# Essays in Development Economics and Political Economy

by

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# Declaration

This thesis is submitted to the University of Warwick in accordance with the requirements of the degree of Doctor of Philosophy in Economics. I declare that it has not been submitted for a degree at another university. Chapter 2 is a joint work with James Fenske (University of Warwick) and Namrata Kala (Massachusetts Institute of Technology) and Chapter 3 is co-authored with Aiman Farrukh (Lahore School of Economics).

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## Abstract

This thesis is a collection of three chapters studying the role of formal and informal institutions in economic development within low income countries. The first chapter emphasizes the importance of informal institutions and social capital in a setting where formal institutions are generally weak. I show that water scarcity can have a different effect on cooperation over water, depending on whether the shortage is transitory or long term. Using daily outlet-level water theft data from Pakistan, I first show that an unexpected short-term water shortage increases the probability of the over-extraction of surface water. Then, I examine how farmers respond to long-term scarcity by exploiting a natural experiment that decreases the effective availability of groundwater — the key substitute for surface water — through an increase in groundwater pollution. The instrumented difference-in-differences estimates show that, in contrast to a short-term shock, long-term scarcity increases inter-village cooperation. Finally, I provide evidence that informal institutions and caste networks are important for managing water theft under prolonged scarcity. Taken together, these results suggest that long-term environmental change can push communities to adapt by investing in informal mechanisms that enforce cooperation.

In the second chapter, co-authored with James Fenske and Namrata Kala, the focus shifts away from the informal institutions and we explore how formal institutions affect firm size distribution in India. Specifically, we study the impact of environmental regulations on firm entry and size. We assemble novel data on more than half a million environmental permit applications by Indian firms. Using event study framework, we show that a 2016 re-categorization reform that lowered the regulatory burden in several industries has heterogeneous effects - lowering the regulatory burden from high to medium increased the number of applications from new firms, and reduced the size of the marginal new entrant in terms of both labor and capital. New entrants spent less on pollution abatement. However, lowering the regulatory burden from medium to low has no effect. Further, we demonstrate several discontinuities in the firm size distribution that correspond to regulatory and fee thresholds.

The first chapter documents the importance of collective action in developing countries. In the last chapter, co-authored with Aiman Farruk, we highlight the short- and long-term impacts of collective action in a different context. We explore whether women's involvement in a social movement can affect their labor force participation and change household investment in girls' education. To do so, we study one of the largest land struggles in Pakistan – *Anjuman-e-Mazareen* Punjab (AMP). The movement had pushed tenant farmers from several districts in Punjab to protest against a proposed change in lease agreements on state-owned land. At a time when most women in the country were mobility constrained, these circumstances forced some women to join the AMP movement and become an integral part of it. We use 16 years of individual-level data and use difference-in-differences and triple difference approach, to document two key results. First, the movement increased women's labor force participation, but this affect disappears after 2008. Second, the movement led to an increase in school enrollment and secondary school completion among the younger cohort. Finally, consistent with qualitative studies, we find suggestive evidence that the effect on increased investment in girls' education is driven by increased involvement of women in decision making within households.

# 1 Resource Scarcity and Cooperation

## 1.1 Introduction

Nearly 1.6 billion people live in countries with water scarcity. If the current trend in climate change persists, water scarcity will propagate to other regions, and will exacerbate conditions in regions where it is already present (World Bank, 2016). In the least developed countries, where the risk of future water shortages is disproportionally high, the livelihood of a large number of people depends on agriculture — the largest user of water.[1] Agriculture water is usually jointly managed, and thus, its effective management requires mutual cooperation across farmers and communities. This cooperation becomes essential in developing countries, where formal institutions are generally weak and communities often rely on relational contracts.[2] Yet, we know little about how resource scarcity will shape the incentives to cooperate for communities sharing joint resources.

In this paper, I study how resource scarcity affects inter-village cooperation in Pakistan — a country that is ranked among the most water stressed in the world and is expected to face severe water shortages in the future.[3] Due to the arid climate, farmers primarily rely on surface water irrigation that is centrally managed. Water is allocated to farmers according to landholdings, but the allocation is often far below total crop requirements. The combination of insufficient water and locational asymmetry gives upstream farmers an incentive to illegally divert water.[4] These illegal diversions (or water thefts) have severe implications for farmers downstream, particularly in areas without any alternative sources of water (e.g. groundwater and rainfall). A decrease in water availability will increase the incentives to cheat for upstream farmers. However, significant and long-lasting water scarcity could provide both time and incentives for downstream farmers to organize better and enforce cooperation through informal mechanisms such as group monitoring and social fines (Wade, 1989).

I use two natural experiments to test the effects of water scarcity on inter-village cooperation. To construct a proxy for cooperation across villages, I collect unique panel data on daily outlet-level water theft incidents, and complement it with

[1] See Mendelsohn *et al.* (2006), World Bank (2016)

[2] See Ostrom (1990), Greif (1993), McMillan and Woodruff (1999), Macchiavello and Morjaria (2015), Bubb *et al.* (2018), Macchiavello and Morjaria (2019)

[3] See Kugelman (2009), Cheema *et al.* (2016)

[4] See World Bank (1994), Bandaragoda and Saeed (1995), Azam and Rinaud (2000), World Bank (2002), Anwar and Ul Haq (2013)

administrative data on water discharge. Then, I exploit random variation in weekly rainfall across regions in a difference-in-differences framework in order to study how an unexpected short-term water scarcity affects water thefts. To explore how farmers react to long-term scarcity, I use an instrumented difference-in-differences design and rely on exogenous changes in the groundwater quality induced by an increase in the industrial effluent from a nearby city.

I find that an unexpected short-term water scarcity shock increases theft. To construct the shock, I calculate the difference between the village-week level number of rainy days and its long-term average.[5] The identification comes from random variation in rainfall across regions, and controlling for both village and week fixed effects, which capture aggregate shocks and village-specific time-invariant characteristics. A negative rainfall shock during the cropping season increases demand for alternative sources of water, and since the allocation of surface water is fixed and far below total crop requirements, the decrease in rainfall increases incentives to over-extract for upstream farmers. I find that one fewer rainy day than the weekly average increases water theft by 14%. This pattern also appears in the administrative water discharge data, where water availability to tail-end villages decreases under a negative rainfall shock, even after controlling for the overall decrease in water availability.

I use changes in the effective availability of groundwater — which is the key substitute for surface water in this region — to construct a measure of prolonged scarcity, and find that in contrast to a short-term shock, long-term scarcity leads to an increase in cooperation. The effective availability of groundwater partly depends on quality, which is measured in terms of salt content. A decrease in quality negatively affects crop yield, and thus, increases demand for surface water. However, the changes in quality could be endogenous to farmers' choices, such as the groundwater extraction rate and use of pesticides. Therefore, I exploit a natural experiment that decreases the groundwater quality in several villages. In 2008 and 2009, there was a significant rise in industrial activity in a district with a sizeable textile industry.[6] The entry of new firms coincides well with an increase in groundwater contamination in the neighboring regions, located mostly downstream from the contamination area. I use this exogenous and indefinite change in groundwater quality in an instrumented difference-in-differences framework.

[5] I use the rainy days to define short-term scarcity because the distribution of rainfall is an important factor for crop yield (Fishman, 2011). However, I will also use the more traditional measures of rainfall shock in the robustness section.

[6] This trend in firm entry could partly be due to an increase in international market share after European Union reduced the anti-dumping duty on Pakistani bed linen (Ghori, 2012) and also partly due to the construction of new industrial estates by Faisalabad Industrial Estate Development & Management Company.

I find that a one standard deviation increase in pollution increases the ratio of water discharge at the tail to its allocated amount (i.e. inter-village cooperation) by 13%. The reduced-form estimates show that the change in inter-village cooperation was very small for the first two years after the treatment, but there is a statistically significant and large increase afterwards that stays at the same level for the next four years. This pattern is consistent with the explanation that farmers might need time to understand the changes in water quality and to resolve the collective action problem.

Looking at prolonged and transitory water shortages together, I find that areas with high groundwater pollution are relatively less likely to encounter water theft under a negative rainfall shock. I explore this pattern further by comparing water channels before and after the groundwater contamination, and find that contaminated areas no longer respond to short-term scarcity shocks. This indicates that part of the increase in inter-village cooperation in the treated areas is due to better management of water theft. Put differently, the long-term shortage of water has increased the cost of stealing in these regions.

I explore possible mechanisms through which long-term scarcity could increase inter-village cooperation. Following Wade (1989), I look at the role of social organization in enforcing cooperation, and provide three pieces of evidence in support of this channel. First, I show that the long-term scarcity is very strongly correlated with the presence of active informal village-level institutions. Second, on distributaries where head- and tail-end villages are less likely to share the same caste, farmers struggle to resolve water theft disputes under long-term scarcity. Third, in a community survey that I recently conducted, farmers in the contaminated areas were more likely to report use of informal mechanism to deal with inter-village water theft problems. I do not find much support for political patronage or changes in local politicians. This channel is known to be important in improving allocation of water (Beg, 2019) and other public goods (Besley *et al.*, 2004) in developing economies.[7] There is also not much support for changes in the formal enforcement mechanisms or fines: I exploit the phase-in structure of devolution reforms that removed the role of legal institutions (i.e. the Irrigation Department) in the management of surface water on a distributary. The results show that the communities that were facing long-term scarcity did not see any

[7]Too look at the role of local politicians in enforcing inter-village cooperation, I match villages with boundaries of constituencies of Provincial Assembly, and political association of Members of Provincial Assembly during the 2008-2013 and 2013-2018 election cycles. This allows me to include political party fixed effects and control for party-specific favouritism. I find no significant difference in the results

change in water theft due to reforms, suggesting that they had already resolved their collective action problem beforehand.

To ensure that the increase in cooperation is not driven by new job opportunities created by the industrial activity, I show that the villages that do not see an increase in groundwater pollution due to their proximity to rivers, but that are equally affected by job opportunities due to their proximity to factories, do not show any change in inter-village cooperation. I also rule out the possibility that the industrial activity affected surface water quality and decreased its demand. Furthermore, I do not find any evidence that the results are driven by a drop in land use in contaminated areas. The results are also robust to exclusion of villages that are very close to industrial activity.

To understand the implications of inter-village cooperation for economic activity, I match villages with two datasets that contain information on crop productivity and choice. First, I use satellite data (Net Primary Productivity), that has been used as a proxy for cropland productivity (Hicke *et al.*, 2004; Heinsch *et al.*, 2005; Strobl and Strobl, 2011), to show that the increased inter-village cooperation resulted in relatively lower productivity dispersion across villages on the same sub-channel, after controlling for the overall drop in productivity due to groundwater contamination. Second, I use the Agricultural Census of 2010 to show that on distributaries where inter-village cooperation is high, tail-end villages are more likely to choose cash crops. This correlation further underlines the importance of surface water availability for disadvantaged farmers.

This project falls at the intersection of two broad literatures. The first literature studies the role of institutions in the management of collective goods (Olson, 1965; Wade, 1989; Ostrom, 1990; Poteete *et al.*, 2010), and the relationship between social capital and contributions towards public goods (Alesina and La Ferrara, 2000; Bardhan, 2000; Dayton-Johnson, 2000; Khwaja, 2009). The second literature is relatively young and looks at the impact of historical and contemporary long-term environmental change on adaptation (Hornbeck and Keskin, 2014; Burke and Emerick, 2016; Taraz, 2017; Henderson *et al.*, 2017). Blakeslee *et al.* (2019) have looked at the impacts of long-term water scarcity in India and find that permanent water shortage negatively affects farm income, but has little impact on agricultural adaptation.[8] I contribute by providing causal evidence of both short-term and long-lasting resource scarcity on cooperation. A few studies have looked at the impact of historical land productivity (Litina, 2016) and economic

[8]Fishman *et al.* (2017) and Hornbeck (2012) also find that the long-term environmental change does not lead to much agricultural adaptation, but both studies find evidence of out-migration.

risk (Buggle and Durante, 2017) on cooperation, but the evidence of contemporaneous resource scarcity on cooperation, especially inter-group cooperation, is limited.[9] This is particularly challenging because the data on cooperation is not usually available to researchers, and exogenous variation in prolonged resource scarcity is rare. This paper makes use of panel data on cooperation and exploits exogenous variation in long-term resource scarcity, which makes it one of the first studies that provides causal evidence of resource exhaustion on the emergence of cooperation.

Since this study looks at an environment where third party enforcement is partly ineffective and social means are important for resolving conflict, it also contributes to the empirical literature on contracting failure in the developing world. This literature looks at the importance of relationships across firms in the absence of formal contract enforcement (McMillan and Woodruff, 1999; Macchiavello and Morjaria, 2015, 2019; Bubb *et al.*, 2018) and also the effectiveness of informal risk-sharing schemes in poor countries (Townsend, 1994; Udry, 1994; Fafchamps and Lund, 2003; Mobarak and Rosenzweig, 2012). A relatively small number of papers have also used lab-in-the-field experiments to look at the role of social networks in supporting cooperation in such an environment (Chandrasekhar *et al.*, 2018; Ligon and Schechter, 2012). This paper contributes to the literature by providing evidence on the effectiveness of social networks and informal institutions in supporting cooperation under a long-term environmental change.

Finally, this paper is also related to the literature that studies how the availability or management of irrigation water affects farm income, welfare, and agricultural adaptation (Jacoby *et al.*, 2004; Duflo and Pande, 2007; Sekhri, 2014; Gine and Jacoby, 2016; Fishman *et al.*, 2017; Blakeslee *et al.*, 2019). A recent paper (Fatima *et al.*, 2016) has looked at how the decentralization reform in Pakistan affected water thefts over a sub-canal. That paper finds that transfer of irrigation management responsibilities to farmers led to an increase in water theft, especially in channels where large landowners are present in the upstream villages. I contribute to this literature by providing evidence that prolonged resource scarcity can push farmers to adapt by investing in informal mechanisms that enforce cooperation.[10]

[9] A recent study (Nie and Yang, 2017) has also looked at effects of long-term water scarcity on cooperation. I further contribute to this relationship along three dimensions. First, rather than focusing on a cross-sectional relationship between cooperation and long-term scarcity, this paper looks at the emergence of cooperation over time in a panel setting. Second, the focus of this paper is on inter-group cooperation rather than within group cooperation. Lastly, instead of using reported measures, this paper make use of actual incidents of water theft.

[10] This project is also connected to the literature studying community-driven development and participatory programs. Most of the studies in this literature evaluate programs that are

In the next section, I present background information on surface water irrigation in Pakistan and describe the data sources. In Section 1.3, I present the empirical framework and identification strategy. Section 1.4 provides results and discusses possible mechanisms. The discussion on crop choice and productivity is in Section 1.5, followed by the conclusion.

## 1.2 Context and Data

Agriculture employs 45% of the total labour force in Pakistan. Apart from being the primary source of livelihood for many households, it is also a major source of raw materials for the manufacturing sector. There are two main seasons for crop production: *Kharif* and *Rabbi*. The cultivation of water intensive cash crops – primarily cotton, rice and sugarcane – takes place in *Kharif* season which generally lasts from May to November. The period of Monsoon rains overlap with this season. In contrast, *Rabbi* is generally dry. During both seasons, farmers use a combination of the following three sources of water for crop production: surface, ground, and rainfall. The focus of this paper is to evaluate how cooperation over surface water use is affected by the availability of groundwater and rainfall. In the rest of this section I provide background information on surface water irrigation in Pakistan, describe types of water theft, and discuss the factors that affect the availability of water. Part of the section relies on the information I collected through the community survey conducted in 2018. At the end, I describe the key datasets used in the empirical section.

### 1.2.1 Surface Water Irrigation in Pakistan

The Indus Basin Irrigation System accounts for 80% of the total agricultural production in the country. The river water feeds into main canals and then into branch canals, distributaries, minors, and finally into over 107,000 farmers' managed watercourses. Figure A5 is an illustration of a typical surface water irrigation system in Pakistan.

designed to increase community involvement in development projects. Overall, these studies have found that such programs do show positive impacts on self-reported pro-social values (Labonne and Chase, 2011; Avdeenko and Gilligan, 2015), but actual behaviour is not affected (Casey *et al.*, 2012; Humphreys *et al.*, 2012). A recent study (Fearon *et al.*, 2015) has suggested that this might be due to the unwillingness of communities to adopt these institutions. In this project, rather than looking at the imposed institutions, I study the effectiveness of self-evolved institutions in increasing cooperation across villages. Apart from that, exploiting the timing of devolution, I provide evidence on whether communities that have accumulated some social capital through self-evolved institutions are better able to adopt participatory programs.

Since independence in 1947, the overall irrigation network has been controlled by the Irrigation Department, and at all levels water is allocated according to the weekly rotational system called *warabandi*. Farmers have fixed weekly turns that are proportional to their landholdings. However, farmers can set up their own plan if they can reach a mutual agreement. This centralized system started facing operational and fiscal constraints in the 1980s (World Bank, 1994). A number of studies have linked inefficient water delivery to illegal diversion at the head-end (water theft), elite capture, and corruption (World Bank, 1994; Bandaragoda and Saeed, 1995; Azam and Rinaud, 2000; World Bank, 2002; Anwar and Ul Haq, 2013; Rinaudo, 2002; Rinaud *et al.*, 1997).

These concerns increased pressure on the government for decentralization, and eventually resulted in transfer of responsibilities from the Irrigation Department to user groups in a three tier participatory organizational structure. Under the reforms, second tier organizations (Farmer Organizations) took charge of distributary-level responsibilities, including management of water taxes, maintenance of a distributary, and monitoring and reporting of illegal diversion or water theft. Fatima *et al.* (2016) used the phase-in structure of the reforms and found that water theft increased after farmers took over the responsibilities. This result further highlights the need to study why some communities are able to establish cooperation while others, not, and which channel do successful communities use to enforce cooperation. Since reforms increase the salience of informal mechanisms, I will also exploit this exogenous change to study whether villages facing long-term scarcity prior to the reforms, exhibit different outcomes under the reforms compared to the rest.

### 1.2.2 Water Thefts

Each village on a distributary is entitled to receive water in proportion to their command area. To ensure that farmers do not over-extract, outlets are designed to only deliver the allocated amounts to each village.[11] Any further extraction of water from the distributary is illegal and considered theft. Figure A1 is an illustration of how outlets are usually placed on a distributary. In this figure, each box represents a village that receives its allocated amount through an outlet. Farmers within a village then share that water according to village-specific rules. Farmers within a village could face similar problems that villages are facing over a distributary. However, farmers rarely steal from other farmers in the same

[11] If the overall supply of water decreases, outlets are designed such that the water supply to each village decrease proportionally.

village, as communities are generally small and cheating is relatively easy to observe. This is also evident in the community survey, where more than 70% of the villages did not experience any water theft within a village in the last *Kharif* season.[12] In contrast, it is much harder to enforce cooperation across villages within a distributary, as social capital across villages is generally not as high.

There are three ways to over-extract surface water from a distributary. First, farmers can illegally alter the dimensions of an outlet. In most cases, they break the concrete structure to create a larger area for water to enter (Azam and Rinaud, 2000; Mustafa, 2001). Second, they can create a new temporary outlet: this is not always a viable option if a distributary is lined. Lastly, they can insert a siphon pipe directly into a distributary. In this case, only the farmers that are adjacent to a distributary are likely to benefit from additional water. The data on water theft incidents shows that in 60% of cases farmers changed the dimensions of an outlet to extract more water.

The over-extraction reduces the water supply at the end of a distributary. This change is observable to irrigation officials and farmers from other villages. To avoid detection, farmers usually over-extract during the night. However, any changes made to the size of an outlet is likely to last until the outlet is restored to its original size by the irrigation officials (Azam and Rinaud, 2000; Rinaudo *et al.*, 2000; Fatima *et al.*, 2016). In interviews, farmers and irrigation officials said that in most of the cases outlets are restored within 2-3 days, but in some cases the changes take up to a week. In some distributaries where villages were actively involved in monitoring the distributary, it was also possible to stop the farmers before they could make any changes to an outlet. Generally, downstream villages can only minimize the losses by pushing irrigation officials to restore the outlet quickly.

### 1.2.3 Cost of Stealing

The farmers involved in water theft face constraints from the Irrigation Department (legal fines) and farmers downstream (social fines). When the Irrigation Department receives information on water theft, it sends an official to gather further information about who might have been involved in the theft. Before penalizing farmers, irrigation officials investigate and calculate the fine for each farmer. If farmers fail to pay the fines then the Irrigation Department can refer the matter to the police. However, this process has been ineffective in constrain-

[12] Nearly all the water-intensive cash crops are grown during *Kharif* season that generally lasts from May to November.

ing farmers for two reasons. First, the penalty is not sufficiently high. The Irrigation Department still follows the old colonial law, which limits the fines (*tawan*) to no more than 20 times the amount of water taxes (*abiana*) paid by the farmer. Since the surface water tax is minimal, farmers usually do not have to pay a large amount for a single incident (Mustafa, 2001). Second, many studies have pointed out that farmers are able to avoid penalties by bribing the irrigation officials (World Bank, 1994; Bandaragoda and Saeed, 1995; Azam and Rinaud, 2000; World Bank, 2002; Anwar and Ul Haq, 2013; Rinaudo, 2002; Rinaud *et al.*, 1997). The ineffectiveness of legal fines is evident in the community survey where only 20% of the farmers found "reporting to the Irrigation Department" to be useful.

Apart from legal fines, the pressure from downstream villages might also restrict upstream villagers from over-extracting. As mentioned earlier, active monitoring is effective in reducing losses. Farmers can also meet directly with other villagers to settle the disputes. In the community survey, most of the farmers mentioned that they raise concerns with the elders in the other village who are of the same caste. Thus, the social capital or caste linkages across head and tail villages are important in enforcing cooperation. However, actively monitoring a distributary or resolving the collective action problem could be costly for downstream farmers. If the cost is too high the farmers might consider relying mostly on substitutes for surface water.

### 1.2.4 Groundwater quality

There are two alternative sources of water: rainfall and groundwater. On a typical farm, during *Kharif* season, groundwater supplies nearly 40% of the total crop water requirement (Qureshi *et al.*, 2004).[13] Groundwater is much more expensive to extract. On average, the cost of groundwater is 30 times the cost of surface water (Qureshi *et al.*, 2010). The substantial price difference between groundwater and surface water is another incentive for over-extracting canal water.

Apart from price, the availability of groundwater can also affect farmers' behaviour towards cheating. The demand for groundwater is a function of water level and quality. The height of water table determines whether farmers can extract the water through typical water pumps. Quality is measured in terms of salt content, and when the water is very saline it has to be diluted with water from other sources (i.e. surface or rain water). In extreme cases, the groundwa-

[13] Rainfall contribute less than 15% towards total crop water requirement (Qureshi *et al.*, 2004)

ter might not be useful at all. Water quality affects crop yield, as a result when groundwater quality is low, demand for surface water increases and so does the propensity to steal (Jurriens *et al.*, 1996; Basharat, 2019).

The groundwater level changes over the year depending on rainfall. Since changes in groundwater quality also depend on the water table, a sufficiently large change in rainfall during a year can temporarily affect groundwater quality. However, a sudden and indefinite change in groundwater quality is rare. There are two aspects of water quality that are important for the empirical analysis. First, farmers generally know the groundwater quality. Figure A3 shows that self-reported water quality aligns well with laboratory reports. My focus group discussions revealed that the farmers usually learn about quality by either noticing the change in soil after applying the water or by tasting it. Second, they do not believe that a change in groundwater quality affects surface water quality. Figure A4 shows that there is no relationship between self reported surface water quality and groundwater quality taken from laboratory reports.

### 1.2.5 Data

The Punjab accounts for more than 60% of the total cultivated area in Pakistan and is divided into 17 canal circles, of which 5 went through phased-in reforms during 2008-2016. I select two circles out of these five: Lower Chenab Canal West and East. The main reason for restricting the sample to just two circles is the high cost of collecting and digitizing water theft registers from the field offices of the Irrigation Department and access to administrative data on water discharge. These two circles cover nearly 400 distributaries and 2,100 villages across eight districts in central Punjab.

I use two data sources to look at water theft. Most the of the analysis will be based on distributary-level water discharge readings. This data is taken from gauges installed on both head and tail ends of each distributary and minor.[14] The irrigation officials collect these readings every day, and forward them to the Program Monitoring and Implementation Unit (PMIU). This information is well maintained, and is regularly cross-checked. The readings are taken in the first and last village from all distributaries and minors.[15] Figure A6 plots raw day-level data from 2015 for two distributaries. For each figure, we have Authorized Tail Discharge — it stays constant throughout the year and is never zero —

[14]Minor distributary usually offtake from a major distributary and have a relatively lower amount of discharge

[15]Figure A1 shows a sketch of network of villages on a distributary.

and Actual Tail Discharge. The figure at the top is for Dhaular Distributary where the water availability at the tail-end was very volatile in 2015, and it is in contrast to the figure at the bottom from Aminpur Distributary where tail nearly always received at least the authorized amount of water. If the actual reading is below the horizontal line, which indicates allocated amount, then the difference between two lines provides a measure of water leakages on a distributary over time. This variation allows me to construct a proxy for inter-village cooperation that I discuss in more detail in section 1.3.1.

To complement this data, I collect and digitize outlet-level reported water theft cases from part of the area under study. These cases are observed by field teams from the Irrigation Department on a regular basis and include three kinds of thefts: enlargement or breach of outlets, illegal construction of outlets, and using pipes for over-extraction. This data could have two biases. First, it is possible that due to rent-seeking some officials might not report a case. However, given that most of these cases do not lead to a punishment, there is little incentive to bribe field teams. Second, this data only shows cases that have been approved by the administration after inquiry and thus any systematic delays in approval could induce processing bias.[16] Figure A7 plots average theft incidents over the whole sampled time period. It shows that most of the theft took place during the *Kharif* period when mostly cash crops are grown.[17]

Since my paper looks at the impact of effective availability of alternative water sources on inter-village cooperation over surface water management, I also collect data on the other two sources. Groundwater is the second biggest source of irrigation in central Punjab. The data on groundwater quality and levels is collected by the Directorate of Land Reclamation Punjab, twice each year, before the start of each season. The water samples are collected from gauges installed on privately owned wells sampled from each 6 x 6 km grid. They provide three measures of groundwater quality for irrigation: Electrical Conductivity (EC), Sodium Adsorption Ratio (SAR), and Residual Sodium Carbonate (RSC). I will discuss more about these measures in Section 1.3.3.

The data on rainfall and temperature is taken from publicly available sources. The data on rainfall comes from Tropical Rainfall Measuring Mission (TRMM) readings. The product used in this paper is gridded at 0.25 x 0.25 degrees, and provides total daily precipitation for the 2000-present time period. The data on

[16] In my discussion with Irrigation Department I learned that nearly 90% of the cases from this sampled period have been processed

[17] There are two cropping seasons; Dry (*Rabbi*) November-April and Rainy (*Kharif*) May-October.

mean near-surface temperature is taken from the Climate Research Unit, which provides monthly averages on a 0.5 x 0.5 degrees grid for the 1901-2015 time period.

To match the water discharge data with the rest of the information, I collected data on outlet and village listings from the Programme Monitoring and Implementation Unit (PMIU). With this data I could map each village on a distributary. Then I geo-coded each village so that each distributary can be linked to the geo-coded groundwater and satellite data. For most of the analysis, to aggregate the groundwater and satellite data at a distributary level I simply take the average over all the villages.[18]

Finally, I use both primary and secondary data to look at the mechanisms farmers use to enforce cooperation. In summer 2018, I conducted a community survey covering 644 villages from 278 distributaries. The details of respondent selection and sampling are in Appendix C. The survey questions include channels farmers use to resolve water theft disputes, farmers' perceptions of surface and groundwater quality, and castes in the village. I also use secondary data (Mouzza Census 2008) collected by the census organization in Pakistan to look at village-level institutions. This census covers all the villages in Pakistan and provides information on basic demographics including the presence of informal institutions, such as Jirga, Panchayat, or village council.

The data on crop productivity, choice and land use comes from several sources. First, the Agricultural Census 2010 provides information on land use and crop choice.[19] Using the village names, I was able to match nearly 36,800 plots from the Agricultural Census with rest of the data. Second, a satellite measure (Net Primary Productivity) provides a proxy for corpland productivity at week-level at a 0.1 x 0.1 grid cell-level from 2008-2015. I further discuss this data in Section 1.5. Third, I use Global Land Cover data that allow me to see how land use changes over time from 2008 to 2012. This data comes from Terra and Aqua combined Moderate Resolution Imaging Spectroradiometer (MODIS), and is available from 2001-2016 at pixel size of 500 meters. Lastly, to look at the temporal changes in crop choice, I use district-level data from from 2007-2016, from the Directorate of Agriculture, Punjab. This data has crop-wise information on total land area under cultivation and total production.

There are multiple household surveys that record information on migration. However, since these surveys anonymize the village names, it is not possible to look

[18] In part of the estimations, I also change the aggregation method to explore the differences across head and tail of a distributary. Any such change in approach will be explicitly mentioned.

[19] The Agricultural Census does not cover all the plots, but has a fairly large sample size

at the changes in total population at a distirbutary level using these secondary datasets. Instead, I use the WorldPop spatial dataste that employs land cover-based methods to estimate the total population at a very fine pixel size of 100 meters (0.00083 decimal degrees). One key limitation of of this dataset is that for Pakistan it is only available for 2010 and 2015. Therefore, I can only capture the changes in population over these two years.

To reduce the noise, I aggregate all the variables at a distributary-week or village-week level.[20] The summary statistics are in Table 1.1. In the first panel, I have distributary-week level data on water discharge. The authorized water discharge stays constant throughout this sample period. It is important to note that the actual discharge both at the head and tail varies from the allocated amount. This variation makes it possible to observe the aggregate leakages on a distributary. This will be further discussed in section 1.3.1. In the third panel, I present the summary statistics on distributary-village-week level water theft data. The mean theft count of 0.061 indicates that a large number of observations are zero for this variable. This is partly because most of the over-extraction usually takes place during the mid-season (see Figure A7), and hence there are many weeks where the theft is either very low or zero. Another important thing to note in the panel is that the same village could be connected to multiple distributaries. Therefore, I use the distributary-village-week as the level of analysis for all the estimations requiring theft data. The second panel presents summary statistics from satellite data on precipitation and mean temperature. The precipitation data has already been converted to rainy days and given in terms of the difference from its long-term mean. Finally, the last panel has summary statistics on the measure of groundwater pollution. There are two reasons why the number of observations is smaller than in the other panels. First, these measures are only collected before the start of each season and since I am only looking at the $Kharif$ season, this is essentially yearly data. Second, the data from 2014 onwards has not been processed by the Directorate of Land Reclamation and therefore it is not available for analysis.

## 1.3 Empirical Framework

### 1.3.1 Water Theft and Inter-village Cooperation

Since water theft restricts tail-end farmers from extracting their allocated share of surface water, it provides a measure of cheating across head- and tail-end villages.

[20] In appendix, Table A4, I show that the main results remain the same whether the data is collapsed at the distributary-village-week level or village-week level.

