Environmental Regulation and Industrial Performance: Evidence from Unexpected Externalities in China*

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Abstract

The neoclassical model argues that environmental regulations impede industrial performance. In this paper, we shed light on two features of environmental regulations in developing countries that have received little attention and that give rise to unexpected outcomes with respect to industry performance. First, compliance to regulations is likely to provide positive forces leading to improved productivity because a) induced innovation and/or the adoption of cleaner technologies among polluting firms enhance industrial activities, and b) regulations stimulate market dynamics through the entry of more efficient firms and the exit of less efficient ones. Second, regulations carry externality effects for non-polluting energy-intensive firms, when the energy sector is prone to regulations. By exploiting plausibly exogenous variation in regulatory stringency generated by the Two Control Zone policy in China across cities and across industries, we find evidence that pollution-intensive firms substantially improved economic performance, whereas energy-intensive firms received negative externalities. The findings are robust to the inclusions of city-specific trends, industry-specific trends, and key *ex ante* determinants of firm growth.

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I. Introduction

The neoclassical economics model argues that environmental regulations impose substantial costs, thereby impede industrial performance, which has been central to historical debates over the social benefits of reducing pollution (i.e., improved public health) vs. the private costs of abating pollution (i.e., production distortion). In the United States, studies have found that stricter air pollution regulations resulted in substantial welfare losses (Henderson 1996; Eskeland and Harrison 1997; Becker and Henderson 2000; Greenstone 2002; Hanna 2010). Such evidence has rendered efforts to regulate pollution controversial around the world, and has amplified the debate in developing countries where economic growth is prioritized over environment.

However, the existing literature predominantly focuses on developed countries, and empirical evidence on the magnitude and even the sign of the effect of environmental regulations on industrial performance in low-income countries is inconclusive. Two unique features in developing countries that have gained little attention may give rise to unexpected outcomes. First, environmental policy may lead to increased productivity and competitiveness, if regulations induce innovation and/or the adoption of cleaner production technologies among polluting firms (Porter 1991; Porter and van der Linde 1995). Such innovation effects are greater particularly in developing countries that continue to rely on low technologies, causing both high emissions and low economic performance. Further, environmental regulations stimulate market dynamics, as more productive firms displace less productive ones, leading to aggregated productivity growth in the regulated industry. Second, developing countries heavily depend on coal to generate electricity, which makes the power sector especially prone to the effect of environmental regulations. Common approaches in the literature measuring the relative effect of regulations on polluting to non-polluting firms would be biased in this case, because non-polluting firms are likely to receive externality effects through energy supply shortage.

In this paper, we examine the extent to which a stricter environmental protection regime in China affected industrial performance. Specifically, we focus on the Two Control Zone (TCZ) policy, legislation that imposed stringent requirements to reduce pollution emissions in more than 200 prefectures exceeding the nationally-mandated pollution standards, requiring the use of lower-sulfur coal and the development and adoption of clean technologies. Further, since the electric power industry, a primary contributor to local pollution, was subject to heavy regulations, we simultaneously test whether the TCZ regulatory policy had externality effect on clean yet energy-intensive industries.

Our empirical strategy is designed to estimate the effect of TCZ regulations on industrial activities for different levels of pollution and energy intensity. Essentially, it is based on a difference-in-differences framework that exploits plausibly exogenous variation in regulatory intensity: pollution-intensive firms located in a TCZ city are subject to relatively more stringent environmental regulations than pollution-intensive firms in a non-TCZ city. Importantly, this approach allows the estimated regulation effect to be purged of industry-specific and city-specific trends. This rectifies shortcomings associated with (a) a simple comparison between TCZ and non-TCZ cities that captures heterogeneities other than TCZ regulatory status (i.e., differential patterns of economic growth), and (b) a simple comparison between polluting and non-polluting industries

that confounds factors that may have had differential impacts on industry-level growth (i.e., demand or supply shocks).

To implement the analysis, we use a plant-level dataset from the Annual Surveys of Industrial Production (ASIP) from 1998 through 2005, collected by the National Bureau of Statistics of China. The ASIP provides detailed information on annual financial and operational status for the census of state-owned firms and all non-state-owned firms with annual revenues above five million yuan.[1] We use the plant's location at the county level to identify its TCZ status, reported by the State Council. We further collect information on industry-level pollution intensity, measured by the share of coal consumption or SO_2 emissions, and energy-intensity, measured by the share of energy consumption, from the National Bureau of Statistics of China. The complete dataset includes more than 1 million observations at the plant-by-year level across 33 power and manufacturing industries.

Such a unique and comprehensive dataset adds credibility to our analysis in two ways. First, the longitudinal nature of the dataset allows us to trace individual firms throughout the period, enabling us to observe market dynamics through entries and exits. This is important in developing countries, where many inefficient firms can survive despite extensive resource misallocation and low total factor productivity (Hsieh and Klenow 2009). It is an open question as to what types of policies and practices encourage efficiency and productivity dynamics within industries in developing countries. Second, detailed information on various facets of firms' activities allows us to carry out a comprehensive analysis of regulation effects (i.e., profits, revenues, costs, assets, liabilities, capital stock, and employment). Many previous studies, on the other hand, have looked at only a single variable, such as an abatement cost, to measure the cost of regulation. However, if compliance with environmental regulations can spur higher industrial performance, then such benefits may partially offset or even more than offset the compliance costs.

We find evidence that the TCZ regulation substantially improved economic performance among pollution-intensive firms but retarded it among energy-intensive firms. In particular, increased revenues more than offset increased total costs for firms consuming greater amounts of coal in TCZ cities, holding their energy consumption constant, resulting in increased profits. The estimates suggest that a 1 percent increase in coal consumption resulted in a 0.58 percent increase in profits, indicating that the regulations had net *positive* effects on productivity and competitiveness of polluting firms. Further analysis provides evidence that the environmental regulation encouraged greater market dynamics by inducing the entry of more productive firms and the exit of less productive ones, thereby enhancing industry competitiveness.

On the other hand, we also find that the TCZ regulation had substantial negative externalities on energy-intensive firms, holding their coal consumption constant: a 1 percent increase in energy consumption resulted in a 0.48 percent decrease in total profits. Our estimates are robust to the inclusions of city-specific trends, industry-specific trends, and key *ex ante* determinants of firm growth. The patterns of findings are also essentially unchanged when using SO_2 emissions to measure regulation intensity at the industry level.

[1] The yuan appreciated substantially after our study period, and currently 1 US dollar is worth about 6 yuan. The five million yuan threshold is based on the nominal value of revenues, and using the then exchange rate, five million yuan was worth about $600,000 in the 1998-2005 period.

The rest of the paper is structured as follows. Section II provides background on related studies on effects of environmental regulation and highlights our contributions to the literature and introduces the policy we focus on. Section III describes the data and present descriptive statistics. Section IV explains the identification strategy, and Section V presents empirical results and probes their robustness. In Section VI, we discuss the implications of our empirical findings in the context of total welfare analysis and policy instruments. Finally, Section VII concludes.

II. Background

A. Related Studies on Effects of Environmental Regulation and Our Contributions

Environmental regulations have been contentiously debated around the world. A central component in advocating a regulatory framework is strong associations between air pollution and human health, such as respiratory infections, cardiovascular diseases, lung caner, and infant mortality (Schwartz, Dockery, and Neas 1996; Chay and Greenstone 2003a; Neidell 2004; Kumar and Foster 2007; Currie, Neidell, Schmieder 2009; Jayachandran 2009; Greenstone and Hanna 2012; Tanaka 2012).[2] In the United States, the Clean Air Act Amendments had led to substantial reductions both in air pollution and in infant mortality (Chay and Greenstone 2003b). Further, air quality has been found to affect various other economic outcomes, such as housing prices across the United States (Chay and Greenstone 2004); school absences among elementary and middle school children in Texas (Currie et al. 2009); and labor supply in Mexico City (Hanna and Oliva 2011).

Despite all these benefits associated with air pollution regulations, the conventional neoclassical theory has provoked disagreements of regulatory policies, as regulations impose substantial costs on productivity growth, thereby lower total welfare losses and hamper competitiveness of domestic firms in international markets. In the United States, the Clean Air Act Amendments are found to have caused distortions on productivity (Gollop and Roberts 1983; Barbera and McConnell 1990; Greenstone, List, and Syverson 2012), firm's location decisions (Henderson 1996; List et al. 2003); employment (Greenstone 2002; Deschenes 2010; Walker 2011; Walker 2012); and foreign direct investment inflows and outflows (Eskeland and Harrison 1997; Keller and Levinson 2002; Hanna 2010).[3]

Our study makes three major contributions to such historical debates. First, we focus on a developing country. Although much pollution contributing to global climate change is emitted from

[2] Types and sources of pollution also matter. It is generally known that smaller particulates are more detrimental to human health. For example, PM_{10} or $PM_{2.5}$, whose particles are less than 10 or 2.5 μg/m^3 in diameter, respectively, or toxic gas, such as SO_2, are considered to be the most hazardous because, when inhaled, these particulate matters or gas can penetrate deep into the lungs and interfere with internal gas exchange. Currie and Neidell (2005) find that reductions in carbon monoxide (CO) had greater impacts on reductions in infant mortality in California over the 1990s, compared to PM_{10} and ozone (O_3).

[3] Vast literature finds evidence in the same line. See also Fleishman et al. (2009); ###.

developing countries, evidence on environmental regulations there is scarce. Extrapolating the results from developed countries is not plausible. For example, the magnitude of benefits associated with improved air quality is likely to be amplified in developing countries, due to less capacity to avoid pollution. Tanaka (2012) examines the effect of TCZ policy on infant mortality in China in the period of 1991-2000 and finds greater elasticities of infant mortality reductions with respect to air pollution reductions than those in developed countries, suggesting greater benefits of environmental policy when air pollution is initially higher.[4] Further, the anticipated adverse effect on economic activities is central to regulation aversion in developing countries, where economic growth is often prioritized over environment. Thus, quantitative knowledge stemmed from developing countries is necessary to correctly assess the cost-and-benefit analysis.

Second, we present new evidence on a more recent approach to view environmental policy interventions as *positive* forces leading to increased productivity and enhanced competitiveness in regulated industries. Since it was first proposed by Porter (1991), increasing discussions have been made on constraints and incentive created by regulation, inducing technological change or innovation among affected industries, yet evidence is still inconclusive.[5] For example, Jaffe and Palmer (1997) find that pollution abatement expenditure is significantly correlated with R&D expenditure yet not with tangible outcomes, such as successful patent applications, in the U.S. manufacturing industries.[6] Likewise, Berman and Bui (2001), on one hand, show increases in productivity among oil refineries in Los Angeles despite heavy compliance costs in response to local air pollution regulation, but Gray and Shadbegian (1998), on the other hand, show productive investments to be crowded out by pollution abatement investment in the pulp and paper industry, whereas Greenstone (2002) finds the overall regulation impact on costs and productivity to be small in magnitudes.

Two reasons unique to developing countries may give rise to different outcomes. One is that firms in developing countries tend to rely on low technologies of production, a main cause of both high pollution emissions and low productivity. Hence, the rate of technological change is expected to be larger, relative to firms in developed countries that have already been producing at the efficient production frontier. Another, previously less examined mechanism, is that environmental regulation stimulates industry dynamics, as more productive firms displace less productive ones, leading to aggregated productivity growth in the regulated industry.[7] In the United

[4] In general, developing countries suffer from much higher levels of air pollution, among which China is one of the most polluted. For example, TSP concentration level in China was close to 400 $\mu g/m^3$ (WHO standard is 100 $\mu g/m^3$) and SO_2 concentration level was close to 100 $\mu g/m^3$ (WHO standard is 50 $\mu g/m^3$) in 1995.

[5] This idea is often known as the Porter hypothesis. See Jaffe et al. (1995), Porter and van der Linde (1995), Jaffe, Newell, and Stavins (2003), and Popp, Newell, and Jaffe (2010) for detailed overview of the related literature. Induced technological changes in response to environmental regulation are found in Goulder and Schneider (1999); Goulder and Mathai (2000); Nordhaus (2000); Snyder, Miller, and Stavins (2003); Greaker (2006); Lee, Veloso, and Hounshell (2007). Induced innovation is also examined in another context in response to changing energy prices (Newell, Jaffe, and Stavins 1999; Popp 2001, 2002; Linn 2008).

[6] In a different context, Brunnermeier and Cohen (2003) find an association between pollution abatement expenditures and increased successful patent applications.

[7] A similar argument has recently been highlighted in the context of international trade. See, for example, Melitz (2003) for dynamic industry model that leads international trade to induce more productive firms to enter the export market, while less productive firms exit. Essentially, we probe environmental regulations provide similar constraints and incentives, whose empirical evidence is limited.

States, Ryan (2012) finds that the 1990 Clean Air Act Amendments on the U.S. Portland cement industry resulted in a large increase in the sunk costs of entry, thereby dynamic effects through lower entry rates lead to decreased overall welfare. However, there is little comparable study of productivity dynamics in developing countries, whereas recent evidence highlights a long left-hand tail of extremely low productive firm in China, India, and other low-income countries than the United States and other developed countries (Hsieh and Klenow 2009). Under such a circumstance, regulation may promote Darwinian selection of market dynamics.

Third, we simultaneously examine the externality effects of an air pollution control on non-polluting energy-intensive firms. Coal, the world's most abundant fossil fuel, has been and will continue to be a primary energy source for developing countries, many of whom enjoy its plenty reserves. Hence, whether air pollution regulation, which heavily affects the power sector, has any impacts on clean, yet energy-intensive, firms is an important empirical question. The net effect is theoretically ambiguous, as it depends on the magnitudes of negative forces of energy shortage in lowering economic activities among energy-intensive firms and positive impacts through deploying energy-saving productivity-enhancing technologies or greater market dynamics among the energy-intensive industries.[8]

B. Two Control Zone Policy

China is infamous for its air pollution, due to emissions from a power sector that relies heavily on coal to generate electric power. As the world's largest coal producer, China possesses abundant and relatively cheap coal, which constitutes the country's primary energy resource endowment, accounting for 75.5 percent of total energy production in 1995 (National Bureau of Statistics of China 2006). However, coal generally emits more pollutants than other fossil fuels. As China underwent rapid economic growth, total SO_2 emissions increased from 18.4 million tons in 1990 to 23.7 million tons in 1995, and the ambient air pollution rose to levels detrimental to human health (State Environmental Protection Agency [SEPA] 1996).

During that decade, elevated air pollution gave rise to increasing public concern about adverse impacts on human health. In response, the Chinese government formulated a series of environmental regulatory policies, among which the most notable was the Two Control Zone (TCZ) policy implemented in 1998.[9] This legislation designated prefectures exceeding nationally mandated thresholds as either acid rain control zone or SO_2 pollution control zone. Based on the records in preceding years, prefectures were designated as SO_2 pollution control zone if;

- Average annual ambient SO_2 concentrations exceeded the Class II standard[10],
- Daily average concentrations exceeded the Class III standard, or
- High SO_2 emissions were recorded.

Alternatively, prefectures were designated as acid rain control zone if;

[8] Various studies find the positive effects of energy-efficient technologies on productivity. See #####.

[9] See Tanaka (2012) for detailed history of environmental regulations in China.

[10] According to the Chinese National Ambient Air Quality Standards (CNAAQS) for SO_2, Class I standard designates an annual average concentration level not exceeding $20\mu g/m^3$, Class II ranges $20\mu g/m^3 < SO_2 < 60\mu g/m^3$, and Class III ranges $60\mu g/m^3 < SO_2 < 100\mu g/m^3$. Cities should meet Class II, which is considered to be less harmful.

- Average annual pH values for precipitation were less than or equal to 4.5,
- Sulfate deposition was greater than the critical load, or
- High SO_2 emissions were recorded.

In total, 175 prefectures across 27 provinces were designated as TCZs (Figure A1).[11] They accounted for 11.4 percent of the nation's territory, 40.6 percent of its population, 62.4 percent of GDP, and 58.9 percent of the total SO_2 emissions in 1995 (Hao et al. 2001).

The TCZ status enforced more stringent regulations mandating the use of less high-sulfur coal and the development of clean coal technology. For example;

- No new coal mines producing coal with a sulfur content higher than 3-percent can be established, and existing mines that produce such coal must gradually be shut down or reduce output.
- Construction of any new coal-burning thermal power plants in large and medium-sized prefectures is prohibited.
- All new and renovated power plants are required to use coal with less than 1 percent sulfur content.
- Existing power plants using coal with sulfur content above 1 percent are required to install flue gas desulfurization (FGD) equipment.

C. Effectiveness of the TCZ Policy

Various pieces of evidence provide support of the efficacy of TCZ regulatory actions in reducing pollutant emissions and improving air quality. First, pollution emissions were cleared out at the sources. For example, by the end of 1999, mines producing more than 50 million tons of high-sulfur coal had been closed in TCZs (Hao et al. 2001). Further, small thermal power plants, with output capacity below 50MW, were actively shut down because they were relatively less efficient, had high coal consumption rates, and emitted massive amounts of pollutants.

Second, the amount of pollution emissions fell substantially more in TCZ cities. In total, SO_2 emissions fell by about 3 million tons, and about 71 percent of all factories producing over 100 tons of emissions per year reduced their SO_2 emissions to the standard between 1998 and 2000 among TCZs (He, Huo, and Zhang 2002). This translated into improved overall air quality; Tanaka (2012) shows that TSP concentration level fell by ## and SO_2 concentration fell by ## in TCZ cities between 1991 and 2000. Between 1998 and 2005, the number of prefectures in the SO_2 pollution control zone (the acid rain control zone) meeting the Class II standard rose by 12.3 (3.3) percent, those meeting the Class III standard increased by 4.2 (7.9) percent, and those not meeting the Class III standard fell by 16.5 (11.2) percent (United Nations Environment Programme 2009).

[11] The SO_2 pollution control zone was concentrated in the north due to high SO_2 emissions for heating,[11] whereas the acid rain control zones were primarily in the south, where heat, humidity, and solar radiation combine to create high atmospheric acidity. Hence, acid rain in the south cannot necessarily be attributed to SO_2 emissions traveling down from the north, but is rather due to local emissions. This is even more evident because acid deposition is the greatest in the summer, when wind direction is generally south to north.

Lastly, increasing number of firms was equipped with green technologies. By the end of 2000, the total power capacity with FGD equipment exceeded 10,000MW.

D. Variation in Regulatory Stringency

This subsection describes the sources of variation in TCZ regulations, which provide insight into identifying the effects of stricter environmental regulations on industrial activities.

The first variation is regulation intensity across cities. The designation of TCZ status is highly correlated with regulation intensity across cities given that the regulations were implemented and enforced mainly within the TCZ cities, and the regulations were less stringent in non-TCZ cities. This has resulted in substantial improvements in air pollution in TCZ cities relative to non-TCZ cities (Tanaka 2012). Also, because the designation of TCZ status was based on a uniform standard across the nation, it is less likely to reflect differences in local production decisions.

The second variation is regulatory intensity within cities. The regulations had a greater impact on firms that produced high initial levels of pollution, whereas non-polluting industries, even within TCZ cities, were not subject to the regulations. Firms heavily reliant on coal thus experienced greater regulatory impact, not only because the regulations emphasized the use of clean coal and the adoption of technologies to clean coal, but also because they were initially heavy polluters. On the other hand, because the power sector was a primary contributor of pollution and consumer of coal, the regulations were likely to affect firms initially using more energy or electricity through energy supply shortage.

The interactions of these two sources of variation allow the subsequent analysis to address a number of concerns. First, TCZ status is likely to covary with various other heterogeneities. In particular, because the pollution level is highly correlated with local economic activities, TCZ cities are likely to be more responsive to rapid economic growth during the study period of 1998-2005. The within-city variation across industries controls for variables associated with time-varying shocks common to all firms within a city.

Second, a simple comparison of more and less polluting industries within a city is confounded by heterogeneous industry-specific shocks. For example, the power industries may have faced increased demand for electricity to accelerate greater production. Since the same industries, whether they are polluting or energy-intensive, exist both in TCZ and non-TCZ cities, the across-cities variation defined by TCZ status helps isolate effects through time-varying shocks unrelated to the regulation and common to all firms within an industry.

III. Data and Descriptive Statistics

A. Data

Plant-level Information – Our data on industrial activities come from Annual Surveys of Industrial Production (ASIP) from 1998 through 2005, collected by the National Bureau of Statistics of China. The ASIP covers the census of state-owned firms and all non-state-owned firms whose

annual revenues exceeded five million yuan (about $600,000). For each firm, the survey reports detailed information on their financial and operational characteristics, covering more than 165,000 firms in 1998 to 270,000 firms in 2005. Importantly, ASIP uses a unique identifier for each firm across years, making it possible to construct a panel dataset following individual firms over time. In addition, we use the two-digit industry code to identify their polluting and energy intensive levels, whereas the six-digit geographical code traces down to the county level, making it possible to identify their TCZ status.[12]

We restrict the sample in our main analysis in two ways. First, it is restricted to the manufacturing sector (which consists of 30 industries and accounts for 91.4% of the original data) and the power sector (which consists of 3 industries and accounts for 4% of the original data). Second, we restrict the sample to firms whose total number of units of industrial activity is one, allowing the main analysis to focus on observations at the plant level. The original data contains information regarding geographical location for the firm, in some cases where the headquarters are located, but does not identify exact locations of each plant. This causes measurement error in determining the extent to which the firm is effectively regulated under the TCZ policy, when multiple plants operate in both TCZ and non-TCZ cities. The majority of the original sample (86.7%) is indeed plant-level observations.

