the test statistic recommended by Borusyak and Hull (2023) (the sample covariance of the difference between Exposure and Exp. Exposure).

Table 9. Effects of Other Channels of Exposure on Adoption

	Network Exposure		Shared-Supplier Exposure		Shared-Mosque Exposure	
	Purchased 1+ servo motors	Purchased 2+ servo motors	Purchased 1+ servo motors	Purchased 2+ servo motors	Purchased 1+ servo motors	Purchased 2+ servo motors
	(1)	(2)	(3)	(4)	(5)	(6)
T1	0.102	0.050	0.109	0.054	0.097	0.047
	(0.036)***	(0.018)***	(0.036)***	(0.018)***	(0.036)***	(0.019)**
	[0.042]**	[0.034]	[0.043]**	[0.034]	[0.042]**	[0.034]
Exposure	0.103	0.093	-0.029	-0.044	0.045	0.039
	(0.093)	(0.093)	(0.048)	(0.029)	(0.051)	(0.036)
	[0.085]	[0.069]	[0.074]	[0.054]	[0.075]	[0.057]
Exp. Exposure	-0.116	-0.133	0.110	0.082	0.039	0.007
	(0.119)	(0.154)	(0.059)*	(0.041)**	(0.116)	(0.086)
	[0.113]	[0.095]	[0.085]	[0.065]	[0.093]	[0.068]
Observations	325	325	325	325	325	325
Upazila FE	Y	Y	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y	Y	Y
Rand. Inf. p-value (Exposure)	[0.132]	[0.090]	[0.607]	[0.362]	[0.476]	[0.462]

Notes: Estimates are of equation 1 in text. Data are from endline survey. T2 firms are excluded from these regressions (as in Columns 3-4 of Table 3). In columns 1-2, Exposure is a 0/1 indicator for whether the firm reported asking for advice about new technology with one or more T2 firms at the baseline or midline survey rounds. In columns 3-4, Exposure is a 0/1 indicator for whether a manager shares a supplier or repair technician with one or more T2 firms at endline survey. In columns 5-6, Exposure is a 0/1 indicator for whether a manager shares a mosque with one or more T2 firms at endline survey. Dependent variables are 0/1 indicators for having purchased or received (from us) 1+ or 2+ servo motors between baseline and endline. The standard errors in parentheses use the spatial correction from Conley (1999). The standard errors in square brackets are heteroskedasticity-robust (without the correction for spatial correlation). *p <0.10; **p <0.05; ***p <0.01. The randomization inference p-values for the coefficient on the realized exposure term are based on the test statistic recommended by Borusyak and Hull (2023) (the sample covariance of the difference between Exposure and Exp. Exposure).

Table 10. Effects of Other Channels of Exposure on Information Flows

	Network Exposure		Shared-Supplier Exposure		Shared-Mosque Exposure	
	Shown report by T2 firm	Discussed servos w/ T2 firm	Shown report by T2 firm	Discussed servos w/ T2 firm	Shown report by T2 firm	Discussed servos w/ T2 firm
	(1)	(2)	(3)	(4)	(5)	(6)
T1	-0.010	0.015	-0.009	0.037	-0.011	0.024
	(0.014)	(0.039)	(0.015)	(0.033)	(0.016)	(0.031)
	[0.018]	[0.045]	[0.019]	[0.047]	[0.018]	[0.047]
Exposure	-0.011	0.219	0.025	-0.023	0.027	0.036
	(0.018)	(0.071)***	(0.013)*	(0.055)	(0.040)	(0.079)
	[0.030]	[0.097]**	[0.013]*	[0.073]	[0.038]	[0.079]
Exp. Exposure	0.029	0.186	-0.010	0.145	-0.004	0.011
	(0.095)	(0.242)	(0.008)	(0.066)**	(0.061)	(0.126)
	[0.055]	[0.153]	[0.022]	[0.089]	[0.053]	[0.103]
Observations	325	325	325	325	325	325
Upazila FE	Y	Y	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y	Y	Y
Rand. Inf. p-value (Exposure)	[0.769]	[0.038]	[0.002]	[0.741]	[0.358]	[0.593]

Notes: Estimates are of equation 1 in text. Data are from endline survey. T2 firms are excluded from these regressions (as in Columns 3-4 of Table 3. For columns 1-2, Exposure is a 0/1 indicator for whether the firm reported asking for advice about new technology with one or more T2 firms at the baseline or midline survey rounds. For columns 3-4, Exposure is a 0/1 indicator for whether a manager shares a supplier or repair technician with one or more T2 firms at endline survey. For columns 5-6, Exposure is a 0/1 indicator for whether a manager shares a mosque with one or more T2 firms at endline survey. Exp. Exposure is mean of Exposure from 1,000 counterfactual treatment assignment draws. Outcome in columns 1 and 4 is a 0/1 indicator for whether the manager reported going to the same mosque as the manager of a T2 firm. Outcome in columns 2 and 5 is a 0/1 indicator for whether the firm reported having seen an electricity report from a T2 firm. Outcome in columns 3 and 6 is a 0/1 indicator for whether the firm reported discussing servo motors with a T2 firm. The standard errors in parentheses use the spatial correction from Conley (1999). The standard errors in square brackets are heteroskedasticity-robust (without the correction for spatial correlation). *p <0.10; **p <0.05; ***p <0.01. The randomization inference p-values for the coefficient on the realized exposure term are based on the test statistic recommended by Borusyak and Hull (2023) (the sample covariance of the difference between Exposure and Exp. Exposure).