Table 1.1: Summary Statistics

| | (1) N | (2) Mean | (3) SD | (4) Min | (5) Max |
|---|---|---|---|---|---|
| *Surface water (distributary-week level)* | | | | | |
| Water discharge at head | 77,616 | 378.4 | 2,322 | 0 | 46,200 |
| Water discharge at at tail | 77,616 | 113.8 | 1,309 | 0 | 28,819 |
| Raito of actual to authorized | 77,616 | 0.723 | 0.421 | 0 | 3.908 |
| *Satellite data (distributary-week level)* | | | | | |
| Rainy day below weekly average | 77,616 | -0.0362 | 1.098 | -5.067 | 2.200 |
| Mean Temperature | 77,616 | 32.05 | 1.878 | 25.12 | 36.40 |
| *Reported theft (distributary-village-week level)* | | | | | |
| Theft count | 61,600 | 0.0618 | 0.399 | 0 | 6 |
| *Groundwater –(distributary-year level)* | | | | | |
| Sodium Adsorption ratio | 2,322 | 9.49 | 7.14 | 0.20 | 31.58 |
| Electrical Conductivity | 2,322 | 1.98 | 1.03 | 0.20 | 4.60 |
| Years | | | | | 2008-2016 |
| Weeks | Kharif Season – (1st May - 30th September) | | | | |
| Distributaries | | | | | 392 |
| Villages | | | | | 2,124 |

Notes: All the variables in the first panel are winsozried. The data on rainfall and temperature is matched with geocodes of villages. Apart from the groundwater and temperature data, all the other variables were aggregated to week level from day level observations. The groundwater data is collected twice during the year at the start of each season. Since the paper is focusing on *Kharif* season, the groundwater data is essentially at distributary-year level. The distributary level groundwater measures were constructed by matching the location of each village to the nearest well and then taking the average of quality readings over all the villages in a distributary. The groundwater data is also only available until 2013. The number of observations for "Reported Theft" data only cover 116 out of the 392 commons. Since the "Theft count" is aggregated over the entire village, it sums up the theft over all the outlets in a village.

In sharing joint resources, an increase in cheating results from lower cooperation. Accordingly, in the empirical analysis I interpret an increase in water theft on the same distributary as a decrease in cooperation across villages. I complement this data with administrative water readings in order to measure inter-village cooperation. As mentioned earlier, the discharge data at the head and tail of a distributary, can be used to calculate aggregate water leakages. This data reports the availability of water at the head and tail of a distributary along with their allocations. One simple measure of leakages would be a difference between head and tail discharge. However, any change in this measure would make it difficult to tell whether the effect is driven by increased overall water availability (higher head discharge) or whether farmers have improved the water delivery to the tail-ends (higher tail discharge). Therefore, I follow Fatima *et al.* (2016) and look at the ratio of tail discharge to its authorized amount:

$$D_{dt} = \frac{TailDischarge_{dt}}{TailAuthorized_{d}} \tag{1.1}$$

To account for overall water availability on a distributary, I also control for head discharge in the empirical analysis. A higher value of $D_{dt}$ would indicate that there is more water available for the tail-end village. This could be achieved through control over illegal diversions or water theft. Since this requires collective action both with-in the tail villages and across head and tail, I interpret $D_{dt}$ as a proxy for inter-village cooperation.

Tables A1 and A2 provide some correlates of village and distributary-level water theft. As expected, the first column shows that villages at the head-end are more likely to steal water. The next column shows that market linkages are also correlated with the number of water theft incidents. At the distributary level, the size of a distributary, the presence of markets, and the position of a distributary are all important attributes for the overall water availability at the tail end of a distributary.

### 1.3.2 Short-term Scarcity

In this section, I combine water theft and precipitation data to look at the potential impact of negative rainfall shocks on the probability of surface water theft. Furthermore, water discharge data complements this analysis by allowing me to observe how water availability at the tail-end changes when villages experiences such shocks. The distributary-village-week level data on water thefts allow me to estimate:

$$Theft_{vdt} = \beta_1 ScarcityShock_{vdt} + x^{'}_{vdt}\lambda + \delta_v + \psi_t + \upsilon_{vdt} \tag{1.2}$$

$Theft_{vdt}$ is a count of incidents of illegal diversion by village $v$ on distributary $d$ in week $t$. To construct a measure of unexpected and short-term scarcity ($ScarcityShock_{dt}$), I subtract the number of rainy days in a week from the long-term mean. I take an average of rainy days for each distributary-week pair from 2000-2016 to construct the long-term mean. This measure captures not just the availability of rainwater but also its distribution over a week, which is an important factor affecting crop yield (Fishman, 2011). To make sure that results are not driven by this measure, I also report estimates using volume-based rainfall shocks (Jayachandran, 2006; Shah and Steinberg, 2017; Kaur, 2014). The term $x_{dt}$ includes the following controls: temperature, groundwater level, monthly variation in rainfall, discharge at the head-end of a distributary, a dummy to capture reforms, and a dummy to account for areas that were hit by 2010 floods. Finally, I have village-distributary ($\delta_d v$) and week fixed effects ($\psi_t$). The random variation in rainfall along with fixed effects allows me to identify the effect of unexpected and short-term scarcity that is given by the coefficient $\beta_1$. Given a negative rainfall shock increases the incentives to cheat, I expect the $\beta_1$ to be positive and statistically significant. Since $Theft$ is a count variable with a large number of zeros, I use negative binomial regression to estimate the coefficient and use conditional fixed effects to control for village-specific time invariant unobserved characteristics. I cluster standard errors by distributary and in the robustness sections also report standard errors with two-way and spatial clustering. For the distributary-level discharge data, I estimate:

$$D_{dt} = \alpha_1 ScarcityShock_{dt} + \alpha_2 HDischarge_{dt} + x^{'}_{dt}\lambda + \delta_d + \psi_t + \epsilon_{dt} \tag{1.3}$$

$D_{dt}$ is ratio of actual to allocated tail discharge, as described in section 1.3.1. To account for overall availability of surface water in a distributary, I also control for the head discharge ($HDischarge_{dt}$) in this specification.[21] Since most of the cash and water-intensive crops are grown in the *Kharif* season, I restrict the sample to May-September. Apart from that, I also exclude those distributaries that

[21]The distributary fixed effects pose a risk of running into temporal dependency. I deal with this problem by including the lag and leads of the key independent variable, as well as estimating equation 1.3 with the inclusion of a lagged dependent variable.

are very small and have only one village.[22] Finally, I winsorize the 0.1% of the top-tail of the discharge data to mitigate the influence of extreme observations.[23]

### 1.3.3 Long-term Scarcity

In this section, to identify the effect of long-term scarcity on inter-village cooperation, I exploit a quasi-experiment that exogenously changes the quality of groundwater after 2009, in response to increase in industrial activity. Since farmers fulfil nearly half of their total crop requirement from groundwater and its effective availability depends both on the water level and quality (Qureshi *et al.*, 2010), any significant change in quality would affect total effective water availability in a season. In this section, I first show the spread and potential source of changes in groundwater quality. Then, I discuss the empirical strategy employed in estimating effects of contamination on cooperation.

The data on groundwater quality is available at the well level, and each well was sampled from a 6 x 6 km grid. As mentioned earlier, there are three measures: Electrical Conductivity (EC), Sodium Adsorption Ratio (SAR), and Residual Sodium Carbonate (RSC). An increase in conductivity indicates a higher content of salt in the water, which is likely to prevent growth of most crops. The other two measures capture the concentration of sodium relative to calcium and magnesium. A higher SAR makes it difficult for water to infiltrate freely through the soil, and will affect crop yield. For most of the paper, I use SAR as the main measure of water quality and conduct robustness checks with EC.

I use the variation in water quality over time to define the control and treated distributaries. To look at the distributary-level changes in water quality, I first match the well-level data with the geographic coordinates of each village and then take a simple average of all the villages on a distributary.[24] Figures 1.1 and 1.2 plot distributary level SAR values for the pre and post treatment time periods (before and after 2009). I have defined three categories; low (0-10), medium (10-20), and high (20 or more). There are three points to note here. First, nearly all the dots close to two rivers show very low levels of pollution. We should expect this given that such areas are likely to be recharged by good quality river water. In my analysis, these are the places that always have good quality

[22]This is solely due to the irrelevance of inter-village interaction in those distributaries. However, the results are not sensitive to this exclusion.

[23]I also winsorize the pollution data from the top-tail and in the appendix show that symmetric winsorization does not change the results.

[24]Later, I will also change the aggregation method to study whether the effect is mostly driven by head or tail-end village

groundwater available.[25] The second point to note is that the area in the middle (Faisalabad District) shows higher pollution. This is to be expected given that it is an industrial hub and has a large number of textile factories (Yamin *et al.*, 2015). This part of the sample will always be treated. Finally, comparing pre and post treatment figures we see a number of places outside Faisalabad and further away from the two rivers, especially downstream, becoming contaminated. These are the places that see a significant change in their water quality after 2009.

These observations in the groundwater quality indicate that there are three groups: pure control, always treat, and treat after 2009. To further examine changes in water quality over time, I define distance cut-offs for the pure control group (areas close to rivers) and the always treated group (areas inside the treatment district) and plot these along with the treatment group in Figure 1.3.[26] This graph shows the stability in the SAR of the control groups, while the SAR of the treatment group jumps by almost half a standard deviation after 2009. In Figure A8, I plot the treatment group trend along with entry into new industries in the Faisalabad district. A large number of firms joined the market in 2008 and 2009, and this trend aligns well with the increase in groundwater contamination.[27] Since water quality is important for irrigation, this variation in SAR that comes from industrial effluents provides me with an exogenous change in effective groundwater availability. Finally, I interpret this change as long-lasting since the groundwater quality is unlikely to improve significantly unless there is flooding and/or effluents are treated.

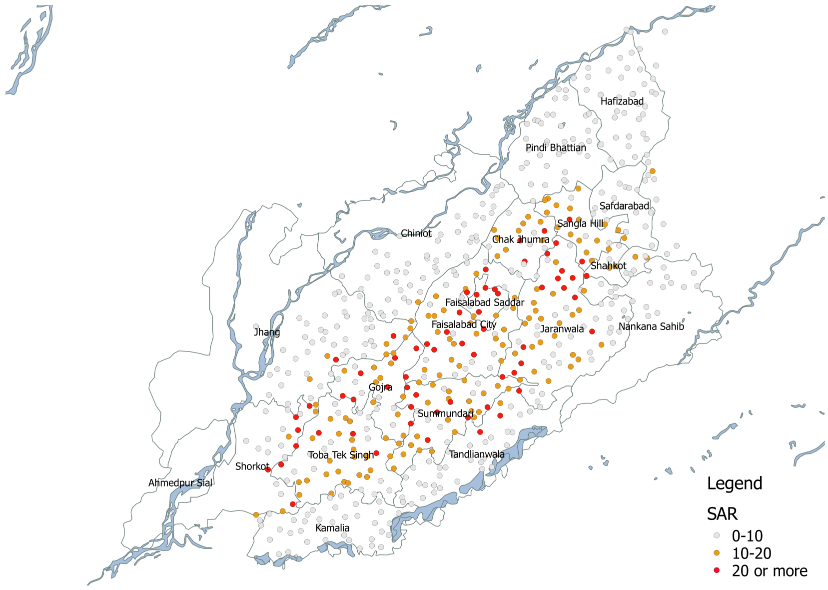
Eisena and Anderson (1979) show that contamination of groundwater could take place through the infiltration of wastewater drains or leakages from sewer lines. In the case of Pakistan, this problem intensifies as most of the wastewater is not treated, and drains are open and unlined (World Bank, 2018). Studies that have analyzed the effluents from the textile industry found that contaminants decrease the concentration of dissolved calcium which increases the Sodium Adsorption Ratio (Sellamuthu *et al.*, 2011; Subrahmanyam and Yadaiah, 2001; Kahlown

[25]The distance to river also explains most of the variation in terms of levels in any given year in my sample.

[26]The distance cut-offs help me in graphing the changes in water quality across treatment and control group. However, in the main analysis, I will use the continuous measure of distance. Apart from that, I will provide sensitivity analysis on cut-off values to show that these are not driving the results.

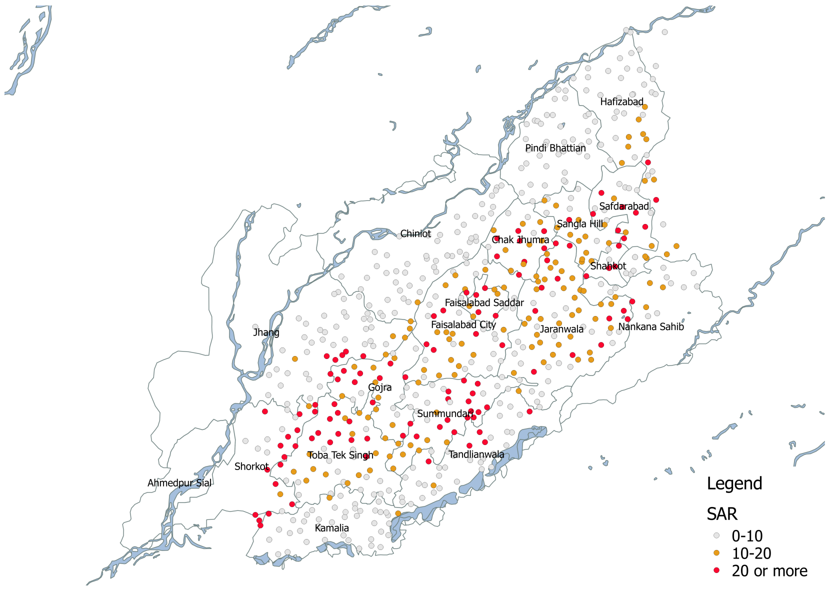
[27]This trend in firm entry could partly be due to an increase in international market share after European Union reduced the anti-dumping duty on Pakistani bed linen (Ghori, 2012) and also partly due to the construction of new industrial estates by Faisalabad Industrial Estate Development & Management Company. Furthermore, there was a large increase in the demand of cotton yarn due to global shortage in the availability of cotton (Pakistan Economic Survey, 2009).

Figure 1.1: Sodium Adsorption Ratio – Pre Treatment (2008)



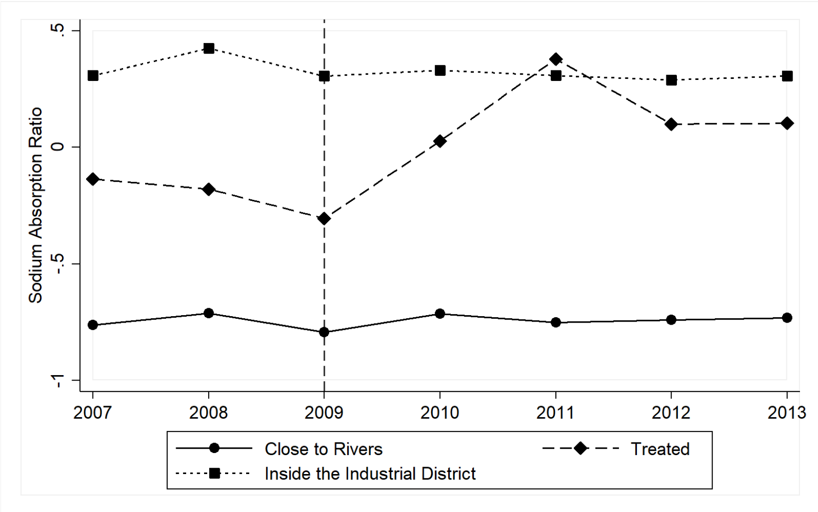

Notes:This figure plots the distributary level mean of Sodium Adsorption Ratio from 2008. There are two patterns to note. First, the areas closer to two rivers show very low amount of SAR (good quality groundwater for irrigation). Second, the distributaries situated closer to Fasilabad have relatively higher quantity of Sodium.

Figure 1.2: Sodium Adsorption Ratio – Post Treatment (2011)




This figure plots the distributary level mean of Sodium Adsorption Ratio from. As compared to 2008, there are many more distributaries in the regions below Faislabad Sadar that have relatively polluted groundwater.

Figure 1.3: Changes in Ground Water Quality




The graph plots distributary-level means of SAR (pollution) for distributaries that are close to rivers, inside the industrial district, and treated areas. A distributary is considered to be "close to rivers" if it is with-in a 10km radius of either Chenab or Ravi river. The distributaries that are with-in the 50km radius of the centre of Faisalabad city considered as "Indside the Industrial district". The rest of the distributaries are part of treatment group. The trends are very similar before 2009 and then there is a sharp increase in pollution in the treatment group. The SAR data comes from the lab reports prepare by the Directorate of Land Reclamation Punjab.

*et al.*, 2006). Ghafoor *et al.* (1994) has looked at the areas close to two primary drains in Faisalabad District, which also pass through several villages in my sample, and found that the Sodium Adsorption Ratio was higher than the recommended level. To ensure that industrial effluents are indeed affecting the groundwater pollution in the treated areas, I digitize maps of key drains in the sample and then look at the changes in pollution in Table A3.[28] The results show that areas closer to drains experienced an increase in pollution after 2009. The table also points out that areas downstream from the industrial districts are the worst affected ones, especially those that are further away from the rivers and outside the industrial area.

The changes in groundwater quality directly affect demand for surface water. In the areas where groundwater has become completely unfit for irrigation, farmers have to rely on surface water. In the areas where quality has partially deteriorated, the mixing of canal and surface water is recommended (Qureshi *et al.*, 2010). Therefore, I have a quasi-random variation in long-lasting water scarcity

[28]The two drains that initially digitize were Maduhana and Pharang. The digitization was based on the maps received from the Irrigation Department during the field work in 2018. Later, I was also able to get georeferenced maps for number of other drains from Irrigation Research Institute.

that increases the demand for surface water. This, in turn, increases the incentive to over-extract for head-end farmers who are already getting the maximum allocated amount. On the other hand, the tail-end farmers have greater incentive to invest in mechanisms to control water theft. Since water quality measures are available at the season level, and the focus is on the $kharif$ season, I aggregate all the variables at the distributary-year level and estimate:

$$D_{dt} = \beta_1 Pollution_{dt} + \beta_2 HDischarge_{dt} + x_{dt}^{'}\lambda + \delta_d + \psi_t + \upsilon_{dt} \tag{1.4}$$

$D_{dt}$ and $HDischarge$ are the same as in equation 1.3 in last section.[29] $Pollution$ is a continuous measure of SAR.[30] The other controls – average rainfall, inter-year variation in rainfall, average temperature, a dummy variable capturing devolution reforms, a dummy variable capturing the 2010 floods, and a constant term – is included in $x_{dt}$. Finally, there are also time ($\psi_t$) and distributary fixed effects ($\delta_d$) that capture both distributary-specific time invariant characteristics and aggregate shocks in each year.[31] Therefore, we essentially have a difference-in-differences equation with $\beta_1$ capturing the effect of contamination on inter-village cooperation. Since long-term scarcity could also push farmers to invest in mechanisms that can enforce cooperation, I expect $\beta_1$ to be positive and statistically significant from zero. However, as mentioned earlier, the groundwater quality is endogenous to farmers' choice of pesticides and the rate of groundwater extraction. This implies that areas that are already less-cooperative are more likely to pollute their groundwater and therefore we should expect a downward bias in $\beta_1$. To deal with this issue I instrument $Pollution_{dt}$ with distance from the industrial hub interacted with a dummy for post the 2009 time period. In the first stage, I estimate:

$$Pollution_{dt} = \gamma_1 Treat_d \times post_t + \gamma_2 HDischarge_{dt} + X_{dt}^{'}\lambda + \delta_d + \psi_t + \upsilon_{dt} \tag{1.5}$$

The patterns in Figures 1.1 and 1.2 suggest that the variable $Treat$ in equation 1.5 should capture the location of a distributary relative to both Faisalabad,

[29] Since water theft data is only given for part of the distributaries and years, it is not possible to look at the effect of this change in groundwater quality on actual water theft incidents.

[30] In the robustness checks, I will also use EC as another measure of groundwater quality. Apart from this, I abstained from using a categorical variable for quality, since the cut-off values are sensitive to crops and vary across areas. However, later in the robustness checks, I will define categories based on typical Kharif crop and estimate equation 1.4 using dummy variable.

[31] I also include a linear time trend for each of the two circles in the study area. However, the results do not change much if I instead include circle specific year fixed effects.

as well as to the rivers. To take this into account, I compute the distance of each distributary to Faisalabad City and the closest nearby river. Then, I define cut-off values to separate the distributaries that are both further away from a contamination source and rivers. To define the area outside the industrial city, I use the cut-off value of 50km. This value captures the average distance from the centre of the Faisalabad District to its south-end.[32] However, the areas that are closer to a river are not polluted by the industrial activity, and therefore, I exclude the distributaries that are within 10km of either of the two rivers. In short, $Treat$ is equal to 1 for distributaries that are 50km away from the centre of Faisalabad City and 10km away from rivers, and zero otherwise. To ensure that my results are not driven by the selection of cut-offs, I take the following steps. First, I also provide estimates using a continuous distance variable, that captures the location of a distributary from the city centre of Faisalabad. Second, I also present results of a specification that also takes into account distance to the drains that carry industrial effluents, and thus also use the areas that are outside the industrial region but are further away from the drains as a control group.[33] Third, I perform a sensitivity analysis to study how estimates change with changes in cut-off values. Since the industrial activity accelerated in 2008 and peaked in 2009, the *post* term is equal to 1 if the time period is after 2009. I cluster standard errors by distributary and the rest of the terms are the same as in equation 1.4.

## 1.4 Results

### 1.4.1 Short-term Scarcity

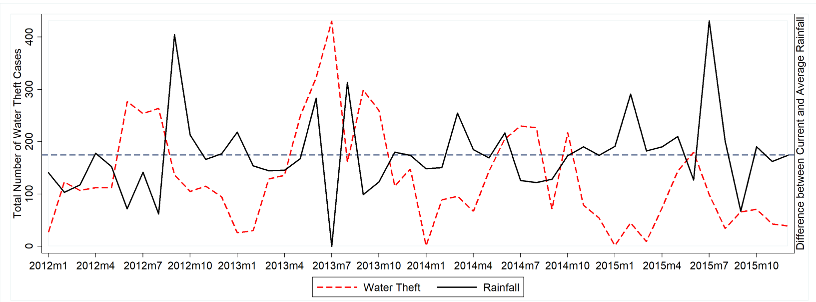
Before moving to the estimation of equation 1.2, I present graphical evidence of the relationship between water theft and rainfall shocks in Figure 1.4. The graph plots outlet-level water theft as reported in the books of irrigation officials and deviation of rainfall from its long-run mean. Many spikes in water theft align well with negative rainfall shocks. This suggests that short-term scarcity provides sufficient incentives to farmers to cheat and over-extract the surface water. I present results from the estimation of equation 1.2 in Table 1.2. The first column does not include any controls, the next column controls for week and year fixed

[32] To ensure that the Faisalabad City is not deriving the results, I conduct robustness checks by excluding it from the sample.

[33] A map of network of drains in the study area is given in Figure A9. This information allows me to use a) the distributaries that are downstream but are further away from a drain and b) areas that are further upstream from Faisalabad districts as an additional control. However, given that not all the drains are present in this data, some treated areas are likely to be misclassified as control.

effects, followed by a column that captures both week-year and conditional fixed effects. I repeat the same specifications in the last three columns after including additional controls. Finally, Panel B uses an alternative measure of rainfall shock (Jayachandran, 2006; Shah and Steinberg, 2017; Kaur, 2014), and Panel C looks at the extensive margin effects.[34] The coefficients in the first two panels were estimated using a negative binomial model. All specifications consistently show statistically significant and positive coefficients on the measure of unexpected and short-term scarcity. Overall, I find that one fewer rainy day than the weekly average increases water theft by nearly 14%. Table A5 reports estimates from the distributary-level data. The first two columns do not include any controls apart from distributary and week fixed effects, the next two columns control for additional variables, and the last two columns also include lags of rainfall shocks. Columns 2, 5, and 7 also control for distributary-specific linear time trends. The estimates show the same pattern as Table 1.2; a negative rainfall shock leads to a decrease in the availability of surface water at the tail relative to the allocated.

Figure 1.4: Water Theft and Rainfall Shocks




The graph plots water theft cases and the deviation of rainfall from its long-term mean. Both series have been aggregated at the monthly level. The water theft data comes form the logbooks of the Irrigation Officials and shows the number of time an outlet has been breached or a new outlet has been created on a distributary to over-extract the surface water. The rainfall data comes from Tropical Rainfall Measuring Mission (TRMM) readings.

### Alternate Explanations and Robustness Checks

The results in Table A5 could possibly be mechanical, as rainfall could affect water availability in the canals by directly changing the river levels. However, since the results do not change after controlling for head discharge and the results using reported theft data (Table 1.2) show a similar picture, the estimates do not seem to be driven by changes in the total available surface water. Alternatively, results could be driven by the time periods when rainfall is very high

[34]For estimating the extensive margin effects, rather than using the count of thefts as a dependent variable, I define a dummy variable that is equals to 1 for any number of thefts.

Table 1.2: Unexpected and Short-term Scarcity — Water Theft Data

| | (1) | (2) | (3) | (4) | (4) | (4) |
|---|---|---|---|---|---|---|
| | theft | | | | | |
| **Panel A: Negative Binomial** | | | | | | |
| Rainy days less than average | 0.134*** | 0.0904** | 0.122** | 0.146*** | 0.122*** | 0.138** |
| | (0.0291) | (0.0406) | (0.0518) | (0.0325) | (0.0372) | (0.0573) |
| Observations | 61600 | 61600 | 26752 | 52008 | 52008 | 23034 |
| Dependent Variable mean | 0.0660 | 0.0660 | 0.0660 | 0.0710 | 0.0710 | 0.0710 |
| **Panel B: Alternative Measure** | | | | | | |
| Negative Rainfall Shock | 0.131*** | 0.178*** | 0.171*** | 0.185*** | 0.223*** | 0.199*** |
| | (0.0480) | (0.0655) | (0.0542) | (0.0492) | (0.0632) | (0.0727) |
| Observations | 61600 | 61600 | 26752 | 52008 | 52008 | 23034 |
| Dependent Variable mean | 0.0660 | 0.0660 | 0.0660 | 0.0710 | 0.0710 | 0.0710 |
| **Panel C: Extensive Margin** | | | | | | |
| Negative Rainfall Shock | 0.00437*** | 0.00618*** | 0.00490** | 0.00635*** | 0.00749*** | 0.00532** |
| | (0.00153) | (0.00191) | (0.00224) | (0.00179) | (0.00208) | (0.00259) |
| Observations | 61600 | 61600 | 61600 | 52008 | 52008 | 52008 |
| Dependent Variable mean | 0.0350 | 0.0350 | 0.0350 | 0.0370 | 0.0370 | 0.0370 |
| Week FE | No | Yes | No | No | Yes | No |
| Year FE | No | Yes | No | No | Yes | No |
| Week-Year FE | No | No | Yes | No | No | Yes |
| Conditional or Village FE | No | No | Yes | No | No | Yes |
| Controls | No | No | No | Yes | Yes | Yes |

All regressions are estimated using distributary-village-week level data. A day is considered rainy if the rainfall is more than 0.1 mm. The long-term average is calculated from the data ranging from 2001-2016. The estimates in Panel A and B are from Negative Binomial Model whereas Panel C was estimated using OLS. The decrease in number of observations in column (3) and (6) is due to use on conditional FE model. The controls include mean temperature, variation in rainfall, a dummy for reform time period and a dummy to indicate the weeks when head discharge was zero. Clustered standard errors are reported apart from conditional fixed effect model where bootstrapped standard errors are given in the parenthesis.

and villages at the head-end shut down their outlets: this would increase the water availability at the tail. To rule out this possibility, I estimate columns 1 and 3 from Table A5 after excluding weeks of very high rainfall (top decile). The results (Columns 1 and 2 of Table A6) do not change. I repeat the same exercise on theft data (see Table A7) and find the results are robust to the exclusion of weeks with very high rainfall. In Panel B of Table 1.2, I show that the results are robust to alternative measure of short-term scarcity that is volume based and has been used in other studies (Jayachandran, 2006; Shah and Steinberg, 2017; Kaur, 2014). I do a similar test on estimates from distributary-level data in Table A8. Both tables show that the results are not sensitive to the choice of short-term scarcity measure. I also use an alternative dependent variable that captures water availability on the basis of height of the water at the tail (Table A11), and results remain consistent. Estimates are also robust to controlling for groundwater quality (Table A12). In the survey, farmers pointed out the *kharif* season months during which the water demand is high and I find that the effect is strong during those months (see Table A9).

To make sure that results are not driven by any dynamic panel data problems, I do a placebo test by including forward lags of the treatment variable. Table A13 shows that the coefficients on nearly all the forward lags are statistically insignificant, and thus, suggests that autocorrelation is not driving the results. Table A14 provides further evidence that results are not driven by dynamic data problems. In this table, I present estimates from equation 1.3 and control for a lagged dependent variable, and find that results stay consistent. I also adjust the standard errors for spatial correlation (Conley, 2010; Hsiang, 2010) and find that results (see Table A15) are still statistically significant. Finally, the results stay consistent after clustering the standard errors at both the distributary and week of the year level (see Table A16).

### 1.4.2 Long-term Scarcity

#### Descriptive Correlations

In this section, I show how long-lasting scarcity affects inter-village cooperation. First, I present motivating evidence in Figure A10. It shows a correlation between inter-village cooperation and the downstream position of a distributary.[35] A village that is further away from the main water source is more likely to have

[35] The downstream position captures the number of channels between a distributary and source of water.

a lower and more uncertain supply of surface water, and thus, faces prolonged water scarcity. In contrast to the results of the previous section, this graph shows that downstream distributaries that face water scarcity are also better at managing the surface water. This is also in line with the findings of Wade (1989), who found better water management in villages located further away from the water source. In Figure A11, I reconstruct the above evidence with another measure of long-term water scarcity; groundwater quality. The result is surprisingly similar. These two correlations suggest that, in comparison to a negative rainfall shock, prolonged water scarcity affects inter-village cooperation positively. However, these graphs are only correlations and likely to be affected by selection bias. In Table 1.3, I start accounting for some of the factors driving these correlations. In the first column there are no controls. I then include year fixed effects, followed by sub-division, and then channel fixed effects. The column (5) includes distributary fixed effects and in the last column I also a control for circle specific linear time trend. The coefficients are positive and statistically significant, indicating a similar story as in the figures that long-term scarcity is positively correlated with inter-village cooperation. Columns (3) and (4) show that even within the same sub-division or same channel, long-term scarcity is associated with higher inter-village cooperation. The effect is also the same with distributary fixed effects. Looking across panels, the coefficient stays consistent whether we include controls or not. In the last panel, I use a cut-off value for SAR, that has been recommended for $Kharif$ crops in Pakistan (Rashid and Memon, 1996), to construct a dummy variable for pollution and go on to find similar results.

### Instrumented Difference-in-Differences Estimates

As mentioned earlier, groundwater quality could be endogenous to farmers' choice of pesticides and rate of groundwater extraction that would bias the OLS estimates downwards.[36] To identify the effect of long-term scarcity on inter-village cooperation, I present estimates using the empirical strategy discussed in the previous section. I use the changes in groundwater extraction that were induced by the industrial effluents in an instrumented difference-in-differences framework to identify the effect of groundwater pollution on inter-village cooperation. This approach relies on the parallel trends assumption that requires treatment and control group, in the absence of treatment, to have the same difference over time. Since there is not statistical test for this assumption, I look at the pre-treatment

[36] Apart from the endogeneity issue OLS estimates could also be biased due to measurement error in the groundwater quality data.