It is worth noting that we do not have observations on small non-state firms whose revenues are less than five million yuan.[13] Furthermore, the data may be skewed by the fact that firms are dropped out of the sample if their revenues went below the threshold or they exited the market. These were generally smaller firms, especially small power plants, disproportionately affected by the regulations since they tended to be inefficient and to produce great pollution per output level. . The main analysis will address this bias in a number of ways.

TCZ Information – The TCZ regulatory status is reported in the document "Official Reply to the State Council Concerning Acid Rain Control Areas and Sulfur Dioxide Pollution Control Areas," published by the State Council in 1998. The document lists the names of all places that were designated either acid rain control zone or SO_2 control zone (Figure A1). We follow Tanaka (2012) in determining the TCZ status at the county level. The assignment was made primarily at the county level, which can be directly linked to ASIP. If the assignment was made at the prefecture level, all districts and cities that belong to the prefecture are given as the TCZ status assigned to the prefecture. The document states that impoverished counties are exempt from the regulations, even when they belong to a TCZ prefecture. Most prefectures specify exact counties that are or are not exempted, but when the names of exempted counties are not listed, we eliminate observations in these counties to reduce measure error in the TCZ status. The TCZ status may have

[12] Chinese administrative divisions consist of six levels. Six-digit geographical codes can be decomposed into three two-digit parts; one for the provincial level, the second one for the prefecture level, and the third one for county level. The county level includes districts (*shixiaqu*), cities (*xianjishi*), and counties (*xian*). In general, districts and cities are urban areas in the prefecture, while counties are outer, rural areas.

[13] How big is this share out of all industries in China? Jefferson and Su (2005) note there were 7.97 million enterprises in 1998, and "the middle circle" (which corresponds to the sample above 5 million yuan in revenue) makes up less than five percent. Our observations are much lower – possibly because we only collect observations in manufacturing industries.

changed over time when a county was upgraded to a district or a city. Though this is rare, we drop observations in these places to further avoid contamination in the TCZ status.

Pollution- and Energy-Intensity Information – The TCZ policy disproportionately targeted polluting firms relative to non-polluting firms, and because the power sector was heavily affected, energy-intensive firms were likely to receive externality effects. However, as we lack information on emission or energy consumption levels at the firm level, we use the two-digit industry-level observations in the baseline year, reported by the National Bureau of Statistics of China.[14] An industry-level pollution-intensity is measured based on the national share of SO_2 emissions, or the national share of coal consumption. Coal consumption is highly correlated with emissions, as coal is a primary contributor of SO_2. Accordingly, the TCZ policy imposed strict requirements for the use of less polluting coals and the adoption of technologies to clean coal. Industry-level energy-intensity is measured by the national share of electricity consumption or energy consumption.

B. Descriptive Statistics

The economic variables used in the main analysis and their descriptive statistics are reported in Table 1. These are: employment, capital, total assets, total liabilities, total revenues, total costs, and total profits. All monetary values are in the 2000 RMB. See Data Appendix for detailed variable definitions.

In total, we have a sample of close to 140,000 firms in 1998 up to more than 250,000 firms in 2005, with 33 two-digit industries over the period of 1998 through 2005, resulting in a total of more than 1 million firms by year observations. Panel B shows that about 70% of these firms operated in TCZ cities. The share of SO_2 emissions and coal consumption from each industry was small – the mean share was only about 2% for both variables. This was also true for energy consumption – the mean share was only about 3% by using either energy or electricity consumption. Table 2 provides more detailed variation in pollution- and energy-intensity across industries, revealing that the electric power production industry had the highest SO_2 emissions of 55.9%, while it was also the largest coal consumer, using 32% of total national coals. In total, the power and manufacturing sectors emitted more than 95% of SO_2 and consumed 78% of coal. On the other hand, the smelting and pressing of ferrous metals industry accounts for the largest energy consumption of 14.1%. The power and manufacturing sectors jointly consumed 68% of total electricity and 65.8% of total energy across the nation.

IV. Identification Strategy

Our empirical strategy is designed to estimate the effect of TCZ regulations on industrial activities for different levels of pollution and energy intensity. Essentially, it is based on a difference-in-differences framework with two sources of variation. The first variation comes from the com-

[14] We use 1995 as the baseline year for three variables discussed below, except SO_2 emissions that use 1996 information, simply because no such information was reported for SO_2 in 1995. Using 1995 instead of 1997 provides pre-treatment information less affected by the amendment of APPCL in 1995. No information for any of the four measures was reported prior to 1995.

parison across geographical locations between TCZ cities and non-TCZ cities. This comparison captures not only the impact of the more stringent regulations imposed by TCZ policy but also inherent heterogeneities across cities. For example, firms in TCZ cities were initially polluting more, suggesting greater industrial activities in the first place. The second variation compares more pollution-intensive (or energy-intensive) industries and less pollution-intensive (or less energy-intensive) industries, effectively removing the effect common within the city but differing across pollution (or energy) intensity levels.

In particular, the data are fit to the following regressions:

$$(1) \qquad Y_{fict} = \beta_0 + \beta_1 \text{Pollution}_i \times \text{TCZ}_c + \beta_2 \text{Energy}_i \times \text{TCZ}_c + \eta_{ct} + \lambda_{it} + \varepsilon_{fict},$$

where Y_{fict} is an outcome of interest for firm *f*, in industry *i*, located in city *c*, in year *t*, *Pollution* measures the share of pollution emissions, and *Energy* measures the share of energy consumption at the industry level. The outcomes include total number of employees, capital stock, total asset, total profit, sales revenue, and sales costs.[15] ε_{fidt} is the idiosyncratic error term, and all standard errors are clustered at the city level.

The coefficient of interest, β_1, captures the direct effect of TCZ policy on a firm belonging to an industry with an increasing share of pollution, operating in a TCZ district. The estimate is expected to be positive if the regulation led to improved productivity or more industrial activity; it is expected to be negative if the regulation had adverse effects on firm performance. The coefficient, β_2, measures the indirect effect of TCZ policy on a firm belonging to an industry with a greater share of energy consumption, in a TCZ district. Because the power industry was the most heavily targeted by TCZ policy, it is expected to have positive externalities if energy supply shortage led to improved technology among energy intensive industries; it is expected to have negative externalities if it retarded industrial activities among industries consuming more energy.

The estimates may be biased if there is a differential trend across cities and/or between more and less pollution- (or energy-) intensive industries. The panel structure of our dataset allows us to circumvent these endogeneity issues in two ways. First, we include city-by-year fixed effects (η_{ct}), which non-parametrically control for time-varying shocks common to all firms in a particular city. The inclusion of city-by-year fixed effects is particularly important because firms in a TCZ city are more likely to be located in industrial areas prone to factors associated with citywide growth trends. Second, the inclusion of industry-by-year fixed effects (λ_{it}) helps remove trends among all firms in a particular industry that are unrelated to the environmental regulation. This controls for unobservable demand and/or supply shocks to a particular industry during this period (i.e., the differential pattern of demand structure for polluting products in a rapidly growing economy or of supply structure based on price/quantity of coal or electricity).

Overall, the estimated regulation effects are purged of many potential sources of bias and are robust to all unobserved transitory determinants of growth common to more polluting and less polluting industries as well as all unobserved factors contributing to firm's growth within a city whose effects are allowed to vary over time. However, there may be several other sources of bi-

[15] We keep these values in level. Although log transformations of these values would allow the effect of the regulations to be proportional to the size of the firms, they cannot be applied since our data contain a large number of zeros or even negatives.

as, such as differences in the permanent characteristics of firms in TCZ. The inclusion of firm fixed effects is, unfortunately, not feasible because an industry's pollution- or energy-intensity level and the TCZ status do not change over time. Instead, we control for a vector of variables that are pre-determined as of 1998 (i.e., firm size, age, and ownership type) that have been identified as important determinants of firm growth (Dunne, Roberts, and Samuelson 1989; Greenstone 2002; Huang, Jin, and Qian 2012).

Another potential bias results from the fact that firms more severely affected by the regulation may drop out of the sample due to the revenue threshold or exited the market. In this case, the regression would attribute an increased industrial performance to the regulation, though the actual mechanism was shifts in industry dynamics. Firm-level of observations in our dataset allows us to probe this eventuality by investigating various samples of firms that operated throughout the entire period (stayers) and that did not operate in either the first year (entrants) or the last year (dropouts).

V. Empirical Results

This section provides regression results based on the econometric framework discussed above. In the first subsection, we present the mean effect of TCZ policy over the entire period for different levels of pollution and energy intensity. We show different specifications for profits to probe the robustness to inclusions of various controls, and we present estimated effects on various other measures of industrial activity. In the following subsection, we examine the effect on industry dynamics via entry and exits as a potential mechanism explaining the main results. Then, the third subsection addresses differential impacts of TCZ policy, depending on the size of firms or types of ownership. Lastly, we conduct additional robustness checks.

A. Main Results

Table 3 reports the main coefficients of interests, β_1 and β_2 in equation 1, using firm-by-year level of observations in the period of 1998-2005. Across all specifications, city-by-year fixed effects and industry-by-year fixed effects are controlled, and thus the estimates effects are purged of spurious correlations arising from differential city-specific trends or industry-specific trends. Positive values of the coefficients imply that the TCZ regulation led to an increase in the outcome variable, whereas negative values indicate the opposite.

We first investigate the effect of TCZ policy on total profits. The column 1 specification presents the estimated policy effect on pollution-intensive firms within a TCZ city. The estimated effect is positive and significant at the 1 percent level, suggesting that profits were increased in response to the TCZ regulations among polluting firms. The estimate indicates that a 1-percentage point increase in the share of coal consumption leads to a 430 yuan increase in a firm's profits in a TCZ city. However, in this specification, the estimate may be biased if the regulations had externality effects on non-polluting energy-intensive firms. The bias can go either direction; the estimated effect is understated if the regulation exhibited negative externalities on energy-intensive firms due to energy shortage, or is overstated if the regulation also led to enhanced performance among energy-intensive firms.

Column 2 thus includes an additional control for energy-intensive firms in an effort to disentangle externalities, if any. The estimated effects on polluting firms increase in magnitude and remain highly significant, whereas the coefficient for energy-intensive firms is negative and significant at the 1 percent level. This suggests that, on the one hand, the regulation increased profits among polluting firms, and, on the other hand, led to decreased profits among firms using higher share of energy. The estimate indicates that a 1-percentage point increase in energy share leads to a 450 yuan decrease in profits in a TCZ city.

In column 3, we additionally control for firms' pre-characteristics that are likely to affect subsequent growth (firm's size, age, and ownership type). As expected, their inclusion captures some of the differences in profits, but the estimated effect remains highly significant, and the order of magnitudes are essentially changed. The elasticity from this preferred estimate in column 3 suggests that a 1-percent increase in coal share led to 0.58 percent increase in profits, whereas a 1-percent increase in energy share led to 0.48 percent decrease in profits.

Columns 4-9 report estimated effects on various other financial and employment status, in an effort to assess overall impacts on industrial performance. First, the comparison between column 4 and 5 reveals whether changes in revenues or costs were the driving factor contributing to the changes in profits.[16] It shows that although costs of principal operations increased among polluting firms, increases in product sales revenue, the main business income, were much greater. This reflects the fact that firms were required to install costly technologies, which in turn generated higher revenues through improved productivity.

On the other hand, energy-intensive firms experienced decreases in revenues and increases in costs, while neither point estimates is statistically significant. The negligible and insignificant effects on costs among energy-intensive firms can be explained by the fact that, after controlling for pollution intensity, they were free from TCZ regulation, and thus assumed no additional cost burden.[17]

Similar patterns are observed for assets, liabilities, and capital. The point estimates indicate that employment has decreased among polluting firms but increased among energy-intensive firms, although neither estimates are significantly different from zero.

Overall, evidence suggests that the environmental regulation had positive effects on industrial performance among the targeted polluting industries, despite requiring them to incur some costs of adjustment. On the other hand, the environmental regulation imposed some negative spillover effects among the non-targeted, energy-intensive industries, potentially due to energy shortage.[18]

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[16] Note that the profit is not defined here as a simple difference between the revenues and costs, although these two variables are still key determinants. See the Data Appendix for detailed definitions of these variables.

[17] Note that because energy-intensity and pollution intensity are likely to be correlated, energy-intensive firms had increased costs, without controlling for pollution-intensity (results are not shown). The similar pattern is true for most of the dependent variables we investigate in this paper. This highlights an importance of controlling for both pollution- and energy-intensity simultaneously to isolate direct and indirect effects. Accordingly, previous studies that have focused on only either one of the two are likely to suffer from biased estimates.

[18] Ideally, we can also examine whether there was energy shortage by looking at fluctuations in energy prices. However, this is not feasible for two reasons. First, we could not find information on energy prices at the county level for

B. Effects on Industry Dynamics via Entry and Exit

In this subsection, we explore the mechanism leading to the results presented thus far. The previous subsection concluded that the TCZ policy resulted in improved performance among polluting industries and diminished performance among energy-intensive industries. These results can be explained either because existing firms were required to install better technologies leading to increased production efficiency, or because of changing industry dynamics due to firms' entry and exit, or both. The longitudinal nature of our dataset using the unique firm identification allows us to observe their performance over time. Formally, we re-estimate the main analysis with controlling for firms' entry and exit decisions. The results are reported in in Table 4, and all specifications include city-by-year fixed effects and industry-by-year fixed effects as well as predetermined characteristics of firm's growth.

First, we assess the effect of the environmental regulation on the balanced panel of firms. Specifically, we restrict the sample to firms whose information is recorded in all years throughout the period of 1998-2005 (we call them the "stayers"). If the TCZ policy only caused non-profitable firms to shut down, without any effect on existing firms, the estimates would become zero or even negative. Panel A of Table 4 provides evidence that this is not the case. The point estimate for profits is positive and the magnitude is not statistically significantly different from the main analysis.

Second, we address the effect of the environmental regulation on firms that entered the market after 1998. Now the sample includes firms that started operation at some point after 1998, called entrants.[19] Because the control group is also composed of entrants, the estimated coefficients can be interpreted as an impact of the environmental regulation on characteristics of firms entering the market. Panel B of Table 4 suggests that the more the entrants were targeted by the regulation, the better economic performance they began with. The point estimate on profits more than doubles and one on costs more than triples, suggesting that the regulation increased entry costs, which in turn encouraged the process of selecting only productive firms into the market.

One may argue that highly efficient firms self-select into more growing areas, which are spuriously correlated with the TCZ designation. Though this is a plausible argument, evidence on energy-intensive firms refutes this is the case. It shows that new energy-intensive entrants did worse in the TCZ cities. Given a high correlation between coal share and energy share, it is unlikely that there are intrinsic differences in location decisions or differential management systems between pollution- and energy-intensive firms.

Third, we explore potential characteristics of those who shut down or exited the market. The sample is restricted to firms that reported in 1998 but did not continue reporting throughout the period, called "dropouts." As expected, Panel C of Table 4 shows somewhat lower economic

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the same period. Second, even if we did, the energy prices were severely regulated by the government, and thus price changes do not truly reflect the market conditions.

[19] Note that although we call them "entrants," they may have entered the market a long time ago without operation in 1998 or have total revenue below the threshold and thus were not reported.

performance among dropouts. Interestingly, though, their outcomes are not quite different from those on stayers. It remains unclear what differentiated the shut-down decisions between the two groups. Overall, the results indicate that firms shut down or exited the market if their economic performance did not improve in response to the environmental regulation, leading to a more dynamic industry structure.

Lastly, the regulation effects are estimated for existing firms as of when the policy was introduced.

C. Heterogeneities in Effect

In this subsection, we investigate whether the regulation had heterogeneous effects across firm's characteristics.

First, we explore whether the size of firms, an important pre-determined characteristics of firm's growth, led to differential effects. As mentioned above, our dataset contains all state-owned firms but non-state-owned firms only if their revenues exceed five million yuan. If large firms were relatively more responsive to the regulation, the estimated effects in the previous analysis may be biased. Table 5 presents the results for different sizes of firms. It shows smaller firms generally benefited more from the regulation than larger firms did among pollution intensive firms. Notably, small firms increased revenues, assets, capital, and employment, and the point estimates are statistically significant at 1 percent level. Large firms, however, decreased revenues, assets, capital, and employment, most of which are highly statistically different from zero. The finding is reassuring that the positive efficacy of the regulation on polluting firms, estimated in the main analysis, may be understated, if even smaller non-state-owned firms are included into the sample.

On the other hand, Table 5 presents the opposite pattern for energy-intensive firms. It shows that the magnitude of negative externality effects was larger for small firms; large firms indeed increased revenues, assets, capital, and employment, in response to the regulation. This indicates that large energy-intensive firms received positive externality effects. The findings again show that the negative externality effect on energy-intensive firms, estimated in the main analysis, may be understated, if even smaller firms are included into the sample.

Second, we investigate whether the effects differed by the types of ownership, which is another important determinant of firm's growth. In particular, we repeat the main analysis, using the samples of state-owned firms and non-state-owned firms, whose results are presented in Panel A and Panel B of Table 6 respectively. This exercise provides suggestive evidence whether the close connection with the central government helps amplify or avoid the effect of the regulation. Contrary to popular belief, regardless of the types of ownership, polluting firms increased profits, thought the magnitude is larger for state-owned firms. On the other hand, both types of energy-intensive firms decreased profits, thought the magnitude is larger for state-owned firms. The results indicate that the main findings do not appear to be driven much by the types of ownership.

D. Robustness Checks

We now probe the robustness of the main findings to an alternative measure of pollution intensity.[20] In particular, we use the national share of SO_2 emissions by industry as of 1996 as a measurement of pollution intensity. Table 7 presents the results, showing that the patterns of findings are essentially the same with the main analysis.[21]

VI. Conclusion

This paper examines the effect of a stricter environmental protection regime on industrial performance in China. The findings suggest that pollution-intensive firms increased profits in response to the environmental regulation, whereas clean yet energy-intensive firms decreased their profits in response to the environmental regulation. Additional pieces of evidence suggest that industry performance is enhanced both through improved profits among existing firms but through encouraging greater industry dynamics via the entry of efficient firms and the exit of low productive firms. The results are robust to various specifications and inclusions of city-by-year fixed effects, industry-by-year fixed effects, and pre-determined characteristics of firm's growth.

Our findings shed light on two outcomes that have been paid little attention. First, although the direct effect of environmental protection, measured by compliance costs, may not be trivial, induced innovation and/or adoption of cleaner and more productive technologies are efficiency-enhancing, translating into a positive spillover impact on industrial performance. In addition, changes in the industry structure give rise to net positive improvements in aggregated productivity. Second, when the power sector is subject to stringent environmental regulations, as is often the case with developing countries, clean energy-intensive firms are likely to receive negative externality effects.

These outcomes may be unique to circumstances in developing countries, since firms in developed countries are relatively already equipped with high efficient technologies, and environmental regulations are more likely to translate into reduced production, reduced employment, or increased plant's death. Further, many developed countries rely relatively more on nuclear or other clean sources of electricity generation, including renewable sources, compared to developing countries, and thus the power sector is not disproportionately affected.

As China continues to be the largest CO_2 emitter in the world, since overtaking the United Stated in 2005, producing more than 20 percent of world emissions in 2008, this study highlights important implications not only for future environmental protection in China but also for global worming.

[20] As previously discussed, coal consumption is strongly associated with SO_2 emissions. The correlation coefficient between the two in Table 2 is 0.95.

[21] Indeed, we repeat all analyses above using SO_2 emissions to measure pollution intensity and find the pattern of findings to be essentially unchanged. The results are available from the author upon request.

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Table 1: Descriptive Statistics of Key Variables

Variables	Obs.	Mean	Std. Dev.
Panel A: Key Economic Variables			
Total profits	1,133,839	2.429	41.888
Total revenues	1,133,839	56.056	457.820
Total Costs	1,133,839	47.567	403.428
Total assets	1,136,101	63.364	568.499
Total liabilities	1,133,839	38.685	298.795
Capital	1,133,839	16.203	112.318
Employment	1,136,101	253.135	761.447
Panel B: Other Variables			
Firms in TCZ	1,136,103	0.69	0.46
State-owned firms	1,134,139	0.20	0.40
Private firms	1,134,139	0.74	0.44
Share of coal consumption	1,136,103	0.024	0.040
Share of SO_2 emissions	1,136,103	0.023	0.050
Share of energy consumption	1,136,103	0.030	0.041

Notes: The level of observation is a firm-by-year in 1998-2005 for 138,617 firms in 1998, growing up to 250,844 firms in 2005. All monetary values are in constant thousands of 2000 RMB.