Table 11. Effects of Other Channels of Exposure on Beliefs and Willingness-to-Pay

	Network Exposure		Shared-Supplier Exposure		Shared-Mosque Exposure	
	beliefs about servo kWh/day	BDM bid	beliefs about servo kWh/day	BDM bid	beliefs about servo kWh/day	BDM bid
	(1)	(2)	(3)	(4)	(5)	(6)
T1	-0.023	0.251	-0.029	0.274	-0.020	0.245
	(0.014)	(0.078)***	(0.016)*	(0.094)***	(0.014)	(0.093)***
	[0.018]	[0.141]*	[0.018]	[0.145]*	[0.018]	[0.142]*
Exposure	-0.025	-0.367	0.029	0.133	0.004	0.437
	(0.021)	(0.195)*	(0.029)	(0.400)	(0.029)	(0.262)*
	[0.032]	[0.286]	[0.025]	[0.283]	[0.029]	[0.234]*
Exp. Exposure	0.068	0.730	-0.104	0.051	-0.031	-0.210
	(0.037)*	(0.593)	(0.030)***	(0.410)	(0.037)	(0.330)
	[0.058]	[0.489]	[0.033]***	[0.339]	[0.040]	[0.301]
Dep var at baseline	0.052	0.129	0.050	0.130	0.048	0.140
	(0.054)	(0.029)***	(0.052)	(0.022)***	(0.051)	(0.019)***
	[0.040]	[0.050]***	[0.039]	[0.049]***	[0.040]	[0.050]***
Observations	325	325	325	325	325	325
Upazila FE	Y	Y	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y	Y	Y
Rand. Inf. p-value (Exposure)	[0.460]	[0.160]	[0.312]	[0.509]	[0.909]	[0.066]

Notes: Estimates are of equation 1 in text. Data are from endline survey. For columns 1-2, Exposure is a 0/1 indicator for whether the firm reported asking for advice about new technology with one or more T2 firms at the baseline or midline survey rounds. For columns 3-4, Exposure is a 0/1 indicator for whether a manager shares a supplier or repair technician with one or more T2 firms at endline survey. For columns 5-6, Exposure is a 0/1 indicator for whether a manager shares a mosque with one or more T2 firms at endline survey. Dependent variable is the mean of the distribution of beliefs about electricity use of a servo motor, in levels or logs. The standard errors in parentheses use the spatial correction from Conley (1999). The standard errors in square brackets are heteroskedasticity-robust (without the correction for spatial correlation). *p $<$0.10; **p $<$0.05; ***p $<$0.01. The randomization inference p-values for the coefficient on the realized exposure term are based on the test statistic recommended by Borusyak and Hull (2023) (the sample covariance of the difference between Exposure and Exp. Exposure).

Table 12. MVPF Calculation

	Adopter type			
	Adopter T2	Inframarginal T2	Spillover adopter	Total
(A) Number of adopters per subsidized motor	0.84	0.16	0.59	1.59
(B) Private WTP per adopter	PDV of electricity savings (=98.95)	Subsidy value (=46.00)	PDV of electricity savings minus cost of motor (=52.95)	197.90
(C) Social WTP per adopter	PDV of C02 averted valued using SCC (=80.26)	None	PDV of C02 averted valued using SCC (=80.26)	160.52
(D) Total WTP per subsidized motor = A*(B + C)	150.54	7.36	78.36	236.26
(E) Public cost per subsidized motor	46.00	46.00	46.00	46.00
MVPF = D/E	3.27	0.16	1.70	5.14

Notes: The calculation uses the US Environmental Protection Agency (EPA) estimate of the Social Cost of Carbon (SCC) of $193 in 2020 (EPA, 2023) and a discount rate of 2% per annum for consistency with the MVPF estimates reported in Hahn et al. (2024). New servo motors are assumed to have a life of 10 years. Benefits from the abatement of other greenhouse gases and local pollution benefits, such as PM2.5 abatement, are not included. The number of Inframarginal T2 adopters per subsidized motor is estimated as the predicted value of the control group adoption rate after accounting for spillovers, based on the estimates reported in Table 3, column 3. The number of Adopter T2 firms per subsidized motor is estimated as 1 minus the number of inframarginal adopters per subsidized motor. The estimate of the number of spillover adopters per subsidized motor includes first and second degree spillovers, assuming a seeding rate of 1 in 3, as discussed in Section 6.