Table 1.3: Long-term Scarcity and Inter-village Cooperation

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | tail discharge relative to allocated | | | | |
| ***Panel A: No Controls*** | | | | | |
| Pollution | 0.056*** | 0.033*** | 0.037*** | 0.015* | 0.011 |
| | (0.012) | (0.010) | (0.012) | (0.008) | (0.008) |
| ***Panel B: Controls*** | | | | | |
| Pollution | 0.066*** | 0.026*** | 0.036*** | 0.010 | 0.013* |
| | (0.012) | (0.009) | (0.012) | (0.008) | (0.008) |
| ***Panel C: Using Pollution Cut-off*** | | | | | |
| Pollution (Dummy) | 0.144*** | 0.091*** | 0.080*** | 0.034** | 0.030* |
| | (0.025) | (0.024) | (0.024) | (0.017) | (0.016) |
| Observations | 2,322 | 2,016 | 2,196 | 2,322 | 2,322 |
| Mean of Dependent Variable | | | | | |
| Number of distributaries | | | | | |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Sub-division FE | No | Yes | No | No | No |
| Channel FE | No | No | Yes | No | No |
| Distributary FE | No | No | No | Yes | Yes |
| Circle Specific Linear Time Trends | No | No | No | No | Yes |

Notes: All regressions were estimated using distributary-week level data. The controls include rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, and a dummy for areas that were flooded in 2010. Standard errors are clustered at the distributary level. Panel C aggregates the pollution using the SAR value of village at the tail-end only. The drop in number of observations in column 2 and 3 is due to limited information on the sub-division and channel of each distributary.

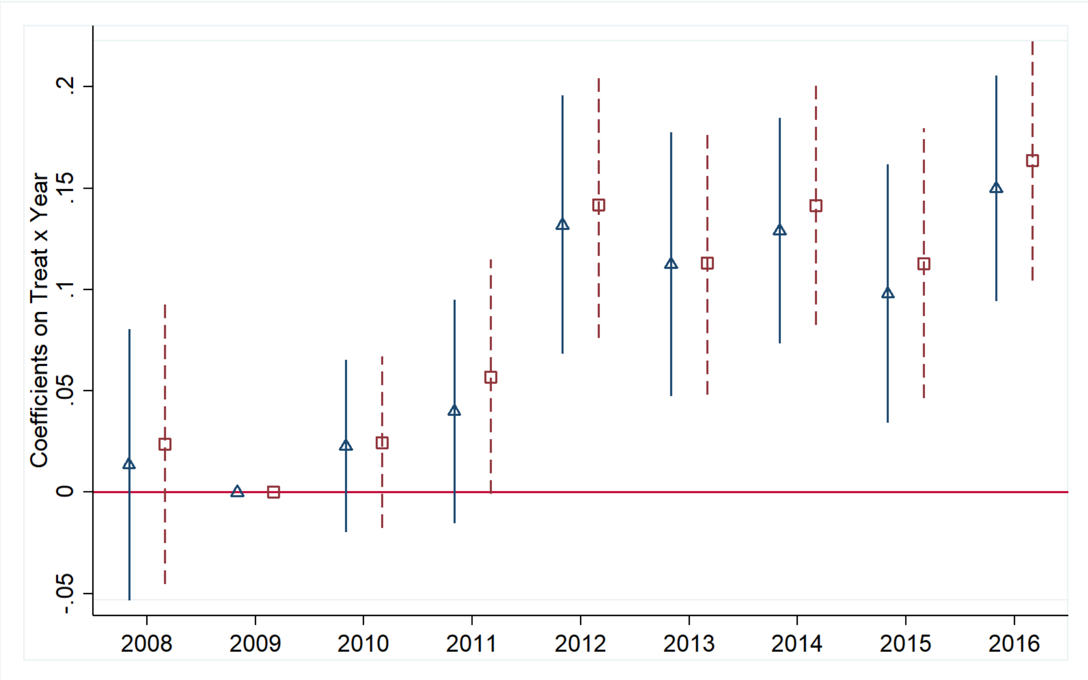
trends in both groups, in Figures 1.3 and A13.These provide suggestive evidence of counterfactual parallel trends. These graphs show that the control and treatment group followed the same trends before 2009. This graph uses the definition of treatment and control based on the distance cut-offs explained in section 1.3.3: the treated distributaries are further away from the rivers (>10 km) and outside the industrial center (>50 km), and the rest of the distributaries are considered control. However, plotting this graph using the continuous measure of distance to the contamination area (Figure A12) shows similar results. Apart from indicating common trends across the treatment and control groups before 2009, these graphs also clearly show that groundwater quality in the treatment areas deteriorated sharply afterwards. Apart from the parallel trends in the first stage, this methodology also requires parallel trends assumption to be satisfied in the reduced form. I look at the evolution in the outcome variable across treatment and control, both before 2009 and afterwards by estimating following specification:

$$D_{dt} = \sum_{i=2008}^{2016} \alpha_i Treat_d \times Year_i + X_{dt}^{'}\lambda + \delta_d + \psi_t + \epsilon_{dt} \tag{1.6}$$

The terms in the above equation are identical to those in equation 1.5. Figure 1.5 plots the estimates. The difference between treatment and control is not statistically significant before 2009 and is very small until 2011, then it increases by 0.12 in 2012 and stays more or less at the same level for rest of the time period. The estimates are given in Table A17 and sensitivity to the distance cut-off is assessed in Table A18. The estimates are robust and the sensitivity analysis shows that these are not driven by the choice of cut-off. Overall, the reduced-form estimates show that before the industrial growth, inter-village cooperation evolved similarly in the treatment and control groups, and the trend does not change much until 2011. After that there is a significant increases in water availability to the tail. The small and insignificant effect in the first two years makes sense as it is likely that farmers took time to understand the changes in groundwater quality and to enforce cooperation.

Lastly, the instrumented difference-in-differences require the exclusion restriction to be satisfied. One key threat to the exclusion restriction is that the effect might be driven by new job opportunities in the industrial area. To test this, I can look at how the effect differs in distributaries that are close to both the industrial areas and to rivers. These distributaries are equally affected by job opportunities, due to their proximity to Faisalabad District, but due to their proximity to rivers, do not experience any change in groundwater pollution. To test this, I estimate a reduced form equation with treatment defined as "distributaries close to rivers"

Figure 1.5: Evolution of Cooperation – Reduced form Estimates




This graph plots estimates from the reduced form regression. The estimates represented by triangle markers control for year FE, distributary FE, and circle-specific time trends. The regression coefficients represented by square markers also control for rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, a dummy for flooded areas, and inter-year variation in rainfall. The standard errors are clustered at the distributary level. The dependent variable is a ration of surface water discharge at the tail-end of a distributary to its allocated amount. This discharge data comes from independent readings collected by Program Monitoring and Implementation Unit in Punjab.

and present results in Figure A14. There is no indication that after 2009 inter-village cooperation increased in distributaries that are close to rivers and as much close to Faisalabad District as treated areas. This suggests that new job opportunities are not driving the results.

I now estimate equation 1.5 (first stage) and present results in Table 1.4. There are four panels. The first three panels differ in terms of how the instrument is defined: I start with the variable *Treat* that takes into account both proximity to contamination site and also the rivers, and then in panel B, I use the variable *Drain* that also takes into account the proximity to drains. Panel C uses continuous distance to contamination site. Finally, in the last panel, I construct the pollution measure by only aggregating the SAR in the tail-end villages. All the coefficients are positive and statistically significant. These estimates further verify that the groundwater pollution increased in the areas that are outside the Faisalabad District. The second panel further shows that increase in pollution was concentrated in the villages that were outside Faisalabad district, but close to drains that carry industrial effluents.

The second stage results (Table 1.5) provide evidence on how this increase in contamination affects inter-village cooperation. Following the structure similar to first-stage estimates, this table also has four panels. All specifications consistently show a positive and statistically significant effect of groundwater contamination on inter-village cooperation. A one SD increase in pollution increases the water availability at the tail by about 13%. The estimates stay consistent after including controls (Column (3)), or using an instrument that also incorporates proximity to drains (Panel B), or using a pollution measure that only takes into account the SAR at the tail-end villages (Panel D).

Table 1.4: Long-term Scarcity – First Stage

| | (1) | (2) |
|---|---|---|
| | Pollution | |
| ***Panel A: Pollution Site and Rivers (Dummy)*** | | |
| Treat x Post | 0.455*** | 0.464*** |
| | (0.083) | (0.082) |
| ***Panel B: Pollution Site, Rivers, and Drains (Dummy)*** | | |
| Drain x Post | 0.431*** | 0.429*** |
| | (0.121) | (0.119) |
| ***Panel C: Distance to Pollution Site*** | | |
| Distance x Post | 0.007*** | 0.007*** |
| | (0.002) | (0.002) |
| ***Panel D: Distance to Pollution Site – Only Tail-ends*** | | |
| Distance x Post | 0.007*** | 0.008*** |
| | (0.002) | (0.002) |
| Observations | 2,322 | 2,322 |
| Distributary and Year FE | Yes | Yes |
| Controls | No | No |

All regressions were estimated using distributary-week level data. The controls include rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, and a dummy for areas that were flooded in 2010. Standard errors are clustered at the distributary level. Panel C aggregates the SAR by taking average of only villages at the tail of a distributary. Treat is equal to 1 if a distributary is outside the contamination site (>50 km from Faisalabad Center) and away from rivers (>10km), and 0 otherwise. Drain is equal to 1 if on average villages are within 5km of a drain, and 0 if the distributary is more than 5km from a drain, close to rivers (10km) and within the contamination site (50km). All regressions also include a linear time trend for each of the two circles. The dependent variable is a ratio of actual to authorized tail surface water discharge, and is used as a proxy for inter-village cooperation.

Table 1.5: Long-term Scarcity – Second Stage Results

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | tail discharge relative to allocated | | | |
| | 2SLS | Reduced Form | 2SLS | Reduced Form |
| ***Panel A: Pollution Site and Rivers (Dummy)*** | | | | |
| Pollution | 0.134** | | 0.164*** | |
| | (0.056) | | (0.058) | |
| Treat x Post | | 0.061*** | | 0.076*** |
| | | (0.023) | | (0.024) |
| KP F-State | 30.19 | | 31.78 | |
| ***Panel B: Pollution Site, Rivers, and Drains (Dummy)*** | | | | |
| Pollution | 0.176* | | 0.200** | |
| | (0.092) | | (0.094) | |
| Drain x Post | | 0.076** | | 0.086*** |
| | | (0.033) | | (0.033) |
| KP F-State | 12.63 | | 12.91 | |
| ***Panel C: Distance to Pollution Site*** | | | | |
| Pollution | 0.129** | | 0.158** | |
| | (0.060) | | (0.062) | |
| Distance x Post | | 0.001** | | 0.001*** |
| | | (0.000) | | (0.000) |
| KP F-State | 19.67 | | 21.65 | |
| ***Panel D: Distance to Pollution Site – Only Tail-ends*** | | | | |
| Pollution | 0.111* | | 0.135** | |
| | (0.057) | | (0.057) | |
| Distance x Post | | 0.001** | | 0.001*** |
| | | (0.000) | | (0.000) |
| KP F-State | 17.23 | | 20.54 | |
| Observations | 2,322 | 2,322 | 2,322 | 2,322 |
| Distributary and Year FE | Yes | Yes | Yes | Yes |
| Controls | No | No | Yes | Yes |

Notes: All regressions were estimated using distributary-week level data. The controls include rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, and a dummy for areas that were flooded in 2010. Standard errors are clustered at the distributary level. Panel D aggregates the SAR by taking average of only villages at the tail of a distributary. Treat is equal to 1 if a distributary is outside the contamination site (>50 km from Faisalabad Center) and away from rivers (>10km), and 0 otherwise. Drain is equal to 1 if on average villages are within 5km of a drain, and 0 if the distributary is more than 5km from a drain, close to rivers (10km) and within the contamination site (50km). All regressions also include a linear time trend for each of the two circles. The dependent variable is a ratio of actual to authorized tail surface water discharge, and is used as a proxy for inter-village cooperation.

### Alternate Explanations and Robustness Checks

One possible explanation for an increase in surface water availability to tail-end villages is that the industrial pollution spilled over to canals, rendering surface water unsuitable for irrigation. Figure A4 provides evidence against this mechanism. It shows that there is no relationship between reported surface water quality and groundwater pollution. As expected, most of the villages report having access to good quality surface water. Alternatively, the main result could also be driven by farmers' decisions to change other inputs. First, pollution could either push farmers to reduce land use and/or completely quit farming. Since the agricultural census does not record changes in land use, I rely on satellite data on global cropland to test this channel. Figure A15 shows there is no indication that, after an increase in pollution, fewer areas were classified as cropland in the treated areas relative to the the control. Second, farmers could also respond to long-term water scarcity by shifting to less water intensive crops. This channel could only explain the results if a sufficiently large number of head-end farmers moved away from water-intensive crops. Since none of the datasets record crop choice at a village level both before and after the treatment, I use aggregate district-level statistics to provide suggestive evidence that there is no major shift towards less water intensive crops in treated districts (see Figure A16). The trend does not show any major drop in the cultivated area for water intensive crops (i.e. cotton, rice, and sugarcane).

An increase in pollution could also push farmers to migrate out. Since tail-end villages usually receive relatively less amount of water, we should expect that the probability of migration should increase as we move downstream on a distributary. This would result in lower demand for water in the tail end of a distributary, which would not explain the key results. Nevertheless, I explore how population changes over time in treated and control distributaries using an open access spatial demographic dataset. The results provide (see Table A19) some indication of out-migration in the treated districts. However, it is important to note three points in interpreting these results. First, only two years' worth of data is available and thus it is not possible to test the parallel trend assumption. Second, the data does not separate the farmers from the rest, making it difficult to conclude whether the decrease in population in treatment districts directly affected farming activities. Third, since there is no change in the land use in the treated areas, the migration does not lead to significant decrease in agricultural activity.

I use variation over the year to perform another placebo test. Since winter season crops are usually less water intensive and have a higher tolerance for salt content in the groundwater, we should expect that, relative to the *kharif* season, the results would be weaker for the dry season. I find in Table A20 that changes in groundwater contamination do not have any effect on inter-village cooperation during the *rabbi* season. Apart from this, I test further robustness of the main results by controlling for Electrical Conductivity (Table A21), including sub-division specific time trends (Table A22), excluding areas that are not in the close proximity of Faisalabad District or areas that are in the Faislabad City (Table A23), and estimates stay consistent across these specifications.

Since upstream areas are less likely to be affected by industrial pollution, there should be relatively smaller or no effect in these areas. I divide the sample into upstream and downstream distributaries and provide second-stage results in Table A24. In line with rest of the evidence, I find that the effect is driven by the downstream areas.

Lastly, I use data on the drains to show robustness to as alternative set of instruments in Table A25. The first four columns use three instruments and take into account distance to the industrial site, rivers and drains. The last four columns redefine the $Treat$ variable by assigning 1 to a distributary where on average villages are within 5km distance of a drain, and zero if it is not or if it is close to rivers or close to the industrial site. The coefficient on pollution is positive and statistically significant from zero in all the specifications.

### 1.4.3 Mechanisms

In this section, I look at possible channels through which the water availability to tail-end villages would increase in response to long-lasting water scarcity. Following Wade (1989), I start by looking at the role of social organization and present three pieces of evidence in support of this mechanism. First, I use the *Mouzza* Census data to show that villages facing long-term scarcity are more likely to have active informal village-level institutions (Table 1.6). The results are robust and statistically significant, and hold for two different measures of pollution. Furthermore, evidence from the cross-section IV framework is in line with the OLS estimates.[37] Overall, this suggests that long-term scarcity increases the need to organize better and resolve the collective action problem. Second, with the data

[37] As noted earlier, in level terms distance to river explains the groundwater quality very well, and since I am now looking at a cross-sectional data, I exploit this distance measure as an IV for SAR.

collected through the 2008 community survey I can look at the correlation between the use of informal mechanisms in order to resolve inter-village disputes and long-term water scarcity. The results (Table A29) show that villages at the tail-end are more likely to use informal mechanisms to resolve conflicts related to water theft. Third, caste networks might be an important channel through which social fines could be enforced. Farmers in distributaries where head- and tail-end villages do not share the same caste might find it harder to resolve water theft disputes. To test this hypothesis, I use two datasets. First, I use primary data from a sample of distributaries, collected using the community survey in 2018. The survey includes information on village-level caste distribution. [38] Second, I use land records data from 2010 to obtain the caste distribution across villages. These two datasets allow me to construct a caste distance measure. I follow Spolaore and Wacziarg (2009) in calculating the expected caste distance between two randomly selected farmers, one from the head-end village and other from the tail-end.[39] Since landholdings are likely to be unequally distributed across caste, and water requirements are proportional to landholdings, an unweighted caste distance measure might understate the effective linkages across villages. To take this into account, I also compute a land-weighted caste distance measure. Table 1.7 shows how the effect of long-term scarcity on inter-village cooperation varies with caste distance. The results show that distributaries where head- and tail-end villages are less likely to share their caste find it difficult to establish cooperation under long-term scarcity. This result further suggests that farmers rely on social means to establish cooperation and is in line with findings of Wade (1989).

Another possible explanation for the results presented above is political patronage. A recent paper finds that the political association of a region is an important determinant of canal water availability in Pakistan (Beg, 2019). The paper finds that the supply of surface water for irrigation increases in districts where elected official is from the national ruling party. In my context, the patronage could be acquired in terms of lining of distributaries or using a political channel to enforce cooperation. The time period of my sample covers two terms of the Provincial Assembly. I match each distributary to a constituency using GIS data, and then include political party fixed effects in Equation 1.4. The results (Table A26) do not show any substantial variation and thus indicate that the political connection

[38]The caste distribution was obtained by asking the respondent to provide the number of households residing in the village against each caste.

[39]The caste distance is defined as $F = \sum_{i=1}^{I} \sum_{j=1}^{J} (s_{1i} \times s_{2j} \times d_{ij})$ where $s_{1i}$ is share of population (or land) of caste $i$ in village 1 and $d_{ij}$ is distance between caste $i$ and $j$; I assume distance is zero if $i = j$, otherwise 1

of a region is not an important channel through which cooperation could have increased.

Yet, another mechanism through which cooperation could be enforced is the Irrigation Department. If farmers in the contaminated areas are more active in contacting irrigation officials, then they might be able to increase the legal cost of cheating for head-end villages, and, therefore, increase water availability at the tail. Although the data on the activities of irrigation officials is not available, I can use the timing of the devolution reforms to provide indirect evidence. Fatima *et al.* (2016) present a detailed and rigorous discussion on how decentralization reforms affected water availability at the tail. They find that, under the reform, the ratio of actual to authorized discharge at the tail decreases significantly and interpret this change as an increase in water theft. Since the reforms completely transferred the management responsibilities to farmers, it increases the salience of social organization and suppresses the importance of legal fines. I study how distributaries that were contaminated before the reform performed under the devolution. If the higher cooperation observed in previous sections is due to increased legal activities, then we should also see a drop in inter-village cooperation when responsibilities were transferred to the farmers. I test this hypothesis by interacting the pre-reform water quality measures with a variable that indicates that the responsibilities of a distributary have been transferred to farmers in each tenure. The estimates are given in Table A30 and A31. There are two important findings; first: in line with Fatima *et al.* (2016), I find that water availability at the tail decreases under the reform, and much more in the first tenure. However, I find that areas facing long-term scarcity (SAR or EC) do not show much change under the reform. In short, it does not matter whether contaminated areas are under the Irrigation Department or farmer organizations, they continue to show relatively higher availability of surface water at the tail. Therefore, I do not find much support for the official channel in increasing the inter-village cooperation.

### 1.4.4 Unexpected Shocks Under Long-term Scarcity

The previous section shows that inter-village cooperation increases under long-term scarcity. The higher availability of water at the tail could be due to: cleaning of a distributary, improvement in infrastructure, or decreases in illegal diversion of water. I do not observe the first two, but I can look at the effect on water theft by interacting the unexpected scarcity shock from Equation 1.3 with the groundwater quality measure. The results (Table 1.8) show two things. First, most of the response of unexpected short-term scarcity shocks comes from areas

Table 1.6: Informal Village Institutions

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Any village level institution? | | | | | | | |
| | OLS | OLS | 2SLS | 2SLS | OLS | OLS | 2SLS | 2SLS |
| EC | 0.053*** | 0.054*** | 0.200*** | 0.243*** | | | | |
| | (0.013) | (0.015) | (0.037) | (0.040) | | | | |
| SAR | | | | | 0.045*** | 0.046*** | 0.246*** | 0.312*** |
| | | | | | (0.015) | (0.016) | (0.051) | (0.057) |
| Dependent variable mean | 0.262 | 0.261 | 0.262 | 0.261 | 0.262 | 0.261 | 0.262 | 0.261 |
| District FE | Yes | Yes | No | No | Yes | Yes | No | No |
| Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Kleibergen-Paap F Stat | N/A | N/A | 102.695 | 107.764 | N/A | N/A | 71.97 | 73.102 |
| Observations | 1,850 | 1,716 | 1,850 | 1,716 | 1,850 | 1,716 | 1,850 | 1,716 |

Notes: All regression use village level data from 2008 Mouzza census. Standard errors are clustered at distributary level. The controls include position of a village on a dsitributary, type of a village, a dummy for whether a village is served by a minor, design discharge at the head- and tail-end, and a dummy to indicate whether the distributary is at the tail of a channel. The drop in observations is due to unavailability of location data for some distributaries, that was used as a control variable.

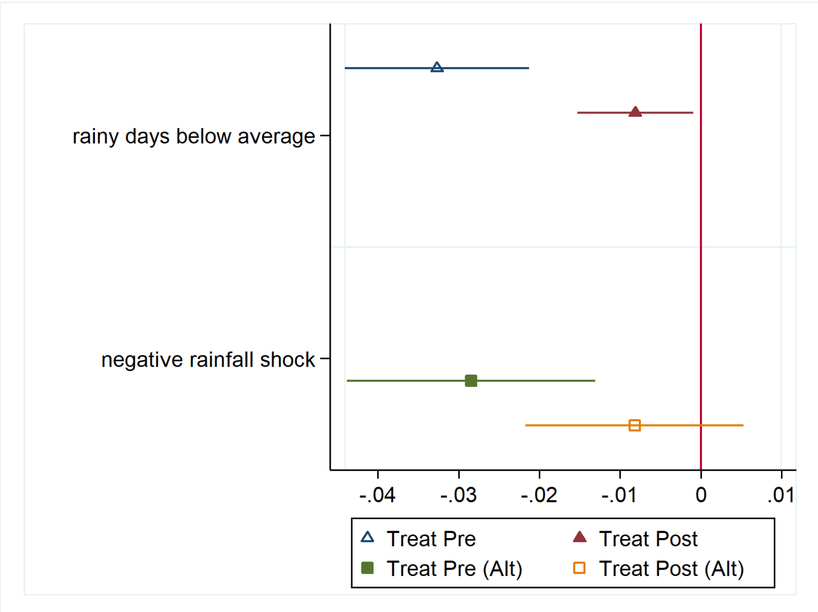
Table 1.7: Long-term Scarcity and Caste Distance

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | actual to authorized tail discharge | | | |
| | Land-weighted | | Survey sample | |
| Polluted | 0.266** | 0.255* | 0.422** | 0.400** |
| | (0.130) | (0.130) | (0.177) | (0.173) |
| Polluted x caste distance | -0.003* | -0.003* | -0.005** | -0.004** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Distributary FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Controls | No | Yes | No | Yes |
| Observations | 1,182 | 1,182 | 960 | 960 |

Notes: Standard errors are clustered at distributary level. *** p<0.01, ** p<0.05, *p<0.1. All standard errors are clustered at distributary level. The time period only include rainy season (19-40th week) from year 2008 to 2013. The controls include mean temperature, total amount of precipitation, supply of surface water, an interaction of temperature and precipitation, a dummy for reform time, a dummy for 2010 floods and monthly variation in rainfall. I follow Spolaore and Wacziarg (2009) to construct caste distance and assume that two castes have zero distance if they are exactly the same, and 1 otherwise. Caste distance measure the overlap of caste across head and tail-end villages; it captures the probability that two randomly selected farmers, one from head-end and other from tail-end, have different caste. It has been rescaled and ranges from 0-100. “Polluted” is the standardized sodium Adsorption ratio. The drop in number of observations in the first two columns is due to a limited match across caste data and discharge data. The lower number of observations in the last two columns reflect the survey sample.

that have relatively better quality groundwater (see coefficient on rainfall shock variable). Second, the areas that have higher pollution respond very little to rainfall shocks (see coefficient on interaction term). These findings, combined with earlier results, show that distributaries facing long-term scarcity are able to resolve the issue of water theft. In the reduced-form setting, I also show how the treated areas responded to negative rainfall shocks both before and after the increase in pollution (Figure 1.6). In line with the estimates from Table 1.8, I find that the treated areas, after the increase in groundwater pollution, do not see a decrease in water availability to the tail in response to a negative rainfall shock. These results highlight that the cost of stealing has increased in the areas facing long-term scarcity. Taken together with the results in the previous section, this increase in cost of cheating is likely to be driven by the informal enforcement mechanisms. In the next section, I explore whether an increase in the availability of surface water have any effect on the cropland productivity.

Figure 1.6: Rainfall Shock and Long-term Scarcity




This graph plots estimates from the reduced-form regression of $D_dt$ on rainfall shock in treated areas, separately for pre- and post-treatment areas. $Treat(Alt)$ indicates the alternative measure of rainfall that is volume based rather than rainy days. Treated areas are the ones that are further away from rivers are outside the contamination site. The post is define as the time period after 2009.

## 1.5 Crop Choice and Productivity

The coordination issue discussed above could have important implications for crop choice and yield. Due to data constraints, I restrict the analysis to the following two datasets. First, I use the Agricultural Census of 2010. There are

Table 1.8: Rainfall Shock under Long-term Scarcity

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | tail discharge relative to allocated | | | |
| Pollution x Rainfall Shock | 0.003* | 0.003** | 0.007*** | 0.006*** |
| | (0.001) | (0.001) | (0.002) | (0.002) |
| Rainfall Shock | -0.012*** | -0.012*** | -0.019*** | -0.020*** |
| | (0.003) | (0.003) | (0.005) | (0.005) |
| Pollution | 0.015* | 0.014* | 0.014* | 0.013* |
| | (0.008) | (0.008) | (0.008) | (0.008) |
| Rainfall Measure | Rainy Days | | Volume Based | |
| Mean of Dependent Variable | 0.75 | | | |
| Observations | 51,084 | 51,084 | 51,084 | 51,084 |
| Distributary and Year FE | Yes | Yes | Yes | Yes |
| Controls | No | Yes | No | Yes |

Notes: All regressions were estimated using distributary-week level data. The controls include, discharge at the head end, temperature, a dummy variable for reform, and intra-month variation in rainfall. Standard errors are clustered at the distributary level.

two limitations of this dataset. First, it is a sample-based census and does not cover either large number of villages or the same villages in each round. Second, it does not collect information on crop production and therefore the only variables that could be of interest are related to land use and crop type. Second, to look at crop productivity I use a satellite measure (Net Primary Productivity). It captures the carbon intake by plants and has been used as a proxy for cropland productivity (Strobl and Strobl, 2011; Hicke *et al.*, 2004). The data is available at a weekly level on a 0.1 x 0.1 grid. I match this data with the location of villages on a distributary. In the rest of this section, I use these two data sources to study the implications of inter-village cooperation for crop choice and productivity.

The satellite data allows me to define within-distributary variation in cropland productivity ($Y_{id}$) by subtracting the distributary's mean ($\overline{NPP}_d$) from each village's average ($NPP_{id}$):

$$Y_{id} = NPP_{id} - \overline{NPP}_d \tag{1.7}$$

I define $Y_{id}$ at the year level by aggregating the weekly data, and then plot the distribution of both control and treatment groups, both before and after 2009 in Figures A17 and A18. These two graphs suggest that the inter-village dispersion in productivity did not change over time in the control group, but decreased in the treatment group. These findings are in line with the earlier result of increased water availability at the tail in the treatment group after 2009. This decrease in productivity dispersion could be due to more equal water distribution along a distributary. However, we should also expect a decrease in average productivity due to overall water scarcity, and therefore the changes in productivity dispersion could be mechanical. To take this into account, I move to a regression framework where I look at the effect of contamination on both average productivity and its coefficient of variation (Table 1.9). The results show that long-term scarcity decreases both average NPP and its variation within a distributary.

The agricultural censuses do not allow me to look at the effects of changes in contamination as there is only one round (2010) that overlaps with discharge and water quality data. Therefore, I look at descriptive correlations between crop choice and availability of water. I match the farm-level data with discharge and groundwater quality data using village names. Since the census was conducted during 2009 and 2010, I take the average of groundwater and discharge data during these two years, and create a village and distributary level cross-section to match with the Agricultural census. I estimate:

$$CashCrop_{ivd} = \beta_1 Tail_{ivd} + \beta_2 D_d + \beta_3 Pollution_{vd} + \beta_3 IrrigationSource_{ivd} + x'_{dt}\lambda + \upsilon_{dt} \tag{1.8}$$

The dependent variable $Cash_Crop_{ivd}$ is simply the proportion of land in which cash crops were grown during the $kharif$ season. $D_d$ and $Pollution_{vd}$ are the same as before, only now they do not have any variation over time. I also control for the proportion of land that is under either full or partial canal irrigation ($IrrigationSource_{ivd}$). Apart from this, there are two sets of controls. The basic controls include type of household, tenure status and Tehsil fixed effects. Other controls include distance to rivers and industrial hubs, and division fixed effects, and EC. Columns 1-3 of Table 1.10 show the basic correlations, Column 4 uses farm-level weights, Column 5 excludes areas that do not have any access to canal water, the next column take into account other controls, and the last one includes distributary fixed effects. There are three points to note. First, as expected, the areas at the tail and with bad quality groundwater use relatively less land for cash crops. Second, the higher availability of surface water is strongly and positively correlated with the proportion of land used for cash crops. Finally, these correlations also show up after I take into account control variables mentioned above, and they also survive the inclusion of distributary fixed effects. Overall, these estimates suggest that access to surface water is an important determinant of both crop productivity and choice in these areas. Moreover, a decrease in inter-village cooperation over surface water can increase the inter-village cropland productivity dispersion.

Table 1.9: Net Primary Productivity

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | NPP | | Coefficent of Variation | |
| Pollution (Dummy) | -0.021*** | -0.021*** | -0.023** | -0.023** |
| | (0.008) | (0.008) | (0.009) | (0.011) |
| Disty FE | Yes | Yes | - | - |
| Year FE | Yes | Yes | - | - |
| Dep Var Mean | 0.037 | | 0.018 | |
| Observations | 10,576 | 10,576 | 10,192 | 10,192 |

Notes: NPP refers to Net Primary Productivity and it captures the carbon intake by plants, and thus, provides a proxy for cropland productivity. The "coefficient of variation" is the ratio of with-in distributary standard deviation of NPP and its average. Pollution is defined as a dummy variable that is equal to 1 if the SAR is above its critical value of 7.5. These estimates exclude cases where distributary was relatively smaller than the grid-cell at which NPP is available. The second and last column also include following controls: average temperature, average rainfall, and variation in the rainfall over the year. Standard errors are clustered at grid-cell level. The first two column of the above table show that pollution led to a decrease in crop land productivity and the last two column suggests that there was also a drop in the inter-village productivity dispersion.