Table 2: Pollution and Energy Intensity, by industry

Code	Industry	SO_2	Coal	Energy
13	Agricultural byproduct processing	0.00%	1.27%	1.50%
14	Food industry / manufacturing of food	3.05%	0.88%	0.92%
15	Beverage industry/ manufacture of beverage	3.05%	0.71%	0.76%
16	Tobacco industry / manufacture of tobacco	3.05%	0.14%	0.17%
17	Textile industry	2.02%	1.84%	2.69%
18	Textile garments, footwear and headgear industry/ manufacturing of textile garments, footwear, footwear and headgear	0.00%	0.09%	0.25%
19	Leather, fur, down and its related manufacturing	0.12%	0.17%	0.22%
20	Processing of timbers and manufacture of wood, bamboo, cane, palm, and straw	0.00%	0.26%	0.29%
21	Manufacture of furniture	0.00%	0.05%	0.08%
22	Manufacture of paper and paper products	2.13%	1.55%	1.63%
23	Printing, reproduction of recording media	0.05%	0.06%	0.16%
24	Manufacture of goods for culture and education, and sports wear	0.00%	0.02%	0.05%
25	Processing of petroleum, coking, processing of nucleus fuel	1.06%	5.83%	4.24%
26	Manufacture of chemical raw material and chemical materials	7.94%	7.85%	12.06%
27	Medical and pharmaceutical manufacturing	0.81%	0.66%	0.92%
28	Chemical fiber manufacturing	1.07%	0.60%	0.97%
29	Manufacture of rubber	0.49%	0.41%	0.49%
30	Manufacture of plastic	0.16%	0.23%	0.41%
31	Manufacture of nonmetal minerals	7.30%	9.75%	9.95%
32	Smelting and pressing of ferrous metals	6.25%	9.38%	14.13%
33	Smelting and pressing of nonferrous metals	4.69%	0.98%	2.17%
34	Metal manufacturing	0.16%	0.34%	0.76%
35	General purpose equipment manufacturing	0.00%	0.60%	1.26%
36	Special purpose equipment manufacturing	0.00%	0.47%	0.83%
37	Transport equipment manufacturing	0.00%	0.62%	1.05%
39	Electrical machinery and equipment manufacturing	2.28%	0.25%	0.48%
40	Communication equipment, computer and other electronic equipment	0.00%	0.10%	0.25%
41	Measuring instrument and machinery for culture and educational activity and office work	0.00%	0.05%	0.11%
42	Art work and other manufacturing	0.00%	0.66%	0.94%
43	Recycling and disposal of waste	0.00%	0.00%	0.00%
44	Production and supply of electric power and heating power	55.85%	31.81%	5.38%
45	Production and supply of gas	0.00%	0.55%	0.26%
46	Production and supply of water	0.00%	0.03%	0.37%
	Manufacturing and power sectors total	95.41%	78.23%	65.75%
	National total	13,098,346	137,676.5	131,175.6

Notes: Each entry, except the last row, indicates the share to the national total for SO2 emissions (in ton), coal consumption (10,000 tons), and energy consumption (in 10,000 tons of SCE). The readings are based on the baseline observations in 1995, except for SO2 in 1996 since SO2 emissions are not available in 1995. SO2 emission is not disaggregated between industry codes 14, 15, and 16, and thus the reported estimates are the total of these three industries. Industries whose SO2 emissions are

not reported are considered to have zero emission to reflect their negligible levels. The original data report the aggregated SO2 emissions for the production and supply of electric, gas, and water, yet we take the number as the emissions of electric industry. Further, among the electric industry, SO2 emissions and coal consumptions are set to be zero for hydro, nuclear, and other energy generations, and supply of electricity and heat production and supply.
Source: Data from National Bureau of Statistics of China

Table 3: Estimated Effect of TCZ Policy on Industrial Performance

	Dependent variables								
	Profits			Revenues	Costs	Assets	Liabilities	Capital	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Coal share × TCZ	43.00***	67.23***	59.15***	161.5*	86.17	582.5***	337.3***	316.4***	-176.9
	(11.18)	(18.78)	(18.42)	(88.84)	(75.25)	(202.2)	(126.5)	(54.42)	(112.3)
Energy share × TCZ		-44.97***	-38.85**	-15.29	36.88	-147.5	-97.38	-188.6***	192.8
		(15.37)	(15.20)	(81.40)	(69.66)	(177.2)	(109.4)	(44.21)	(144.3)
City trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm characteristics	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,133,839	1,133,839	1,100,521	1,100,521	1,100,521	1,101,595	1,100,521	1,100,521	1,101,595
R^2	0.022	0.022	0.032	0.092	0.089	0.126	0.170	0.127	0.187

Notes: All monetary values are in constant thousands of 2000 RMB. Robust standard errors, clustered at the county level, are reported in parentheses. The coal and energy share are in decimal, and TCZ is a binary variable. Robust standard errors, clustered at the county level, are reported in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 4: Estimated Effect of TCZ Policy on Industry Dynamics

	Dependent variables						
	Profits	Revenues	Costs	Assets	Liabilities	Capital	Employ-ment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Stayers							
Coal share × TCZ	38.45	-176.3	-192.0	-207.1	-118.6	124.5	-459.9**
	(30.28)	(187.2)	(170.6)	(346.0)	(216.4)	(81.02)	(226.8)
Energy share × TCZ	0.117	403.3*	350.9*	771.4*	393.8	10.59	540.7
	(31.95)	(217.6)	(191.2)	(410.6)	(241.6)	(86.69)	(489.5)
Observations	217,088	217,088	217,088	217,129	217,088	217,088	217,129
R^2	0.074	0.129	0.124	0.171	0.171	0.182	0.181
Panel B: Entrants							
Coal share × TCZ	103.5***	451.3***	301.7**	1,637***	1,018***	528.9***	-56.20
	(27.56)	(142.8)	(117.9)	(414.5)	(268.6)	(107.4)	(165.6)
Energy share × TCZ	-73.30***	-287.2**	-171.2*	-1,064***	-675.5***	-360.8***	-12.09
	(22.49)	(124.1)	(103.7)	(325.7)	(209.3)	(81.22)	(139.0)
Observations	600,662	600,662	600,662	601,007	600,662	600,662	601,007
R^2	0.031	0.110	0.108	0.193	0.217	0.132	0.233
Panel C: Dropouts							
Coal share × TCZ	23.39	5.606	-42.20	26.41	-40.82	229.9***	53.92
	(32.94)	(110.2)	(83.44)	(224.2)	(125.0)	(85.27)	(152.2)
Energy share × TCZ	-20.57	47.89	91.23	246.6	178.7	-140.7**	92.06
	(25.19)	(94.49)	(75.49)	(202.5)	(109.8)	(65.48)	(164.2)
Observations	282,795	282,795	282,795	283,483	282,795	282,795	283,483
R^2	0.176	0.193	0.192	0.129	0.253	0.226	0.310
Panel D: Starters							
Coal share × TCZ	34.05	-43.88	-75.10	-26.29	-54.20	195.0***	-293.6**
	(22.29)	(116.4)	(103.0)	(209.7)	(126.4)	(56.29)	(139.9)
Energy share × TCZ	-19.43	160.9	172.1*	436.9**	263.3**	-82.07	405.6
	(18.85)	(112.7)	(99.42)	(217.7)	(126.5)	(51.59)	(253.8)
Observations	499,859	499,859	499,859	500,588	499,859	499,859	500,588
R^2	0.060	0.115	0.111	0.118	0.164	0.159	0.186
City trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Panel A includes firms whose observations are available throughout the period of 1998-2005 (stayers), Panel B includes firms whose observations become available after 1999 (entrants), Panel C includes observations whose observations are available in 1998 but become not available at some point before 2005 (dropouts), and Panel D includes firms whose observations are available in 1998 (start-ers). All monetary values are in constant thousands of 2000 RMB. Robust standard errors, clustered at the county level, are reported in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Estimated Effect of TCZ Policy, by Size of Firms

	Dependent variables						
	Profits	Revenues	Costs	Assets	Liabilities	Capital	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Large							
Coal share × TCZ	-42.00	-2,467**	-2,351**	-5,621***	-3,604***	-374.0	-3,413***
	(143.7)	(1,039)	(941.1)	(2,001)	(1,244)	(405.6)	(1,198)
Energy share × TCZ	237.3	4,392***	3,871***	9,988***	5,540***	1,419***	8,180**
	(186.3)	(1,501)	(1,316)	(2,680)	(1,594)	(532.6)	(3,489)
Observations	31,793	31,793	31,793	31,860	31,793	31,793	31,860
R^2	0.348	0.401	0.410	0.518	0.344	0.329	0.282
Panel B: Medium							
Coal share × TCZ	13.43	-187.3	-194.0*	641.9***	495.6***	330.1***	-91.33
	(22.72)	(127.0)	(114.0)	(232.7)	(170.9)	(70.48)	(169.3)
Energy share × TCZ	-13.03	298.0**	329.8**	-137.4	-207.0	-137.2**	-261.9
	(20.46)	(147.4)	(131.5)	(213.7)	(152.0)	(63.68)	(287.3)
Observations	81,788	81,788	81,788	81,946	81,788	81,788	81,946
R^2	0.152	0.148	0.147	0.268	0.237	0.179	0.245
Panel C: Small							
Coal share × TCZ	28.57***	248.9***	196.3***	796.9***	523.4***	270.7***	218.4***
	(10.26)	(47.37)	(36.18)	(145.2)	(93.61)	(41.72)	(61.96)
Energy share × TCZ	-20.77***	-184.8***	-144.2***	-582.2***	-388.6***	-199.4***	-432.9***
	(7.810)	(38.90)	(30.79)	(111.0)	(71.99)	(31.40)	(54.99)
Observations	986,940	986,940	986,940	987,789	986,940	986,940	987,789
R^2	0.026	0.024	0.023	0.087	0.095	0.067	0.073
City trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All monetary values are in constant thousands of 2000 RMB. Robust standard errors, clustered at the county level, are reported in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Estimated Effect of TCZ Policy, by Ownership

	Dependent variables						
	Profits	Revenues	Costs	Assets	Liabilities	Capital	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: State-owned							
Coal share × TCZ	70.01**	53.80	-17.39	220.2	134.6	293.4***	-482.9**
	(27.28)	(164.4)	(147.1)	(319.6)	(203.9)	(90.33)	(226.3)
Energy share × TCZ	-34.19	571.2***	599.6***	1,195***	659.7***	16.11	1,817***
	(28.92)	(207.0)	(181.0)	(419.4)	(242.4)	(94.21)	(594.0)
Observations	219,779	219,779	219,779	220,734	219,779	219,779	220,734
R^2	0.086	0.204	0.209	0.173	0.231	0.180	0.246
Panel B: Private							
Coal share × TCZ	35.54**	255.6***	199.4***	814.7***	484.8***	260.3***	21.66
	(16.05)	(65.59)	(46.17)	(207.6)	(126.9)	(59.11)	(80.57)
Energy share × TCZ	-24.94**	-187.3***	-146.3***	-552.8***	-331.6***	-183.2***	-183.5***
	(12.45)	(57.54)	(43.50)	(158.4)	(96.92)	(44.71)	(70.00)
Observations	815,127	815,127	815,127	815,243	815,127	815,127	815,243
R^2	0.039	0.056	0.051	0.111	0.104	0.126	0.147
City trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes

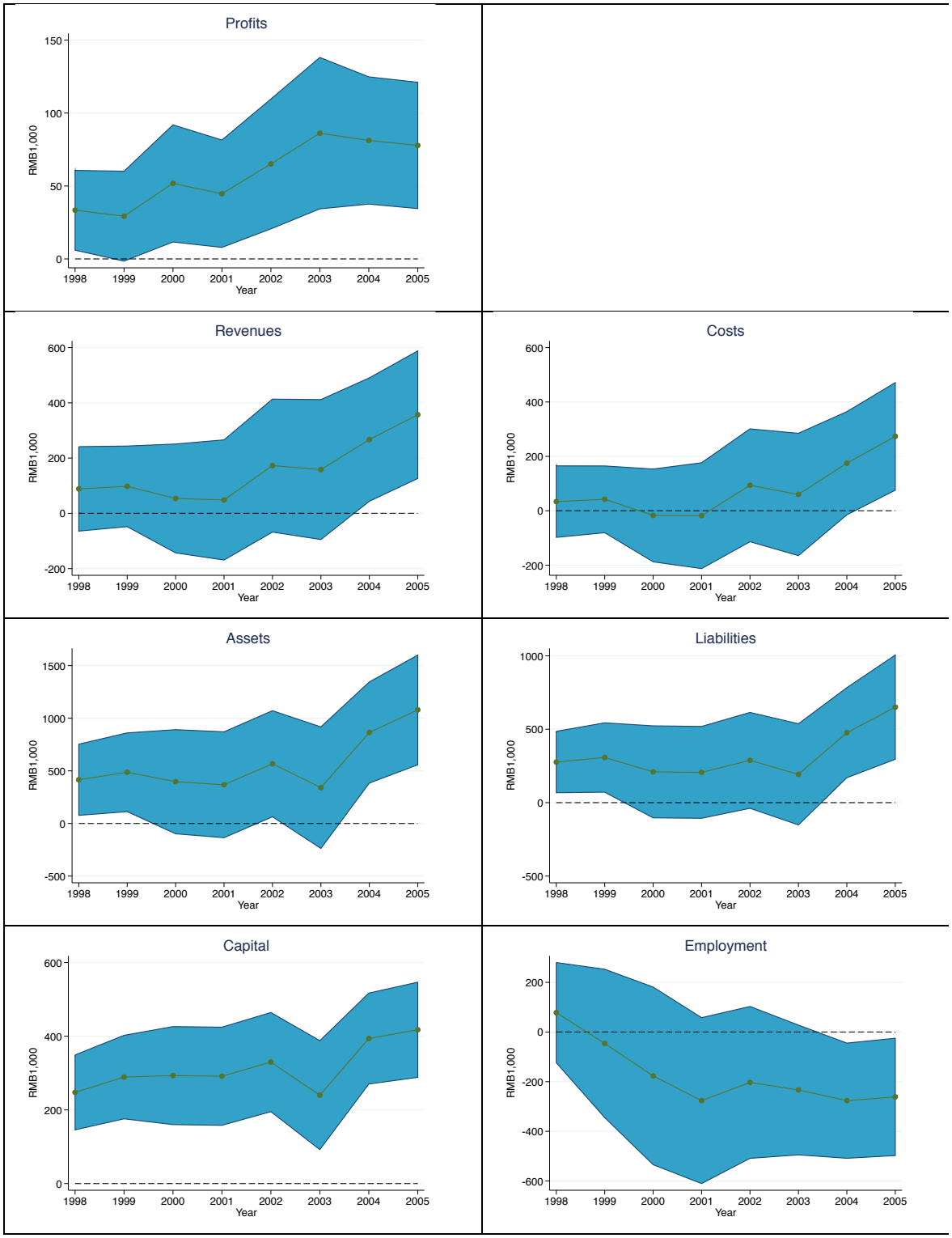
Notes: All monetary values are in constant thousands of 2000 RMB. Robust standard errors, clustered at the county level, are reported in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Estimated Effect of TCZ Policy on Industrial Performance, using SO_2 Share

	Dependent variables								
	Profits			Revenues	Costs	Assets	Liabilities	Capital	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SO_2 share × TCZ	37.49***	41.56***	36.71***	94.61**	47.04	377.0***	212.5***	183.6***	-124.6**
	(9.093)	(10.47)	(10.26)	(46.62)	(38.43)	(106.5)	(66.64)	(29.89)	(59.97)
Energy share × TCZ		-19.43**	-16.48**	49.56	73.80*	62.42	28.11	-60.30***	135.8
		(8.103)	(8.102)	(48.54)	(40.67)	(95.79)	(56.52)	(22.59)	(115.2)
City trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm characteristics	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,133,839	1,133,839	1,100,521	1,100,521	1,100,521	1,101,595	1,100,521	1,100,521	1,101,595
R^2	0.022	0.022	0.033	0.092	0.089	0.126	0.170	0.127	0.187

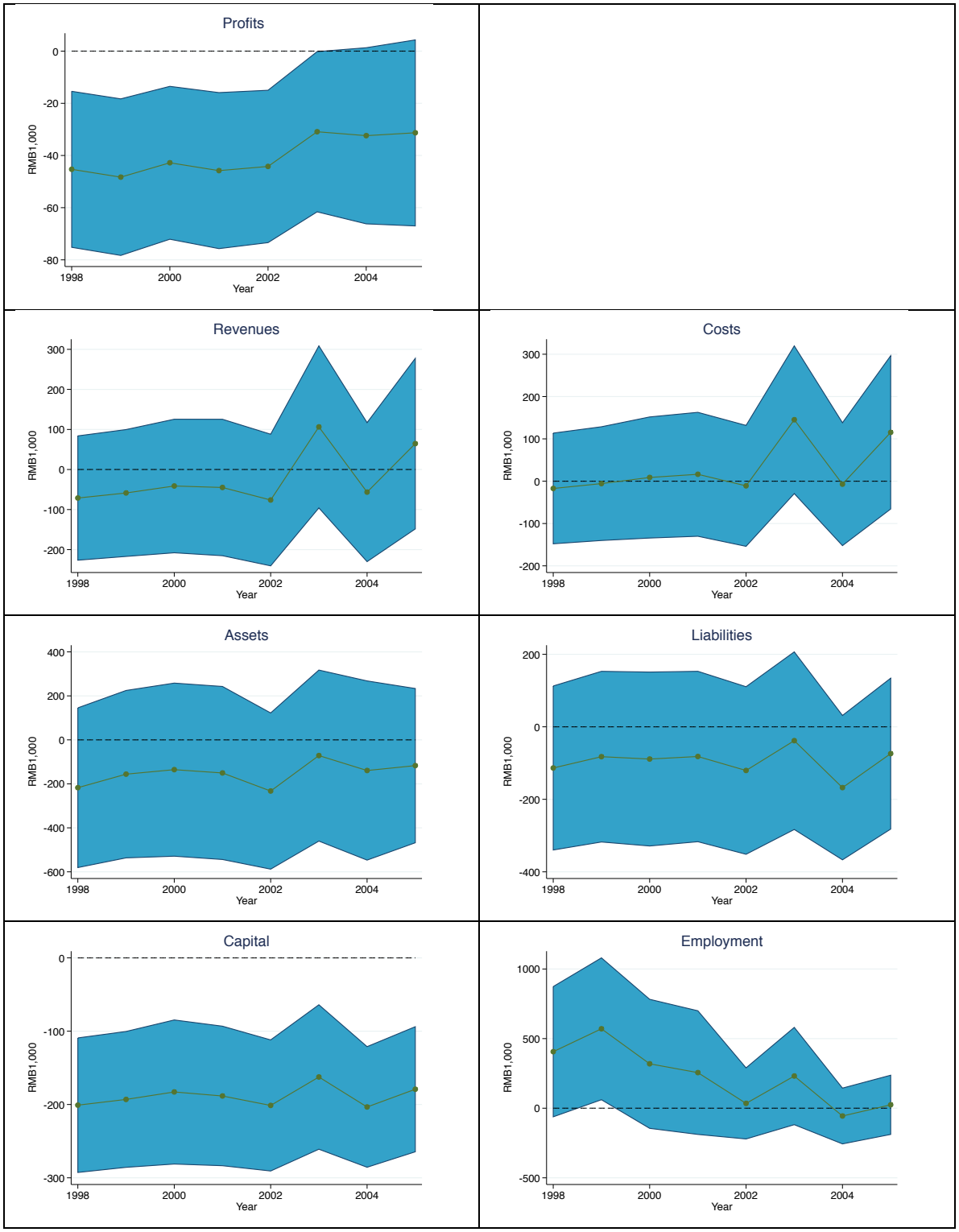
Notes: Robust standard errors are reported in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Figure 1: Year-level Effect of TCZ Policy on Pollution-Intensity Firms



Notes: The area corresponds to the 95% confidence interval for the coefficients of respective year, estimated using equation (#). All regressions control for city-specific trends, industry-specific trends, pre-determinants of firm growth, and energy share times TCZ.

Figure 2: Year-Level Effect of TCZ Policy on Energy-Intensive Firms



Notes: The area corresponds to the 95% confidence interval for the coefficients of respective year, estimated using equation (#). All regressions control for city-specific trends, industry-specific trends, pre-determinants of firm growth, and coal share times TCZ.