## 1.6 Conclusion

Developing countries are likely to face severe water shortages in the future. In the absence of strong formal institutions, water scarcity might push villages to over-extract water for irrigation. However, prolonged water scarcity could give disadvantaged farmers with an incentive to organize better and enforce cooperation. This paper studies how short- and long-term resource scarcity affects inter-village cooperation over surface water in Pakistan. For identification, I use two natural experiments. First, I use random variation in village-week level rainfall to define a short-term shock. Second, I exploit changes in groundwater quality induced by industrial effluents to measure effective availability of alternative resources. Since groundwater is the key substitute for surface water and changes in its quality persist for multiple seasons, I use this exogenous variation to define a proxy for long-term scarcity. I find that an unexpected short-term scarcity shock leads to an increase in water thefts, or decreases inter-village cooperation. However, when the scarcity is long-lasting, inter-village cooperation over surface water increases. The instrumented difference-in-differences estimates show that

Table 1.10: Crop Choice and Scarcity

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Proportion of Area Under Kharif Cash Crops | | | | | | |
| Tail | -0.075** | -0.093*** | -0.069** | -0.077** | -0.067** | -0.052* | -0.063* |
| | (0.029) | (0.029) | (0.030) | (0.032) | (0.033) | (0.030) | (0.033) |
| Actual to authorized discharge | | 0.246*** | 0.221*** | 0.224*** | 0.231*** | 0.156** | |
| | | (0.071) | (0.071) | (0.077) | (0.083) | (0.073) | |
| Pollution | | | -0.056*** | -0.052*** | -0.053*** | -0.030* | -0.061*** |
| | | | (0.016) | (0.017) | (0.019) | (0.018) | (0.015) |
| Irrigation Source | | | 0.105*** | 0.133*** | | 0.116*** | 0.102** |
| | | | (0.033) | (0.037) | | (0.031) | (0.040) |
| Dependent variable mean | 0.53 | | | | 0.55 | 0.53 | |
| Number of Villages | 274 | | | | | | |
| Number of distributaries | 137 | | | | | | |
| Observations | 15,302 | 15,302 | 15,302 | 15,302 | 12,027 | 15,302 | 15,302 |
| Basic Control | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Other Controls | No | No | No | No | No | Yes | No |
| Tehsil FE | Yes | Yes | Yes | Yes | Yes | Yes | No |
| Weights | No | No | No | Yes | No | No | No |
| Disty FE | No | No | No | No | No | No | Yes |

Notes: All regressions use plot-level data from 2010 Agricultural Census. Standard errors are clustered at the distributary level. “Tail” refers to the villages that are at the tail-end of a distributary. ”Actual to authroized discharge is a ratio of surface water discharge at the tail-end and its allocated amount. ”Pollution” is a standardized sodium Adsorption ratio. ”Irrigation Source” is a self reported measure of surface water availability that captures the plot areas being that receive water from a distributary. The controls include tenure type, and household type. The outcome variable is only for the *Kharif* season as nearly all the cash crops are grown in this season and it is a ratio of all the land under cultivation divide by the total land owned and rented in. The drop in the number of distributaries are due to low number of matches across villages in Agricultural Census and villages in discharge data.

a one standard deviation increase in groundwater pollution increases availability of surface water at the tail relative to its allocated amount by 13%.

To provide evidence that the increase in cooperation under long-term scarcity is achieved through improvements in social organization, I present three pieces of evidence. First, I show that long-term scarcity is associated with the presence of active informal village-level institutions. Second, villages facing long-term scarcity are more likely to use informal means to resolve inter-village water disputes. Finally, in distributaries where head and tail-end villages are less likely to share a caste, farmers find it harder to establish cooperation under long-term scarcity. Consistent with this explanation, I find that communities facing long-term scarcity cease to respond to transitory water shortages. I rule out the possibility that decreases in water theft are due to changes in legal enforcement mechanisms or increased political patronage. The effect is also not explained by spillovers of groundwater pollution to surface water.

Taken together, these results suggest that local institutions that help to enforce cooperation over the management of joint resources have the potential to improve endogenously under long-term resource scarcity. Put differently, environmental change can push communities to resolve their collective action problem. However, there is an important caveat. The findings on the effectiveness of caste linkages in enforcement suggest that communities require sufficient social linkages to adapt successfully.

These results also have implications for community-driven participatory programs, especially Irrigation Management Transfer reforms. Fatima *et al.* (2016) evaluate the effectiveness of decentralization reforms in this region and find that the externally imposed farmers' organizations are unable to prevent water theft. This paper shows the communities that were facing long-term scarcity did not see a significant change in water theft due to reforms, suggesting that they had already resolved their collective action problem. Taken together, these findings suggest that such participatory programs might require sufficient social capital to operate properly.

# 2 Environmental Regulation, Firm Entry, and Firm Size

*with James Fenske, and Namrata Kala*

## 2.1 Introduction

The trade-off between employment and environmental quality has been at the heart of sustainable development issues since its inception. This trade-off is particularly salient in developing countries - in India, over half the population face air pollution exposure over the National Air Quality Standard (Greenstone *et al.*, 2015), and industrial sectors, often a primary contributor to pollution, employ over a quarter of the labor force. If environmental regulations change the number and types of firms that enter a market, this can lead capital or labor to be misallocated or go unused (Greenstone, 2002; Tombe and Winter, 2015). Regulation can also reduce competition, which can harm consumers and workers (Heyes, 2009). Conversely, if regulation disproportionately falls on firms with market power, it can correct distortions, while mandated abatement measures can increase innovation, competitiveness, and productivity (Becker *et al.*, 2013; Berman and Bui, 2001; Porter and Van der Linde, 1995; Ambec *et al.*, 2013).

Standard data sources frequently have limitations that make it difficult to disentangle these effects. Survey data often only provide information on large, active, and formal firms, are collected at an annual frequency, overlook failed attempts to enter the market, and only describe the firm as a whole rather than specific processes within the same firm. In this paper, we assemble a large database of applications made by Indian firms seeking permission to pollute. We use an event-study framework to evaluate the effects on the quantity, size, and pollution abatement of new entrants of a re-categorization policy that lowered the regulatory burden for some industries. Further, we test whether several size thresholds in environmental regulations predict discontinuities in the firm size distribution.

We have assembled a dataset of more than half a million applications submitted to the State Pollution Control Boards of nine states of India. In these applications, firms seek permission to engage in a number of polluting activities. These include both new applications by entrant firms as well as renewals. These data contain information on a wide set of firm characteristics and behaviors, including employment, capital investment, quantity of pollution generated, and expenditure on pollution abatement. These capture application events at the daily level, and cover both new and incumbent firms.

We subject these data to two broad analyses. First, we use an event study approach to evaluate a 2016 re-categorization reform in which regulatory restrictions were relaxed for selected industries. Industries that were re-categorized generally saw their renewal periods lengthened, frequency of inspections reduced, and restrictions on locating in ecologically fragile zones removed. Our identification strategy exploits variation within industries, within states, within months, and, critically, compares industries with equal pollution potential that had been classified differently prior to the reform but whose regulatory statuses were harmonized by the reform. Second, we use a combination of data visualization and formal density tests to identify "bunching" in the firm size distribution. That is, we examine whether there is an excess mass of applications below cutoffs in terms of measures of firm size that determine how an application will be classified, and hence the fees and regulations that will be applied to it.

Our event study results reveal that the re-categorization policy did change the self-reported characteristics of new entrants. Industries in which regulatory restrictions were loosened saw an increase in new applications, particularly from smaller firms with fewer workers, and less total capital investment. New entrants in these industries submit less complete applications, and yet are more likely to see their applications accepted. These effects are sizable: applications increased by 31% in re-categorized industries relative to other similar industries, while the marginal entrant had 19% fewer workers and 17% less capital. Because these firms are smaller, they pollute less, though they also spend less on pollution abatement.

Our bunching results show the presence of multiple statistically significant discontinuities in the firm size distribution that correspond with regulatory cutoffs. Some of these are specific to a single industry. For example, rice mills that produce more than ten tons of rice per day face greater regulatory burdens. More than 6% of all rice mills with capacity below twenty tons per day report a capacity of *exactly* ten tons per day. Some discontinuities are common across several industries, coinciding with nonlinearities in the mapping of total capital investment to application fees charged by the state of Haryana. Firms systematically report an excess mass at values just below those that trigger higher fees. We follow a strategy similar to Velayudhan (2018), and show no systematic changes in the relationship between capital investment and other inputs at the relevant thresholds, suggesting that this represents actual bunching, and not simply the deliberate underreporting of capital investment.

We interpret our results as evidence that environmental regulations in India do impose real costs on firms, and that these costs discourage entry, particularly by smaller firms. By reducing the recurrent but fixed costs of operating, India's re-categorization policy allowed smaller, less capital-intensive firms to enter in the expectation of profitability. Although applications in recategorized industries were more likely to be accepted, this is not significant at conventional levels.

These results represent a progress report on a larger project that is currently underway. We are currently working to examine impacts of the re-categorization reform on additional outcomes, such as renewals by existing firms, acceptance or rejection of applications, and pollution discharge. For some applications, we have text data on the internal correspondence between officials of the State Pollution Control Board, and are currently working with these to examine the conditions that lead an application to be inspected or referred up the administrative hierarchy for further attention.

### 2.1.1 Contribution

We contribute to two broad literatures. The first focuses on how environmental regulations affect firms, both in developed countries and in developing countries. Environmental regulations shape firm pollution behavior (Fan *et al.*, 2019; Foulon *et al.*, 2002), productivity (He *et al.*, 2019), where firms locate (Lipscomb and Mobarak, 2016), and the distribution of production across firms (Boomhower, 2019). Enforcement of these regulations is targeted unevenly across firms (Duflo *et al.*, 2018) and often corrupt (Duflo *et al.*, 2013). We make several contributions to this literature. We examine several dimensions of how regulation affects both the rate of entry and the composition of firms that attempt to enter the market, including their levels of employment and capital investment. Our data allow us to look not only at successful entrants, but also at those firms who attempt to enter the market, and their decision to apply again conditional on initial rejection by the regulator.

The second literature to which we contribute links regulation more generally to the firm size distribution in developed countries, as well as in developing countries, where the prevalence of small firms is particularly acute (Hsieh and Olken, 2014). The literature has identified a number of regulatory incentives that keep firms inefficiently small, often with substantial welfare consequences (Garicano *et al.*, 2016). These include revenue thresholds that trigger stricter tax enforcement (Almunia and Lopez-Rodriguez, 2018), compulsory registration for value added tax (Liu *et al.*, 2019), the costs of formalization, (Ulyssea, 2018), and environmental regulations (Balietti *et al.*, 2018). We make a number of new contributions here. We identify relevant regulatory cutoffs across a range of dimensions that have largely been overlooked in the literature (e.g. (Kleven, 2016)). These include thresholds in employment and capital investment that do not feature as important in the literature on labor regulations (e.g. Besley and Burgess (2004); Amirapu and Gechter (2020)) or that are based on other, often product-specific, variables, such as waste-water discharge, and built-up area. Bunching is, then, much more widespread in Indian manufacturing than is generally supposed. We construct a data novel set that contains many firms too small to be captured in standard sources such as CIME's Prowess database and the Annual Survey of Industries. Our data allow us to examine at a granular level the timing of application, acceptance or rejection, and reapplication.

In section 2.2, we provide background on environmental regulation in India, as well as the details of India's re-categorization policy. We also outline the key thresholds that determine how an industry is regulated, and that we expect may lead to bunching across several dimensions of firm size. In section 2.3, we outline our sources of data and provide descriptive statistics. In section 2.4, we present our empirical strategies, both for evaluating the impacts of recategorization in an event-study framework and for testing for the presence of bunching on firm size. In section 2.5, we present the results of these analyses. Section 2.6 concludes.

## 2.2 Context and Re-Categorization Policy

### 2.2.1 Environmental regulation in India

The framework for environmental regulation in India is governed largely by the Water (Prevention and Control of Pollution) Act of 1974 and the Air (Prevention and Control of Pollution) Act of 1981 (Ghosh, 2019). These helped establish the Central Pollution Control Board and the State Pollution Control Boards, which advise both the national and state governments on the control, prevention, and abatement of water and air pollution (Ghosh, 2019). The Central Pollution Control Board coordinates with and provides assistance to the State Pollution Control Boards, which undertake a wide set of functions such as setting standards, investigation and research, and organizing training programmes. The State Pollution Control Boards have a number of powers, including inspection, information gathering, and refusing or withdrawing consent for the establishment of any industry (Bhat, 2010; Paranjape, 2013). Despite this legal framework, a combination of poor enforcement and monitoring, slow enforcement by courts, corruption, and small-scale production have weakened the effectiveness of environmental regulation (Gronwall and Jonsson, 2017).

Polluting firms must obtain approval from the State Pollution Control Board both before establishment and before beginning operations (Ghosh, 2019). In our data, we will see applications for both these types of permission, under the headings of Consent To Establish (CTE) and Consent to Operate (CTO). State pollution control boards can make inquiries as these applications are received, including site inspections (Ghosh, 2019). CTO must be renewed regularly, though the frequency of the renewal depends on the color category assigned to the industry: Red, Orange, Green, or White. After consent is given, State Pollution Control Boards monitor firm compliance with environmental regulations, for example through inspection, and consent can be either not renewed or withdrawn for failure to comply (Ghosh, 2019).

### 2.2.2 India's re-categorization policy

In 2015, the government of India announced a plan to re-categorize industries by color, based on their pollution potential (Aggarwal, 2015). Directions to all State Pollution Control Boards were sent in March 2016, bringing these changes into effect (CPCB, 2016). Industries now receive a pollution score between 0 and 100, which determines whether they are classified as Green (21-40), Orange (41-59), or Red (60 and above) (Aggarwal, 2015; CPCB, 2016).[1] The pollution score itself is based on three sub-indices: a water pollution score, and air pollution score, and a hazardous waste score (CPCB, 2016). These are broken into further sub-indices that are rule-based. For example, one portion of the water pollution index will assign an industry 25 points if it emits high-strength but non-toxic polluted waste water with biological oxygen demand in the range of 1000-5000 milligrams per litre, so long as the pollutants are biodegradable. These are then aggregated into a single pollution score.[2]

India's 17 "critically polluting industries," such as distilleries and thermal power plants, remained in the red category (Aggarwal, 2015). In addition, these industries are not permitted in ecologically fragile or protected areas (CPCB, 2016). While a small number of industries were classified upwards, most industries that changed color categories were downgraded, such as synthetic detergents and soaps

[1] Aggarwal (2015) reports different cutoffs corresponding to those that were initially considered but later revised. We report here the cutoffs in CPCB (2016), which also correspond to those observed in our data.

[2] Details on the scoring methodology can be found on pages 8-14 of (CPCB, 2016).

(excluding formulation) (from Red to Orange) or digital printing on polyvinyl chloride (PVC) clothes (from Orange to Green). 26 of 85 initially Red industries became Orange, and 3 became Green (CPCB, 2016). 19 of 73 initially Orange industries became Green, and 2 became white (CPCB, 2016). At the same time, the requirement that certification was to be renewed annually was removed. Renewal periods were lengthened to five years (Red industries), ten years (Orange industries), or removed altogether (Green industries) (Aggarwal, 2015). In addition, a new "White" category was introduced for industries with pollution scores of 20 and below, for which only notifying the State Pollution Control Board was necessary, and so no CTO was required. This was meant to include industries with negligible pollution levels, such as the use of vacuum forming machines to make biscuit trays from rolled PVC sheet (CPCB, 2016). After the reform, 60 industries were classified as Red, 83 as Orange, 63 as Green, and 36 as White (CPCB, 2016). Some industries were split into multiple categories based on their production process or use of raw materials (CPCB, 2016).

There were several motivations for this change in policy. It was expected to reduce paperwork, improve administrative efficiency, speed up the consent process, create a more user-friendly and industry-friendly environment, and give pollution control boards more time for inspection and reporting (Kulkarni, 2015). It was hoped the move would help improve economic growth, remove red tape, and increase the ease of doing business, particularly for small and medium enterprises, though critics worried it would increase both air and water pollution, favoring industrial interests over environmental concerns (Chauhan, 2015; Kanchan, 2020). Another aim of the policy was to harmonize how industries were classified throughout India (CPCB, 2016). Previously, classification had largely based on industry size and resource use, or rather than on pollution and likely health impacts (CPCB, 2016). The pollution scoring system was meant to overcome the perceived "random" basis of classification (CPCB, 2016). The Central Pollution Control Board also claimed the policy would aid self-assessment by industries (CPCB, 2016).

It is the downward re-categorization that we take as our principal measure of exposure to treatment. Firms that were reclassified from Red to Orange or Green, or from Orange to Green saw their costs of remaining in business fall, because their renewal periods were made longer.[3] Further, they could expect fewer inspections, since inspections are mandated less frequently in lower categories. Prior to December 2019, Red industries were to be inspected every 3 years, while Or-

[3]We do not consider industries classified as White, since they are not required to appear in our data.

ange industries were to be inspected every 5 years, and Green industries every 7 (Kanchan, 2020). Firms reclassified from Red to Orange or Green also saw restrictions on locating in ecologically fragile or protected areas removed.

### 2.2.3 Size-based regulations

In the first part of our bunching analysis, we focus on three industries for which size-based cutoffs are applied, and for which there are sufficiently large numbers of applications for us to conduct standard tests for bunching (Kleven, 2016). These are: industries emitting wastewater, building and construction, and rice mills. The applicable cutoffs are as follows:

1. For industries emitting wastewater, the relevant cutoff is a discharge of 100 kiloliters per day. Before 2016, this cutoff was not relevant to how industries were categorized. Afterwards, applications with more than 100 kiloliters per day in wastewater discharge were generally classified as "red," while those at or below the cutoff were generally coded "orange."
2. For building and construction, the relevant cutoff is a built-up area of 20,000 square meters. Applications above this cutoff are generally classified as "red," while those at or below the cutoff are generally coded "orange."
3. For rice mills, the relevant cutoff is the capacity to produce ten tons of rice per day. Applications above this cutoff are generally classified as "orange," while those at or below the cutoff are generally coded "green."

In the second part of our bunching analysis, we focus specifically on the state of Haryana. The fees charged for both CTE and CTO applications are based on total capital investment, and follow a step function with several notches. We show the fees charged and how these depend on capital investment in Table 2.1. It is clear that there are notches at several points. Expressed in *lakhs* (units of 10,000 rupees in which the raw data are reported), these are: 2, 10, 25, 50, 100, 300, 1,000, 5,000, and 10,000. We test for bunching it capital investment at these notches.

## 2.3 Data

### 2.3.1 Overview of data

The data on individual applications is taken from each state's Online Consent Management & Monitoring System (OCMMS). This system allows online submis-

Table 2.1: Haryana Fee Schedule

| | Fee in Rs. | | |
|---|---|---|---|
| | | CTO | |
| Size (in 10,000s of Rs.) | CTE | First Year | Subsequent Years |
| *Red* | | | |
| $> 10{,}000$ | 105,000 | 150,000 | 75,000 |
| $> 5{,}000$ but $\leq 10{,}000$ | 60,000 | 120,000 | 60,000 |
| $> 1{,}000$ but $\leq 5{,}000$ | 36,000 | 90,000 | 45,000 |
| $> 300$ but $\leq 1{,}000$ | 24,000 | 60,000 | 24,000 |
| $> 100$ but $\leq 300$ | 17,700 | 30,000 | 11,000 |
| $> 50$ but $\leq 100$ | 14,500 | 15,000 | 4,500 |
| $> 25$ but $\leq 50$ | 7,500 | 6,000 | 3,000 |
| $> 10$ but $\leq 25$ | 4,500 | 1,500 | 1,500 |
| $> 2$ but $\leq 10$ | 2,250 | 600 | 600 |
| $\leq 2$ | 600 | 300 | 300 |
| *Orange and Green* | | | |
| $> 10{,}000$ | 35,000 | 50,000 | 25,000 |
| $> 5{,}000$ but $\leq 10{,}000$ | 20,000 | 40,000 | 20,000 |
| $> 1{,}000$ but $\leq 5{,}000$ | 12,000 | 30,000 | 15,000 |
| $> 300$ but $\leq 1{,}000$ | 8,000 | 20,000 | 8,000 |
| $> 100$ but $\leq 300$ | 5,700 | 10,000 | 3,700 |
| $> 50$ but $\leq 100$ | 4,500 | 5,000 | 1,500 |
| $> 25$ but $\leq 50$ | 2,500 | 2,000 | 1,000 |
| $> 10$ but $\leq 25$ | 1,500 | 500 | 500 |
| $> 2$ but $\leq 10$ | 750 | 200 | 500 |
| $\leq 2$ | 200 | 100 | 200 (water), 100 (air) |

*Notes:* The table presents fee charged for both CTE and CTO applications along with relevant capital investment cutoffs for Haryana. The top half of the table provides this information for the "red" category while the bottom half refers to both "orang" and "green". This information is take from OCMMS of Haryana State Pollution Control Board.

sion of Consent to Establish (CTE) and Consent to Operate (CTO) applications (MoEFCC, 2019).[4] Under The Water Act 1974 and The Air Act 1981, all firms likely to discharge sewage, trade effluent, or air pollution are required to obtain Consent to Establish before establishing or expanding (Ghosh *et al.*, 2018). In our data, we observe firms that apply for CTE either to establish new units, expansion of existing units, or renewal or extension of existing CTE. Firms that require environmental clearance and have already obtained it can get a CTE

[4]Firms can also use this system to apply for authorization for bio-medical waste.

with up to 7 years validity, and for rest of the firms the validity period is 5 years (HSPCB, 2017).

Once the new unit has been established or expansion has taken place, a firms must apply for Consent to Operate before commencing operation. A CTO application could be for consent to pollute air, water, or both. A CTO has a limited validity period that depends on the pollution category of an industry, and a firm can apply for any number of years up to or including than the maximum limit. Thus, the total fee for CTO depends on the pollution category as well as the validity period requested by a firm. At this stage, firms are also likely to be inspected on the status of pollution control measures taken by the firm and whether these are consistent with the measures the firm has reported (HSPCB, 2017). Lastly, firms are required to apply for CTO renewals 90 days before the expiry of existing consent (HSPCB, 2017). At the time of renewals, firms are also required to submit analysis reports on effluent and emissions, if applicable.

Firms submit consent applications at their respective State Pollution Control Board's OCMMS. The OCMMS is active in 24 States and Union Territories (MoEFCC, 2019).[5] It is further integrated with the State Government Single Window System for nine States.[6] In an application form, some details and documents are mandatory. Additionally, each State can also include further fields to collect more data. However, these restrictions vary by state and thus some variables we collect are only available for some states. The required documents usually verify the information provided in the form, such as the proposed location of the firm or its anticipated capital investment. For example, the Kerala Pollution Control Board requires firms to submit an Affidavit "regarding under-appreciated value of the fixed assets of the industry".

An application can only be submitted by an authorized official of a firm (HSPCB, 2017). Each application is forwarded to the concerned Assistant Environmental Engineer (AEE) who can also return the application if it is incomplete or if any required document is missing. The final order on an application is an administrative decision (Ghosh *et al.*, 2018). After a complete application has been submitted, authorities might visit the unit if an inspection is required. On the basis of the inspection report and recommendations of inspectors, the Environmental Engineer can decide whether to approve or refuse an application. In our

[5]Andhra Pradesh, Bihar, Chhattisgarh, Chandigarh, Daman & Diu, Haryana, Jharkhand, Jammu & Kashmir, Kerala Meghalaya, Odisha, Punjab, Puducherry, Telangana, Tripura, Tamil Nadu, Uttar Pradesh, Sikkim, Delhi, Goa, Himachal Pradesh, Assam, Uttakhand and Andaman & Nicobar

[6]Andhra Pradesh, Chattisgarh, Haryana, Jharkhand, Kerala, Odisha, Tamil Nadu, Telengana and Punjab.

data, we observe both successful and unsuccessful CTE and CTO applications. Pending applications are also present in the data. For each application, we download a filled form available on each State's OCMMS and also an accompanying PDF version of an application. The data used in the analysis is extracted from either of these two files for each application.

We are able to download individual applications from the following 9 States: Andhra Pradesh, Haryana, Jharkhand, Kerala, Odisha, Tamil Nadu, Telengana, Punjab, and Uttar Pradesh. Since in each State OCMMS was implemented at a different time, the coverage over time is not consistent across States. Uttar Pradesh is the last State to adopt OCMMS in our data and, given there is no data before 2017, we are unable to use it in our difference-in-differences framework. Andhra Pradesh is another State where we observe hardly any applications before 2017. For rest of the 7 States, we have data for at least four quarters before the re-categorization policy came into effect, and so our difference-in-differences results rely on these 7 States. Finally, the data used in the analysis was last accessed on January 2019 and therefore our temporal coverage is limited to 2015-2018. For the 7 States in our analysis, we have identified a listing of 376,528 CTE and CTO applications over the period 2015-2018 that is taken from Central Pollution Control Board OCMMS.[7] We have been able to download 367,916 individual applications from this listing. For the remaining 2.3% of applications, we have been unable to access the data.

### 2.3.2 Summary statistics

Summary statistics for the samples relevant to our bunching analyses are reported in Table 2.2. We report the variables on which the size-based thresholds are based for each of the relevant categories. Further, in the case of Haryana, we report descriptive statistics for both total capital investment and the capital-labour ratio.

## 2.4 Empirical Strategy

### 2.4.1 Difference-in-Differences: Recategorization

In order to estimate how the policy of re-categorization affects the characteristics of new entrants and how their applications are treated, we consider both

[7]The number for each State is as follows: Haryana (38,304), Jharkhand (28,099), Kerala(138,501), Odihsa (21,698), Punjab (77,113), Tamil Nadu (62,583), and Telangana (10,230)

Table 2.2: Summary Statistics for Bunching Analysis

| | (1) Mean | (2) s.d | (3) Min | (4) Max | (5) N |
|---|---|---|---|---|---|
| Industries Reporting Wastewater | | | | | |
| Effluent Discharged | 391.00 | 21,428.72 | 0 | 1,600,000.00 | 5,780 |
| *Building and construction* | | | | | |
| Built-Up Area | 11,353,722.20 | 202,727,441.66 | 0 | 5,391,696,384.00 | 4,092 |
| *Rice Mills* | | | | | |
| Rice Capacity | 160.38 | 1337.18 | 0 | 41,340.00 | 4,954 |
| *Applications in Haryana* | | | | | |
| Capital | 42,639,791.00 | 4197537701.21 | 0 | 504041406000.00 | 43,553 |
| Capital Labour Ratio | 248,710.35 | 23,526,374.28 | 0 | 3,360,275,968.00 | 41,537 |

*Notes:* For building and construction, the relevant cutoff is a built up area of 20,000 square meters. Applications above this cutoff are generally classified as red while those at or below the cutoff are generally coded as orange. For rice mills, the relevant cutoff is the capacity to produce ten tons of rice per day. For industries emitting wastewater, the relevant cutoff is a discharge of 100 kiloliters per day. This cutoff was only introduced in 2016. The data presented here is neither winsorized or trimmed. The bottom part of the table presents statistics only for applications submitted in Haryana. The effluent discharge is in kiloliter per day. Build-up area is in square meters. Rice capacity is in tons per day. Capital investment is expressed in *lakhs Rs.*.

difference-in-difference and event study approaches. In particular, we estimate the following two specifications:

$$y_{ijst} = \alpha + \beta Treatment_j \times Post_t + \delta_{st} + \eta_{sj} + \theta_{jt} + \epsilon_{ijst} \tag{2.1}$$

and

$$y_{ijst} = \alpha + \sum_t \beta_t Treatment_j + \delta_{st} + \eta_{sj} + \theta_{jt} + \epsilon_{ijst} \tag{2.2}$$

Here, $y_{ijst}$ is an outcome variable for application $i$ in industry $j$, submitted in state $s$ in month $\times$ year $t$. We consider measures of firm size, such as the natural logarithm of the total number of workers or the natural logarithm of total capital investment reported in the application. For some states, we have additional outcome variables. For Tamil Nadu, the reported data include reported expenditure on pollution abatement. We also consider measures of the application process, namely whether an application was accepted and, conditional on being rejected, whether we see the same firm apply again for permission for the same activity within twelve months. For Kerala, the internal email records of the SPCB will allow us to consider outcomes such as whether the site was inspected in future work.

$\alpha$ is a constant. The variable $Treatment_j$ is an indicator for whether industry $j$ was re-categorized downwards during the 2016 policy change. In our baseline specification, we take re-categorization from Red to Orange as an indicator of treatment, retaining only those that remained Red or remained Orange throughout as the comparison group. $Post_t$ is an indicator that is equal to 1 once the policy is enacted, i.e. beginning in the second quarter of 2016. In an alternative, we use Orange to Green re-categorization as the measure of treatment, keeping comparison categories only applications that were Orange throughout or were Green throughout.

We control for a number of fixed effects. $\delta_{st}$ is fixed effects for state $\times$ year $\times$ month. $\eta_{sj}$ are state $\times$ industry fixed effects. $\theta_{jt}$ are fixed effects for pollution score $\times$ year $\times$ month. Because pollution scores have been used to determine color categories since the policy came into effect, these are critical to our estimation. They narrow our focus to comparisons between industries that have equal pollution potential, but that were classified differently prior to the second quarter of 2016. That is, we identify $\beta$ by comparing how applications in the same industry, in the same state, change after re-categorization, compared to the change in other industries, while allowing for flexible time trends that can differ across states and for each discrete pollution score. We cluster standard errors by industry.

In the event study specification (equation (2.2)), we estimate a different coefficient on treatment, $\beta_t$ for each quarter in the data. The omitted category is the first quarter of 2016, which is the final pre-treatment quarter in the data. All coefficients, then, can be interpreted as the divergence of industries re-categorized downwards relative to other industries, benchmarked against the gap that existed just before the policy took effect. If estimates of $\beta_t$ before 2016 are not statistically different from zero, this is evidence in favor of the parallel trends assumption.

In order to evaluate whether the recategorization policy led to a change in the number of applications received, we estimate modified versions of (2.1) and (2.2). In particular, we estimate:

$$y_{jst} = \alpha + \beta Treatment_j \times Post_t + \delta_{st} + \eta_{sj} + \theta_{jt} + \epsilon_{jst} \tag{2.3}$$

and

$$y_{jst} = \alpha + \sum_t \beta_t Treatment_j + \delta_{st} + \eta_{sj} + \theta_{jt} + \epsilon_{jst} \tag{2.4}$$

In both equations, $y_{jst}$ is the log number of applications received in industry $j$, in state $s$, in year $\times$ month $t$. $Treatment_j$ and $Post_t$ are defined as in (2.1) and (2.2). As before, we include state $\times$ year $\times$ month fixed effects $\delta_{st}$, state $\times$ industry fixed effects $\eta_{sj}$, and pollution score $\times$ year $\times$ month fixed effects $\theta_{jt}$. The unit of observation is now the industry $\times$ state $\times$ year $\times$ month cell, and we limit our sample to cells in which at least one application was made. We continue to cluster by industry.

### 2.4.2 Bunching Analysis

We will begin our bunching analysis by plotting simple descriptive histograms of the number of applications observed in various size bins. More formally, we test whether these deviations from a smooth distribution around the cutoffs are statistically significant using the local polynomial density estimators proposed in Cattaneo *et al.* (2017). In particular, we employ unrestricted density estimation, a triangular kernel, the optimal bandwidth based on the mean squared error, local quadratic approximations both to construct the density point estimators and the bias-corrected density point estimators, and jackknife standard errors. We report p-values corresponding to the robust bias-corrected statistic.

In order to provide suggestive evidence for whether bunching is due to misreporting or to actual changes in firm size, we draw on Velayudhan (2018). If, in the case of Haryana, firms are simply misreporting the amount of total capital investment, without misreporting other inputs that are not relevant to the fee schedule, we would expect the level of reported capital relative to the number of workers to be conspicuously low for levels of capital investment just below the threshold in the fee schedule. To test for this, we estimate:

$$Y_i = \alpha + \beta BunchingRegion_i + \gamma Capital_i + \delta Capital_i^2 + \epsilon_i \qquad (2.5)$$

Here, $Y_i$ is, in alternative specifications, the ratio of total capital investment to the total number of workers (the capital-labour ratio) or the total number of workers. $\alpha$ is a constant. $BunchingRegion_i$ is a dummy for a level of total capital investment that between 95% of the relevant threshold and the threshold itself. $Capital_i$ is total capital investment. We estimate (2.5) separately for each of the relevant cutoffs in the data, and retain applications within a neighborhood defended by the next adjacent fee thresholds in the data. We treat red industries as one sample, and orange and green industries taken together as a second sample. We report robust standard errors. If firms misreport capital to stay at or below the relevant cutoff, we expect $\beta$ to be negative when the outcome is the capital-labour ratio (capital is low relative to the number of workers) or positive when the outcome is the total number of workers (labor is high relative to capital).

# 2.5 Results

## 2.5.1 Recategorization

We present our estimates of (2.1) and (2.3) in Table 2.3. In the top panel, we consider industries that were recategorized from Red to Orange, in comparison with those that were always Red, or always Orange. In the bottom panel, we consider industries recategorized from Orange to Green in comparison to those that were always Orange or always Green. The sample consists only of new, i.e. Consent to Establish, applications. For a sub-set of outcomes, we are concerned that results may result mechanically from the fact color categorization depends on a size-based threshold such as total wastewater discharge. In these cases, we also report results for a sub-sample that omits these categories.

In the first column of Table 2.3, we show that industries that were classified downwards from Red to Orange saw an increase in the number of applications. In the full sample, this corresponds to a roughly ($e^{0.270} - 1 \approx 0.31$) 31% increase in the number of applications. Excluding size-based classifications, the magnitude (24%) is similar, but not statistically significant. Although applications in these categories were more than 4 percentage points more likely to be accepted after the change (column 2), this is not statistically significant. Applications that were rejected initially are less likely to be followed by another application by the same firm for the same activity – what we call reapplication – within one year (column 3). New entrants in treated industries were smaller, both in terms of total workers (column 4) and total capital investment (column 5). In the full sample new firms have 23% fewer workers. Without size-based classifications, the magnitude is 32%. The comparable reductions in total capital investment are 20% and 26%, though the estimate is only statistically significant when excluding size-based classifications.

The marginal new entrant is, as a result of the policy, one that spends less on pollution abatement, wether this is measured in abatement costs per worker (column 6) or in total pollution control costs (column 7). Abatement per worker declines by 8-10%, though this is only significant in the full sample. Total pollution control costs fall between 51% and 100%, and this is significant in both samples. In the bottom part of the table, we show that these effects are largely confined to firms that were reclassified from Red to Orange. Excepting a reduc-

Table 2.3: Differences in Differences Results: CTE Applications

| | (1) ln(Number of Applications) | (2) Accepted | (3) Reapplication within one year | (4) ln(Total Workers) | (5) ln(Total Capital Investment) | (6) ln(Abatement Expenditure per Worker) | (7) ln(Pollution Control Costs) |
|---|---|---|---|---|---|---|---|
| Panel A: Treatment is Recategorization from Red to Orange | | | | | | | |
| *Including Size-Based Categories* | | | | | | | |
| Treatment X Post | 0.270** | 0.0463 | -0.115* | -0.208*** | -0.187 | -0.0844* | -0.413* |
| | (0.131) | (0.0308) | (0.0606) | (0.0717) | (0.119) | (0.0436) | (0.224) |
| Observations | 4,580 | 13,887 | 1,666 | 11,123 | 14,216 | 1,544 | 1,566 |
| Outcome Mean | 0.640 | 0.820 | 0.559 | 2.677 | 4.926 | 0.154 | 1.288 |
| *Excluding Size-Based Categories* | | | | | | | |
| Treatment X Post | 0.215 | 0.0117 | -0.0671 | -0.278*** | -0.231* | -0.0990 | -0.713*** |
| | (0.156) | (0.0152) | (0.0720) | (0.0549) | (0.123) | (0.0633) | (0.194) |
| Observations | 3,584 | 10,602 | 1,294 | 9,154 | 10,828 | 1,325 | 1,341 |
| Outcome Mean | 0.633 | 0.813 | 0.611 | 2.610 | 4.659 | 0.165 | 1.206 |
| Panel A: Treatment is Recategorization from Orange to Green | | | | | | | |
| *Including Size-Based Categories* | | | | | | | |
| Treatment X Post | -0.206 | 0.0781 | -0.00745 | -0.119 | -0.00562 | -0.0128 | -0.166* |
| | (0.171) | (0.0556) | (0.0780) | (0.115) | (0.199) | (0.0169) | (0.0967) |
| Observations | 3,847 | 16,512 | 1,486 | 12,281 | 16,873 | 592 | 605 |
| Outcome Mean | 0.703 | 0.875 | 0.477 | 1.542 | 3.151 | 0.0950 | 1.039 |
| *Excluding Size-Based Categories* | | | | | | | |
| Treatment X Post | -0.196 | 0.0894 | -0.0119 | -0.0985 | 0.00504 | -0.00952 | -0.148 |
| | (0.175) | (0.0540) | (0.0825) | (0.110) | (0.207) | (0.0175) | (0.102) |
| Observations | 3,149 | 13,555 | 1,133 | 10,597 | 13,826 | 451 | 462 |
| Outcome Mean | 0.662 | 0.881 | 0.510 | 1.344 | 2.634 | 0.107 | 0.816 |

*Notes:* *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors clustered by industry in parentheses. In Panel A, Treatment is defined as industries recategorized from Red to Orange, and the sample also includes industries that were categorized as Orange before and after the policy change. In Panel B, Treatment is defined as industries recategorized from Orange to Green, and the sample also includes industries that were categorized as Green before and after the policy change. Column 6 and 7 only use data from Tamil Nadu as information on Pollution Control Cost is not available for other tion in pollution control costs, we do not find statistically significant responses to the Orange to Green recategorization treatment.

The corresponding event study estimates of (2.2) and (2.4) are in Figures 2.1 and 2.2 for the Red-to-Orange treatment. Our event study results confirm these results. In most panels of figures 2.1 and 2.2, there is no evidence of violation of the parallel trends assumption. In two cases (number of applications and total capital investment, both excluding size-based categories), significant deviations reflect a one-off deviation in a single pre-treatment period, and not a broader trend. The number of applications rises gradually in treated industries, reaching a new level after roughly six quarters. The responses of entry of firms with fewer workers and lower levels of capital investment are more gradual, emerging after four to six quarters. The entry of firms that spend less on abatement emerges relatively early, though the gap between treated and untreated firms closes within three years. Excluding size-based categories gives very similar patterns.

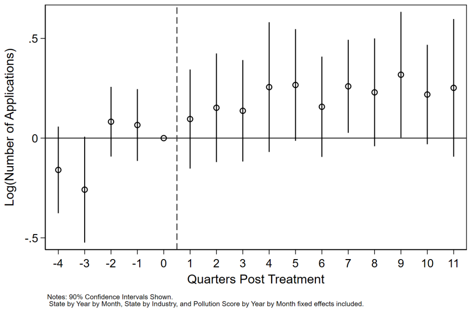
### 2.5.2 Bunching

#### Industry-Specific Cutoffs

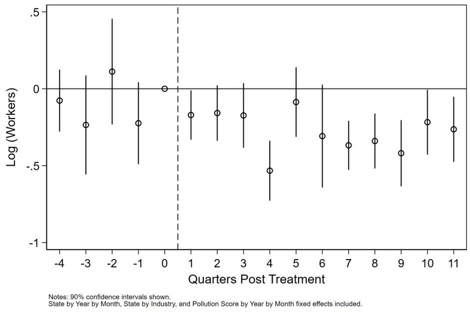
We plot histograms of the number of applications observed in each of the industries we consider by size, and we split the sample of firms that emit wastewater by whether they applied before 2016 or in 2016 and after. These are presented in Figure 2.3. For wastewater, we truncate the figures from the left, since there is a large mass of firms that have either very low levels of wastewater discharge or total capital investment. In all cases, there is evidence of substantial bunching at or below the relevant cutoffs. That is, the distribution of firms is not smooth around each cutoff, and usually exhibits a large spike at or just below the cutoff.

Turning to the formal analysis, we report p-values for discontinuities in the distribution using Cattaneo *et al.* (2017) local polynomial density estimators in Table 2.4, and plot the density estimates in Figure 2.4. The results show that the deviations from a smooth distribution around the cutoff are significant at the 1% level for all but one of the distributions. The exception is the placebo check: bunching at or below 100 kiloliters of wastewater is not significant at conventional levels prior to 2016. In each of the density plots, the deviation of the distribution from the local polynomial is plainly visible.

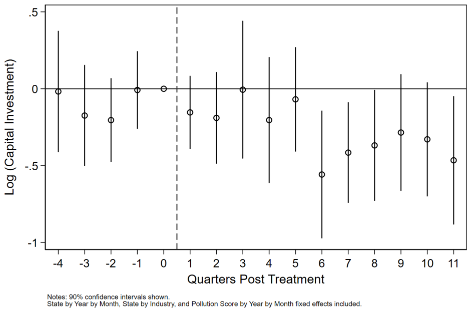
We note a caveat with these results. Since we are only able to observe the applications, rather than independently measured values of the cutoff values, we



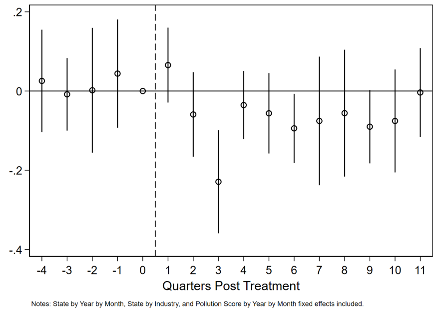

(a) ln(Number of Applications)



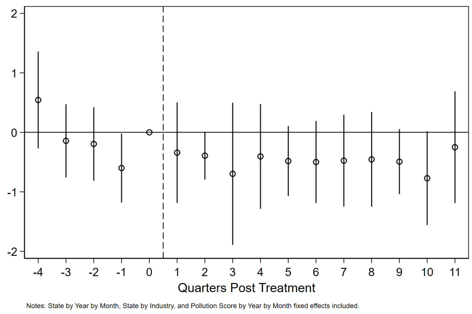

(b) ln(Total Workers)




(c) ln(Total Capital Investment)




(d) ln(Abatement Expenditure per Worker)



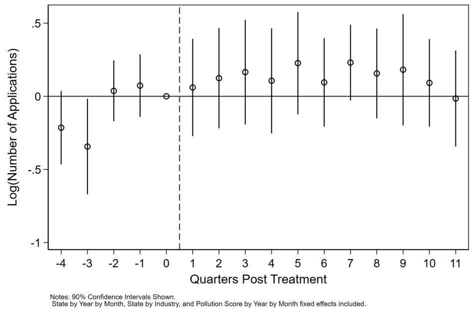

(e) ln(Pollution Control Costs)

Figure 2.1: Event Study Results: With Size-Based Categories

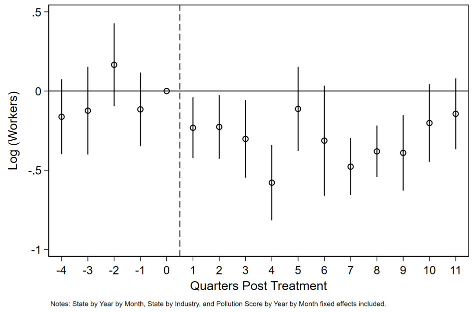
are unable to distinguish actual bunching from misreporting. Similarly, we have many fewer observations for each specific cutoff than Velayudhan (2018), and so cannot use the distributions of other firm characteristics around these thresholds to distinguish misreporting from bunching.

### Notches in the Haryana fee schedule in Haryana

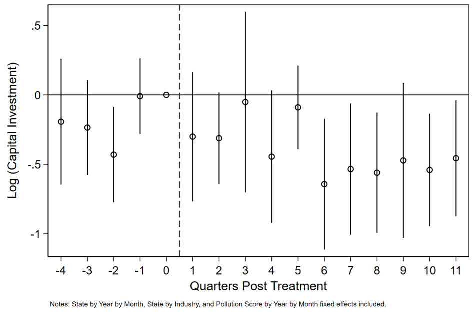
We now turn to considering notches in the fee schedule in Haryana. As before, we begin by plotting descriptive histograms of the number of applications observed by level of capital investment in lakhs – i.e. units of 10,000 rupees. Histograms for red industries in a window around each cutoff are presented in Figures 2.5



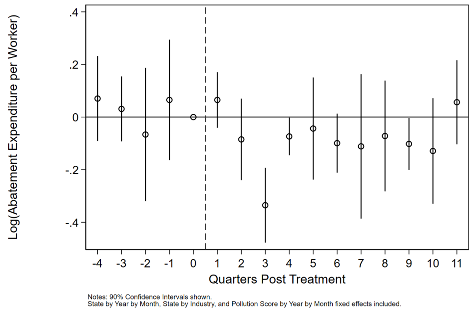

(a) ln(Number of Applications)



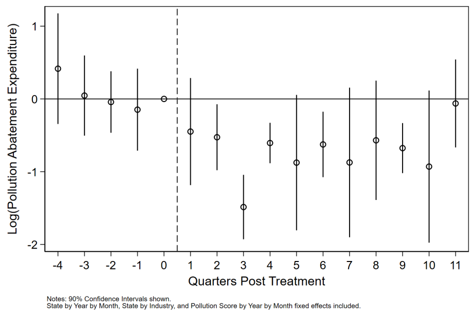

(b) ln(Total Workers)




(c) ln(Total Capital Investment)




(d) ln(Abatement Expenditure per Worker)




(e) ln(Pollution Control Costs)

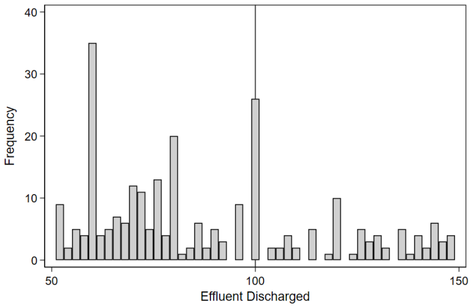
Figure 2.2: Event Study Results: Without Size-Based Categories

and 2.6. Because the distribution of capital investment is highly skewed, we plot these histograms for neighborhoods around each cutoff.

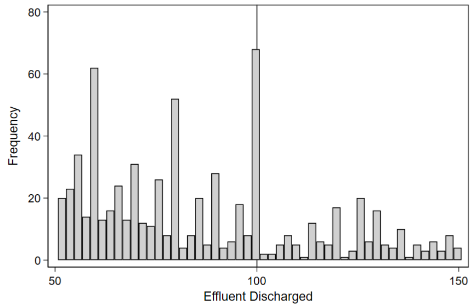
For red industries, there is substantial evidence of bunching below many of the relevant thresholds, and the excess mass of firms with capital investment just below the cutoffs of 5,000, 1,000, 300, 100, 50, 25, and 10 is particularly apparent.

For orange and green industries, there is again evidence of bunching both below and in some cases just at the relevant thresholds. This is most visually plain at cutoffs of 5,000, 1,000, 300, 100, 50, and 25.

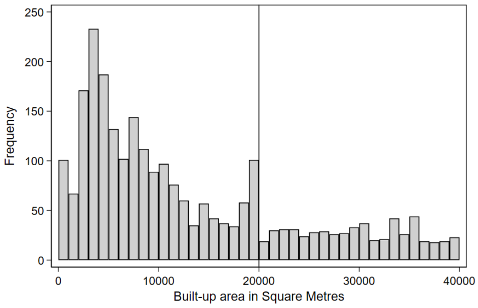
As with the industry-specific cutoffs above, we test whether this bunching is statistically significant using the Cattaneo *et al.* (2017) local polynomial density



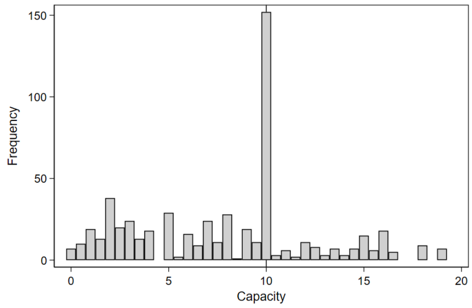

(a) Wastewater: Before 2016




(b) Wastewater: 2016 and later

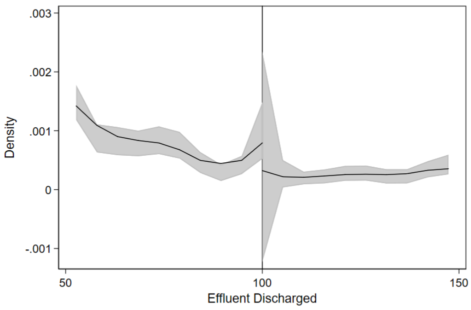



(c) Building and construction

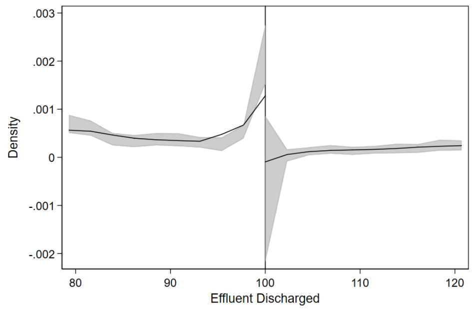



(d) Rice mills

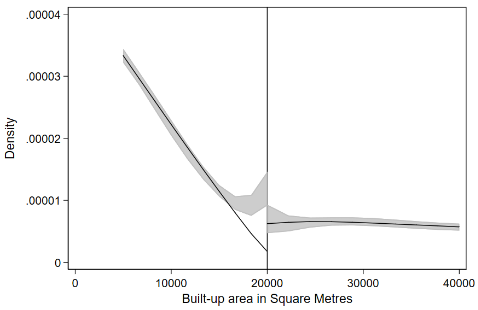
Figure 2.3: Histograms



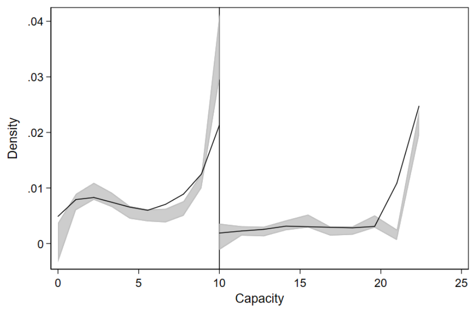

(a) Wastewater: Before 2016




(b) Wastewater: 2016 and later




(c) Building and construction




(d) Rice mills

Figure 2.4: Density Tests

estimators. We report p-values in Table 2.4. Because of the large number of tests, we omit density plots for space. The results show that the deviations

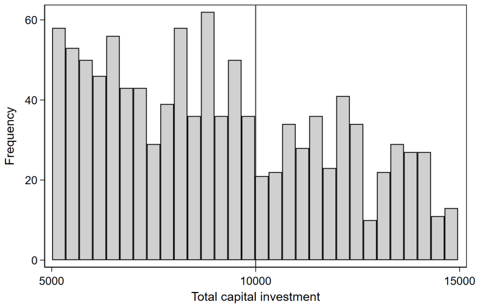
Table 2.4: McCrary Density Tests

| | p Value | |
|---|---|---|
| *Sample* | | |
| Building and construction | 0.008 | |
| Rice Mills | 0.000 | |
| Wastewater: 2016 and later | 0.001 | |
| Wastewater: Before 2016 | 0.640 | |
| | | |
| *Investment Cutoff* | Orange and Green | Red |
| 2 | 0.042 | 0.795 |
| 10 | 0.091 | 0.115 |
| 25 | 0.000 | 0.000 |
| 50 | 0.172 | 0.000 |
| 100 | 0.000 | 0.000 |
| 300 | 0.000 | 0.001 |
| 1000 | 0.000 | 0.000 |
| 5000 | 0.157 | 0.000 |
| 10000 | 0.131 | 0.042 |

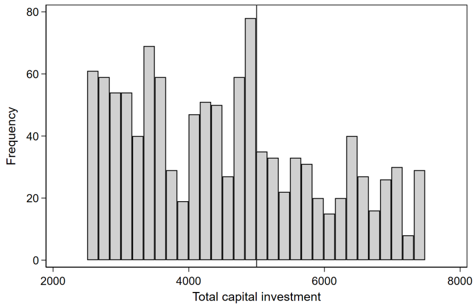
*Notes:* For building and construction, the relevant cutoff is a built up area of 20,000 square meters. Applications above this cutoff are generally classified as red while those at or below the cutoff are generally coded as orange. For rice mills, the relevant cutoff is the capacity to produce ten tons of rice per day. For industries emitting wastewater, the relevant cutoff is a discharge of 100 kiloliters per day. This cutoff was only introduced in 2016. The bottom part of the table refer to the fee charge for both CTE and CTO applications at different cutoffs of total capital investment in Haryana.

from a smooth distribution are significant at the 1% level for ten of the eighteen cutoffs considered, are significant at the 5% level for two others, and at the 10% level for one other. The cutoffs where bunching is insignificant at conventional levels include the two largest cutoffs for orange and green industries and the two smallest for red industries – parts of the distribution that is more sparse, given that red industries tend to be relatively large, while orange and green industries are relatively small.

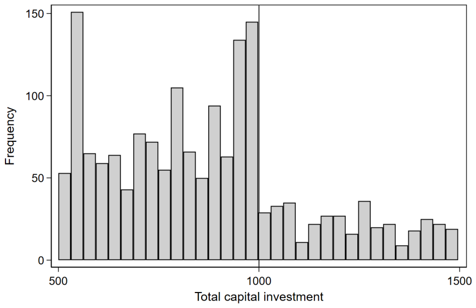
Because of the large number of applications to which these size-based cutoffs apply, we have sufficient power to provide suggestive evidence for whether this bunching constitutes misreporting. Our estimates of equation (2.5) are reported in Table 2.5. There is only limited evidence of misreporting. For red industries, the capital-labor ratio deviates significantly downward from the level predicted by a quadratic polynomial in labor in the regions just below the cutoffs of 5,000 and 1,000. For orange and green industries, this is the case at the cutoff of 10,000. For total workers, there is evidence at the 1,000 lakh cutoff that there are



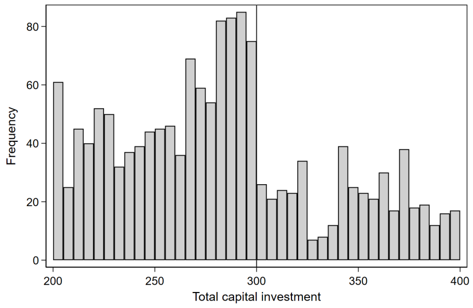

(a) Neighborhood of 10,000 lakhs




(b) Neighborhood of 5,000 lakhs

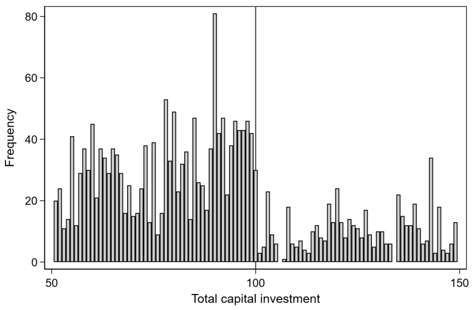



(c) Neighborhood of 1,000 lakhs

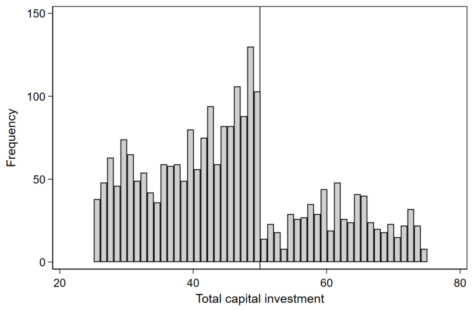



(d) Neighborhood of 300 lakhs

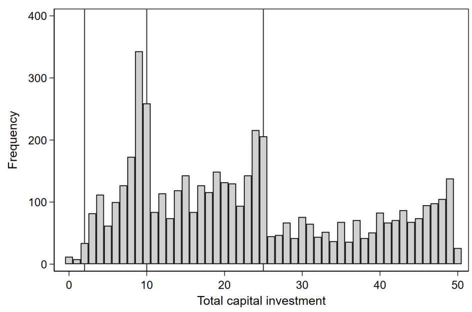
Figure 2.5: Histograms by level of capital investment: Larger cutoffs for red industries




(a) Neighborhood of 100 lakhs

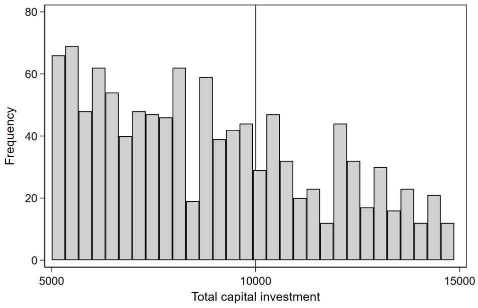



(b) Neighborhood of 50 lakhs

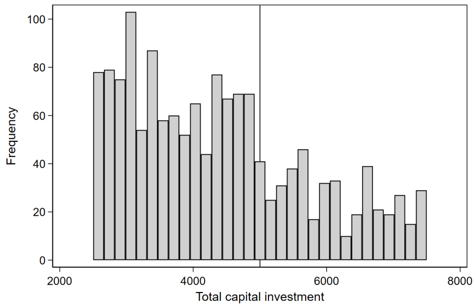



(c) Smaller cutoffs

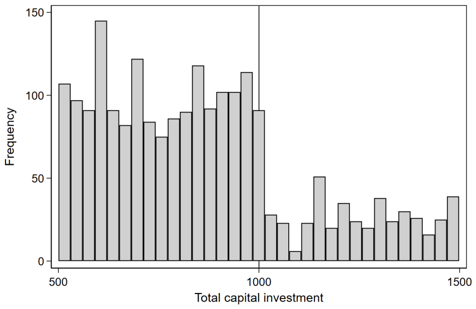
Figure 2.6: Histograms by level of capital investment: Smaller cutoffs for red industries



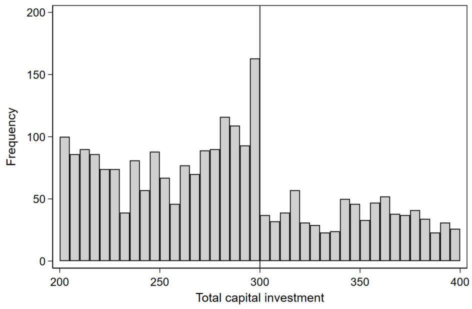

(a) Neighborhood of 10,000 lakhs




(b) Neighborhood of 5,000 lakhs

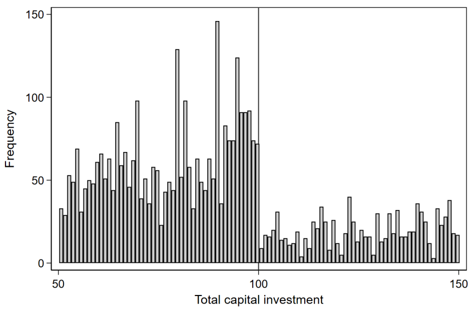



(c) Neighborhood of 1,000 lakhs

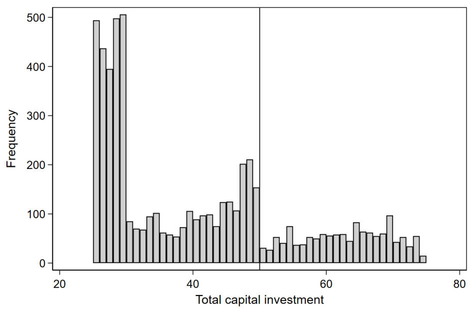



(d) Neighborhood of 300 lakhs

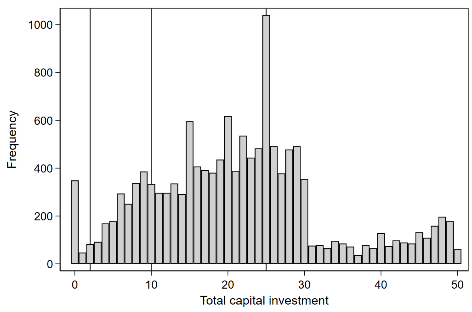
Figure 2.7: Histograms by level of capital investment: Larger cutoffs for orange and green industries




(a) Neighborhood of 100 lakhs

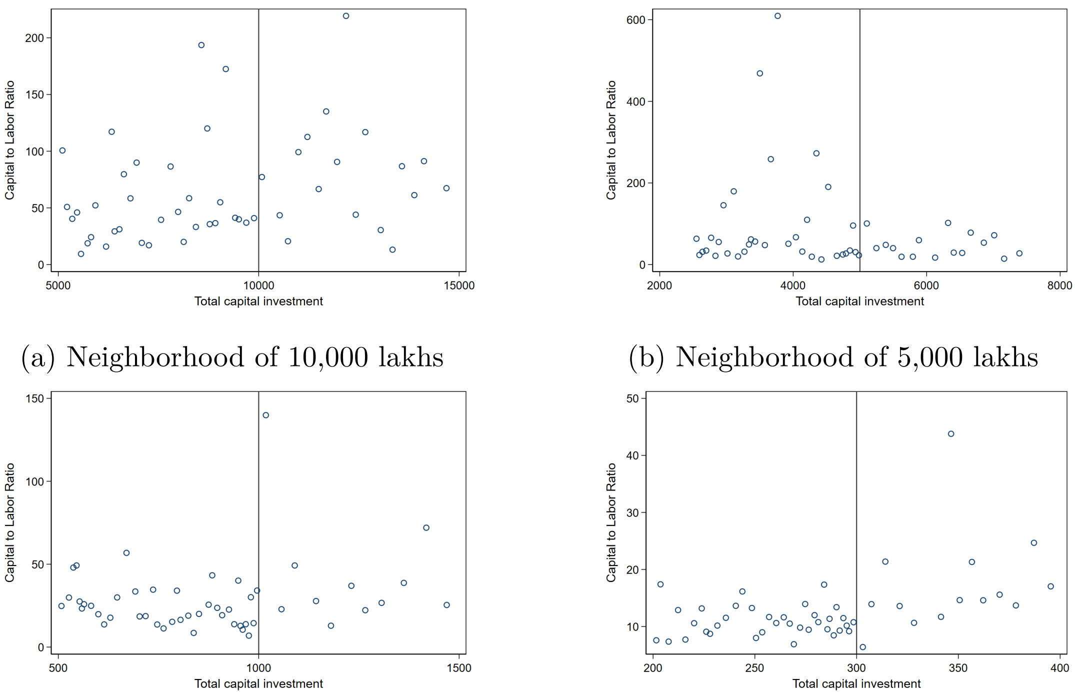



(b) Neighborhood of 50 lakhs




(c) Smaller cutoffs

Figure 2.8: Histograms by level of capital investment: Smaller cutoffs for orange and green industries




(a) Neighborhood of 10,000 lakhs

(b) Neighborhood of 5,000 lakhs

(c) Neighborhood of 1,000 lakhs

(d) Neighborhood of 300 lakhs

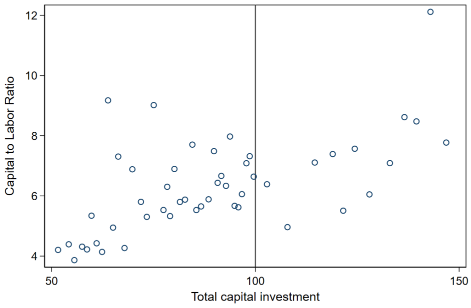
Figure 2.9: Binscatter plots of the capital-labor ratio against capital: Larger cutoffs for red industries

more workers than otherwise expected in the bunching region. For the majority of cutoffs in the data, however, there is either no significant deviation from a quadratic polynomial in the bunching region, or the deviation suggests there is less labor relative to capital than expected, rather than more. Importantly, this is true for the cutoffs where we have the largest samples, and hence greatest power to detect deviations of this type.