Why do Firms Hire using Referrals? Evidence from Bangladeshi Garment Factories

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Abstract

I argue that firms use referrals from current workers to mitigate a moral hazard problem. I develop a model in which referrals relax a limited liability constraint by allowing the firm to punish the referral provider if the recipient has low output. I test the model's predictions using household survey data that I collected in Bangladesh. I can control for correlated wage shocks within a network and correlated unobserved type between the recipient and provider. I reject the testable implications of models in which referrals help firms select unobservably good workers or are solely a non-wage benefit to providers.

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1 Introduction

Firms in both developed and developing countries frequently use referrals from current workers to fill job vacancies. However, little is known about why firms find this practice to be profitable. Since hiring friends and family members of current workers can reinforce inequality (Calvo-Armengol and Jackson, 2004), policy measures have been proposed to promote job opportunities to those who lack quality social networks. For instance, policymakers who believe referrals reduce search costs might require companies to publicize job openings. Such measures will succeed only if they address the underlying reason firms hire using referrals.

I argue that firms use referrals to mitigate a moral hazard problem. I develop a model in which the ability of a worker to leave for an alternate firm limits the original firm's ability to punish a worker after a bad outcome. Instead, a firm must provide incentives for high effort by raising wages after a good outcome. This incentive scheme compels firms to lower initial wages in order to avoid paying workers prohibitively high wages over the course of her employment, but a minimum wage constraint limits firms' ability to do this for lower-skilled workers.

I model a referral in which the provider of the referral agrees to forgo her own wage increase if the referral recipient performs poorly. This agreement allows the firm to satisfy the recipient's incentive-compatibility constraint without violating the minimum wage constraint or paying prohibitively high expected wage. If a social network can enforce contracts between its members, the recipient will have to repay the provider later for any wage penalties she suffers. The recipient then acts as though the punishment is levied on her own wages and thus exerts high effort in response. While a sufficiently long relationship between the firm and worker would allow the firm to use multiple periods of future wages to provide incentives for high effort and thus limit the need for the firms to use referrals, employment spells are relatively short in many developing country labor markets. For instance, in this paper's empirical setting – the Bangladeshi garment industry – demand shocks lead to frequent churning of workers between firms, workers often drop in and out of the labor force, and careers are relatively short.

The contract between the firm, provider, and recipient in my model is analogous to group liability in microfinance. In both cases, a formal institution takes advantage of social ties between participants to gain leverage over a group of them. Varian (1990) shows that in a principal-agent

set-up, principals can use agents' ability to monitor each other to reduce moral hazard. Bryan et al. (2010) provide evidence of this social pressure in microfinance,[1] which supports one of the primary assumptions of my model: the recipient works hard if the provider has monetary gain from her doing so. More broadly, this paper illustrates that firms can benefit from social ties between workers.

The model generates several predictions on the labor market outcomes of referral providers and recipients, which I test using household survey data that I collected from garment workers in Bangladesh. I construct a retrospective panel for each worker that traces her monthly wage in each factory, position, and referral relationship. The wage histories of the referral provider and recipient can be matched if they live in the same *bari* (extended family residential compound).

I use these matched provider-recipient pairs to confirm the key testable premise of the model: the provider is punished when the recipient performs poorly, so that the referral pair has positively correlated wages. I compare the wages of bari members conditional on factory and individual fixed effects to account for permanent unobserved heterogeneity at the factory and individual level. I then conduct a different in difference test that assesses whether the correlation in these wage residuals between a provider and recipient, relative to the correlation in wage residuals of other bari members, is stronger when they are part of the referral relationship (versus when they are working in different factories). This test allows me to account for within-bari wage shocks at a certain time and the possibility that these shocks might be stronger if the bari members are in the same factory or have ever been in a referral relationship. Detailed data on the type of work done by each respondent further allow me to control for factory or industry-level wage shocks to workers in a certain position or using a specific type of machine or within-factory shocks to a production team.

This joint contract between the firm and referral pair has further testable implications for the wage variance and observable skills of the provider and recipient. A provider's wage is tied both to her own output and that of the recipient. Therefore the wage variance of a provider will exceed that of other workers of the same observable skill. Furthermore, since the wages of observably higher skilled workers are higher relative to a constant minimum wage, firms can levy higher punishments

[1] Specifically, they offer a reward to a referral provider if the referral recipient repays back a loan, which increases loan repayment rates. In one of the treatment arms they do not tell the participants about the reward until after the referral has been made, so they can tell that the effect is due to social pressure and not selection.

on higher skilled workers without violating their own incentive compatibility constraint for high effort. Referral providers are thus observably higher skilled than non-providers. Recipients, by contrast, are observably lower skilled than other hired workers, since referrals allow the firm to hire workers it would not otherwise.

While other hypothesized explanations for referrals – namely selection models (Montgomery 1991; Galenianos 2010) or patronage models (Goldberg, 1982) – can also predict the wage correlation between a referral provider and recipient, I show that a moral hazard model has different predictions on the wage path of referral recipients with tenure. Specifically, the moral hazard model in this paper predicts that both the wage level and variance of referred workers increase with tenure (relative to the wage level and variance of non-referred workers) as the firm uses both the recipient's own wages and those of the provider to provide incentives for high effort. By contrast, I develop a selection model that predicts that as firms learn about non-referred workers after hiring, these workers have either increasing wage variance or higher rates of dismissals than referred workers. Neither of these patterns are not found in the data.[2] A patronage model suggests that firms use referrals to decrease the provider's wage in order to pay recipients de-facto wages below the minimum wage. However, this model provides no reason for firms to give wage increases to referral recipients and thus cannot explain the increased wage level with tenure of the moral hazard model.

The empirical evidence that the provider's wage reflects the recipient's output confirms that the provider has incentive to prevent the recipient from shirking. Previous literature arguing that referrals provide information about recipients either proposes that the workers are passive and the firm infers information about the recipient based on the provider's type (Montgomery, 1991) or must assume that the provider and firm's incentives are aligned without having the data to validate the assumption.[3] This assumption may not always hold: referral providers may favor less qualified family members (Beaman and Magruder, 2010) or refer workers who leave once a referral bonus is received (Fafchamps and Moradi, 2009).

[2]By contrast, Simon and Warner (1992), Dustmann et al. (2009), and Pinkston et al. (2006) provide evidence of differential learning about referral recipients. They study developed country labor markets, where the prevalence of heterogeneous higher-skilled jobs likely make match quality and unobserved ability more important. They also lack the matched provider-recipient pairs that provide evidence of moral hazard; therefore it is also possible that referrals address moral hazard in their scenario as well.

[3]For instance, Kugler (2003) assumes that referral recipients have a lower cost of effort due to peer pressure from providers. Simon and Warner (1992) and Dustmann et al (2009) posit that the provider truthfully reports the recipient's type, which lowers the variance in the firm's prior over the recipient's ability.

This paper suggests a context where strong network ties are important in labor markets. While in some contexts weak ties may be more able to provide non-redundant information about job vacancies than close ties (Granovetter, 1973), the existence of networks in my model allows one member to be punished for the actions of another. This mechanism depends on strong ties to enforce implicit contracts through mutual acquaintances and frequent interactions. Accordingly, almost half of the referrals in my data are from relatives living together in the same extended family compound. My results then suggest that strong ties are important for job acquisition in markets where jobs are relatively homogeneous but effort is difficult to induce through standard mechanisms. Indeed, studies in the U. S. have found that job seekers of lower socioeconomic status are more likely to use referrals from close relatives (Granovetter, 1983).

The rest of the paper proceeds as follows. In section 2, I provide information about labor in the garment industry that is relevant to the model and empirical results. Section 3 develops a theoretical model of moral hazard and shows how referrals can increase firm's profits in that environment. Section 4 contrasts the predictions of a moral hazard model with those of alternative explanations for referrals: namely, patronage, selection, or search and matching models. Section 5 describes the data and section 6 explains the empirical strategy. I provide results in section 7. Section 8 concludes.

2 Labor in the Garment Industry in Bangladesh

The labor force of the Bangladeshi garment industry has experienced explosive yearly growth of 17 percent since 1980. It has become an integral part of Bangladesh's economy, constituting 13 percent of GDP and 75 percent of export earnings (Bangladesh Export Processing Bureau, 2009). Garment production is labor-intensive. While specialized capital such as dyeing machines is used to produce the cloth that will be sewn into garments, the garments themselves are typically assembled and sewn by individuals at basic sewing machines. Production usually takes place in teams, which typically consist of helpers (entry-level workers who cut lose threads or fetch supplies), operators (who do the actual sewing), a quality control checker, and a supervisor.

A worker's effort determines the quantity and quality of her output. It is relatively easy for firms to assess the quantity of a worker's output, but the quality of a garment can only be determined if

a quality checker examines it by hand. It is thus prohibitively costly for firms to observe workers' effort perfectly, creating the potential for moral hazard. Firms' ability to assess effort is further complicated by the arrival of new orders with uncertain difficulty come in and instances where a worker's output is affected by others on her team. However, factory managers can use reports from quality checkers and supervisors to assess worker's effort and give rewards to the workers who appear to have performed well. The theoretical model therefore considers allows firms to give contracts based on output (which is correlated, but not perfectly, with a worker's effort).

The key way that firms reward high output is through wage increases. Workers are typically paid a monthly wage[4] – 88 percent of workers in the sample receive one – and receive raises if they have performed well. Sometimes raises are explicitly promised (conditional on good performance), and other workers describe seeing colleagues in the same firm getting raises and anticipating that they can do the same. Wages thus reflect – albeit noisily – the worker's performance. This performance assessment and subsequent wage updating happens relatively rapidly, as depicted in figure 1. By 12 months after hiring, for instance, only 36 percent of the workers who have remained with the firm are still making the original wage offered to them upon hiring. Wages are relatively downwardly nominally rigid; in only 0.67 percent of worker-months did the worker receive lower wages in the next month, while 6.62 percent of worker-months culminated with a wage increase.[5]

The official minimum wage in Bangladesh at the time of the survey (August to October, 2009) was 1662.5 taka per month, around 22 U.S. dollars. The minimum wage does appear to be binding: only 9 out of 974 of the workers in the sample reported earning below the minimum wage, and figure 2 shows evidence of bunching in the wage distribution around the minimum wage. Anecdotally, even if the government does not have the resources to enforce the minimum wage, upstream companies fear the bad publicity that will result if journalists or activists discover that firms are paying below the minimum wage.

There is rarely a formal application process for jobs in the garment industry. After hearing

[4]Explicit piece rates are therefore rare; only 10 percent of workers in my sample are paid per unit of production. Since firms would have to monitor workers under a piece-rate regime anyway to monitor the quality of their work, managers told me that piece rates are not worth the administrative cost, especially since they would have to redefine a new piece with each order.

[5]While this downward rigidity is not modeled explicitly, if wage decreases reduce workers' effort (Bewley, 2002) or are otherwise undesirable to firms, then firms have even greater reason to provide incentives for effort by increasing wages after a good outcome rather than decreasing wages after a bad outcome. This further raises the cost to firms of providing incentives for effort and increases the potential efficiency gain of referrals.

about a vacancy, hopeful workers show up at the factory and are typically given a short interview and sometimes a "manual test" where they demonstrate their current sewing ability. Referrals are common: 32 percent of workers received a referral in their current job. Sixty-five percent of referrals came from relatives, most of which (and 45 percent of referrals overall) occurred between workers living in the same extended family compound, called a *bari*. These workers often work in close contact with each other; sixty percent of referral recipients began work on the same production team as the provider of a referral. Receiving a referral is more common in entry level positions: 43 percent of helpers (vs. approximately 30 percent of operators and supervisors) received referrals. By contrast, 44 percent of supervisors, 25 percent of operators, and only 10 percent of helpers have provided referrals.

While I am unaware of the existence of contracts that explicitly tie the wage of the referral provider to the performance of the referral recipient, workers do describe implicit contracts between the firm, provider, and recipient that reflect the contracts modeled in this paper. Workers explain that if a relative or friend has referred them into a job, they want to do perform well because the referral provider looks bad if they do not. The provider may then be passed up for promotions or raises which I model as wage penalty.[6] When current workers were asked if they knew anyone who they wouldn't give a referral because she "wouldn't be a good worker", only 7 percent of respondents said yes, while 85 percent said no, providing prima facie evidence that the performance of referred workers relates to the effort of a recipient rather than a mechanism for selecting unobservably good workers.

A final important characteristic of the labor market in Bangladeshi garment factories is the relatively high turnover and short time that most workers spend in the labor force, which together imply that the average time that a worker spends in particular factory is relatively low. The median worker in my data has 38 months of total experience in the garment industry and 18 months experience in the same firm. A worker's experience is often interrupted as workers spend time out of the labor force in between employment spells, usually to deal with care-taking of children, sick or the elderly. Thirty-one percent of current workers spent time out of the labor force before their current job. Even garment workers who work continuously tend to switch factories

[6]This penalty does not necessarily contradict the nominal downward wage rigidity discussed earlier in this section. The provider receives a lower wages than she would have otherwise, which could have included a raise due to her own performance.

frequently, as competing factories get large orders and expand their labor force rapidly by poaching workers from other factories. As shown in figure 1, by twelve months after the time of hiring, for instance, only 64 percent of all hired workers who are still working in the garment industry remain in that factory. Accordingly, the theoretical model in section 3 has two periods, giving firms the option to use one period of future wages to induce effort, but not a long enough horizon to permit for firms to be able to use multi-period contracts that induce the efficient level of effort even in the presence of a lower bound on wages. That is, I consider the duration of employment spells to be a middle ground between a spot market and a market in which a firm uses contracts spanning a worker's entire career to provide incentives for effort (Lazear, 1979).

3 Model

The model has two periods, and firms use higher wages in the second period to provide incentives for high effort in the first. However, since workers have the option to leave and work for another firm in the second period, limiting firms' ability to punish workers after a bad outcome, firms must provide incentives for high effort by increasing wages after a good outcome. This means that second period wages that satisfy the incentive-compatibility constraint for effort are relatively high, and firms must decrease the first period wages to avoid paying workers more in expectation than their output. This wage scheme generates the relatively high average returns to experience found in the industry (5.8 percent per year) and fits with workers' reports that firms reward good workers with raises.

However, the minimum wage limits firms' ability to use this wage scheme for observably lower skilled workers who have lower output and thus are paid lower wages. The firm will not be able to provide incentives for high effort for these workers without paying them more than their output, absent a referral. This referral allows the firm to punish the provider if the recipient has low output, thus satisfying the recipient's incentive compatibility constraint for high effort with lower expected second period wages than would be required without a referral and allowing the firm to hire workers it would not otherwise.

3.1 Set-up

In each of the two periods, output is given by $y = \theta + X$, where θ is a worker's observable quality and X is a binary random variable, $X \in \{x_h, x_l\}$, with $x_h > x_l$. In each period, workers can choose between two effort levels, e_h or e_l. If the worker chooses e_h, the probability of x_h is α_h. If a worker chooses e_l, the probability of x_h is α_l, with $\alpha_h > \alpha_l$. For notational convenience, I define the worker's expected output at high effort to be π_h and the worker's expected output at low effort to be π_l. That is,

$$\begin{aligned} \pi_h &= \alpha_h x_h + (1 - \alpha_h) x_l \\ \pi_l &= \alpha_l x_h + (1 - \alpha_l) x_l \end{aligned}$$

Each workers works for two periods. Between the first and the second period of work, a worker can choose whether to stay with the current firm or leave and work with another firm. Firms must offer a worker a wage before the period's work take place, but can make second period wages contingent on first period output. Specifically, the firm can offer a menu where the worker receives w_1 in the first period and in the second period earns

$$w_2 = \begin{cases} w_{2h} & \text{if} \quad X_1 = x_h \\ w_{2l} & \text{if} \quad X_1 = x_l \end{cases}$$

Labor markets are competitive, so wage competition between firms bids wages up to a worker's expected production. I also assume that firms can commit to the wage contract, so that the worker is not worried that the firm will renege on a given w_2 wage offer.[7] There is also a lower bound of $\underline{w}$ on wages.[8]

Low effort has zero cost to workers, while high effort costs c. Workers are risk neutral[9] and utility is separable in expected earnings and effort cost. The worker discounts wages in the second

[7] The fact that fellow workers notice good work and whether it is rewarded helps make firms' offers credible. If firms did not follow through on this implicit agreement, workers would notice and the firm's reputation would suffer, leading workers to choose to work in other firms.

[8] One possible interpretation of this constraint is $\underline{w} = 0$: workers are credit-constrained and they cannot be charged to work. However, gains from referrals will be even greater if there exists a $\underline{w}$ which is strictly greater than zero, such as a minimum wage.

[9] This assumption is made for analytical tractability. Adding risk aversion would only compound the moral hazard problem and reinforce the importance of referrals in providing incentives for high effort.

period at rate $\delta \leq 1$, yielding utility of high and low effort respectively:

$$\begin{aligned} u(e_h) &= w_1 - c + \delta(\alpha_h w_{2h} + (1-\alpha_h)w_{2l}) \\ u(e_l) &= w_1 + \delta(\alpha_l w_{2h} + (1-\alpha_l)w_{2l}) \end{aligned}$$

3.2 Non-Referred Workers

After output is realized from the first period, a worker can choose to stay at the initial firm or work for another firm for one more period of work. The original firm can provide a worker incentives to work hard in the first period by offering a w_{2h} that is sufficiently high, relative to w_{2l}, to make high effort incentive-compatible:

$$\begin{aligned} -c + \delta\Big(\alpha_h w_{2h} + (1-\alpha_h)w_{2l}\Big) &\geq \delta\Big(\alpha_l w_{2h} + (1-\alpha_l)w_{2l}\Big) \\ w_{2h} &\geq w_{2l} + \frac{c}{\delta(\alpha_h - \alpha_l)} \end{aligned} \tag{1}$$

Akin to a back-loaded compensation model, a worker works hard in the first period for the promise of higher future wages. Note that even though the worker is paid less than her output in the first period, firms' ability to commit to high wages means that the worker decides where to work based on total wages and not just first period wages.

In the second period, an outside firm would bid wages up to the worker's second period output with low effort of $w^0(\theta) = \theta + \pi_l$,[10] as long as this amount is above the minimum wage $\underline{w}$.[11] Thus any wage below this amount offered to the worker in the second period by her original firm will be rejected in favor of an alternative firm, and so the minimum earnings a worker can get after a bad outcome is $w^0(\theta)$.[12] Accordingly, the firm must offer a w_{2h} of at least $w^0(\theta)$ plus the wedge

[10] I assume parameter values such that any worker worthwhile to hire and induce effort in during the first period is worthwhile to hire for one period with low effort. See equation 5.

[11] The utility of workers with $\theta + \pi_l < \underline{w}$, who wouldn't be hired by another firm in the second period, is given by the value of their outside option. I develop the role of outside options more in appendix B, focusing in particular on determining the parameter values that rule out a solution in which some workers with $\theta + \pi_l < \underline{w}$ are hired with high effort, because their outside option is so much worse than the wage they would be making in garment work. While the testable implications of the model would still go through in this case – there will still be some workers the firm cannot punish severely enough – the visual interpretation of the model given in figures 3 through 5 is clearer if the relationship between θ and high effort is monotonic.

[12] The moral hazard problem requires some limit on a firm's ability to punish workers after a bad outcome. I model this as the possibility that a worker can leave for another firm. This seems reasonable, given that it is unlikely that workers could commit to stay with a firm (even if firms can commit to future wages). However, even if workers could commit to stay with firms, the minimum wage would still serve as a lower bound on wages in the second period and

between w_{2l} and w_{2h} needed to satisfy the IC constraint given in equation (1), making the expected second period wage needed to satisfy a worker's IC for high effort of

$$\begin{aligned} Ew_2^{high} &= (1-\alpha_h)(\theta+\pi_l)+\alpha_h(\theta+\pi_l+\frac{c}{\delta(\alpha_h-\alpha_l)}) \\ &= \theta+\pi_l+\frac{c\alpha_h}{\delta(\alpha_h-\alpha_l)} \end{aligned} \tag{2}$$

If $\delta < 1$, the firm will pay exactly this wage in the second period; otherwise another firm will offer the same expected payment but with more of the payment in the first period and the worker would prefer this offer.[13]

The firm's formal maximization problem is given in appendix A. For a worker of observable quality θ, the firm has three options: (i) hire and induce high effort, (ii) hire but settle for low effort, or (iii) not hire the worker. Figure 3 depicts the wages the firm must pay to induced high effort ($\underline{w}$ in the first period and Ew_2^{high} in the second) and the worker's output at high effort ($2\theta+\pi_h+\pi_l$). Since output is increasing in θ more steeply than the minimum wages necessary for the minimum wage and incentive compatibility constraints, if θ is sufficiently high a firm can offer a wage contract $\{w_1, w_{2h}, w_{2l}\}$ that satisfies the IC and minimum wage constraints and still pays the worker her expected output. Call $\underline{\theta}_{high}$ the minimum θ required for a worker to be profitable to hire at high effort:

$$\underbrace{2\theta+\pi_h+\pi_l}_{\text{output}} \geq \underbrace{\underline{w}+\pi_l+\theta+\frac{c\alpha_h}{\delta(\alpha_h-\alpha_l)}}_{\text{minimum wages to satisfy IC and LL}} \tag{4}$$

$$\underline{\theta}_{high} = \underline{w}+\frac{c\alpha_h}{\delta(\alpha_h-\alpha_l)}-\pi_h$$

create a moral hazard problem.