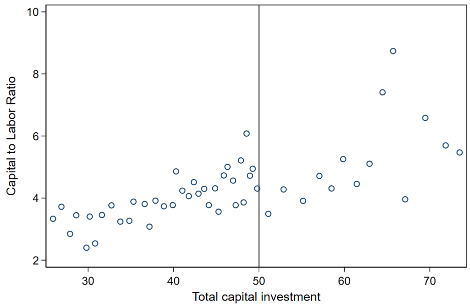
In Figures 2.9, 2.10, 2.11, and 2.12, we show binned scatterplots that supplement the above regression analysis. They show how the capital-labor ratio varies with total capital investment. There is, again, only very limited evidence of misreporting in these figures. The capital-labor ratio is not conspicuously lower to the left of the relevant thresholds. Where downwards deviations from the quadratic polynomial were significant in Table 2.5, it is clear from the figures that this may simply reflect the degree of volatility in the data, and not a systematic pattern.

## 2.6 Conclusion

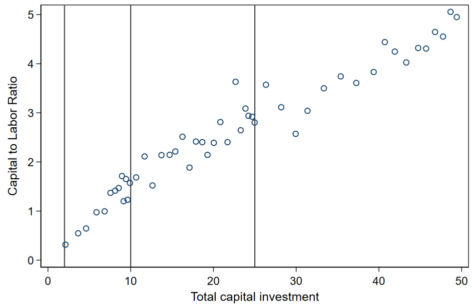
In this paper, we have assembled a novel web-scraped database of applications by Indian firms seeking permission to pollute from their States' Pollution Control Boards. Using more than half a million records with daily observations of both attempts to enter and of successful entry, we have shown that environmental




(a) Neighborhood of 100 lakhs

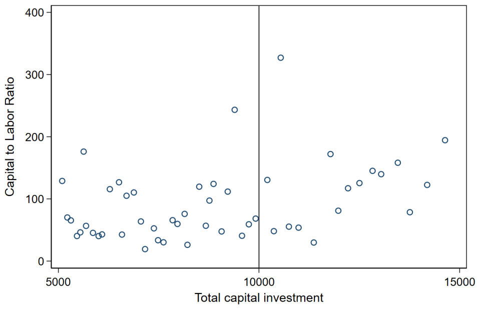



(b) Neighborhood of 50 lakhs

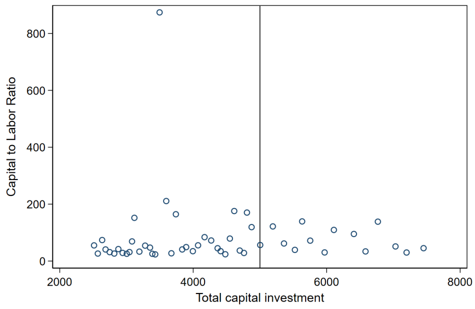



(c) Smaller cutoffs

Figure 2.10: Binscatter plots of the capital-labor ratio against capital: Smaller cutoffs for red industries

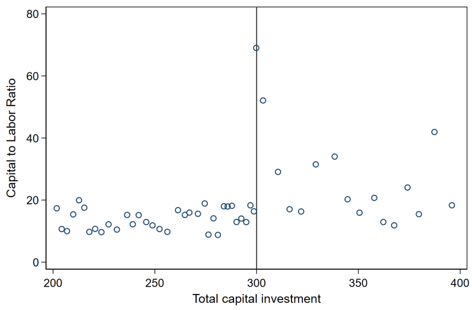



(a) Neighborhood of 10,000 lakhs



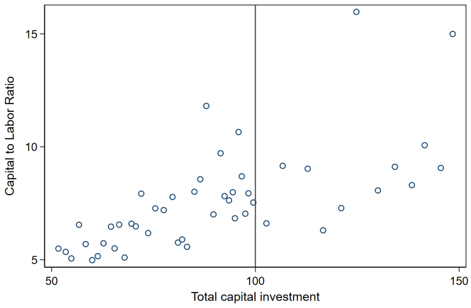

(b) Neighborhood of 5,000 lakhs


(c) Neighborhood of 1,000 lakhs

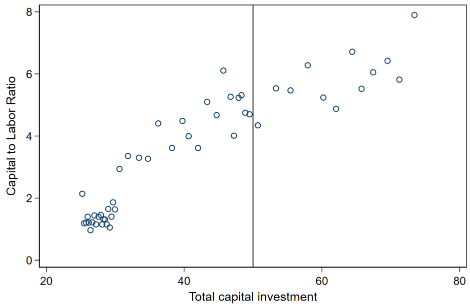



(d) Neighborhood of 300 lakhs

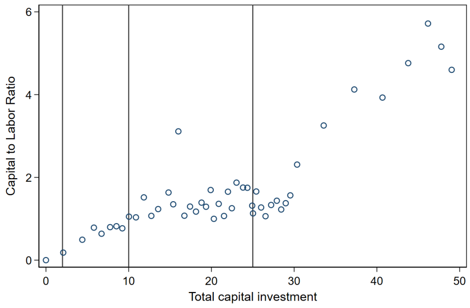
Figure 2.11: Binscatter plots of the capital-labor ratio against capital: Larger cutoffs for orange and green industries




(a) Neighborhood of 100 lakhs




(b) Neighborhood of 50 lakhs




(c) Smaller cutoffs

Figure 2.12: Binscatter plots of the capital-labor ratio against capital: Smaller cutoffs for orange and green industries

regulations do substantially affect the size of firms in the market. Cutoffs in terms of size, output, and capacity that affect how a firm is classified lead to bunching of firms below these thresholds, as do notches in the fee schedule in the state of Haryana. We show that the country's 2016 re-categorization reform, by reducing the burdens of inspection, reapplication, and location restrictions, encouraged the entry of smaller firms in terms of both capital and labor.

These results have consequences for the literatures on environmental regulations and the firm size distribution, in particular in developing countries. We have shown that environmental regulations do indeed change the composition of firms that both try to enter the market and that do successfully enter the market. There are a large number of environmental regulations and fee schedules that induce firms to report lower values of output, inputs, and size, or to actually distort these. Evidence from tests similar to those in Velayudhan (2018), as well as descriptive results, suggest that many of these effects are real, and not solely due to misreporting.

Table 2.5: The relationship of capital to labour near thresholds in the fee schedule

| | | Capital Labour Ratio | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | *Red Industries* | | | | *Orange and Green Industries* | | | |
| *Cutoff* | *Sample* | $\beta$ | (s.e.) | | N | $\beta$ | (s.e.) | | N |
| 10000 | 5000 to 15000 | -17.617 | [ 11.618 ] | | 966 | -36.428 | [ 13.128 ] | *** | 966 |
| 5000 | 2500 to 7500 | -67.087 | [ 17.478 ] | *** | 1,074 | 3.198 | [ 25.711 ] | | 1,358 |
| 1000 | 500 to 1500 | -11.438 | [ 3.772 ] | *** | 1,587 | 2.184 | [ 2.411 ] | | 2,012 |
| 300 | 200 to 400 | -0.351 | [ 1.052 ] | | 1,426 | 2.969 | [ 2.391 ] | | 2,369 |
| 100 | 50 to 150 | -0.16 | [ 0.327 ] | | 2,018 | 0.169 | [ 0.467 ] | | 3,970 |
| 50 | 25 to 75 | 0.385 | [ 0.169 ] | ** | 2,267 | -0.32 | [ 0.194 ] | * | 5,515 |
| 25 | 10 to 50 | 0.163 | [ 0.102 ] | | 3,525 | -0.099 | [ 0.060 ] | | 10,814 |
| 10 | 0 to 25 | 0.069 | [ 0.072 ] | | 3,091 | 0.156 | [ 0.100 ] | | 8,052 |
| | | Total Workers Including Officials | | | | | | | |
| | | *Red Industries* | | | | *Orange and Green Industries* | | | |
| | | $\beta$ | (s.e.) | | N | $\beta$ | (s.e.) | | N |
| 10000 | 5000 to 15000 | -331.012 | [ 85.010 ] | *** | 978 | -39.708 | [ 81.491 ] | | 980 |
| 5000 | 2500 to 7500 | -38.69 | [ 35.884 ] | | 1,086 | -43.98 | [ 72.159 ] | | 1,378 |
| 1000 | 500 to 1500 | 44.143 | [ 14.263 ] | *** | 1,593 | -30.358 | [ 22.867 ] | | 2,026 |
| 300 | 200 to 400 | -12.058 | [ 9.786 ] | | 1,447 | -12.754 | [ 7.818 ] | | 2,379 |
| 100 | 50 to 150 | -19.328 | [ 10.613 ] | * | 2,019 | -14.389 | [ 9.658 ] | | 3,985 |
| 50 | 25 to 75 | -12.407 | [ 11.518 ] | | 2,267 | 1.301 | [ 4.462 ] | | 5,525 |
| 25 | 10 to 50 | 22.797 | [ 24.830 ] | | 3,538 | 2.872 | [ 5.782 ] | | 10,833 |
| 10 | 0 to 25 | -1.236 | [ 2.228 ] | | 3,107 | -3.862 | [ 2.503 ] | | 8,073 |

*Notes:* This table report estimate from equation (2.5). It follows Velayudhan (2018) and provides suggestive evidence on whether bunching reported in the figures constitutes misreporting. Since the cutoff are different for red industries, we present results for red industries separately, and orange and green industries separately. Robust standard errors are in square brackets.

# 3 Social Movements and Gender Integration

*with Aiman Farrukh*

## 3.1 Introduction

Social movements and protests are forms of collective action where participants generally have mutual interests in a social or policy change. The literature on factors that drive protest participation is vast and growing (Finkel *et al.*, 1989; Finkel and Opp, 1991; Barbera and Jackson, 2019; Enikolopov *et al.*, 2019; García-Jimeno *et al.*, 2018). A small number of papers have also looked at the causal effects of protests on political outcomes and attitudes (Madestam *et al.*, 2013; Mazumder, 2019). However, there is limited evidence on the causal effects of protests or social movements on the economic outcomes of participants.

In this paper, we examine how a struggle for property rights by tenant farmers in Pakistan affected labor force participation of women and household investment in girls' education. The movement was initiated in 1999 by landless farmers across several districts of Punjab in response to a proposed change in lease agreements (Sayeed and Haider, 2010; Mumtaz and Mumtaz, 2012). Over time, nearly a million farmers joined the movement that is known as *Anjuman-e-Mazareen Punjab* (AMP), and the key objective evolved from resisting proposed change in the lease agreements to acquiring land ownership. The arrest of men by police and paramilitary forces at the start of the movement pushed many women to join the struggle. Over time, women became an important part of the movement by actively participating in protests and leading processions (Mumtaz and Mumtaz, 2012). This active engagement of women is unusual for a country where most women at the time were mobility constrained and had relatively low labor force participation, especially in rural areas.

We use 16 rounds of Labor Force Survey Data (1990-2013) and a difference-in-differences approach to study how women labor force participation evolved in districts where AMP movement was active in 1999. To understand whether this movement had any affect on the younger cohort, we also study changes in educational attainment of girls. There are four key features of our empirical strategy. First, since the AMP movement is still on-going and farmers have not been able to gain the ownership of the land, we can study the effect of this social movement on participant without worrying about the effect of actual policy change. Second, to test the validity of the parallel trend assumption, we show how outcomes evolved across AMP and non-AMP districts for nearly a decade before the treatment. Third, since the movement only affected the rural population, we are able to use the sample of women from urban areas as another layer of control in a triple difference specification to account for any differential change among rural women to rule out changes in public good provisions in AMP districts. Lastly, since we have more than 10 years of post-treatment data, we are able to study the long-term effects of AMP movement. To do so, we divide the sample into older cohorts (women that had already completed education by 1999) and a younger cohort (girls that were either in school in 1999, or were younger than school-going age). Then, we compare the educational attainment of the younger and older cohorts within the same household, and study whether it differs across AMP and non-AMP districts. In a separate specification, to further account for any household-level changes, we add men as an additional layer of control in this inter-cohort triple difference specification. These features allow us to estimate the causal effects of participation of women in the AMP movement on their labor force participation and investment in girls' education.

We find that women in rural parts of AMP districts show a sharp decrease in labor force participation in years immediately after the start of the movement. This pattern changes in 2003 where we observe an increase in women working in the AMP districts. Finally the effect disappears after 2008 when police brutality dropped significantly due to a change in provincial and federal government after 2008 elections. Overall, we show that after the start of the movement, women are 6 percentage points more likely to be working in the AMP districts relative to the non-AMP districts. We find that the increase in labor force participation is mostly concentrated in the agricultural sector and is stronger for households that were previously active in that sector. These results suggest that women in the AMP district joined the labor force temporarily during a time when men were facing arrests and harassment.

We observe an increase in investment in girls' education in the AMP districts. We find that after the start of the movement there is 3.3 percentage points increase in the enrollment of girls in AMP districts relative to non-AMP districts. The effects are even stronger when we look at completion of primary schooling where we see an increase of 2.8 percentage points relative to baseline mean. This pattern becomes even more clear when we look at inter-cohort triple difference approach where we compare changes in the gender gap in educational attainment between older and younger cohort across AMP and non-AMP districts. Overall, we find that in AMP districts there is a nearly 3.6 percentage points increase in the probability of finishing secondary school for girls when compared with boys in the same household. Consistent with the evidence above, these results are stronger for households that were involved in agricultural activities or where household members had lower educational attainment at the baseline.

The results are robust to alternative specifications and a number of household and individual-level control variables such as marital status, household head gender, and number of female members in a household. We also find the results to be consistent when we use intensity of AMP movement, measured in terms of land under lease relative to total cultivable land in a district, rather than presence of AMP movement as the treatment variable. We also show that these results are not sensitive to the exclusion of the capital of Punjab province (Lahore) that although an AMP district, might follow differential trends in women labor force participation or girls' school enrollment. We also implement a placebo test where we assign treatment status to all the neighboring districts of AMP districts and show that these effects disappear if we only consider neighboring districts as treated. This suggests that our results are not picking up any changes that are common to those regions where AMP movement started. We show that these results are not explained by differential treatment of AMP districts by the state or an overall increase in new public schools in the rural areas. We also rule out the possibility that rural-urban migration is deriving our results. Finally, the estimates from event studies are also in line with the results described above.

Some qualitative studies suggest that active participation in the movement for long period of time also increased women's role in decision making with-in a household (Mumtaz and Mumtaz, 2012; Basu, 2016; Khan and Kirmani, 2018). This channel might also explain our results related to educational attainment. To test this hypothesis, we put together multiple rounds of publicly available Pakistan Social and Living Standards Measurement surveys. We find some suggestive evidence that, after 1999 women in the rural areas of AMP districts became more actively involved in making decisions related to education, employment and marriage. This effect becomes statistically significant only after nearly eight years of the start of the movement, which indicates that changes in intra-household bargaining power might take several years to institutionalize.

### 3.1.1 Contribution

We contribute to two strands of literature. First, there is literature studying the causal effects of social movements. Studies in this literature have looked at impacts of social movements on political outcomes and attitudes (Madestam *et al.*, 2013; Mazumder, 2019, 2018), stock market performance (Acemoglu *et al.*, 2018), and property value (Collins and Margo, 2007). However, the evidence on how protests or movements affect economic outcomes of participants is very limited. They key challenge is to separate the effect of an actual policy change from that of a long-lasting social movement. Since the AMP movement did not lead to any policy change and protest lasted for long period of time, we have an excellent setting to deal with this challenge. Therefore, we contribute to this literature by providing an estimate of the causal effect of a social movement on both short-term and long-term economic outcomes of the individuals that were exposed to a social movement. For the short-term outcomes, we focus on women labor force participation, therefore this paper is also related to the literature studying the effects of demographic shocks on female labor force participation (Teso, 2019; Acemoglu *et al.*, 2004; Goldin and Olivetti, 2013; Fernández *et al.*, 2004).

We also contribute to the literature studying gender gaps in school enrollment in developing countries. Previous studies have looked at both demand (such as conditional cash transfers (Fiszbein and Schady, 2009)) and supply side interventions (such as school construction (Duflo, 2001; Kazianga *et al.*, 2013) or improved access (Muralidharan and Prakash, 2017)). A related strand of this literature looks at how increase in women's intra-household bargaining power affects investment in girl's education (Doss, 2013; Qian, 2008) or how cash transfers that are targeted towards women leads to an increase in children's education (Duflo, 2012; Saavedra and Garcia, 2012). We contribute to this literature by providing evidence that increase in women's decision making power within a household due to their involvement in a social movement, can also reduce gender gaps in educational attainment in a developing country.

The rest of the paper proceeds as follows. In the next section we discuss the role of women in the AMP movement and describe the data. Then, in Section 3, we present our identification strategy. In Section 4, we present the main results, robustness, and mechanisms. We conclude in Section 5.

## 3.2 Context and Data

### 3.2.1 The Anjuman-e-Mazareen Punjab Movement

Anjuman-e-Mazareen Punjab movement reflects a struggle over tenancy agreement between tenant farmers and the military establishment. Initiated in 2000, the movement currently represents nearly a million tenant farmers contesting around 70,000 acres of agricultural land in ten districts of Punjab, Pakistan.

The movement began independently in two different districts of Punjab, namely, Okara and Khanewal in response to government proposals to sharecropping agreements on state lands into rental contracts.. Subsequently, it spread across other districts and eventually combined under a common banner of the Tenants Association of Punjab or Anjuman-e-Mazareen Punjab (Sayeed and Haider, 2010).

The roots of this conflict can be traced back to the time period of British Raj. Under the Punjab Tenancy Act 1887, the British settled farmers from different parts of Punjab to take care of uncultivated land. Part of the settled farmers cultivated the land under a sharecropping agreement which is commonly know as *battai*; where the Raj retained the property rights and tenant farmers paid rent in kind (Khan and Akhtar, 2014). The Punjab Colonization Land Act 1912 leased the land to the Royal British Army until 1933 which was later extended until 1938 (Rizvi, 2017).

After independence, the Pakistani Army took over the military farms and tenants farmers continued the cultivation under *battai* system. In May 2000, the Army changed the agreement from sharecropping to cash-based contract (Rizvi, 2017). The new contract system impaired land security of the tenant farmers for three reasons. First, although the lease period was set at seven years, it was subject to annual renewal depending on fixed payments paid by the farmers. Second, farmers can be evacuated if the land is required for defense purposes. Third, contractors can not claim occupancy tenancy right or ownership rights (Choudry and Kapoor, 2010; Khan and Akhtar, 2014). The tenant farmers were convinced that the new system will take away the rights promised under Punjab Tenancy Act of 1898, and therefore decided to reject the cash-based contract (Rizvi, 2017; Sayeed and Haider, 2010). These circumstances became the motivation for collective action in the form of Anjuman-e-Mazareen Punjab movement (AMP).

The initial protests staged by the AMP were based on rejecting the new contract leasing system and preventing the military establishment from replacing local tenant farmers with non-village contract farmers. However, with time, as the tenants involved in the movement became more organized, they came to the realization that the military was not the legal owners of the land they were contesting for, rather it was legally under the ownership of the provincial government. This evolved their objectives to demanding land ownership instead (Ali, 2014; Sayeed and Haider, 2010).

The movement was initially active in only two districts, but within a few months, the protests spread to nine other districts of Punjab where the farmers were threatened with evictions by the military for continued protests. Due to a long withstanding resistance from the AMP farmers and condemnation from national and international media, the military rangers were withdrawn in August 2003 and local farmers were able to cultivate on their lands again. To date the AMP is still active in the struggle for achieving legal ownership of the military farmlands.

### 3.2.2 Women's Role

The significant involvement by women is an interesting feature of the movement, particularly when considering their engagement alongside the opposite gender in a conservative country such as Pakistan (Fleschenberg, 2015). Initially, women physically shielded men in police encounters during protests in order to protect them from being arrested, but thereafter, they became involved in planning, mobilizing and moving across villages to spread their message, recruitment, attending court hearing and speaking at public events (Basu, 2016). Qualitative research suggests that affiliation with the movement became the basis of exposure, mobility and confidence among women (Basu, 2016; Khan and Kirmani, 2018). Basu (2016) conducted a number of interviews in AMP districts and reported a reduction in the level of domestic violence towards women, gender segregation owing to increased mobilization and a rise in preference towards education for girls. The following testimonies of a participants (Mumtaz and Mumtaz, 2012) capture the involvement of women:

> "When I saw the police enter our village and ask around for Younus Iqbal, I thought here is this man fighting for us, we should help him. I got the women of the village together and we stopped the police before they could get to Younus Iqbal. We said, 'You have come to arrest Younus Iqbal, well we are all Younus Iqbal; arrest any one of us'. On seeing us resisting like this they left." (Mumtaz and Mumtaz (2012), page 143)

> "Women have worked shoulder to shoulder with men in this movement. In fact women have often gone a step ahead of men. To organise women we went door to door to convince them that if under the contract system their land is taken away how will they meet the needs of their families?" (Mumtaz and Mumtaz (2012), page 143)

These findings motivate us to study how women's involvement in the movement affected their labor market choices and household's decision regarding investment in girls' education.

### 3.2.3 Data

We use 16 rounds (1990-2012) of nationally representative Labor Force Survey (LFS) data to study the impacts of the AMP movement.[1] Motivated by the discussion above, we are interested in studying how the AMP movement affected outcomes for women in rural areas. The data allow us to look at both older and younger cohorts of women. For the older cohort, we focus on labor force participation and occupation choices, whereas for the the younger cohort we look at enrollment and educational attainment.

The data covers seven rounds before 1999 and eight afterwards. This coverage allows us to look at the pre-trends over a long time period. The key challenge we face in assembling our dataset is to have a consistent set of districts across all the rounds. Since during this time, there are number of districts that were split in two, we follow the list of districts that were present in the very first round (1990) and match the remaining districts and their splits into this initial list. This leads to 58 consistent districts across Pakistan.

Next, we use publicly available data to capture whether the AMP movement was active in a district. Furthermore, we collect data on the amount of land under military establishment in each district to calculate the intensity of treatment. This provides us with 9 districts with some land under military.

Lastly, to understand how women's role in decision making has changed over time, we also use five surveys (1998, 2001, 2005, 2007, and 2011) of the Pakistan Social and Living Standards Measurement (PSLM). We use variables that capture the involvement of women in decision making related to education, employment, and marriage. In each survey, women of age 15-49 in a household were asked to report who decides a) whether you can seek or continue to get education b) whether you can seek or remain in paid employment and c) whether and when you should be married?. From the responses, we can pick up whether a women herself or any other women from a household has a say in these decisions. Since AMP movement could also possibly affect involvement of women in decision making, we use PSLM data to to test this hypothesis. Following the same approach as LFS, we collect information on districts and then make adjustments to merge splits into the parent districts. The downside of using PSLM is that we only have one pre-treatment time period (1998). However, given that there are no other surveys that cover such a large number of districts across Pakistan and

[1] The reason for limiting the post-treatment rounds to 2012-13 is a change in the sampling frame adopted by the Labor Force Survey from 2013 on wards. In 2012 the list of enumeration blocks was updated through Economic Census 2003 and the list of villages/mouzas/dehs of 1998 Population Census were taken as sampling frames. But from 2013, the list of enumeration blocks was updated from field on the prescribed proforma by Quick Count technique and revised during House Listing in 2011 for conduct of Population Census taken as sampling frames. (LFS Report)

include consistent set of questions on women decision making, we have limited options in this regard.

Table 3.1 presents summary statistics for rural women. The first panel looks at the older cohort that is within the age range of 16-60 (years). For this cohort, we are interested in labor force participation and occupation choice. The table shows that on average only 17% of women are working and most of the them are employed in the agriculture sector. In the second, we present statistics for the younger cohort (age 10-15) that is within the age range of 5-15. For this group, we are mostly interested in educational attainment. In this sample, less than half of the girls are currently enrolled and only 13% have more than five years of education.

Table 3.1: Summary Statistics

| **Panel A: Older Cohort** | | | | | |
|---|---|---|---|---|---|
| | count | mean | sd | min | max |
| age | 397123 | 32.47 | 12.17 | 16.00 | 60.00 |
| whether currently working | 397123 | 0.17 | 0.37 | 0.00 | 1.00 |
| total number of hours worked last week | 397123 | 7.05 | 14.87 | 0.00 | 99.00 |
| works in agriculture | 71976 | 0.83 | 0.37 | 0.00 | 1.00 |
| **Panel B: Younger Cohort** | | | | | |
| | count | mean | sd | min | max |
| age | 117023 | 12.39 | 1.72 | 10.00 | 15.00 |
| whether currently enrolled | 117023 | 0.45 | 0.50 | 0.00 | 1.00 |
| whether has more than five years of education | 117023 | 0.28 | 0.45 | 0.00 | 1.00 |

*Notes:* Older cohorts include women of age 16-50 and younger cohorts include women of age 5-15. Data taken from LFS surveys 1990-2013. The lower number of observations for variable "works in agriculture" is due unavailability of this information in rounds prior to 1996.

## 3.3 Empirical Strategy

We rely on three empirical strategies. First, we use difference-in-differences where we compare women in AMP districts with non-AMP districts before and after the movement started. Second, we estimate a triple difference model where we use women in urban districts as another layer of control. We use these two strategies to study the impact of AMP on both older and younger cohort as described above. Lastly, we study the differences between two cohort, within the same household in a household fixed effect model. In this section, we will describe each of these three strategies in detail.

### 3.3.1 Difference-in-Differences

We estimate the following difference-in-differences model for both the older and younger cohorts using 16 rounds of repeated cross-section LFS data:

$$Y_{idt} = \beta_1 treat_d \times post_t + \delta_d + \psi_t + \alpha_{idt} + v_{idt} \tag{3.1}$$

$Y_{idt}$ is an outcome variable for woman $i$ in district $d$ in survey year $t$. For outcomes, we look at labor force participation variables for older cohorts and educational attainment variables for younger cohort. Next, $treat_d$ is a dummy variable that is equals to 1 for districts where there is any state-owned land and that was affected by the proposed changes in lease agreement. However, a dummy variable might not capture the intensity of treatment. Therefore, we also estimate the same equation using a "proportion treated" variable instead of a dummy variable. The "proportion treated" variable is the ratio of relevant state-owned land in district $d$ to total cultivable land in district $d$. Since movement started in early 2000, immediately after proposed changes in the contract were announced by the state , *post* is a dummy variable that is equals to 1 for years after 1999. The equation also includes district fixed effects ($\delta_d$) and year fixed effects ($\psi_t$). Finally, we control for age by decade ($\alpha_{idt}$). We cluster standard errors at the district level. To make sure that the results are robust, we also present results with an alternative specification where we account of province by year fixed effects. The variable of interest is $\beta_1$. If the identification assumption is satisfied, then $\beta_1$ should provide causal impacts of AMP movement on the older cohort's labor force participation and the younger cohort's educational attainment. The identification assumption requires that, in the absence of the AMP movement, treatment and control districts should have the same differences over time. Since there is no statistical test for this assumption, we look at the evolution of differences between treated and control districts from 1990 to 1998. For this reason, we estimate following event study equation:

$$Y_{idt} = \sum_{y=1990}^{2013} \gamma_y treat_d \times year_y + \delta_d + \psi_t + \alpha_{idt} + v_{idt} \tag{3.2}$$

We use 1999 as the base year. Therefore, this equation estimates the evolution of differences across treatment and control districts both before and after 1999. The estimates of $\gamma_y$ for the pre-1999 period will provide us an indirect test of parallel trends assumption. The estimates for post-1999 period will show the evolution of treatment effect over time. We continue to cluster the standard errors at the district level.

### 3.3.2 Triple Difference

Since the AMP movement was concentrated only in the rural areas, in the triple difference approach we use urban women as another layer of control. We estimate:

$$\begin{aligned} Y_{irdt} = &+ \mu_4 treat_d \times rural_r \times post_t + \mu_1 treat_d \times post_t + \\ &+ \mu_2 treat_d \times rural_r + \mu_3 rural_r \times post_t + \mu_5 rural_r \\ &+ \delta_d + \psi_t + \alpha_{idt} + v_{idt} \end{aligned} \tag{3.3}$$

The term $rural_r$ is a dummy variable that is equal to 1 if a woman $i$ is from a rural area $r$. The rest of the terms are same as in equation (3.1). The intuition behind this specification is that the AMP movement only mobilized rural women and therefore should not have any impact on urban women. Under the parallel trends assumption, the coefficient on $\mu_4$ would provide us a causal estimate of impacts of AMP movement on rural women. To test the pre-trends, we estimate event study using following equation:

$$\begin{aligned} Y_{irdt} = &\sum_{y=1990}^{2013} \kappa_y treat_d \times rural_r \times year_y + \zeta_2 treat_d \times rural_r + \\ &\sum_{y=1990}^{2013} \lambda_y rural_r \times year_y + \zeta_5 rural_r + \\ &\sum_{y=1990}^{2013} \zeta_y treat_d \times year_y + \delta_d + \psi_t + \alpha_{idt} + v_{idt} \end{aligned} \tag{3.4}$$

Since $treat_d$ and $year_y$ are collinear with district and year fixed effects, we have omitted those terms. The coefficients on $\kappa_y$ for years before 1999 provide us a test for pre-trends. We run the above regression also with "proportion treated" instead of a dummy variable for treated districts and provide robustness to alternative sets of fixed effects. We continue to cluster the standard errors by district.

Another possible way to set up the triple difference is to use rural men rather than urban women as a control. However, given that the literature shows that rural men were directly affected by the AMP movement, it is not clear whether it would be a good control group and therefore we rely on urban women to implement triple difference specification.

### 3.3.3 Inter-cohort Difference

In the two strategies described above, we are looking at older and younger cohorts separately. However, we can also analyze how the difference between these two groups evolves over time. More importantly, we can look at inter-cohort changes over time within a household. We focus on outcomes related to educational attainment in this section. The key reason behind this restriction is that we can focus on the older cohort that was already passed the school going age when the AMP movement started and thus they can not invest any further in their education. Therefore, intuitively we would like to compare the cohorts that, in response to the AMP movement, could not invest in the education (older) with the ones that were still of schooling going age (younger). We estimate:

$$Y_{ihdt} = \beta_1 treat_d \times young_t + \delta_h + \psi_t + \gamma_i + \alpha_{idt} + v_{idt} \tag{3.5}$$

$Y_{ihdt}$ are outcomes related to educational attainment of woman $i$ in household $h$ in district $d$ and year $t$. There are three new terms in this equation as compared to equation (3.1). First, instead of "*post*" we have a dummy variable "*young*" that is equal to 1 for those girls that were school-going age or younger when the movement started and 0 for women that had already completed their education at that time. Second, we have household fixed effects instead ($\delta_h$) instead of district fixed effects. Third, we also have birth year fixed effects ($\gamma_i$) along with survey year fixed effects ($\psi_t$). The key identifying assumption is that in the absence of the AMP movement the inter-cohort differences in educational attainment within a household should not grow at a different rate in the treatment districts as compared to control districts. The household fixed effects absorb any time invariant regional differences that might explain different evolution of inter-cohort differences across treatment and control groups and hence they also absorb district fixed effects. One threat to this identification could be that in treated districts parents' preferences towards investment in children's education could have different growth trajectory as compared to parents' preferences in control group. To address this, we use men's educational attainment as another reference group in the following specification:

$$\begin{aligned} Y_{ihdt} =& \theta_1 treat_d \times young_t + \theta_2 female_{ihd} \\ &+ \theta_3 treat_d \times female_{ihd} + \theta_4 young_t \times female_{ihd} \\ &+ \theta_5 treat_d \times female_{ihd} \times young_t \\ &+ \delta_h + \psi_t + \gamma_{it} + \alpha_{idt} + v_{idt} \end{aligned} \tag{3.6}$$

The term $female$ is a dummy for individuals who are female. The rest of the terms are the same as in equation (3.5). We cluster standard errors by district while estimating equation (3.5) and 3.6. This specification allows us to further control for any differential changes in parents' preferences towards investment in their children's education. Thus we need a weaker identification assumption as compared to the double difference approach. If the identification assumption is satisfied, then $\theta_1$ provides us a causal estimate of the AMP movement on inter-cohort differences in educational attainment.

We go through the following steps in order to define the younger and older cohorts for estimating equation (3.5) and 3.6. First, to explore long-term impacts of the AMP movement, we focus on households that were surveyed 10 years after the movement started (LFS round 2009, 2010, and 2012). Second, we choose an age cut-off of 30 to define the older cohorts since at this age further investment in school education is unlikely. All women that were at least of age 30 in 1999 are part of the older cohort. Third, women that were less than 30 years of age in 1999 or were born after are part of the young cohorts. Finally, we exclude all girls between the age of 0-10 as we are considering completion of primary, middle, and secondary school as our outcome variables.

## 3.4 Results

In this section, we report results from the three empirical strategies described above. We start by examining how the AMP movement affected the older cohort and then we turn our focus towards the younger cohort.

### 3.4.1 Older Cohorts

We are interested in understanding whether the AMP movement affected the labor force participation of rural women residing in treated districts. For this reason, we follow the double and triple difference specification described above and look at the changes in two variables: whether an individual is currently working and the number of hours worked in the previous week. Figure 3.1 reports estimates from equation (3.2) and 3.4. The first row of Figure 3.1 has triple difference estimates with treatment defined as a dummy variable. In the second row, we again have a triple difference estimate but the treatment now captures the intensity as well. Lastly, we look at the double difference estimates in the third row. There are a few important points to note in this figure. First, the estimates on "currently working" (Panel (b), (d), and (f)) show that the pre-trends are parallel. Second, some women in the treated areas stopped working in the year immediately after the movement started. This is in line with qualitative studies that report high number of evictions and arrests right after the start of protests. Third, three years after the treatment we see more women joining the workforce. However, this change starts fading away 8 years post-treatment as we see the coefficient becomes very small and statistically insignificant. This overlaps well with the return of formal democracy in Pakistan in 2008, that also lessened the police brutality against male farmers (Choudry and Kapoor, 2010). This suggest that women in the treated areas might have joined the labor force during the times when male household members were either away from their fields to protest or were more likely to get arrested. The post-treatment pattern in "total hours" estimates (Panel (a), (b), and (c)) is more or less similar to "currently working". However, the pre-trends are not as clean.

We present estimates from equation (3.1) and 3.3 in Table 3.2 and C1 respectively. In each table we have three panels. Panel A reports estimates from the baseline specification. Then, we have estimates from an alternative specification where we also control for province times year fixed effects. Lastly, in Panel C we control for other variables such marital status, total number of household members, proportion of female household members, and whether the household head is female. The estimates for the older cohort are in Column 1 and 2 of both Tables. In line with the evidence presented in the event studies, there is a small increase in the number of women currently working and also the total weekly hours, but the estimates are mostly insignificant partly because the effects were very short-lived. In Panel A of Table 3.2, we find that women in treated districts were 6 percentage points more likely to join the labor force. Compared with baseline mean, this is nearly a 50% increase. This effect is relatively smaller in Table C1 (Panel A, Column 1) where we only see an increase of 3.2 percentage points.

To understand whether there was any change in the occupational choice of women, we look at the probability of working in the agriculture sector in Figure 3.2. The pattern in the figure suggests that women were more likely to join the agricultural sector in treated districts after the AMP movement started. This is in line with the narrative that women entered the workforce to replace absent male farmers that were coping with police brutalities at the time.[2] However, this could also be because there are few jobs available in rural areas, outside of the agricultural sector.

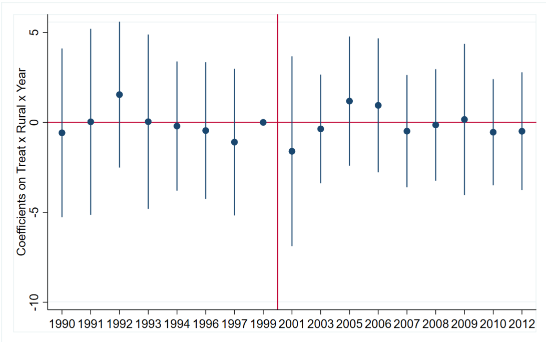
[2]Please note that the number of pre-treatment time periods are smaller in this case relative to earlier results. This is because we were only able to find consistent set of codes for occupation for years after 1996.

Table 3.2: Double Difference Estimates – Proportion Treated

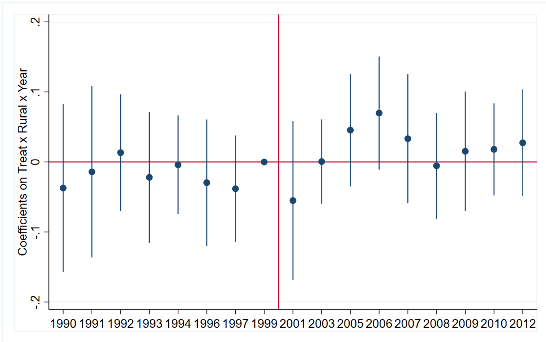
| | Older Cohort | | Younger Cohort | |
|---|---|---|---|---|
| | (1) work | (2) hours | (3) currently enrolled | (4) educ 5+ |
| **Panel A: Baseline Specification** | | | | |
| post=1 × proportion treated | 0.0357** | 0.0173 | 0.0330* | 0.0282*** |
| | (0.0155) | (0.541) | (0.0187) | (0.0103) |
| Observations | 397122 | 397122 | 117023 | 117023 |
| Dependent Variable mean | 0.125 | 4.830 | 0.328 | 0.207 |
| **Panel B: With Province × Year FE** | | | | |
| post=1 × proportion treated | 0.0254 | -0.209 | 0.0365* | 0.0213** |
| | (0.0168) | (0.557) | (0.0202) | (0.00984) |
| Observations | 397122 | 397122 | 117023 | 117023 |
| Dependent Variable mean | 0.125 | 4.830 | 0.328 | 0.207 |
| **Panel C: With Controls** | | | | |
| post=1 × proportion treated | 0.0353** | 0.00143 | 0.0330* | 0.0286*** |
| | (0.0154) | (0.538) | (0.0186) | (0.0103) |
| Observations | 397122 | 397122 | 117023 | 117023 |
| Dependent Variable mean | 0.125 | 4.830 | 0.328 | 0.207 |

*Notes:* ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Standard errors clustered by district in parentheses. All panels present estimates from equation (3.1). Older cohorts include women of age 16-50 and younger cohorts include women of age 10-15. All panels use data on women living in the rural areas. "Proportion treated" is a ratio of state-owned land and total cultivable land in a district. For both set of cohorts, controls include total number of household members, proportion of female household members, and whether the household head is female. For older the older cohorts, we also control for marital status.

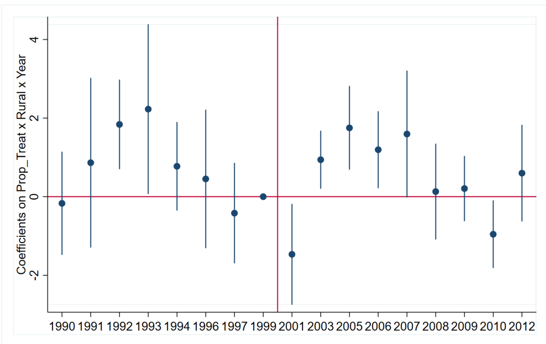
Figure 3.1: Older Cohorts



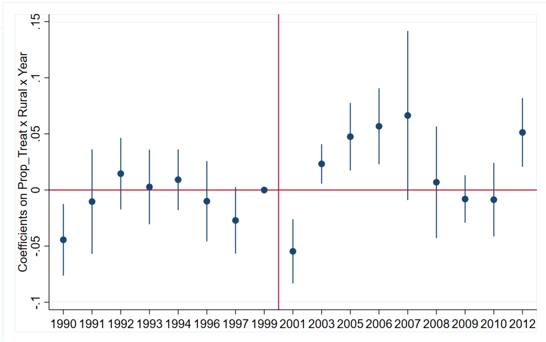

(a) Total Hours – Triple Difference



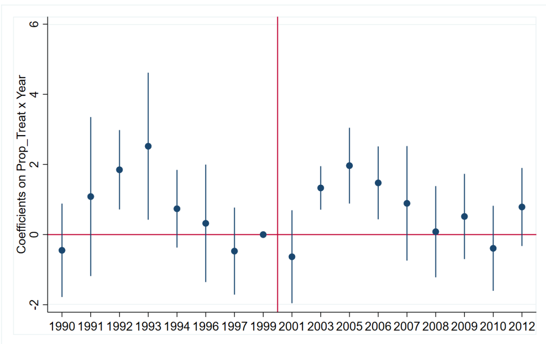

(b) Currently Working – Triple Difference



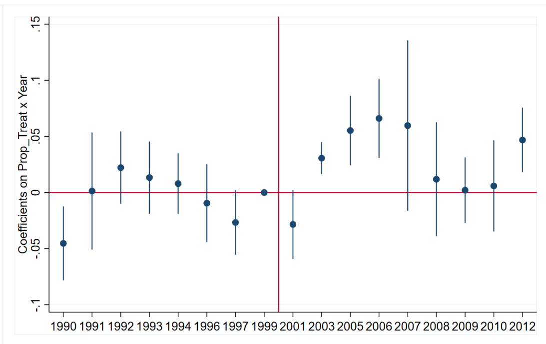

(c) Total Hours – Proportion Treated




(d) Currently Working – Proportion Treated




(e) Total Hours – Double Difference – Proportion Treated



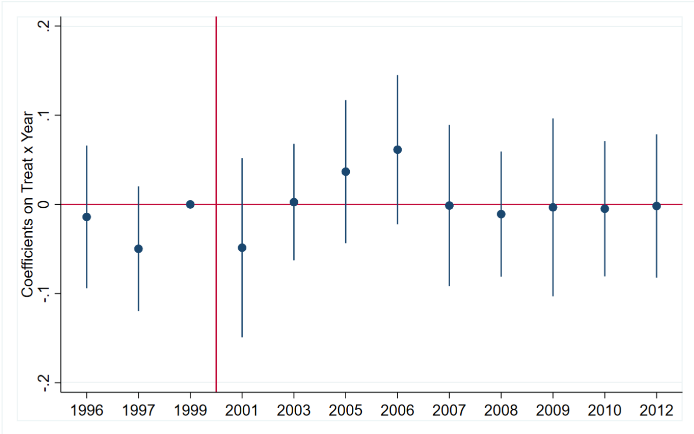

(f) Currently Working – Double Difference – Proportion Treated

*Notes:* Figures plot coefficients from double difference and triple difference specifications. Only data on the older cohort (16-60) is included. Standards errors are clustered by the district and 95% confidence intervals are shown. "Total Hours" is the total number of hours worked in the past week. Panel (a), (b), (c), and (d) plot estimated coefficients of equation (3.4) wheres Panel (e) and (f) plot estimated coefficients of equation (3.2).

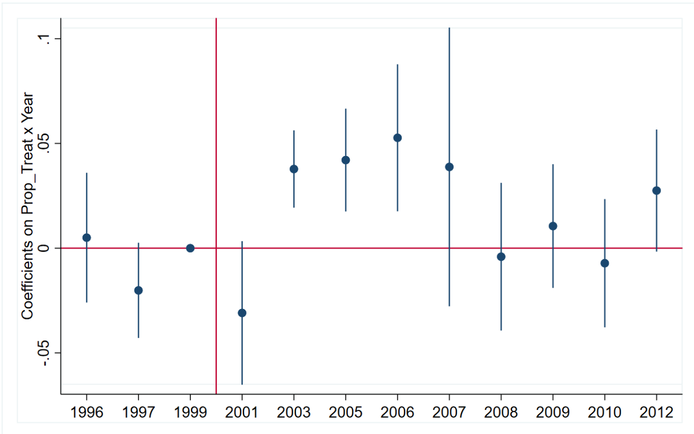
### 3.4.2 Younger Cohorts

For the younger cohort, we are interested in understanding whether there is any change in educational attainment. For this purpose, we keep focus on three

Figure 3.2: Older Cohorts – Occupation Choice




(a) Agriculture – Double Difference

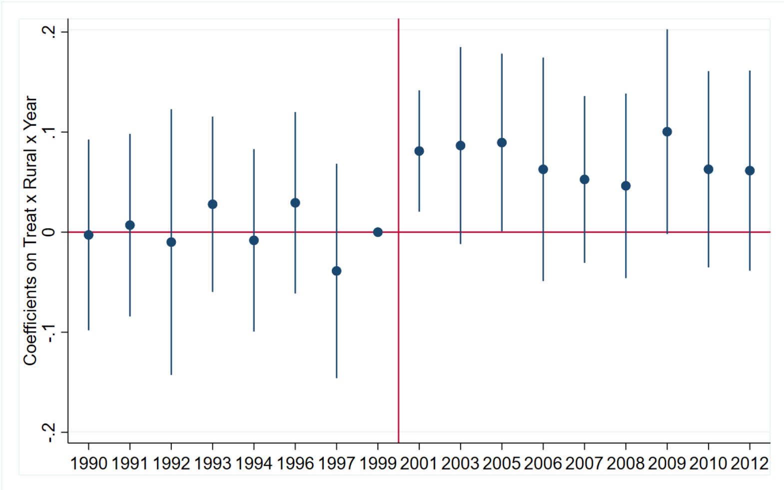



(b) Agriculture – Double Difference – Proportion Treated

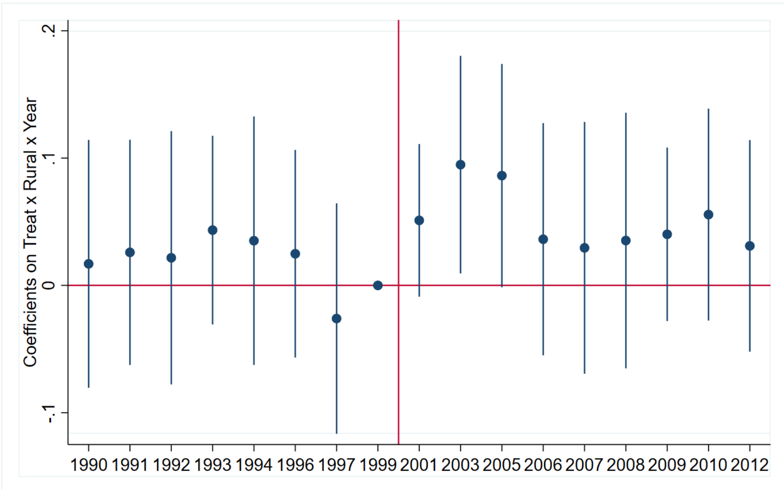
*Notes:* Figures plot estimated coefficients of equation (3.2). Only data on the older cohort (16-60) is included. Standards errors are clustered by the district and 95% confidence intervals are shown.

variables: whether a girl is currently enrolled in a school, and whether her current education is more than or equal to 5 years. Figures 3.3, 3.4, and 3.5 report the results in the form of event studies. Figure 3.3 has triple difference estimates that use a dummy variable to define treatment whereas in Figure 3.4 we used the proportion treated of land to capture the intensity of treatment. In Figure 3.5, we have results from the double difference specification and the treatment variable is defined in terms of intensity. These figures show number of important results. First, all sub-figures show that there are no differential pre-trends across the treated and control groups. Second, there is strong evidence that girls in the treated areas are more likely to be enrolled after the treatment as compared to girls in the control districts. Moreover, the results persist even 12 years after the treatment. Third, there is also evidence that girls are more likely to finish primary school, but the results are not always statistically significant.

Figure 3.3: Younger Cohorts – Triple Difference




(a) Currently Enrolled

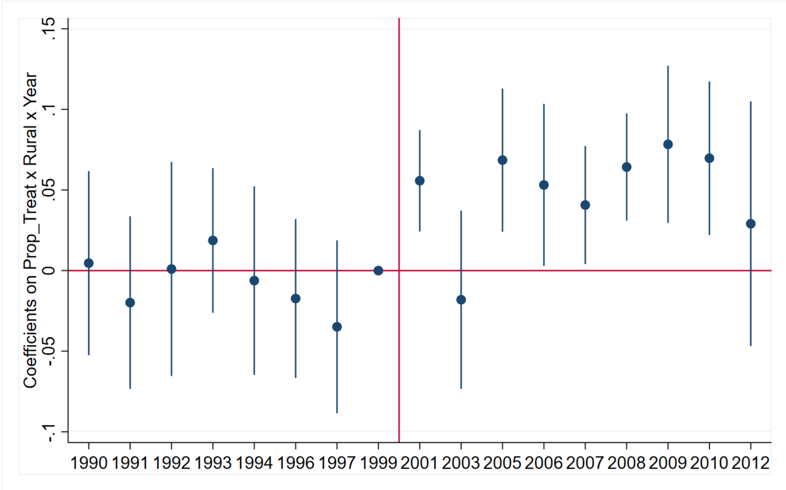



(b) Education 5+

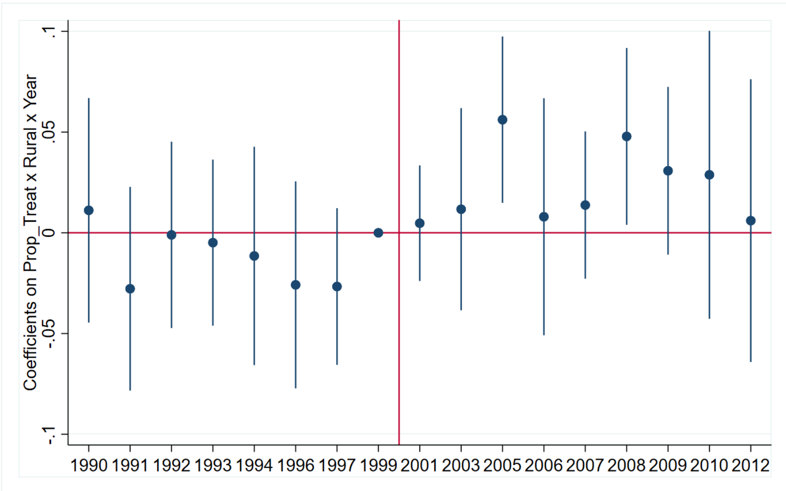
*Notes:* Figures plot estimated coefficients of equation (3.4). Only data on the younger cohort (10-15) is included. Standards errors are clustered by the district and 95% confidence intervals are shown. "Education 5+" is more than or equal to 5 years of education.

These results also show up in Table 3.2 and C1 and survive alternative specifications and also additional control. We find that girls in treated districts were 3.3 percentage points more likely to be enrolled, which is an increase of 10% relative to baseline mean. This effect is more or less similar in the triple difference estimates as compared to the respective baseline mean. In column 4 of Table 3.2,

Figure 3.4: Younger Cohorts – Triple Difference – Proportion Treated



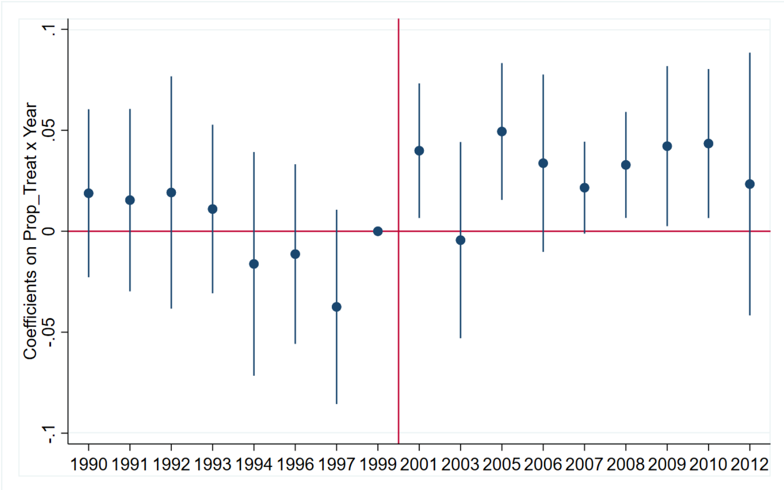

(a) Currently Enrolled – Proportion Treated



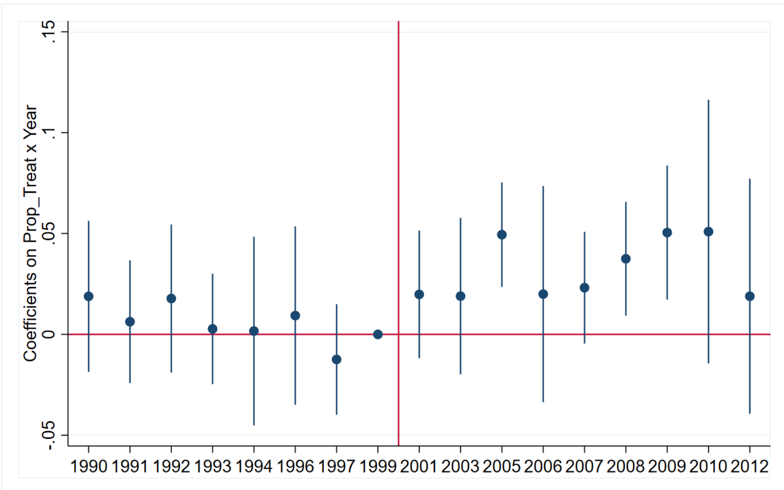

(b) Education 5+ – Proportion Treated

*Notes:* Figures plot estimated coefficients of equation (3.4). Only data on the younger cohort (10-15) is included. Standards errors are clustered by the district and 95% confidence intervals are shown. “Education 5+” is more than or equal to 5 years of education.

Figure 3.5: Younger Cohorts – Double Difference




(a) Currently Enrolled – Double Difference




(b) Education 5+ – Double Difference

*Notes:* Figures plot estimated coefficients of equation (3.2). Only data on the younger cohort (10-15) is included. Standards errors are clustered by the district and 95% confidence intervals are shown. "Education 5+" is more than or equal to 5 years of education.

we find that there is also a 2.8 percentage point increase in the probability that girls have at least primary education. This increase is nearly 13.6% relative to the mean and is statically significant.

As discussed in Section 3.3, we can also look at the long-term changes in educational attainment within a household. We present estimates of equation (3.5) and 3.3 in Table 3.3. The Panel A presents results from double difference specification, and in Panel B we have estimates from a triple difference specification. We now focus on the sample that is not currently enrolled so that we can compare the final education level. Therefore, we also look at completion of middle and secondary school. In both panels, we find that the younger cohorts in the treated districts attain more education relative to older cohorts. More specifically, we find that inter-cohort differences in completing secondary schooling, between young and older cohorts increases by 3.63 percentage points. The effect size is relatively bigger in the triple difference estimates where we find that the difference increases by 6.67 percentage points. Compared to the baseline means, the effect sizes in both specifications are nearly 50%. Qian (2008) finds that an increase in female income has a positive impact on educational attainment of all children. The results presented above suggest that women's involvement in a social movement can also decrease the gender gap in educational attainment. We further explore the mechanisms that derive this results in Section 4.4.

Table 3.3: Within Household Estimates

| | Younger Cohort | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | educ 5+ | educ 8+ | educ 10+ |
| **Panel A: Double Difference** | | | |
| treat=1 × younger cohort=1 | 0.105*** | 0.0662** | 0.0363* |
| | (0.0366) | (0.0296) | (0.0210) |
| Observations | 94155 | 94155 | 94155 |
| Dependent Variable mean | 0.198 | 0.101 | 0.0630 |
| **Panel B: Triple Difference** | | | |
| treat=1 × female=1 × younger cohort=1 | 0.101*** | 0.0761*** | 0.0667*** |
| | (0.0309) | (0.0223) | (0.0163) |
| Observations | 232155 | 232155 | 232155 |
| Dependent Variable mean | 0.339 | 0.199 | 0.125 |

*Notes:* ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Standard errors clustered by district in parentheses. Panel A presents estimates from equation (3.5), whereas Panel B from equation (3.6). "young cohorts" is equal to 1 for women that were less than 30 years old in 1999 or were born after 1999.

### 3.4.3 Robustness

We show robustness to changes in the baseline specification in Panel B of Table 3.2 and C1. In an alternative specification, we add province times year fixed effects. There is no significant change in the results. In Panel C, we also control for marital status for the older cohort and for both cohorts we account for total number of household members, proportion of female household members, and whether the household head is female. The results are robust to the inclusion of these control variables. In the Appendix, we also report results from specifications where we use a dummy treatment variable in the double difference specification (Table 3.5) and use proportion treated in the triple difference specification (Table 3.4).

One possible threat to our identification is selection bias that could arise due migration across districts that is related to the AMP movement. To deal with this, we restrict our sample to households that are residing in a given district since birth. The results present in Table C2, are similar and show that such selection bias is unlikely to drive our results.

Finally, the Lahore district which is the capital of the Punjab province could have different development patterns, and might drive our results. To test this, we present estimates from a sample where we exclude Lahore from the data. The results (Table C3) are not sensitive to this exclusion. Likewise, we consider the possibility that all the AMP districts are part of regions that experienced different infrastructure development, and thus such investment in infrastructure might explain these results. To deal with this, we define a placebo treatment where we define treated areas as all the neighboring districts of those districts where AMP movement was active. For this specification, we also exclude the AMP districts from the data. The results are in Table C4, where we find that all the coefficients are statistically insignificant and small. This suggests that our main results are unlikely to be driven by any changes in the region of AMP districts.

### 3.4.4 Mechanisms

To understand whether the AMP movement affected some subgroups differently than others, we study heterogeneous treatment effects in Table 3.6. Since the movement was led by tenant farmers, we expect the effects to be stronger for households that were involved in the agricultural sector at baseline. We do observe this pattern in the Panel A of Table 3.6. For the older cohort, we find that

Table 3.4: Triple Difference Estimates – Proportion Treated

| | Older Cohort | | Younger Cohort | |
|---|---|---|---|---|
| | (1) work | (2) hours | (3) currently enrolled | (4) educ 5+ |
| **Panel A: Baseline Specification** | | | | |
| rural=1 × post=1 × proportion treated | 0.0321** | -0.181 | 0.0587*** | 0.0350** |
| | (0.0147) | (0.524) | (0.0203) | (0.0173) |
| Observations | 711682 | 711682 | 208696 | 208696 |
| Dependent Variable mean | 0.0960 | 3.757 | 0.533 | 0.370 |
| **Panel B: With Province × Year FE** | | | | |
| rural=1 × post=1 × proportion treated | 0.0384** | -0.0976 | 0.0623*** | 0.0422*** |
| | (0.0151) | (0.567) | (0.0217) | (0.0152) |
| Observations | 711682 | 711682 | 208696 | 208696 |
| Dependent Variable mean | 0.0960 | 3.757 | 0.533 | 0.370 |
| **Panel C: With Controls** | | | | |
| rural=1 × post=1 × proportion treated | 0.0315** | -0.207 | 0.0583*** | 0.0347** |
| | (0.0146) | (0.520) | (0.0201) | (0.0172) |
| Observations | 711682 | 711682 | 208696 | 208696 |
| Dependent Variable mean | 0.0960 | 3.757 | 0.533 | 0.370 |

*Notes:* ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Standard errors clustered by district in parentheses. All panels present estimates from equation (3.3). Older cohorts include women of age 16-50 and younger cohorts include women of age 10-15. All panels use data on women living in both rural and urban areas. "Proportion treated" is a ratio of state-owned land and total cultivable land in a district. For both set of cohorts, controls include total number of household members, proportion of female household members, and whether the household head is female. For older the older cohorts, we also control for marital status.

Table 3.5: Double Difference Estimates

| | Older Cohort | | Younger Cohort | |
|---|---|---|---|---|
| | (1) work | (2) hours | (3) currently enrolled | (4) educ 5+ |
| **Panel A: Baseline Specification** | | | | |
| treat=1 $\times$ post=1 | 0.0623* | 0.945 | 0.0428* | 0.0341** |
| | (0.0341) | (1.149) | (0.0243) | (0.0131) |
| Observations | 397122 | 397122 | 117023 | 117023 |
| Dependent Variable mean | 0.125 | 4.830 | 0.328 | 0.207 |
| **Panel B: With Province $\times$ Year FE** | | | | |
| treat=1 $\times$ post=1 | 0.0449 | 0.609 | 0.0608** | 0.0193 |
| | (0.0417) | (1.293) | (0.0281) | (0.0186) |
| Observations | 397122 | 397122 | 117023 | 117023 |
| Dependent Variable mean | 0.125 | 4.830 | 0.328 | 0.207 |
| **Panel C: With Controls** | | | | |
| treat=1 $\times$ post=1 | 0.0613* | 0.911 | 0.0431* | 0.0352*** |
| | (0.0339) | (1.148) | (0.0242) | (0.0131) |
| Observations | 397122 | 397122 | 117023 | 117023 |
| Dependent Variable mean | 0.125 | 4.830 | 0.328 | 0.207 |

*Notes:* ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Standard errors clustered by district in parentheses. All panels present estimates from equation (3.1). Older cohorts include women of age 16-50 and younger cohorts include women of age 10-15. All panels use data on women living in the rural areas. "Proportion treated" is a ratio of state-owned land and total cultivable land in a district. For both set of cohorts, controls include total number of household members, proportion of female household members, and whether the household head is female. For older the older cohorts, we also control for marital status.

the increase in labor force participation is mostly concentrated in those household where men were active in the agricultural sector before 1999. Overall, we find that the effect is 3.1 percentage points higher for women that were part of an agricultural household at the baseline. The estimates for "number of hours worked" are qualitatively similar but the coefficients are not statistically significant. For the younger cohorts of women, we observe that the effect is 6.6 percentage points stronger for those that where part of an agricultural household at the baseline.

The strong response on girls' educational investment from households in AMP districts drives us to study whether this effect is mostly concentrated in those households where the investment in education is generally low even for male household members. The results are present in Panel B of Table 3.6. The coefficient on "*post*×*proportion treated*" suggests that the effect is indeed concentrated in households with low investment towards education in general. Again, we are unable to say the same for "number of hours worked". The effect on labor force participation is 1.25 percentage points smaller in households where male household members had higher level of education at the baseline. Overall, this table emphasizes that the treatment effect is mostly driven by a subgroup that is likely to be influenced by the AMP movement.

Table 3.6: Heterogeneity by Male Household Members' Occupation and Education

| | Older Cohort | | Younger Cohort | |
|---|---|---|---|---|
| | (1) work | (2) hours | (3) currently enrolled | (4) educ 5+ |
| **Panel A: Male Members in Agricultural Sector** | | | | |
| post=1 × proportion treated | 0.0197 | -0.255 | 0.00796 | 0.0169** |
| | (0.0159) | (0.559) | (0.0189) | (0.00809) |
| agriculture=1 × proportion treated | -0.000921 | -0.718** | -0.0317** | 0.00872 |
| | (0.00971) | (0.339) | (0.0130) | (0.00946) |
| post=1 × agriculture=1 | 0.0460** | 3.150*** | -0.0402** | -0.0318*** |
| | (0.0179) | (0.743) | (0.0175) | (0.0109) |
| post=1 × agriculture=1 × proportion treated | 0.0319*** | 0.888* | 0.0660*** | 0.0190 |
| | (0.0118) | (0.505) | (0.0162) | (0.0125) |
| agriculture=1 | 0.0497*** | 1.950*** | -0.116*** | -0.0699*** |
| | (0.00964) | (0.417) | (0.0130) | (0.0114) |
| Observations | 397122 | 397122 | 117023 | 117023 |
| Dependent Variable mean | 0.169 | 7.048 | 0.449 | 0.285 |
| **Panel B: Educated Male Members** | | | | |
| post=1 × proportion treated | 0.0390** | -0.0272 | 0.0376* | 0.0320*** |
| | (0.0166) | (0.572) | (0.0188) | (0.0116) |
| educated=1 × proportion treated | -0.0408*** | -1.962*** | 0.0307** | 0.0438*** |
| | (0.00872) | (0.379) | (0.0131) | (0.00928) |
| post=1 × educated=1 | -0.0194* | -1.569*** | 0.00728 | -0.0156 |
| | (0.0105) | (0.462) | (0.0170) | (0.0134) |
| post=1 × educated=1 × proportion treated | -0.0125* | 0.312 | -0.0201 | -0.0184* |
| | (0.00704) | (0.237) | (0.0123) | (0.0108) |
| educated=1 | -0.0455*** | -1.579*** | 0.266*** | 0.241*** |
| | (0.00910) | (0.347) | (0.0119) | (0.0113) |
| Observations | 397122 | 397122 | 117023 | 117023 |
| Dependent Variable mean | 0.169 | 7.048 | 0.449 | 0.285 |

*Notes:* ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Standard errors clustered by district in parentheses. All panels present estimates from equation (3.1) where we further include interactions with the heterogeneity variable. Older cohorts include women of age 16-50 and younger cohorts include women of age 10-15. All panels use data on women living in the rural areas. "Proportion treated" is a ratio of state-owned land and total cultivable land in a district. "Male Members in Agricultural Sector" is number of male household members that were work in the agricultural sector and were at least 16 years old in 1999. "Educated Male Members" is number of male household members that had at least 10 years of education and were at least 16 years old in 1999.