[13] That is, I assume firms discount the future less than workers do. Among other reasons, this could be because firms have better access to credit markets than workers. In fact, if δ is too low, then a worker would prefer lower average wages (while exerting low effort) to higher average wages but with more of the payout in the second period. To avoid this possibility, I assume: then it is prohibitively costly for the firm to pay wages high enough to induce effort, since a worker might prefer lower average wages (at low effort both periods) than higher wages but with a lower first-period payoff. A worker with $\theta = \underline{\theta}_{high}$ will be still be willing to accept wages that satisfy the IC for high effort if

$$\begin{aligned} \underline{w}-c+\delta(\pi_l+\underline{\theta}_{high}+\frac{c\alpha_h}{\delta(\alpha_h-\alpha_l)}) &\geq (1+\delta)(\pi_l+\underline{\theta}_{high}) \\ (\pi_h-\pi_l)-c &\geq \frac{(1-\delta)c\alpha_h}{\delta(\alpha_h-\alpha_l)} \end{aligned} \tag{3}$$

If the worker's θ is below $\underline{\theta}_{high}$, however, a worker earning $\underline{w}$ in the first period and Ew_2^{high} in the second would earn more than her output. The firm would hire the worker if it could reduce first period wages, but since the minimum wage constraint prevents this possibility, the worker is not profitable to hire at high effort. The value of output at low effort relative to the minimum wage dictates whether some workers with $\theta < \underline{\theta}_{high}$ are worth hiring at low effort. This occurs if a worker with $\theta = \underline{\theta}_{high}$, whom the firm is exactly indifferent about hiring at high effort, has output at low effort which is greater than the minimum wage:

$$\begin{aligned} 2(\underline{\theta}_{high} + \pi_l) &> 2\underline{w} \\ \frac{c\alpha_h}{\delta(\alpha_h - \alpha_l)} &> \pi_h - \pi_l \end{aligned} \tag{5}$$

So the possibility of hiring at low effort is relevant if effort is costly (high c), workers discount the future considerably (high δ) or the output from high effort are relatively close to the output from low effort (π_h closer to π_l). The presence of these workers are key to the testable implications of the model that compare workers of the same θ who are hired with or without a referral.

Figure 4 depicts firms' hiring and effort decisions in the case where condition (5) applies and some workers are hired at low effort. As in figure 3, workers for whom high effort is profitable (those with $\theta \geq \underline{\theta}_{high}$) are hired at high effort. Additionally, workers with $\theta < \underline{\theta}_{high}$ are hired as long as their output with two periods of low effort (the $2\theta + 2\pi_l$ line) is above the twice the minimum wage. Denote as $\underline{\theta}_{NR}$ the minimum θ for which this condition holds, which is the minimum observable quality of worker hired without a referral.[14]

3.3 Referrals

It would be profitable for the firm to induce high effort in some workers with $\theta < \underline{\theta}_{high}$ if it could lower the worker's wage after low output below the $w^0(\theta)$ another firm would offer. The firm could then satisfy the IC constraint for high effort without paying prohibitively high expected wage. One way that firms could do this is through a referral. Suppose that a current employee in the firm offers to serve as a referral provider (P) to a potential worker, the referral recipient (R). I assume

[14] This statement holds under the relationship derived in appendix B, which states that if workers with $\theta < \theta_{NR}$ have sufficiently high outside option, firms also cannot punish them enough in the second period to ensure that they work hard.

that both P and R are part of a network whose members are playing a repeated game that allows them to enforce contracts with each other that maximize the groups' overall pay-off (Foster and Rosenzweig, 2001). Then a provider is willing to allow her own wages to be decreased by some punishment p if the recipient has low output, since the recipient will eventually have to repay her.[15]

Analogously to the firm's problem with one worker, when considering a potential referral pair with workers of observable quality (θ_P, θ_R) the firm can choose whether to hire and induce effort in one or both workers. Appendix C details the full maximization problem. I will focus here on the characterizing the scenario in which the firm finds it profitable to hire both the provider and recipient and induce high effort in both. In this case the provider receives second period wages:

$$w_2^P = \begin{cases} w_{2h}^P & \text{if P and R both have high output} \\ w_{2h}^P - p & \text{if P has high, R has low} \\ w_{2l}^P & \text{if P has low, R has high} \\ w_{2l}^P - p & \text{if both P and R have low output} \end{cases}$$

The recipient receives w_{2h}^R in the second period after high output and w_{2l}^R after low. The firm can punish the provider to satisfy the recipient's IC constraint for high effort, as long the provider's wage net of p does not drop below $w^0(\theta_P)$, which would prompt both workers to leave for another firm. The firm can then satisfy the recipient's IC constraint without the need to raise the recipient's expected second period wage (the Ew_2^R line on the graph) as high as it would need to be absent a referral (the Ew_2^{high} line on the graph).

The firm will then be able to induce high effort profitably in a recipient with $\theta_R < \underline{\theta}_{high}$ if θ_P is high enough so that the workers' joint output exceeds the wages the firm must pay in order to satisfy IC constraints for both the recipient and provider without dropping either the recipient's wage or the provider's wage net of p below $w^0(\theta_R)$ and $w^0(\theta_P)$ respectively. That is if,

$$2(\theta_P + \theta_R + \pi_l + \pi_h) \geq w_1^R + w_1^P + \alpha_h w_{2h}^P + (1-\alpha_h) w_{2l}^P + \alpha_h w_{2h}^R + (1-\alpha_h)(w_{2l}^R - p) \qquad (6)$$

subject to the incentive compatibility constraints that high effort is worthwhile for the provider and

[15] Moreover, the referral creates a surplus – a worker is hired who wouldn't be otherwise – so that the provider can be made strictly better off once the reimbursement is made. While I will not model the side payments between the provider and recipient that divide the surplus, the key point is that the referral can be beneficial for them both.

for the recipient (given both the recipient's own wages and potential punishment of the provider), that each worker's outside option in the second period determines the maximum punishment, the individual rationality constraint that the referral must give both workers higher utility than they would get with the referral. The exact constraints are given in appendix C.

If (6) holds while satisfying these constraints, then the firm induces high effort in both workers. Figure 5 depicts the minimum observable quality of recipient $\underline{\theta}_R(\theta_P)$ that is profitable for the firm to hire and provide incentives for high effort, given a provider of observable quality θ_P. This is the recipient whose own IC would just bind after the firm levies the maximum punishment p on the provider's wage. This maximum punishment equals the difference between the amount the firm must pay the provider to satisfy her own IC and the minimum wage ($\underline{w} + Ew_2^{high}$) and her output. It can then raise w_{2l}^P by p which guarantees that the provider will not leave in the second period when she is facing punishment, even if she is already receiving w_{2l}^P after receiving low output herself. The minimum observable quality of recipient $\underline{\theta}_R(\theta_P)$ that is then profitable to induce effort has output equal to the total wages of $\underline{w}$ in the first period and expected wage of Ew_2^R in the second.

Decreasing the provider's wage is one particular way that the firm can punish the provider after observing low output from the recipient. The firm could instead, for instance, fire the provider or assign her to unpleasant tasks within the firm. However, if there is any firm-specific human capital (or firing or replacement costs), then the firm has incentive to choose a punishment that retains the worker (as $w_{2l}^P - p \geq w^0(\theta)$ ensures) but still makes the pair worse off than if the recipient had high output. Accordingly, my main empirical focus is on punishment through wages. Providing evidence that punishment takes place in this manner does not, of course, imply that punishment does not occur in other ways. Instead, I provide evidence of one, potentially important, means through which the firm punishes the provider.

3.4 Testable Implications

The joint contract offered to the provider and recipient generates several testable implications about the observable quality and the wages of providers and recipients. The first set of implications provide evidence that the provider's wage reflects the recipient's output:

1. Because the provider's wage decreases by p when the recipient has low output, and thus

receives w_{2l}^R rather than w_{2h}^R, the wages (conditional on observed quality) of the provider and recipient at a given time are positively correlated.

2. $Var(w^P|\theta) > Var(w|\theta)$. A provider's wage reflects not just her own output, but the recipient's as well. For proof, see appendix D.2.

3. $E(\theta|hired\ and\ made\ referral) > E(\theta|hired)$. A firm's scope to punish a provider is increasing in θ, so the higher θ_P, the lower is the minimum $\underline{\theta}_R(\theta_P)$ from that worker and the more willing is the firm to accept a referral from that worker. This result is also discussed in appendix D.1.

A second set of testable implications show that firms provide referred workers wage schedules that satisfy an IC constraint for high effort. This increased effort allows a firm to hire workers with referrals that it wouldn't otherwise be able to hire.

4. $E(\theta|hired\ with\ referral) < E(\theta|hired)$. Because the firm can get positive profits from some observably worse recipients than $\underline{\theta}_{NR}$, recipients on average have lower θ than other hired workers. For proof, see appendix D.1.

5. The wage level of referral recipients is increasing with tenure, relative to non-recipients. The firm provides incentives for effort both by increasing the recipient's wage after a good outcome and punishing the provider after a bad outcome. By contrast, the firm has no incentive to provide wages to non referred workers, who are exerting low effort. For proof, see appendix D.3.

6. The wage variance of referral recipients also increases with tenure. In addition to the increase in average wages of referred workers, the wedge between w_{2h}^R and w_{2l}^R that appears in the second period increases their wage variance, relative to the wages of non-referred workers whose second period wage does not depend on output. For proof, also see appendix D.3.

The predictions on the wages of referral recipients are crucial in distinguishing a moral hazard model from other reasons that firms might use referrals, in particular, from a selection model and a patronage/nepotism model. That is, while selection and patronage models also predict the provider's wage reflects the recipient's output and that referrals allow observably lower skilled

recipients be hired that would not be otherwise, they do not predict that the wage level and variance of referral recipients increases with tenure relative to non-referred workers. The next section briefly summarizes the predictions of a selection and a patronage models in a similar two-period set-up to the moral hazard case, and explains why their predictions differ from a moral hazard model.

4 Contrasting the predictions of the Moral Hazard Model with Alternative Models of Referrals

4.1 Selection

Much of the previous literature on referrals assumes that the referral provides information about the recipient's unobserved type. In some of these papers, the mechanism is correlated unobservable types within a network (Montgomery 1991; Munshi 2003); the firm can estimate the recipient's type based on what it has learned about the type of the provider. However, while this model predicts that there may be correlation between the wages of a referral pair even when they are not working in the same factory, it cannot explain why this wage correlation is differentially stronger when they are in the same factory together. Alternatively, the provider could be reporting the the recipient is high type (Saloner, 1985). Firms would then know more about recipients before hiring[16] and learn more about non-recipients after hiring.

Appendix E characterizes this selection model. If there is any noise in the mapping between type and output (i.e., sometimes high types have low output and sometimes low types have high output), then providers must be punished when the recipient has low output in order to ensure that only the good types are referred. This punishment predicts the same positive wage correlation between referral recipient and provider as the moral hazard model, but the firm's adjustment of recipients' wages yields different predictions on the wages and turnover of recipients after hiring. Specifically, once the firm learns the true type of each worker, if the costs are low to replace a worker, then the firm would fire the non-referred workers that it learns are low type, and there would be higher turnover among non-referred workers. Alternatively, if replacement costs are high

[16]That is, firms cannot learn at least some of the information provided by the referral in any other way. While firms do use manual tests (see section 2) to learn the dexterity and skills of potential hires, the referral would be giving information about motivation, attention to detail, and diligence, which cannot be measured in these tests.

enough that the firm chooses to retain the workers it learns are bad types, it still updates their wages to reflect this new information. Then the wage variance of non-referred workers would spread with tenure.

4.2 Patronage

Another possible model suggests that referrals allow firms to set de facto wages below the minimum wage by lowering the provider's wage by the difference between the minimum wage and its desired wage for the recipient.[17] The firm and the referral pair are both better off in this scenario, since absent a referral the minimum wage would prevent firms from hiring all the workers it would like. The positive correlation between the wages of the recipient and provider would then represent the fact that the "fee" for the referral (as reflected in the lowering of the provider's wages) is decreasing in the quality of the recipient. However, those workers hired with a referral would always receive the minimum wage, since the firm would actually prefer to pay them less than the minimum. So there is no reason that the wages of referral recipients would increase with tenure relative to non-recipients, as predicted by a moral hazard model. Moreover, if there is any reason wages of the more valuable non-referred workers would increase with tenure,[18] the wages of referred workers would fall with tenure, relative to non-referred workers.

4.3 Search and Matching

While a full search and matching model is beyond the scope of this paper, I utilize the predictions of the model of Dustmann et al. (2009). In their model, referred workers are not on average better type than non-referred workers, but the firm has a more precise signal about the true productivity of referred workers. Because referred workers are better matched with their jobs initially than nonreferred workers, their wages are initially higher than those of non-referred workers, who are willing to accept lower wages for the expectation of higher future wage growth (since they are

[17] Note that the moral hazard model presented in this paper also implies that referrals serve to offset the minimum wage: the referral provider might agree to a referral that decreases her current wage, since the recipient will agree to repay her in the future. The question, then, is whether the empirical results could be explained by a patronage model of referrals in an environment where effort is perfectly observable.

[18] For instance, good workers could build up firm specific human capital more rapidly than bad workers. This seems plausible, since entry level work is similar across all firms, whereas supervisors need to understand the details of the work done by a specific firm.

insured against low realizations of their productivity by the ability to leave the firm). So non-referred workers are predicted to have higher wage growth than referred workers.

Note that while the Dustmann et al. (2009) and other search models don't explicitly incorporate joint contracts between the firm, provider, and recipient that would predict the positive wage correlation of the other models considered, other components of a search model might lead to this wage correlation. For instance, if the provider and recipient both have a specialized skill and the factory uses the referral to fill that specialized skill at a time it has a particularly large demand for it. In section 6.1 I argue that the detailed data I collected on the machine type, position, and production team of the provider and recipient alleviate the concern that within-factory wage shocks to certain types of workers generate the positive wage correlation in provider and recipient. However if there was a wage shock to some component of worker type which is unobserved to the econometrician and referrals are used to help find this type of worker, it is useful to note that this type of search model has different implications for evolution of the wages of that referred worker after hiring.

4.4 Summary of predictions of different models

The chart below summarizes the predictions of the moral hazard model with the selection, patronage, and search models discussed in this section. While the moral hazard, selection, and patronage models are predict that the provider's wage reflects the recipient's output (predictions 1 through 3), which allows firms to hire observably lower skilled recipients (prediction 4), they have different implications for the wage level and variance with tenure (predictions 5 and 6).

	Moral Hazard	Selection	Patronage	Search
1. Wage Correlation of R and P	+	+	+	(no pred)
2. Wage Var of P vs non-P	+	+	+	(no pred)
3. Observable Quality of P vs non-P	+	+	+	(no pred)
4. Observable Quality of R vs non-R	-	-	-	(no pred)
5. Wage Level of R vs non R with tenure	+	(no pred)	- or flat	-
6. Wage Var of R vs non R with tenure	+	-	(no pred)	(no pred)

5 Data and Summary Statistics

The data for this paper come from a household survey that I conducted, along with Mushfiq Mobarak, of 1395 households in 60 villages in four subdistricts outside of Dhaka, Bangladesh.[19] The survey took place from August to October, 2009. Households with current garment workers were oversampled, yielding 972 garment workers in total in the sample. Each sampled garment worker was asked about her entire employment and wage history. Specifically, she was asked to list the dates she worked in each factory and spell-specific information about each such as how she got the job (including detailed information about the referral) and the nature of work done.[20] A factory-specific code was recorded, allowing me to match the outcomes of workers in the same factory for the empirical tests that require comparisons of outcomes of workers working in the same factory. The sampled workers worked in 892 factories all together during their careers. Of these factories, 198 had more than one sampled worker at a particular time period and 95 had a within-bari referral with both members captured in the data.

A worker is also asked if she ever changed wages within the factory, and if so, in what month each wage change occurred and whether there was also a change in position associated with the wage change. The surveys I observed suggested that workers did not much have difficulty remembering past wages. Wage information is very salient to workers, most of whom are working outside the home or for a regular salary for the first time and whose wages represent substantial improvements to household well-being. However, there is still likely some measurement error, and I discuss the impact it may have on my results in section 6.

Together, these data yield a retrospective panel of a worker's monthly wage and other outcomes in each of her factories, positions, and referral relationships since she began working. This work history is crucial for several aspects of my identification strategy. Primarily, multiple observations per worker allow me to include worker fixed effects and observing a pair of workers both in and out of a referral relationship allows me to control for correlated unobservables when testing whether their wage correlation is higher in the factory with the referral relationship. Additionally, I know

[19] Specifically, Savar and Dhamrai subdistricts in Dhaka District and Gazipur Sadar and Kaliakur in Gazipur District. For use in other projects, 44 of the villages were within commuting distance of garment factories, and 16 were not. Details of the sampling procedure and survey are given in Heath (2011).

[20] If a worker worked in a given factory for two spells, with a spell at another factory in between (which did occur 42 times), the questions were asked separately about each spell. So if a worker has referred in one spell but not the other, or by different people in each spell, this was recorded.

the timing of worker's decisions to leave the labor force temporarily, allowing me to use these decisions as a proxy for the worker's decision to leave the labor force permanently. Analyzing the relationship between referrals and the decision to leave the labor force temporarily provides some evidence on the influence of attrition out of the labor force in the retrospective panel. I also know how much time workers spent out of the labor force between jobs, so that I can also control for actual experience when constructing measures of a worker's observable skill in the empirical tests. This is important in an industry where the returns to experience are high but workers often spend time out of the labor force between employment spells.

The sampling unit for the survey was the bari. A bari is an extended family compound, where each component household lives separately but households share cooking facilities and other communal spaces. The median number of bari members in sampled baris was 18, with a first quartile of 9 people and the third quartile of 33. Any time a worker indicated receiving a referral from a bari member who was also surveyed, the identity of the provider was recorded. Therefore, in employment spells where the surveyed worker received a referral from someone living in the bari and working in the garment industry at the time of the survey, the work history of the recipient can be matched to the work history of the provider.

The word used for "referral" in the survey was the Bangla word *suparish*, which most literally translates as "recommendation." However, given that I do not know of any factories with policies of making a recommendation/referral official, I did not try to determine whether the factory knew about the bond between workers. That is, I instructed the enumerators to err on the side of coding as a referral any time the recipient found out about the job through a current worker in the factory. The survey form allowed the respondent to name at maximum one referral provider per employment spell.[21]

Table 1 provides information on the personal and job characteristics of workers who have received referrals, those who have given referrals, and those who neither gave nor received referrals. One pattern that emerges from the table is that workers do not seem to use referrals to gain information about unfamiliar labor markets. In fact, those who were born in the city in which they are

[21] In section 7.1 I argue that if I have coded as a "referral" some instances where the firm does not know about the bond between the provider and recipient or if the firm does actually make referral contracts between multiple providers and recipients, it would only work against me finding the relationship that I do between the provider and recipient's wages.

currently residing are more likely to have received a referral than those who have migrated to their current city. Workers are also no more likely to use referrals in jobs that are further from their current residence, as measured in commuting time.

6 Empirical Strategy

6.1 Testing for Punishment of Provider

The test for punishment of the provider based on performance of the recipient (prediction 1) is whether the recipient's wage (conditional on observable characteristics) predicts the provider's wage (also conditional on observable characteristics) at a given point in time. I examine whether this holds among the 45 percent of referrals in the sample that are between bari members, which is the sample where I can match provider and recipient. Specifically, I first obtain obtain wage residuals conditional on observable variables (the θ in my model), since the model's prediction on the wage correlation of R and P is conditional on each worker's θ. I include both factory and individual fixed effects in this specification to allow for the possibility that some factories pay higher average wages than others (and may use referrals differentially more or less) and for unobserved individual-level characteristics (which may be correlated between the provider and recipient).

$$log(w_{ift}) = \beta_0 + \delta_f + \gamma_i + \beta_1 experience_{ift} + \beta_2 experience^2_{ift} + \varepsilon_{ift} \tag{7}$$

Denote the residual from this regression as $\tilde{w}_{ift}$. I then run a regression where the unit of observation is the wage residual $\tilde{w}$ of any pair of bari members i and j that are both working in the garment industry at the same time t. Specifically, I regress the $\tilde{w}_{ift}$ of one of the pair on the $\tilde{w}_{jft}$ of the other, and allow the effect of $\tilde{w}_{jft}$ to vary based on whether i and j are in the same factory, whether there has ever been a referral between i and j, and whether i and j are currently in a referral relationship[22].

$$\begin{aligned} \tilde{w}_{ift} =& \gamma_1\ \tilde{w}_{jft} + \gamma_2\ \tilde{w}_{jft} \times ever\ referral_{ij} \\ &+ \gamma_3\ \tilde{w}_{jft} \times same\ factory_{ijt} + \gamma_4\ \tilde{w}_{jft} \times referral_{ijt} + u_{ift} \end{aligned} \tag{8}$$

[22]Recall that a referral in my model is by definition between two workers in the same factory.

The following table shows the number of observations which identify the different interaction terms in the regression. While the majority of the observations in this regression are bari members in different factories between whom there was never a referral, whose role in the regression is only to identify industry-wide wage shocks, there is still a large absolute number of referral pairs, both together and outside of the same factory.

	ever referred = 0	ever referred = 1
same factory = 0	56,299	366
same factory = 1	8,199	380

Since individual and factory-level heterogeneity have already been accounted for in equation (7), equation (8) tells us whether the wage of one bari members is above her average (relative to others in the same factory) when another bari member's wage is above her average (relative to others in the same factory). This could happen, for instance, if bari members have correlated productivity shocks (due to, for example, a contagious illness). If so, then γ_1 would be positive. The $w_{jft} \times ever\ referral_{ij}$ term allows this correlation to be stronger between members of a referral pair, even when that referral relationship is not in place. The $w_{jft} \times same\ factory_{ijt}$ term lets the correlation in within-bari shocks be stronger between bari members who are working in the same factory. If after accounting for each of these shocks, there is still a differentially stronger wage correlation among members of a referral relationship, then $\gamma_4 > 0$ and I conclude that there is punishment of the provider based on the performance of the recipient. This test is valid if $\tilde{w}_{jft} \times referral_{ijt}$ is uncorrelated with the error term u_{ift}, conditional on the $w_{jft} \times same\ factory_{ijt}$ and $w_{jft} \times ever\ referral_{ij}$ terms. That is, the referral itself must be the only reason that two members of a referral pair can have differentially stronger correlation in wages during the referral relationship.

One might be concerned that this condition fails due to wage shocks to observable job characteristics within the factory – namely, to production team, position, or machine type. That is, the referral pair may do similar work and a within-factory or industry-wide wage shock to that type of work leads to differentially stronger wage correlation between the bari pair relative to other bari members working in the same factory. For instance, the provider might have trained the recipient to sew using a specialized type of machine and their factory gets a large order that necessitates heavy use of that machine, prompting both the provider and recipient's wages to increase at the

same time. To address this concern, I allow for within-factory and industry-wide wage shocks to machine or position by including interactions of $\tilde{w}_{jft}$ and $\tilde{w}_{jft} \times same\ factory_{ijt}$ with indicators for $same\ machine_{ijt}$ and $same\ position_{ijt}$ and verify that the referral pair still has differentially stronger wage correlation during the referral relationship.

It is not possible to do the exact same test for the production team, since I know whether two bari members were on the same production team only if there was a referral between the two. However, I can interact an indicator for $same\ team_{ijt}$ with $\tilde{w}_{jft} \times referral_{ijt}$ in equation 8 to test whether the wages of a referral pair who are not on the same production team are still more strongly correlated than the wages of other bari members working together in the same factory (who may or may not be on the same team). If so, it is unlikely that production complementarities are driving the correlation in wages between the provider and recipient, since their wages remain more strongly correlated than other bari members even when they are not working together on the same team.

This test requires retrospective wage data in order to compare the wages of a provider and recipient in the same factory to their wages when they are not in the same factory. While using retrospective wage data from current garment workers raises the possibility of attrition bias – if one member of a referral pair drops out of the garment industry then I cannot include their wages here – a very particular pattern of turnover would be required to bias the $\tilde{w}_{jft} \times referral_{ijt}$ coefficient away from zero. That is, to make the wages of the provider and recipient appear more strongly correlated than they would without attrition, either the provider or recipient would have to drop out of the labor market when they received a wage shock in the opposite direction of the other. For instance, the recipient would have to drop out of the labor market when her wages would have been low, but only when the provider has high wages. Using data on workers' decisions to drop out of the labor force temporarily as a proxy for the decision to leave the labor force permanently, there is no evidence of any of these patterns.[23]

While the variable $referral_{ijt}$ reported by the participants may not perfectly capture the notion of a referral modeled theoretically, such misclassification is unlikely to bias the $\tilde{w}_{jft} \times referral_{ijt}$

[23]That is, in a probit regression where the dependent variable is equal to one if the worker leaves the labor force temporarily in a particular month (conditional on working in the previous month), the wage residual of the recipient has no effect on whether a provider leaves the labor force temporarily, and similarly the wage residual of a provider has no effect on whether a recipient leaves the labor force temporarily.

coefficient away from zero. For instance, in some cases the respondent might have reported having been referred, but the provider only passed along information about the job without notifying the firm of her connection to the recipient. The firm would then not be able to punish the provider based on performance of the recipient. However considering these instances as referrals would bias the coefficient on $\tilde{w}_{jft} \times referral_{ijt}$ toward zero. Similarly, if in actuality the firm punishes multiple providers if the recipient has low output but only one is considered to be a provider in regression (8), then the wages of the control pairs also reflect wage effects of a referral, and the estimated wage effects of a referral are smaller than they would be otherwise.

Retrospective wage information based on recall data also likely contains measurement error. However, there would need to be differentially stronger correlation in the noise components of the wage reports of a bari pair (relative to other bari members working in the same factory at the same time) to yield a differentially stronger wage correlation between the referral pair. To mitigate this possibility, surveys were done with each worker independently to mitigate the type of information sharing that might occur differentially between a referral pair and lead to correlated measurement error. The remaining recall error likely represents classical measurement error and would only bias the coefficient on $\tilde{w}_{jft} \times referral_{ijt}$ toward zero.

6.2 Wage Variance

Predictions 2 and 5 pertain to the wage variance of referral providers and recipients, conditional on their observed skills. So I first condition out observable measures of skill by estimating a wage equation for worker i in factory f:

$$log(w_{if}) = \beta_0 + \delta_f + \beta_1 experience_{if} + \beta_2 experience_{if}^2 + \beta_3 male_{if} + \beta_4 education_{if} + \varepsilon_{if} \quad (9)$$

Since this test does not require past wages that allow multiple observations per worker–unlike in the test for punishment of the provider–I use only current wages in estimating (9) to avoid concerns about selective attrition. For instance, providers may be less likely to drop out of the labor market after a bad wage shock since they don't want to leave the friends they have referred alone in the factory. I then test whether the squared residual $\hat{\varepsilon}_{if}^2$ (an estimate for wage variance) increases if the worker made a referral.

$$\hat{\varepsilon}_{if}^2 = \alpha_1 x'_{if}\hat{\beta} + \alpha_2 \; made \; referral_{if} + \alpha_3 \; referred_{if} + u_{if} \tag{10}$$

I do this test conditional on the worker's fitted wage $x'_{ift}\hat{\beta}$, since many theories of the labor market would predict that wage variance is higher among high-skilled groups (Juhn et al., 1993). The model predicts that both recipients and providers have higher wage variance than other hired workers of the same θ, which would yield $\alpha_2 > 0$ and $\alpha_3 > 0$.

Since the prediction on the wage variance of recipients is more nuanced – their wage variance grows with tenure, relative to the wage variance of non-referred workers – I further test additionally whether within-worker wage variance increases for non-referred workers. This test shows that the higher wage variance for recipients is not due to permanent characteristics that are reported to the firm by the provider (or observed to the firm but not the econometrician), which would result in larger wage variance for recipients that begins on hiring.

Specifically, I assess whether the squared difference between a worker's wage residual after a certain time in the firm (t_1 = 3, 6, and 12 months) and the worker's initial wage offer at time t_0 varies between recipients and non-referred workers.

$$(\tilde{w}_{it_1} - \tilde{w}_{it_0})^2 = \beta_0 + \beta_1 referred_i + \varepsilon_i \tag{11}$$

These relatively short time windows yields estimates that are relatively uncorrupted by the selection of which workers remain in the firm that long but is long enough to reflect firms' initial observation of worker's output and their subsequent wage updating. The model predicts $\beta_1 > 0$: the wage variance of referred workers raises with tenure, relative to that of non-referred workers.[24]

6.3 Observable Quality

To test predictions 3 and 4, which relate to the observable quality (θ in my model) of providers and recipients, I consider separately two measures of skill: experience and education[25]. So for each

[24] I also control for education, experience, and sex to guarantee that any differences in wage variance with tenure of workers with these characteristics are not driving the coefficient on *referred*.

[25] While literacy and numeracy are not strictly required (except for supervisors, who need to keep written records), employers say that educated workers are more likely be proficient "floaters." Floaters are individuals who fill in in various parts of the production chain when other workers are absent or after a special order has come in. An educated worker can more easily learn the work from a pattern rather from than watching it be done.

worker-employment spell, I estimate:

$$educ_{if} = \beta_0 + \delta_f + \beta_1 referred_{if} + \beta_2 made\ referral_{if} + \beta_3 male_{if} + \varepsilon_{if} \tag{12}$$

$$experience_{if} = \beta_0 + \delta_f + \beta_1 referred_{if} + \beta_2 made\ referral_{if} + \beta_3 male_{if} + \varepsilon_{if} \tag{13}$$

where experience is measured at the beginning of employment. I include factory fixed effects to compare providers and recipients to other workers in the same factory. The model predicts $\beta_1 < 0$ and $\beta_2 > 0$ in both regressions: providers should have more education and experience than other hired workers, while recipients should have less.

6.4 Wage Level with Tenure

Prediction 6 is that the wage level of referral recipients increases with tenure. I again look at wages a short window after hiring (3, 6, and 12 months) to assuage fears that differential attrition is driving changes in wage levels of stayers versus non-stayers. To further allay concerns that differential attrition is driving these results, I repeat each specification only including workers who have survived in the firm up until that point so that the identification of the differential trend of referred workers with tenure only comes from comparison of workers who remain in the firm.

$$\begin{aligned} logw_{ift} =& \beta_0 + \delta_f + \gamma_1 referred_{ift} + \gamma_2 tenure_{ift} + \gamma_3 referred_{ift} \times tenure_{ift} \\ &+ \beta_1 experience_{ift} + \beta_2 experience^2_{ift} + \beta_3 male_{ift} + \beta_4 education_{ift} \end{aligned} \tag{14}$$

The model predicts $\gamma_3 > 0$: the wages of referred workers raise with tenure, relative to those of non-referred workers.

7 Results

7.1 Punishment of Provider

Table 2 reports results from equation (8), a regression of one bari member's residual wage $\tilde{w}_{it}$ on the residual wage $\tilde{w}_{jt}$ of another bari member working in the garment industry at the same time, and on interactions of $\tilde{w}_{jt}$ with whether i and j were in the same factory, whether there has ever

been a referral between i and j, and an interaction between the indicator for same factory and whether there has been a referral between i and j. Standard errors are calculated by bootstrapping the two-stage procedure. Specifically, I take repeated samples with replacement from the set of monthly wage observations. For each replicate I first estimate the wages conditional on observables to get the $\tilde{w}_{ift}$'s, construct pairs of wage observations for baris with multiple members chosen in that replicate, and then estimate equation (8). This procedure, analogous to a block bootstrap, preserves the dependent nature of the wage pairs data by ensuring that if a wage observation is selected, all pairs of wage observations involving that worker at that time will also be in the sample.

In column 1, we see that the coefficients on $\tilde{w}_{jft}$ and $\tilde{w}_{jft} \times ever\ referral_{ijt}$ are close to zero and insignificant: there is no evidence of correlated wage shocks among bari members in different factories, whether or not there was once a referral between the two workers. The coefficients among bari members in the same factory (both with and without a referral), by contrast, are positive. To help interpret these coefficients, consider three bari members in the same factory: P referred R, whereas C works in the same factory but did not participate in a referral with either P or R. So a 10 percent increase in R's wage (above her mean wage, compared to other workers in the same factory) corresponds to an increase of 0.566 percentage points in C's wage (above her mean wage, compared to other workers in the same factory). This effect is much stronger between the provider and recipient, yielding a positive coefficient on the variable of interest, $\tilde{w}_{jft} \times referral_{ijt}$. So if R's wage goes up by 10 percent, P's wage goes up 3.295 percentage points more than it does after a 10 percent increase in C's wage.

Column (2) adds interactions between $\tilde{w}_{jft}$ and $\tilde{w}_{jft} \times same\ factory_{ijt}$ and an indicator variable for whether the pair is using the same machine to allow for industry-wide and within-factory wage shocks to workers skilled in using a particular machine. After including these effects the interaction between $\tilde{w}_{jft}$ and $same\ factory_{ijt}$ becomes zero, so the wage correlation between bari members in the same factory is indeed driven by within-factory wage shocks to workers using the same machine type. However, there is no evidence that wage shocks to certain machine types are driving the referral effect; the coefficient on $\tilde{w}_{jft} \times referral_{ijt}$ remains unchanged. Column (3) suggests that there are industry-wide, but not within factory, wage shocks to workers in a specific position; the coefficient on $\tilde{w}_{jt} \times same\ position_{ijt}$ is positive. The referral effect $\tilde{w}_{jft} \times referral_{ijt}$ again remains unchanged, suggesting that a tendency of referred worked to work in the same position is not

driving their wage correlation. Finally, column (4) verifies that the $\tilde{w}_{jft} \times referral_{ijt}$ coefficient is still significant even among pairs not working on the same production team.

One further robustness check addresses a potential concern that firms hire workers with referrals at particular times in the production cycle, such as after receiving a big order, which would heighten the wage correlation between all workers at that factory at that time. This pattern of wage shocks suggests a specification that compares referred workers to other workers in the same factory at the same time. Accordingly, in table 3 I reestimate equation (8) using the wage observations in bari-factories pairs at times in which there was at least one referral between bari members in the factory. The coefficient on $\tilde{w}_{jft} \times referral_{ijt}$ then identifies the wage correlation of the referral pair compared to the wage correlation of other bari members in the same factory as the referral pair at the same time. While I lose the ability to use the $\tilde{w}_{jft} \times ever\ referral_{ij}$ coefficient to allow for differentially stronger wage between a referral pair, this test provides evidence that the results in table 2 are not driven by wage shocks occuring to the the entire factory at times when it is using referrals. Reassuringly, the coefficient on $\tilde{w}_{jft} \times referral_{ijt}$ is very similar in this sample.

The first column shows that when one member of a referral pair's wage increases by 10 percent, the other's wage increases by 2.541 percent more than it would after an equivalent wage increase from another bari member in the same factory. Analagously to table 2, columns two and three include interactions of $\tilde{w}_{jt}$ with an indicator for whether the two are using the same machine or work in the same position to demonstrate that the wage effects of the referral are not driven by differentially stronger shocks to machine or position in factories using referrals. Column 4 again shows that the differentially stronger correlation in wage between the referral pair relative to other workers in the same factory is not only present in referral pairs on the same production team.

7.2 Unexplained Wage Variance

Table 4 gives the results from regression (10), which tests whether the unexplained wage variance–the residual $\hat{\epsilon}^2_{if}$ from a first stage wage regression–varies with fitted wage $x'_{if}\hat{\beta}$ and whether the worker has made or received a referral. Column (1) indicates that those giving and receiving referrals have higher wage variance than others with their same predicted wage. The coefficient of 0.021 on $referred$ and the coefficient of 0.022 on $made\ referral$ are both large, relative to the average squared wage residual of 0.068. Column (2) includes interactions between $made\ referral$

and position dummies, addressing the potential concern that the variance result for providers is driven primarily by supervisors. If so, we might be concerned that the more capable supervisors are both allowed to give referrals and also manage larger teams or receive wages that are more closely tied to their team's performance, leading to higher wage variance absent effects from the referral. However, there is no evidence that the effect of giving a referral on wage variance is larger among supervisors.

Table 5 reports the estimated coefficients from equation 11, which further tests whether the overall higher wage variance reported in table 4 represents increasing wage variance with tenure (versus higher wage variance that appears from initial wage offer). Wage variance does increase with tenure. The estimated effects of a referral on the squared wage change from initial to 3, 6, and 12 month wages are highly significant and very large, greater than the mean squared change for the 3 and 6 month cases and seventy percent of the mean change after 12 months.

7.3 Observable Quality

Table 6 reports results from regressions (12) and (13), which test for differences in education and experience between providers and recipients versus other hired workers in the same factory. Columns (1) and (4) show that referral recipients on average have 0.67 fewer years of education and 0.59 fewer years of experience than other workers in the same factory. By contrast, providers have on average 0.30 more years of education and 0.51 more years of experience than other workers in the same factory. In columns (2) and (5), I include position dummies. While a literal interpretation of the model would say that only a worker's observable quality θ matters in determining her ability to give, or need for, a referral (and not her θ relative to others in the same position) the inclusion of position dummies shows that observable differences in recipients and providers are not only determined by variation in θ across positions.[26] While smaller in magnitude, the results are still negative and significant for recipients and positive (although insignificant) for providers. Columns (3) and (6) show that providers are observably better and recipients are observably worse than other garment workers in the same bari. These results confirm that bari members with mid-range

[26]That is, a worker's observable quality is increasing in her position level, and section 2 points out that giving referrals is more common in higher positions and less common in lower positions. If the results on observable quality did not hold within position, then they would also be consistent with a story in which referrals are a way to make entry level workers feel comfortable, by ensuring that they have an experienced provider around.

values of θ constitute the control group for the referral pairs in equation (8); they are good enough not to need a referral, but not observably good enough to be able to give one.

7.4 Wage Levels with Tenure

Table 7 reports the results of equation 14 which compares changes in the wage level of referral recipients with those of non-referred workers after 3, 6, and 12 months. Whether the sample includes the wages from all workers or only wages for those workers who have remained in the firm for that long, the $tenure \times referred$ coefficient is large and positive. It is significant in each case using each window for the regressions including only stayers, and after 12 months for both samples. The estimated coefficient on the $tenure \times referred$ after 12 months, for example, shows that after a year in a firm, the wage of a referred worker have gone up 4.56 percentage points (12 months times a coefficient of 0.0038 per month) more than that of an observably identical worker who did not receive a referral but has remained in the firm for those 12 months. This increase corresponds to the higher second period wages for referred workers who have had high output, relative to the flat wage schedule offered to non-referred workers.

8 Conclusion

The results of this paper indicate that referrals can minimize a moral hazard problem caused by firms' inability to perfectly observe workers' effort. Referrals provide allow the firms to use the provider's wages to provide the recipient incentives for high effort, a useful tool in an industry where employment spells are relatively short. I provide empirical evidence from data I collected from the garment industry in Bangladesh that the poor performance of a recipient lowers both her own wage and that of the provider. The joint contract ensures the recipient will work hard even though the firm's ability to punish her is limited, and thus allows the firm to hire observably lower skilled workers than it would otherwise hire.

While the empirical work was limited to the garment industry in Bangladesh, there is little reason to believe that firms' potential to use referrals to solve moral hazard is limited to this context. Many labor markets, particularly in the developing world, are also characterized by the high turnover that makes effort difficult to induce using long-term contracts. Anthropological

evidence from some of these labor markets points out that referral recipients work hard because their providers are held responsible for their performance, fitting with the model presented here (Grieco 1987; kyung Kim 1987).

Furthermore, the ability of referrals to induce effort is also likely relevant in certain lower skilled labor markets with developed countries. For instance, sociologists have pointed out the tendency of employers of immigrants to hire relatives of existing workers (Suarez-Orozco, 2001). Given the high mobility of immigrants, firms may worry that new a new immigrants would remain in a location for long enough to fear the repercussions of low effort in a particularly. However, the presence of a referral provider who is more highly skilled or established in a location can allow the firm to hire newer immigrants.

These findings have important implications for policy-makers attempting to prevent network referrals from restricting access to jobs to members of certain privileged networks. Attempts to disseminate information about job openings will not undo network effects in contexts such as the Bangladeshi garment industry. Firms will still hire an observably bad worker only if she receives a referral from a current worker who is willing to allow her own wages to be decreased if the recipient performs poorly. Nor is it obvious that policymakers should attempt to minimize the role of referrals in job hiring; referrals help firms resolve an asymmetric information problem.

Recent literature has demonstrated the importance of social networks in developing economies in a wide range of situations, from spreading information about new crops (Conley and Udry, 2010) to facilitating productive exchange between traders (Fafchamps and Minten, 2002). This paper demonstrates that these efficiency gains from social networks carry over to employment in large firms. While my results suggest that moral hazard is an issue in these firms, referrals allow firms to implement a second-best outcome that leads workers to put forth higher effort than they would without the referral.

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A Firm's Problem, Baseline Case with No Referrals

For a worker of a given observable quality θ, the firm can choose between hiring the worker and inducing high effort, hiring the worker but accepting low effort, or not hiring the worker:

$$
\begin{aligned}
\pi = \max\Big(0,\quad & \\
& \max \quad 2\theta + \pi_h + \pi_l - w_1 - \alpha_h w_{2h} - (1-\alpha_h) w_{2l} \\
& \qquad \text{subject to} \quad \delta\Big(\alpha_h w_{2h} + (1-\alpha_h) w_{2l}\Big) - c \geq \delta\Big(\alpha_l w_{2h} + (1-\alpha_l) w_{2l}\Big) && (IC) \\
& \qquad\qquad w_{2h}, w_{2l} \geq max(w^0(\theta), \tilde{w}(\theta)) && (max\ punishment) \\
& \qquad\qquad w_1, w_{2h}, w_{2l} \geq \underline{w} && (min\ wage) \\
& \qquad\qquad 2\theta + \pi_h + \pi_l = w_1 + \alpha_h w_{2h} + (1-\alpha_h) w_{2l}, && (zero\ profit) \\
& \max \quad 2\theta + 2\pi_l - w_1 - \alpha_l w_{2h} - (1-\alpha_l) w_{2l} \\
& \qquad \text{subject to} \quad w_1, w_{2h}, w_{2l} \geq \underline{w} && (LL) \\
& \qquad\qquad 2\theta + 2\pi_l = w_1 + \alpha_l w_{2h} + (1-\alpha_l) w_{2l} && (zero\ profit)\Big)
\end{aligned}
$$

B Outside options

Consider the role of the workers' outside option $\tilde{w}(\theta)$, which they would earn if their original firm fires them in the second period and are not worthwhile for another firm to hire ($\theta + \pi_l < \underline{w}$). If the firm wants to induce high effort in one of these workers, it must pay her a $w_{2h} = \tilde{w}(\theta) + \frac{c}{\delta(\alpha_h - \alpha_l)}$.

This is not profitable if the worker's output from high effort in the first and low in the second (with α_h probability) is less than the firm's expected wage bill for that worker, $\underline{w}$ in the first period and $\alpha_h w_{2h}$ in the second:

$$\underline{w} + \alpha_h \Big(\tilde{w}(\theta) + \frac{c}{\delta(\alpha_h - \alpha_l)} \Big) \geq \theta + \pi_h + \alpha_h(\theta + \pi_l) \tag{15}$$

The graphical depictions of the model given in figures 3 through 5 assume this constraint holds, so that only workers with $\theta > \underline{\theta}_{high}$ are profitable to hire with high effort. If this constraint does not hold, so that some workers with $\theta + \pi_l < \underline{w}$ are profitable to hire at high effort, referrals still improve efficiency: the firm would use the ability to punish a provider to drive down the recipient's utility after a bad outcome below $\tilde{w}(\theta)$ and thus be able to profitably hire some workers that it could not otherwise. These testable implications of the model would remain the same, since referrals would still allow the firm to induce high effort in workers with $\underline{w} - \pi_l < \theta < \underline{\theta}_{high}$, who would be exerting low effort absent the referral.

C Firm's Problem with Referrals

The firm's set of possible options are

(i) hire both R and P and induce effort in both

(ii) hire both and induce effort only in P

(iii) hire both and induce effort only in R

(iv) hire both but induce effort in neither

(v) hire only P with high effort

(vi) hire only P with low effort

(vii) hire only R with high effort

(viii) hire only R with low effort

The predictions of the model focus on scenario (i). This decision is profitable if

$$\max_{w_1^R, w_1^P, w_{2h}^P, w_{2l}^P, w_{2h}^R, w_{2l}^R, p} 2(\theta_P + \theta_R + \pi_l + \pi_h) - w_1^R - w_1^P - \alpha_h w_{2h}^P - (1-\alpha_h) w_{2l}^P - \alpha_h w_{2h}^R - (1-\alpha_h)(w_{2l}^R - p)$$

subject to:

$$w_{2h}^P \geq w_{2l}^P + \frac{c}{\delta(\alpha_h - \alpha_l)} \qquad (IC, P)$$

$$w_{2h}^R \geq (w_{2l}^R - p) + \frac{c}{\delta(\alpha_h - \alpha_l)} \qquad (IC, R)$$

$$w_{2h}^P, w_{2h}^P - p, w_{2l}^P, w_{2l}^P - p \geq max(w^0(\theta_P), \tilde{w}(\theta_P)) \qquad (max\ punishment,\ P)$$

$$w_{2h}^R, w_{2l}^R \geq max(w^0(\theta_R), \tilde{w}(\theta_R)) \qquad (max\ punishment,\ R)$$

$$w_1^P, w_{2h}^P, (w_{2h}^P - p), w_{2l}^P, (w_{2l}^P - p) \geq \underline{w} \qquad (minimum\ wage, P)$$

$$w_1^R, w_{2h}^R, w_{2l}^R \geq \underline{w} \qquad (minimum\ wage, R)$$

$$\begin{aligned} & w_1^R + \alpha_h w_{2h}^R + (1-\alpha_h)(w_{2l}^R - p) + w_1^P + \alpha_h w_{2h}^P + (1-\alpha_h) w_{2l}^P \qquad (IR) \\ & \quad \geq \max\Big(w_1(\theta_P) + \alpha_h w_{2h}(\theta_P) + (1-\alpha_h) w_{2l}, 2\tilde{w}(\theta_P)\Big) \\ & \qquad + \max\Big(w_1(\theta_R) + \alpha_h w_{2h}(\theta_R) + (1-\alpha_h) w_{2l}(\theta_R), 2\tilde{w}(\theta_R)\Big) \end{aligned}$$

is greater than or equal to zero. The IR constraint says that in expectation, participating in the referral must be profitable for the recipient and provider together. That is, their wages in the referral must exceed their payoffs from not participating, which are equal to the max of the wages they would be offered by other firms, or their outside options.

D Proofs

Proposition D.1. *Workers with $\theta_R < \underline{\theta}_{NR}$ can be profitably hired by the firm if they have a referral from a provider of sufficiently high θ_P.*

Proof. Consider a worker with $\theta_R < \underline{\theta}_{NR}$ who would not be hired absent a referral. The firm can profitably hire this worker if the constraints in case (i) are satisfied, and both workers exert high effort. Suppose specifically that worker had a referral from a provider with observable quality θ_P

and the firm sets wages:

$$
\begin{aligned}
w_1^R &= \underline{w} \\
w_1^P &= \underline{w} \\
w_{2l}^R &= \underline{w} \\
w_{2h}^R &= \underline{w} - p + \tfrac{c}{\delta(\alpha_h - \alpha_l)} \\
w_{2l}^P &= w^0(\theta_P) + p \\
w_{2h}^P &= w_{2l}^P + \tfrac{c}{\delta(\alpha_h - \alpha_l)}
\end{aligned}
$$

That is, the minimum wage constraints bind for R in the first period and the second period after low output, and for P in the first period. Additionally, the IC constraints for high effort just bind for P and R. For a given punishment p, the firm increases w_{2l}^P (relative to $w^0(\theta_P)$) by p in order to insure that the provider doesn't leave in the second period, even if she and the recipient both had low output in the first period. Then with $w^0(\theta_P) = \theta_P + \pi_l$, the observable quality of referral provider for which joint output just equals the wage bill is:

$$
\begin{aligned}
2\theta_R + 2\theta_P + 2\pi_h + 2\pi_l = & \underbrace{2\underline{w}}_{\text{first period wages}} + \underbrace{w_{2l}^P + \frac{c\alpha_h}{\delta(\alpha_h - \alpha_l)}}_{\text{second period wages for provider}} \\
& + \underbrace{\underline{w} - (w_{2l}^P - \theta_P - \pi_l) + \frac{c\alpha_h}{\delta(\alpha_h - \alpha_l)}}_{\text{second period wages for recipient}} \\
\underline{\theta}_R(\theta_P) = & \frac{3}{2}\underline{w} + \frac{c\alpha_h}{\delta(\alpha_h - \alpha_l)} - \frac{\pi_l}{2} - \pi_h - \frac{\theta_P}{2}
\end{aligned} \tag{16}
$$

Then since $\underline{\theta}_R(\theta_P)$ is decreasing in θ_P, for any θ_R, a referral in which the output from both workers exerting high effort is greater than the wages the firm would need to satisfy the IC and minimum wage constraints is possible if θ_P is sufficiently high. The IR constraint for the $\underline{\theta}_R(\theta_P)$ derived in equation 16 holds as long as the pair's period 1 utility from both working (with high effort) and earning the wages given above is greater than their utility without the referral (where P would be hired at high effort since $\theta_P > \underline{\theta}_{high}$, and R would receive the value of her outside option since

$\theta_R < \underline{\theta}_{NR}$):

$$2\underline{w} - 2c + \delta(\underline{w} + \theta_P + \pi_l + 2\frac{c\alpha_h}{\delta(\alpha_h - \alpha_l)}) \geq \tag{17}$$
$$(1+\delta)\tilde{w}(\theta_R) + \theta_P + \pi_h - \frac{c\alpha_h}{\delta(\alpha_h - \alpha_l)} - c + \delta(\theta_P + \pi_l + \frac{c\alpha_h}{\delta(\alpha_h - \alpha_l)})$$
$$(2+\delta)\underline{w} - c + \frac{c\alpha_h(1+\delta)}{\delta(\alpha_h - \alpha_l)} \geq \theta_P + \pi_h + (1+\delta)\tilde{w}(\theta_R)$$

Whether this constraint is satisfied depends on the value of R's outside option ($\tilde{w}(\theta_R)$) relative to the minimum wage and the discount rate (since P has to delay more of her compensation than she would absent the referral). If this constraint fails, for the $\underline{\theta}_R(\theta_P)$ derived in equation 16, then the $\theta_R(\theta_P)$ that makes (17) hold with equality would be the lowest θ_R accepted from a provider with θ_P. If $\delta = 1$, this constraint simplifies to

$$2\theta_R + \pi_h + \pi_l - c \geq 2\tilde{w}(\theta_R) \tag{18}$$

Accordingly, if workers are relatively patient (δ close to 1) and θ_P is sufficiently high, then the referral is profitable as long as R's output in garments is above her outside option.

Note that there is a possibility for other types of referral contracts to be profitable. For instance, the firm could hire R for only one period and charge P a punishment of $\frac{c}{\delta(\alpha_h - \alpha_l)}$ if R has low output. However, since they key testable implication would be the same (workers with $\theta_R < \underline{\theta}_{NR}$ would be hired), I will not further analyze these contracts. □

Proposition D.2. $Var(w^P|\theta) > Var(w|\theta)$

Proof. Without a referral, the second-period wage distribution of a worker of observable quality θ_P will be:

$$w = \begin{cases} \theta_P + \pi_l + \frac{c}{\delta(\alpha_h - \alpha_l)} & \text{with probability} \quad \alpha_h \\ \theta_P + \pi_l & \text{with probability} \quad 1 - \alpha_h \end{cases}$$

yielding variance $\alpha_h(1-\alpha_h)\frac{c}{\delta(\alpha_h - \alpha_l)}$. If this worker gives a referral, then she will receive some punishment p (whose level depends on the θ_R) if the recipient has low output. Her observed

second-period wage distribution will then be:

$$w = \begin{cases} w_{2l}^P + \frac{c}{\delta(\alpha_h - \alpha_l)} & \text{with probability} \quad \alpha_h^2 \\ w_{2l}^P + \frac{c}{\delta(\alpha_h - \alpha_l)} - p & \text{with probability} \quad \alpha_h(1-\alpha_h) \\ w_{2l}^P & \text{with probability} \quad \alpha_h(1-\alpha_h) \\ w_{2l}^P - p & \text{with probability} \quad (1-\alpha_h)^2 \end{cases}$$

which yields wage variance $\alpha_h(1-\alpha_h)p\frac{c}{\delta(\alpha_h-\alpha_l)}$. For any positive p, this is larger than the variance with no referral. □

Proposition D.3. *For workers with $\underline{\theta}_{NR} < \theta < \underline{\theta}_{high}$, referrals increase both wage level and variance with tenure: $E(w_2^R|\theta) - E(w_1^R|\theta) > E(w_2|\theta) - E(w_1|\theta)$ and $(E(w_2^R|\theta) - E(w_1^R|\theta))^2 > (E(w_2|\theta) - E(w_1|\theta))^2$*

Proof. Consider a worker with observable quality θ_R, where $\underline{\theta}_{NR} < \theta_R < \underline{\theta}_{high}$. Without a referral, that worker will be hired with low effort and be paid her output $\pi_l + \theta_R$ in each period. The minimum p required for that worker to be hired – call this $\underline{p}(\theta_R)$ – sets her output equal to the wages net of this punishment.

$$\begin{aligned} 2\theta_R + \pi_h + \pi_l &= \theta_R + \underline{w} - p + \frac{c\alpha_h}{\delta(\alpha_h - \alpha_l)} \\ \underline{p}(\theta_R) &= \underline{w} + \frac{c\alpha_h}{\delta(\alpha_h - \alpha_l)} - \pi_h - \theta_R \end{aligned} \tag{19}$$

Since $\theta > \underline{w} - \pi_l = \underline{\theta}_{NR}$, and $\frac{c\alpha_h}{\delta(\alpha_h-\alpha_l)} \leq \frac{c}{\delta(\alpha_h-\alpha_l)}$, then $p < \frac{c}{\delta(\alpha_h-\alpha_l)}$ and therefore $w_h^R > w_l^R$. That is, the firm satisfies R's IC for high effort both by punishing the provider and creating a wedge between w_h^R and w_l^R, which increases wage variance among referral recipients relative to the second period wage a non-referred worker gets that does not depend on output. Note also that R's expected wage

$$Ew_2^R = (1-\alpha_h)\underline{w} + \alpha_h(\underline{w} + \frac{c}{\delta(\alpha_h - \alpha_l)}) = \underline{w} + \frac{c\alpha_h}{\delta(\alpha_h - \alpha_l)}$$

is strictly greater than her first period wage of $\underline{w}$. This reflects both the fact that the punishment levied on P does not appear in R's wage and also that the firm uses higher second period wages after a good outcome as provide incentives for effort for R. So the wage trajectory for a recipient is increasing with tenure, relative to the flat wage schedule the firm gives a worker with the same

θ_R who does not have a referral. $\square$

E Selection Model

Production remains the same as in the moral hazard framework except for the fact that effort is now a function of observable quality θ and unobservable quality ϵ, where ϵ can be high or low. A worker's type is revealed (to the current firm and the market overall)[27] between the first and second periods. As in the moral hazard set-up competition between firms bids the worker's wage up to her expected productivity. Let $0 < \beta < 1$ be the proportion of high types in the population. Analogous to the moral hazard framework, the probability of high output if $\epsilon = \epsilon_{high}$ is α_h, which is greater than the probability of high output if $\epsilon = \epsilon_{low}$. Define $\pi_h = \alpha_h \epsilon_h + (1 - \alpha_h)\epsilon_l$ and $\pi_l = \alpha_l \epsilon_h + (1 - \alpha_l)\epsilon_l$. In the second period of work, the firm will retain all workers for whom $\theta + \epsilon \geq \underline{w}$. Workers with θ such that $\theta + \epsilon_l \geq \underline{w}$ are hired and retained in the second period whether they are low type or not. Workers with $\theta + \epsilon_l < \underline{w}$ are not worth retaining in the second period if they turn out to be low type but are worthwhile to hire in the first with some possibility they are high type if

$$\theta + E\pi - \underline{w} + \beta(\theta + \pi_h) - \underline{w} \geq 0 \tag{20}$$

$$\underline{\theta}_{NR} = \underline{w} - \frac{E\pi - \beta\pi_h}{1 + \beta}$$

So workers with $\underline{\theta}_{NR} \leq \theta \leq \underline{w} - \epsilon_l$ are dismissed in the second period, whereas workers with $\theta_{NR} \geq \underline{w} - \epsilon_l$ are retained and then paid their newly revealed productivity.

Referrals allow the firms to hire workers with $\theta < \theta_{NR}$ that it knows are high type. Then the firm sets a punishment p on providers whose recipients had low output that makes it incentive-compatible to refer high-type workers but not low type.[28] Again assuming that R and P can

[27] If learning were asymmetric, the results would persist as long as there is any reason that firms would like to update wages to reflect this new information about her productivity. This would occur as long as outside firms learned anything about workers producitivty. Even if learning is entirely limited to the the current firm, that firm may still pay higher-productivity workers more if there is some chance workers will choose to leave after receiving a wage offer.

[28] A low type worker has no incentive to receive a referral in this model, she would prefer for the firm to have no information and pay her according to the expected productivity of a random worker.

perfectly enforce contracts between each other, the IC and PC constraints for a referral are:

$$-(1-\alpha_h)p + \underline{w} + \theta + \pi_h \geq 0 \quad \text{(PC: Do refer high types)} \tag{21}$$

$$-(1-\alpha_l)p + \underline{w} \geq 0 \quad \text{(IC: Don't refer low types)} \tag{22}$$

In the second period, once the market also knows the recipient's type, the firm must pay the recipient her output $\theta + \pi_h$.[29] Accordingly, conditional on θ, there is no wage variance of referred workers in the second period, yielding a lower variance increase with tenure than with the non-referred workers.

[29]Note that, like the moral hazard model, the selection model also contains an element of the patronage model: the firm will hire workers whose output (even with high type) is below the minimum wage $\theta + \pi_h < \underline{w}$ because it can "charge" the referrer an amount equal to the difference between the recipient's output and the minimum wage. Still, since referrals are also a selection mechanism, referred workers are of higher type than nonreferred workers, and we should see differential learning as firms learn the type of non-referred workers and update wages accordingly.

Figure 1: Cumulative Turnover and Wage Updating by Tenure

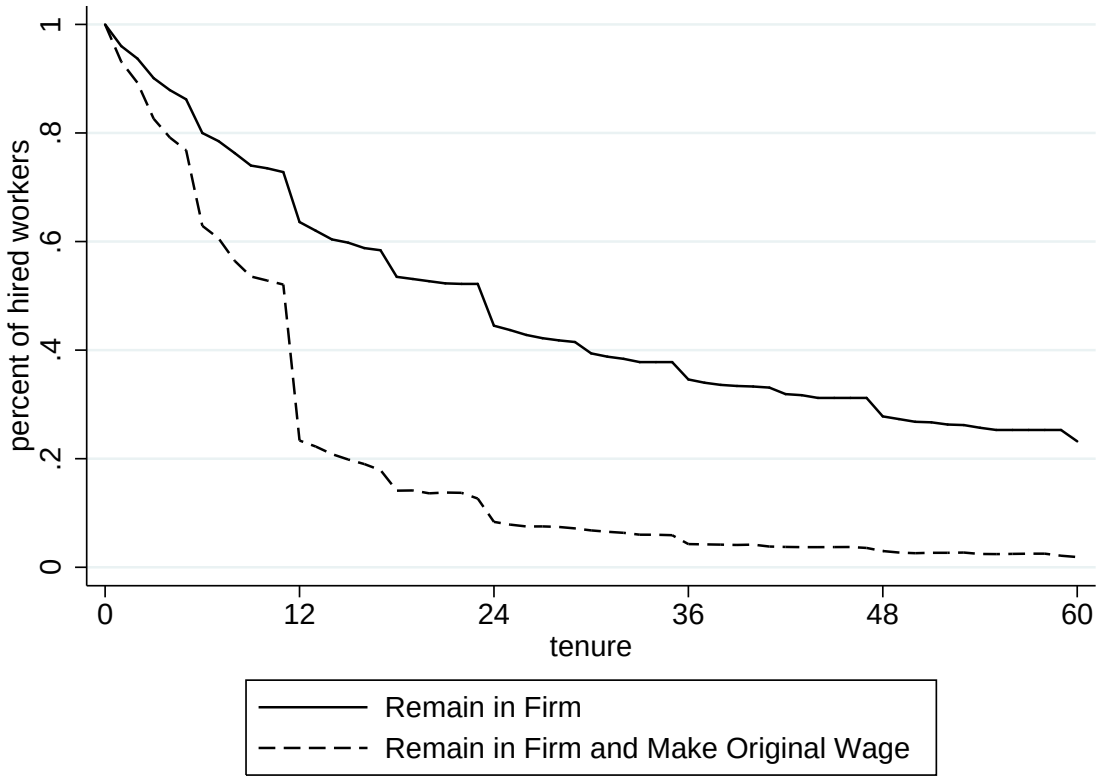
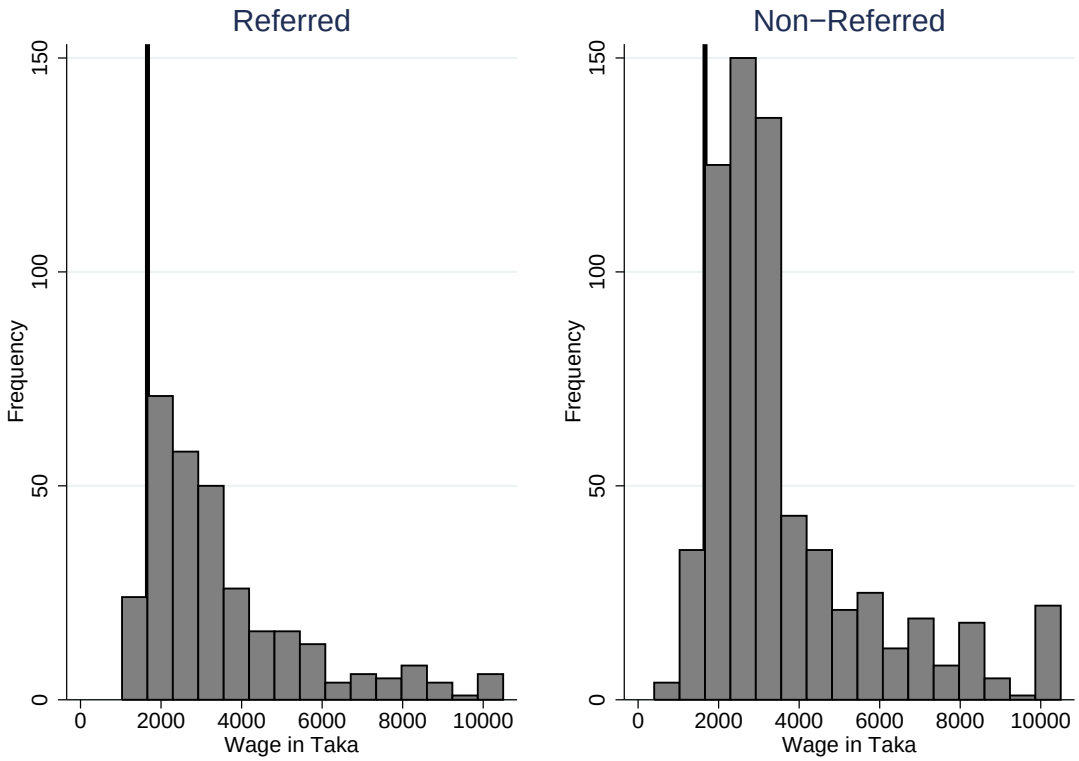


Figure 2: Wage Distribution for Referral Recipients and Non-Referred Workers



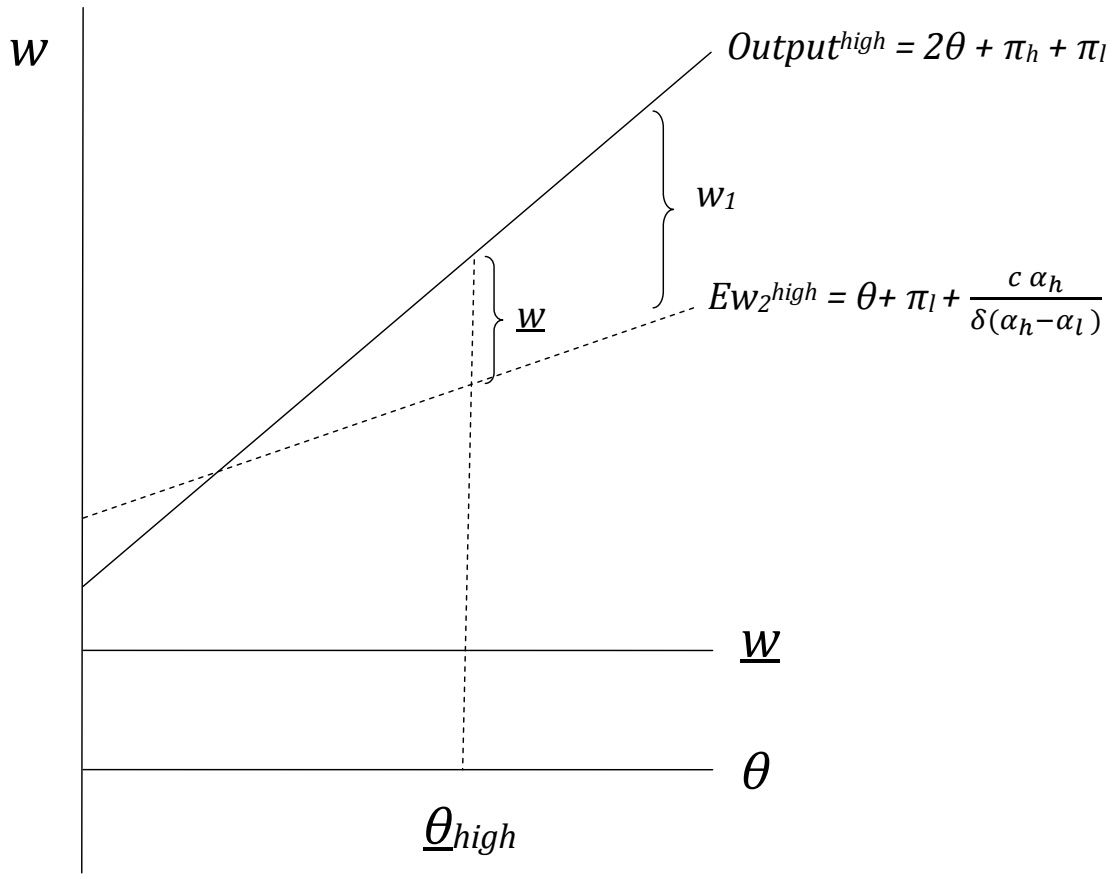


Figure 3: Observable quality and incentives for high effort

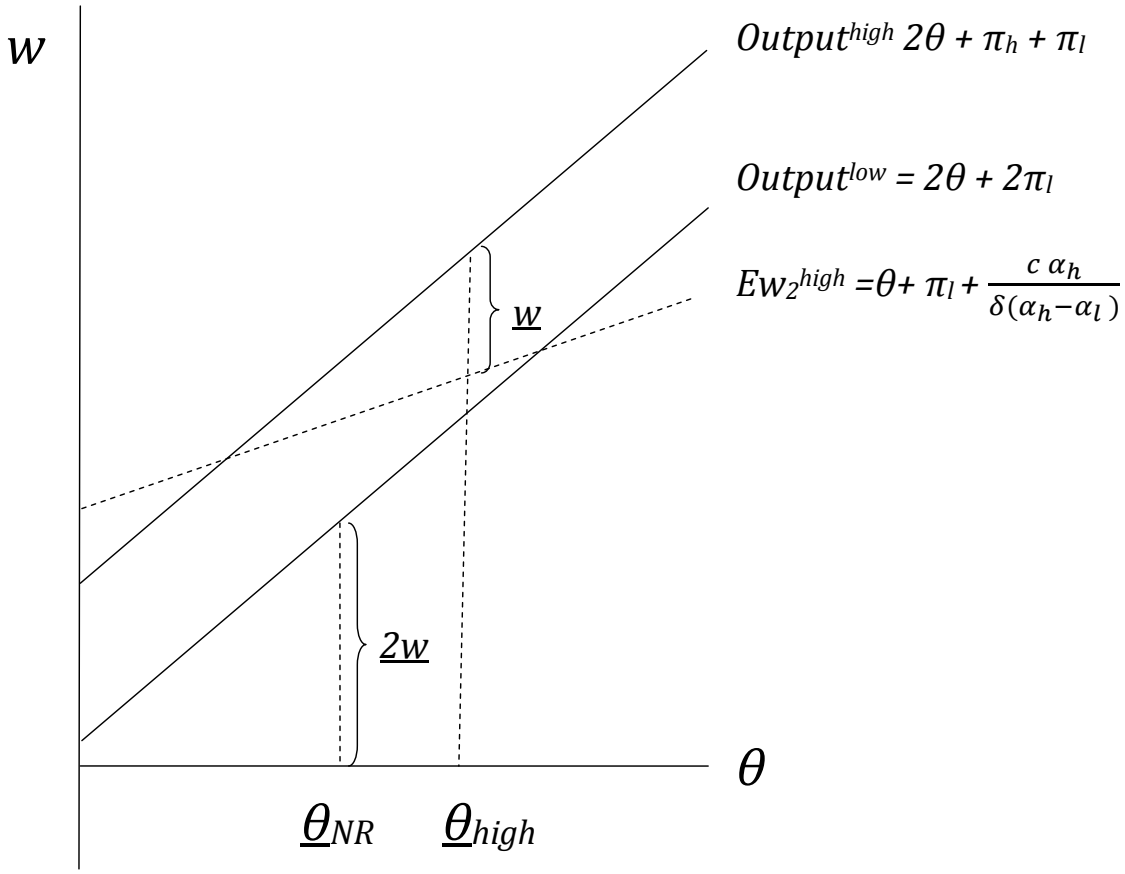


Figure 4: Observable quality and hiring of non-referred workers

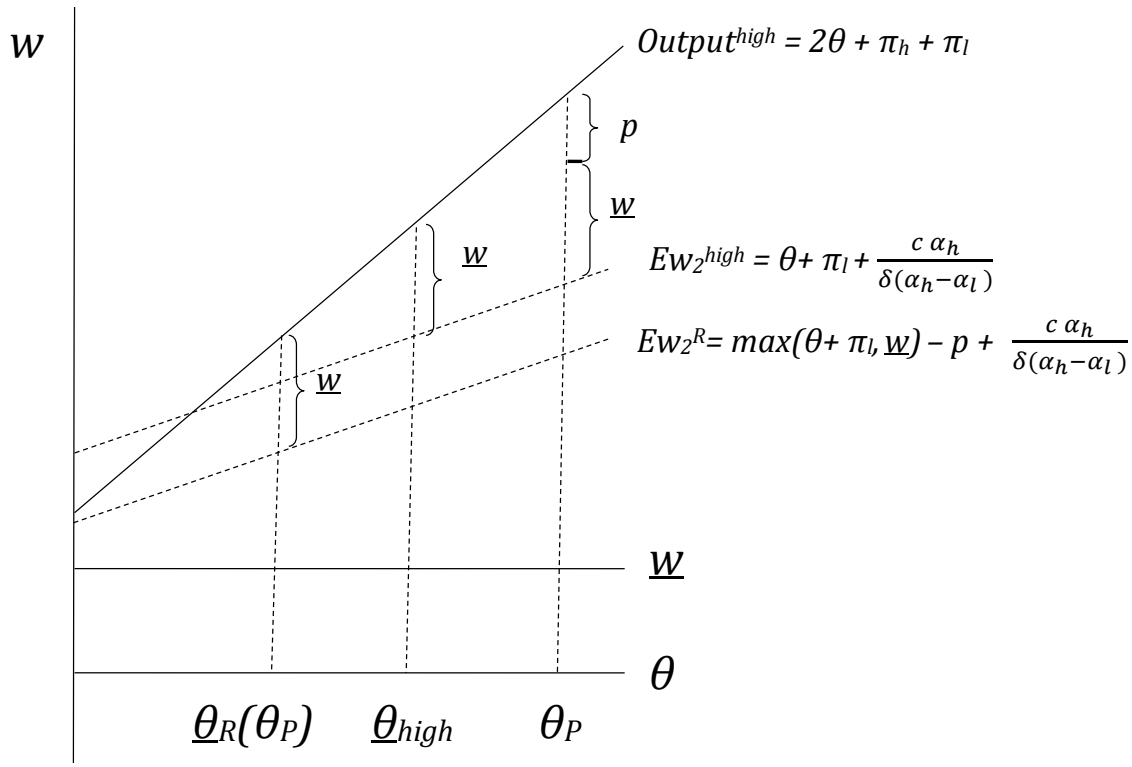


Figure 5: A referral pair where both exert high effort

Table 1: Summary Statistics, Recipients, Providers and Other Workers

	recipient	provider[a]	neither	overall
male	0.436	0.609	0.373	0.433
	[0.029]	[0.032]	[0.022]	[0.016]
experience (months)	14.017	26.285	20.376	19.931
	[1.576]	[2.377]	[1.518]	[1.072]
education (years)	5.354	6.617	5.799	5.870
	[0.203]	[0.253]	[0.155]	[0.116]
married	0.736	0.865	0.769	0.776
	[0.025]	[0.023]	[0.019]	[0.014]
has a child	0.340	0.457	0.415	0.397
	[0.027]	[0.033]	[0.023]	[0.016]
age	26.017	28.448	25.369	26.029
	[0.482]	[0.543]	[0.363]	[0.267]
originally from current village	0.112	0.100	0.059	0.078
	[0.018]	[0.020]	[0.011]	[0.009]
either parent any education	0.520	0.567	0.493	0.515
	[0.029]	[0.033]	[0.023]	[0.016]
good relations with management[b]	0.840	0.853	0.808	0.829
	[0.021]	[0.023]	[0.018]	[0.012]
appointment letter[c]	0.330	0.494	0.293	0.346
	[0.027]	[0.033]	[0.021]	[0.015]
took manual test[d]	0.340	0.463	0.462	0.429
	[0.027]	[0.033]	[0.023]	[0.016]
commute time (minutes)	18.170	19.316	18.868	18.887
	[0.719]	[0.874]	[0.566]	[0.419]
typical daily hours of work	11.801	11.805	11.642	11.722
	[0.123]	[0.149]	[0.113]	[0.077]
N	306	231	485	967
percent	31.6	23.9	50.2	100

Notes: (a) Workers who both received and gave referrals appear in both of the first two columns.
(b) worker reported "good" or "excellent" relationship, out of possible choices "very bad", "bad", "okay", "good", "excellent"
(c) an appointment letter states that the worker cannot be dismissed without cause
(d) a manual test taken before an employment spell consists of an employer sitting the worker down in front of a sewing machine, pre-hiring, and asking her to demonstrate the specific skills and maneuvers that she knows

Table 2: Wage correlation between provider and recipient

	Dep. Var is wage residual $\tilde{w}_{ift}$			
	(1)	(2)	(3)	(4)
$\tilde{w}_{jft}$	0.0006	0.0088	-0.0252***	0.0006
	[0.005]	[0.006]	[0.006]	[0.005]
$\tilde{w}_{jft} \times ever\ referral_{ij}$	0.0051	0.0106	-0.0206	0.0063
	[0.027]	[0.028]	[0.028]	[0.033]
$\tilde{w}_{jft} \times same\ factory_{ijt}$	0.0566***	0.0060***	0.0503***	0.0566***
	[0.012]	[0.015]	[0.015]	[0.012]
$\tilde{w}_{jft} \times referral_{ijt}$	0.3195***	0.3110***	0.3390***	0.3200***
	[0.074]	[0.077]	[0.076]	[0.078]
$\tilde{w}_{jft} \times same\ machine_{ijt}$		-.0.0219***		
		[0.009]		
$\tilde{w}_{jft} \times same\ factory_{ijt} \times same\ machine_{ijt}$		0.1073***		
		[0.028]		
$\tilde{w}_{jft} \times same\ position_{ijt}$			0.0685***	
			[0.010]	
$\tilde{w}_{jft} \times same\ factory_{ijt} \times same\ position_{ijt}$			0.0194	
			[0.031]	
$\tilde{w}_{jft} \times referral_{ijt} \times same\ team_{ijt}$				-0.0045
				[0.064]
Observations	66,784	66,784	66,784	66,784
R-squared	0.001	0.001	0.002	0.001

*Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1*

The unit of observation is a matched pair of the wage residual $\tilde{w}_{ift}$ of a bari member and the wage residual $\tilde{w}_{jft}$ of another bari member working in the garment industry in the same month. Residuals are from the first stage wage regression given by equation 7.

Bootstrap standard errors in brackets, constructed by taking repeated samples of monthly wage observations and then constructing the bari member pairs for each sample chosen (then repeating 100 times)

Table 3: Comparing workers currently in referral relationships to other workers in same factory

	Dep. Var is wage residual $\tilde{w}_{ift}$			
	(1)	(2)	(3)	(4)
$\tilde{w}_{jft}$	0.0523	0.0440	0.0551**	0.0523
	[0.035]	[0.042]	[0.028]	[0.035]
$\tilde{w}_{jft} \times referral_{ijt}$	0.2541***	0.2545***	0.2537***	0.2880***
	[0.053]	[0.053]	[0.050]	[0.069]
$\tilde{w}_{jft} \times same\ machine_{ijt}$		0.0205		
		[0.049]		
$\tilde{w}_{jft} \times same\ position_{ijt}$			-0.0073	
			[0.041]	
$\tilde{w}_{jft} \times referral_{ijt} \times same\ team_{ijt}$				-0.0740
				[0.080]
Observations	1,520	1,520	1,520	1,520
R-squared	0.032	0.032	0.032	0.033

*Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1*

The unit of observation is a matched pair of the wage residual $\tilde{w}_{ift}$ of a bari member who was in the same factory and a referral between two members of that worker's bari and the wage residual $\tilde{w}_{jft}$ of another bari member working in the same factory in the same month. Residuals are from the first stage wage regression given by equation 7.

Bootstrap standard errors in brackets, constructed by taking repeated samples of monthly wage observations and then constructing the bari member pairs for each sample chosen (then repeating 100 times)

Table 4: Unexplained variance, providers and recipients

Dependent Var: $\hat{\epsilon}^2_{if}$ from first stage wage regression		
	(1)	(2)
$x'_{if}\hat{\beta}$	0.0490***	0.0570***
	[0.0162]	[0.0188]
referred	0.0214**	0.0199**
	[0.0099]	[0.0100]
made referral	0.0220*	0.0332
	[0.0114]	[0.0327]
operator		-0.0163
		[0.0126]
supervisor		-0.00613
		[0.0236]
operator × made referral		-0.0101
		[0.0352]
supervisor × made referral		-0.0200
		[0.0434]
Mean Dep Var	0.069	0.069
Observations	939	939
R-squared	0.023	0.026

*Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is a worker's squared wage residual from equation 9, which is then regressed on the worker's fitted wage $x'_{if}\hat{\beta}$ from the same regression, along with dummy variables for referred and made referral.*

Table 5: Within person wage variance, recipients vs. non-referred workers

Dep. Var. is $(\tilde{w}_i \textit{ at tenure } T - \tilde{w}_i \textit{ at tenure } 0)^2$			
T	3 months	6 months	12 months
referred	0.0190***	0.0360***	0.0388***
	[0.005]	[0.011]	[0.015]
Mean Dep Var	0.013	0.033	0.054
Observations	1775	1473	1026
R-squared	0.013	0.008	0.018

*Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1*

The dependent variable is the squared difference between the individual's wage (conditional on observables) $\tilde{w}_i$ after 3, 6, or 12 months minus the individual's initial wage offer (conditional on observables). The dependent variable is then regressed on a referred dummy, and also on experience, sex, and education.

Standard errors in brackets, clustered at person level

Table 6: Observable Characteristics, Providers and Recipients

	(1)	(2)	(3)	(4)	(5)	(6)
Dep Var	Educ	Educ	Educ	Exper	Exper	Exper
referred	-0.670***	-0.500**	-0.611**	-0.590***	-0.257*	-0.570***
	[0.253]	[0.251]	[0.240]	[0.152]	[0.140]	[0.167]
made referral	0.302	0.094	0.256	0.509***	0.194	0.485**
	[0.287]	[0.268]	[0.287]	[0.178]	[0.163]	[0.189]
Mean Dep. Var.	5.909	5.909	5.909	4.059	4.059	4.059
Position dummies	N	Y	N	N	Y	N
Factory FE	Y	Y	Y	Y	Y	Y
Bari FE	N	N	Y	N	N	Y
Observations	2112	2112	2112	2030	2030	2030
R-squared	0.531	0.546	0.629	0.540	0.622	0.573

*Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1; Unit of observation is a worker-factory spell.*

Education and experience measured in years, defined at the beginning of a worker spell; Regression includes control for male; position dummies are indicators for helper, operator, and supervisor

Table 7: Wages with tenure

	Dep. Var. is log(wage)					
Time since hired	3 months	3 months	6 months	6 months	12 months	12 months
referred	-0.0546*	-0.0602*	-0.0592**	-0.0583*	-0.0546*	-0.0387
	[0.030]	[0.032]	[0.030]	[0.034]	[0.030]	[0.040]
tenure	-0.0015	-0.0027*	-0.0019	-0.0020	-0.0007	-0.0011
	[0.002]	[0.002]	[0.001]	[0.001]	[0.001]	[0.001]
tenure×referred	0.0024	0.0049*	0.0038	0.0052**	0.0035*	0.0038**
	[0.003]	[0.003]	[0.003]	[0.002]	[0.002]	[0.002]
stayers only	N	Y	N	Y	N	Y
Observations	7,958	7,375	12,968	10,715	20,794	13,917
R-squared	0.680	0.688	0.677	0.688	0.678	0.713

*Stars indicate significance: *** p<0.01, ** p<0.05, * p<0.1*

Controls: factory fixed effects, experience, experience squared, male, education; Standard errors in brackets, clustered at the person level