We now discuss possible channels that might explain an increased investment in girls' education in the AMP districts. One possible explanation could be that after the change in lease agreement, tenant farmers anticipated a decrease in income from farming or a possible eviction. This uncertainty might push them to seek employment opportunities for their daughters that do not require land. However, given that this uncertainty decreased over time but the results on education persist even until 2012, it is unlikely that this channel is driving the results. Nevertheless, we use data from five rounds of PSLM survey and show that distance to school for children living in the AMP districts has not changed after the movement started (see Table C5).

Another possible channel is an increase in women's participation in decision making. Since the movement increased women's mobility and thus exposure, and we also see a short-term increase in labor force participation, it is possible that women now have more say in household-level decisions, including investment in education. Previous studies also show that an increase in women empowerment leads to higher investment in girls' education (Doss, 2013; Qian, 2008). Since this information is missing in the Labour Force Survey, we turn our focus towards Pakistan Social and Living Standards Measurement survey. As discussed earlier, one key limitation is that we only have one pre-treatment round available for this survey and therefore we are unable to show that pre-trends are parallel. We use our triple difference specification and present results on three outcomes in Table 3.7. In the first column we look at whether there is increased participation of women in making decisions related to education, in the next column we focus on decisions related to employment and in the last column we have decisions regarding marriage. The results show that there is no differential change in women's role until 2005, but then in 2007 we see a significant involvement especially in decisions related to education and marriage. Overall, we find suggestive evidence that over time there is an increase in women role in decision making in the AMP districts. This might explain part of our results on increased investment in girls' education.

One possible channel could also be an increased investment in infrastructure in treated districts. For example, it is possible that there is an increase in entry of public schools which increases the access to public education and thus we see more girls attending schools now. However, results presented above suggest that this is unlikely to be a key channel. First, we do show that this results persists in a triple difference specification where we also compare rural areas with urban areas of the same district. Second, we show that the results are consistent in with-in household specification where we also include male siblings as another layer of control. Therefore, unless the increase in school entry is exactly aligned with the start of AMP movement, only concentrated in rural areas of treated district and mostly for girls schools, this channel is unlikely to explain the result.

Table 3.7: Women in Decision Making

| | (1) decisions regarding education | (2) decisions regarding employment | (3) decisions regarding marriage |
|---|---|---|---|
| treat=1 × year=2001 × 1.rural | 0.0354 | 0.0507 | 0.00370 |
| | (0.0438) | (0.0502) | (0.0661) |
| treat=1 × year=2005 × 1.rural | -0.0709 | -0.0133 | 0.0344 |
| | (0.0509) | (0.0569) | (0.0850) |
| treat=1 × year=2007 × 1.rural | 0.0832** | 0.0422 | 0.158** |
| | (0.0405) | (0.0492) | (0.0782) |
| treat=1 × year=2011 × 1.rural | 0.0868 | 0.105*** | 0.115* |
| | (0.0562) | (0.0382) | (0.0649) |
| Observations | 79985 | 79956 | 40449 |
| Dependent Variable mean | 0.355 | 0.345 | 0.475 |

*Notes:* ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Standard errors clustered by district in parentheses. All panels present estimates from equation (3.3). All panels use data on women from five surveys (1998, 2001, 2005, 2007, and 2011) of the Pakistan Social and Living Standards Measurement (PSLM). In each survey, women of age 15-49 in a household were asked to report who decides a) whether you can seek or continue to get education b) whether you can seek or remain in paid employment and c) whether and when you should be married?. We code "decisions regarding education", "decisions regarding employment", and "decisions regarding marriage" as 1 if either woman herself or any other woman form the household is involve in the decision making.

## 3.5 Conclusion

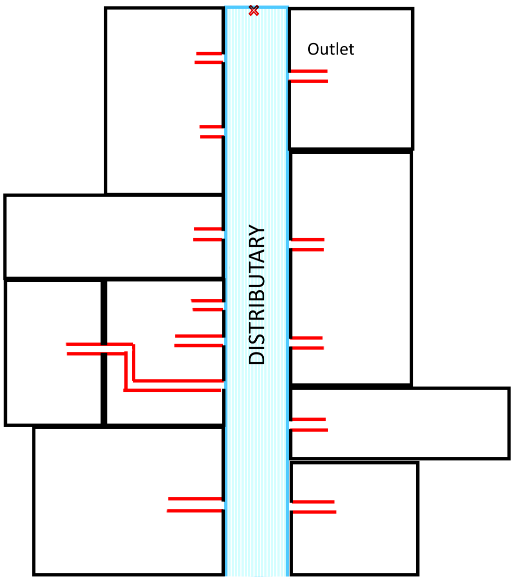
Previous studies have shown that social movements affect political and economic outcomes (Madestam *et al.*, 2013; Acemoglu *et al.*, 2018; Collins and Margo, 2007). However, evidence on how social movements affect economic outcomes for individual participants is limited. In this paper, we study how a long-lasting land struggle by tenant farmers in Pakistan affects women's labor force participation and households' choices regarding girls' education. To do so, we use representative individual-level data that spans over more than 20 years and exploit a difference-in-differences approach. The estimates show that women in affected districts show an increase in labor force participation after the start of the AMP movement. However, this effect is short-lived and disappears after a decrease in police brutalities against male members. The pattern suggests that women temporarily started working in agricultural fields during the times when men were likely to be arrested for protesting. On the other side, we see a strong and consistent effect on the educational attainment for the younger cohorts. We see an increase in girls' enrollment and also find an increase of 3.6 percentage point in the probability of completing secondary school. These estimates are robust to different specifications including a triple difference approach and within household difference. Lastly, we find some suggestive evidence that this effect is driven by increased involvement of women in decision making in rural areas of AMP districts.

The findings have important implications for the literature studying gender gaps in educational attainment in developing countries (Duflo, 2001; Kazianga *et al.*, 2013; Fiszbein and Schady, 2009; Muralidharan and Prakash, 2017). On the supply side, studies in this literature focus on how infrastructure investment affect girls' enrollment. This is one of the first studies to document that women's participation in social movements can increase their involvement in decision making which can affect investment in girls' education. Lastly, the study also highlights how social movements can have long-lasting effects even when they do not lead to any real policy change.

# Appendix A (for Chapter 1)

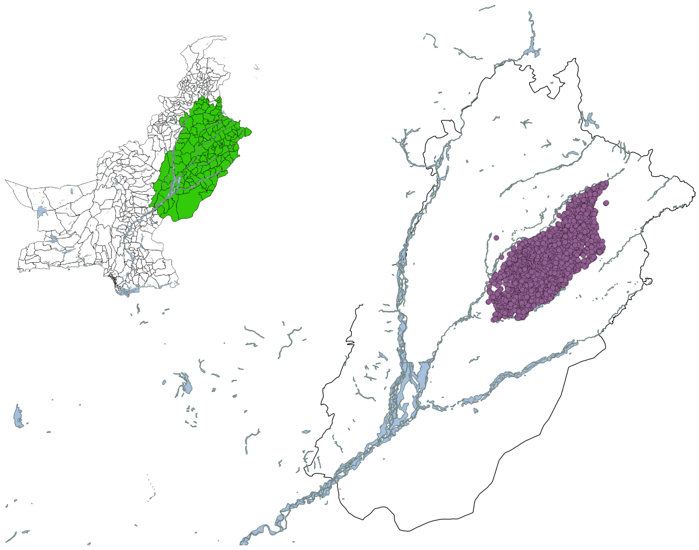
## A Additional Figures

Figure A1: Illustration of Network of Villages on a Typical Distributary



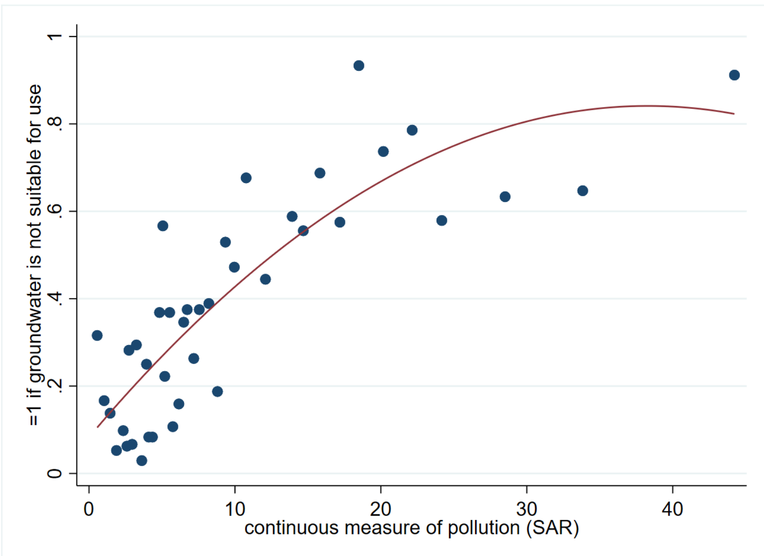

Notes: This figures shows how villages are typically connected with a distributary. Each village has multiple outlets that are designed to only withdraw allocated amount of water. In some cases there the same outlet could be connected to multiple villages.

Figure A2: Illustration of a Typical Irrigation Network in Pakistan



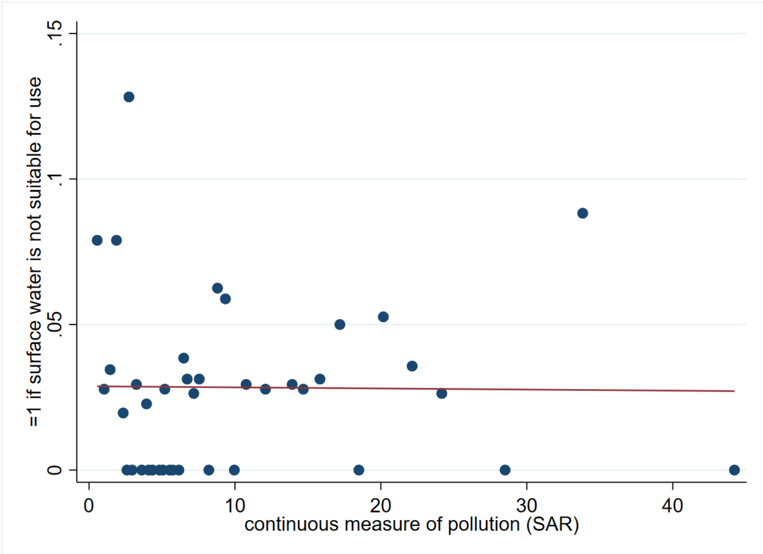
Notes: The inset map highlights the location of Province Punjab on the map of Pakistan. The main graphs plots the location of villages in my sample on the map of Punjab.

Figure A3: Comparison of self-reported groundwater quality and SAR



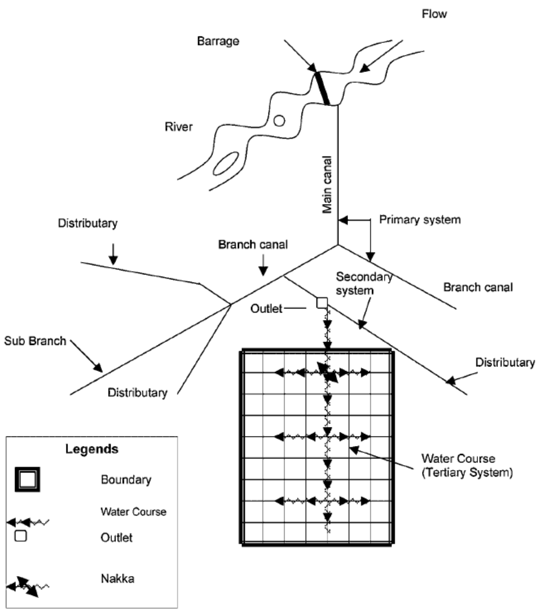

Notes: The self-reported groundwater quality is taken from the community survey conducted in 2018.

Figure A4: Comparison of self-reported surface water quality and SAR



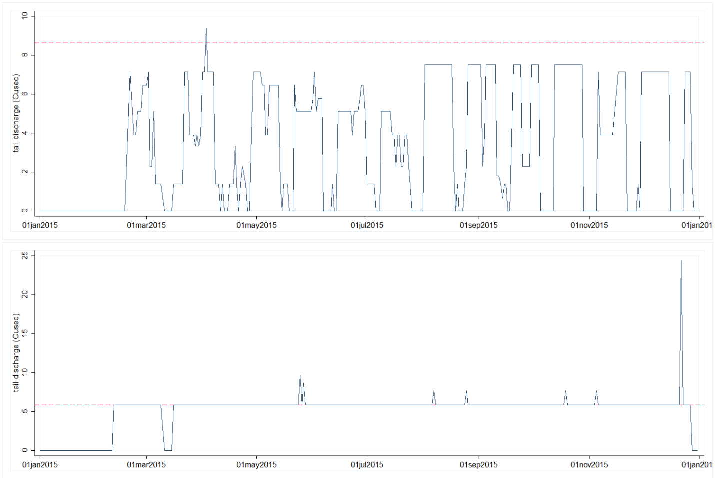

Notes: The self-reported surface water quality is taken from the community survey conducted in 2018.

Figure A5: Illustration of a Typical Irrigation Network in Pakistan



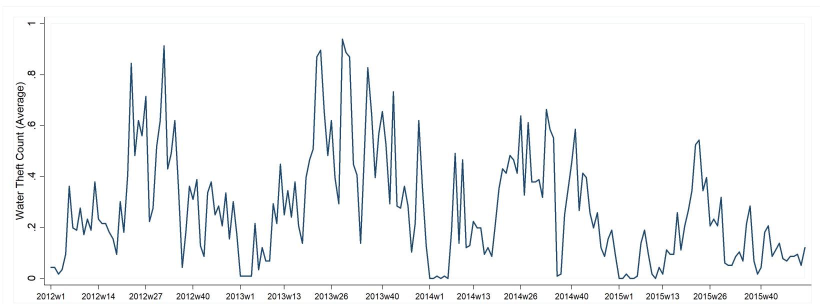

Notes: This figure is borrowed from Latif (2007) and shows a typical network of surface water irrigation. This paper will focus on the secondary system where a distributary supplies water to villages through outlets.

Figure A6: Authorized versus Tail Discharge



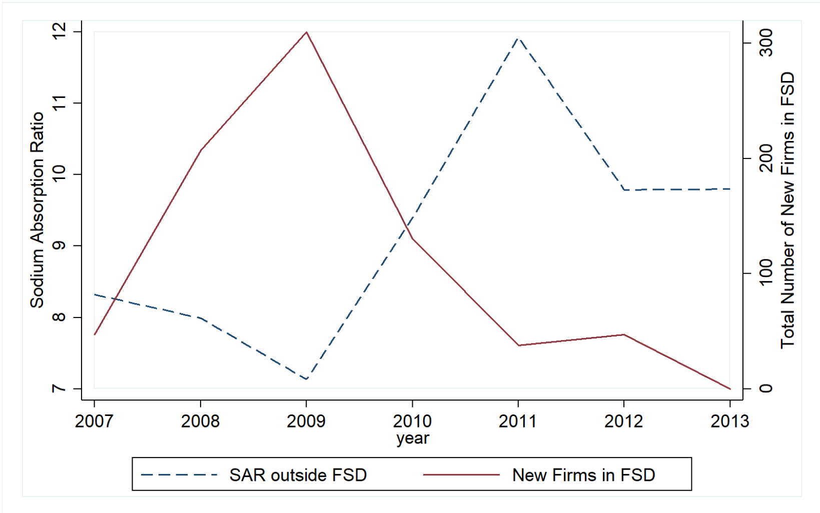

Notes: The figures plot day level data, for year 2015, on water discharge from two distributaries; Douhlar and Aminput respectively. The dashed line represents the allocated amount and solid line plots actual readings.

Figure A7: Water Theft Incidences



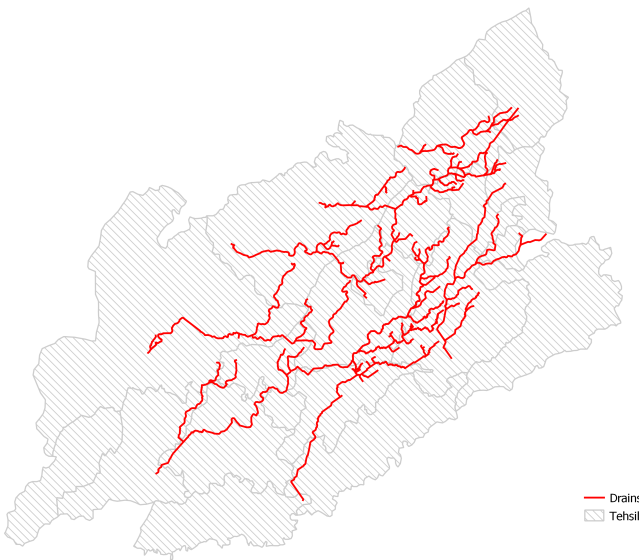

Notes: This figure plots total theft for each month in the sampled villages. The highest peaks are observed during the *kharif* season when usually water intensive cash crops are grown.

Figure A8: Ground Water Quality and Industrial Growth



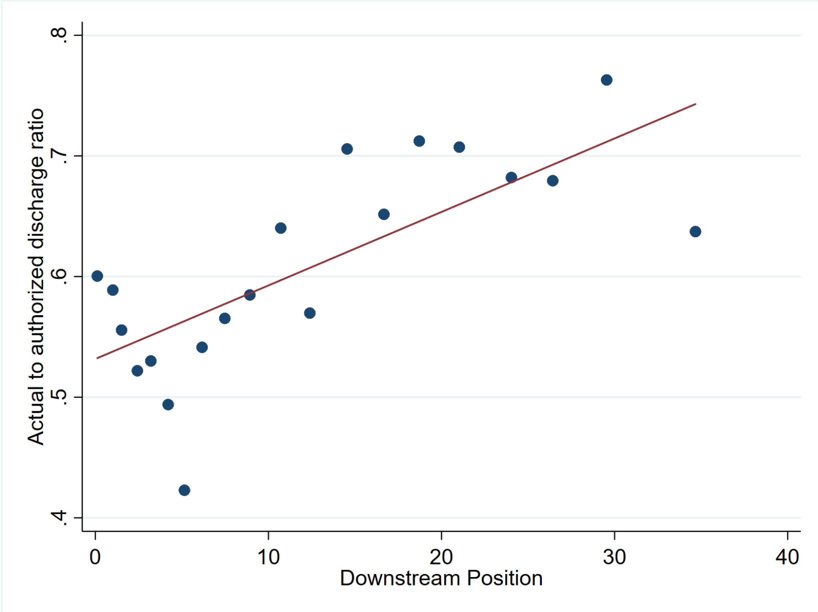

Notes: The graph plot distributary-level means of SAR for treatment group and entry of new firms in Faislabad District. The sharp increase in industrial activity align very well with pollution in neighbouring districts.

Figure A9: Long-term Scarcity and Inter-village Cooperation – Downstream Position



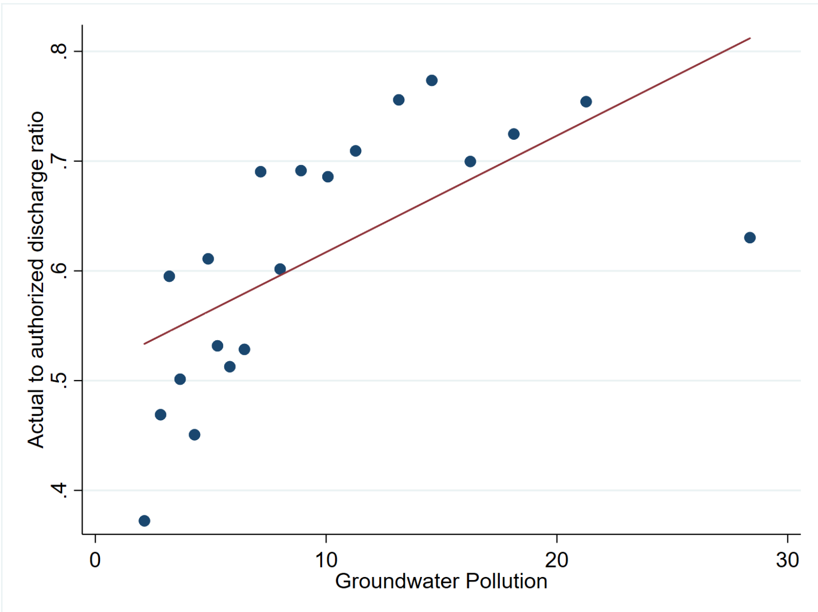

The graphs shows the location of drains in the study area. Part of the data on the location of drains was obtained from Irrigation Research Institute, Lahore. The location of two key drains (Maduhana and Pharang) were digitized using the maps obtained from the Irrigation Department. This map excludes the drains that were connected with the districts upstream from Faisalabad.

Figure A10: Drains



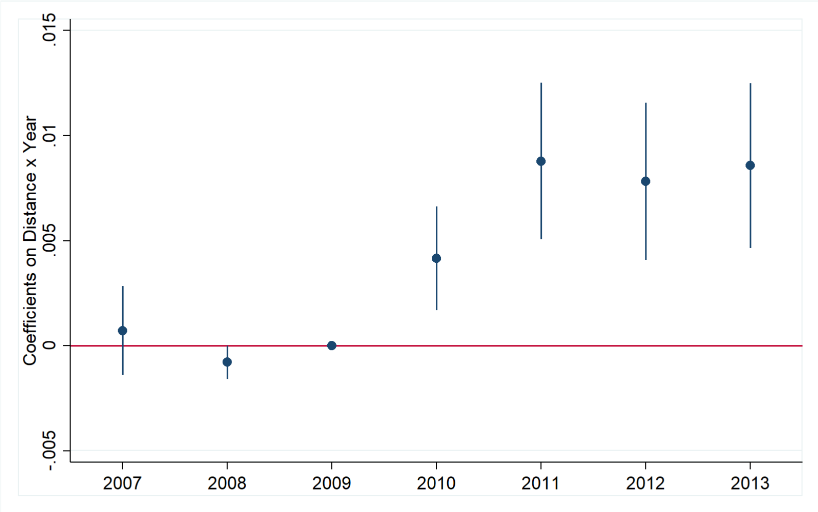

The graph plots binned scatter plot between the proxy of inter-village cooperation and downstream position of a distributary.

Figure A11: Long-term Scarcity and Inter-village Cooperation – Groundwater Quality



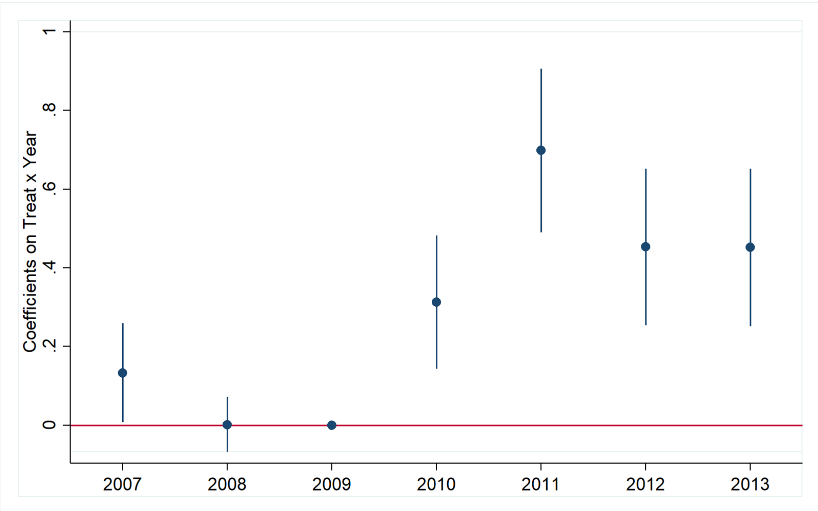

The graph plots binned scatter plot between the proxy of inter-village cooperation and different groundwater quality.

Figure A12: Temporal Changes in Ground Water Quality – First Stage



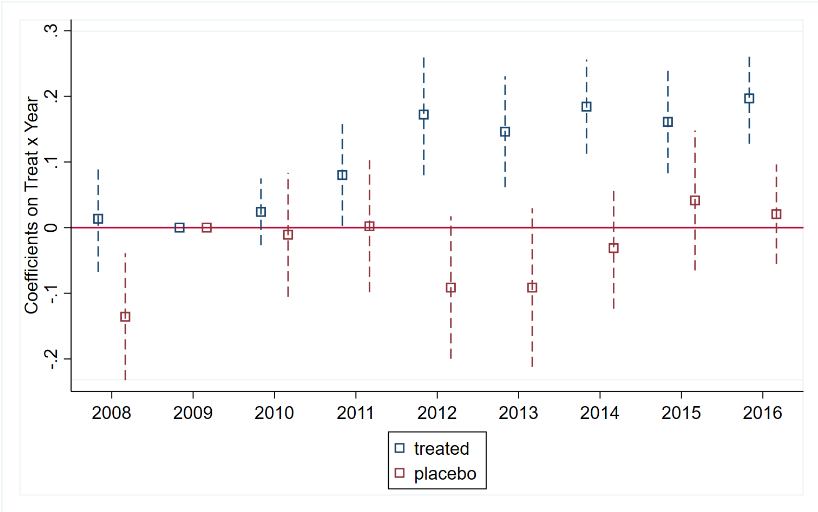

The graph plots the coefficients on treatment variable from the flexible regression of SAR on distance x year and distributary and year fixed effects. The standard errors are clustered at the distributary-level.

Figure A13: Temporal Changes in Ground Water Quality – First Stage with Continuous Measure



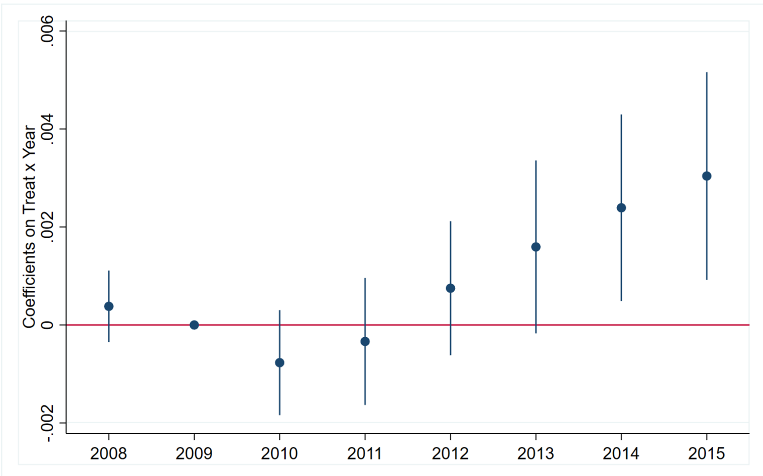

The graph plots the coefficients on treatment variable from the flexible regression of SAR on treat x year and distributary and year fixed effects. The standard errors are clustered at the distributary-level.

Figure A14: Test for Exclusion Restriction



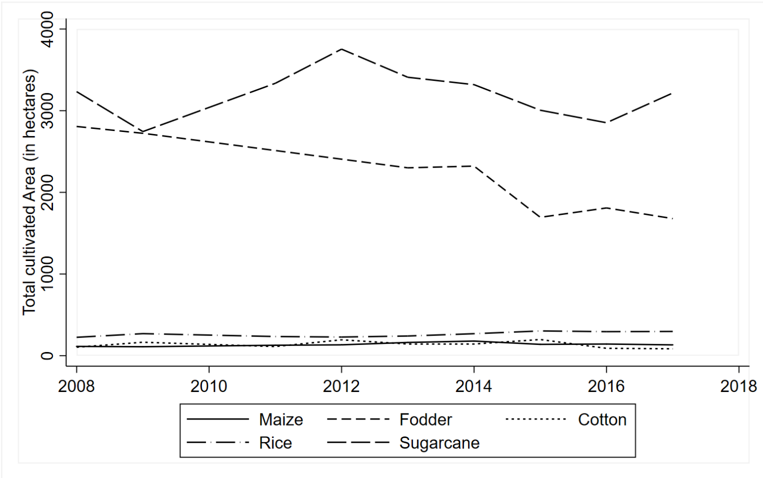

This graph plots estimates from reduced form regression. The treatment variable for placebo regression is equal to 1 if distributary is within 10km radius of a river and zero if distributary is with-in the industrial district. This placebo treatment is used to test whether the effect is drive by job opportunities rather than pollution. In order to compare the areas that are close industrial district, I exclude all the distributaries that are more than 70km away from the industrial centre. The standard errors are clustered at the distributary-level.

Figure A15: Changes in land use



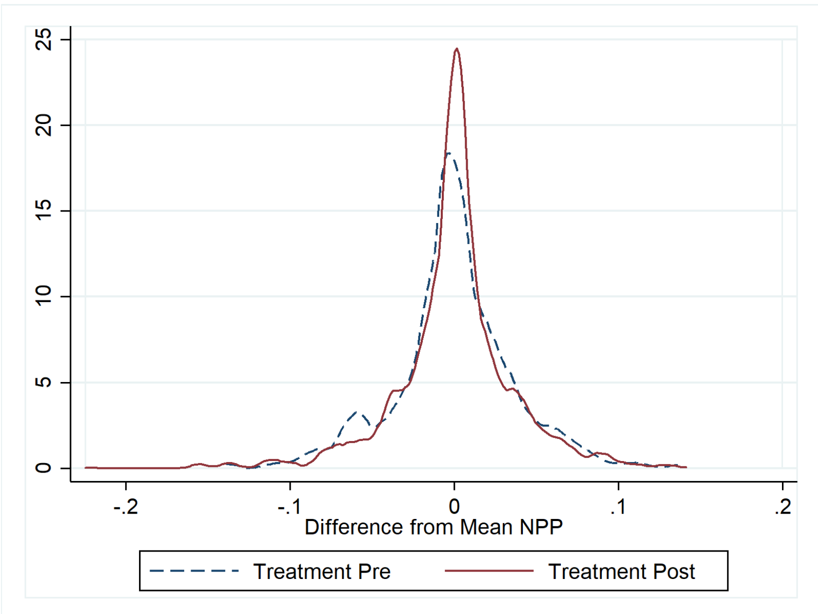

Notes: The graphs plots coefficient and standard errors from a difference-in-differences regression similar to the flexible version of the reduced form regressions estimated in Figure 1.5. The outcome variable is a dummy and equals to 1 if cell has been classified as cropland and zero otherwise. The regression include year and distributary fixed effects and standard errors are clustered at the distributary-level. The treatment variable is 1 if the cell is outside the industrial area (>50 km) and far away from the rivers (> 10 km), and zero otherwise.

Figure A16: Changes in crop choice



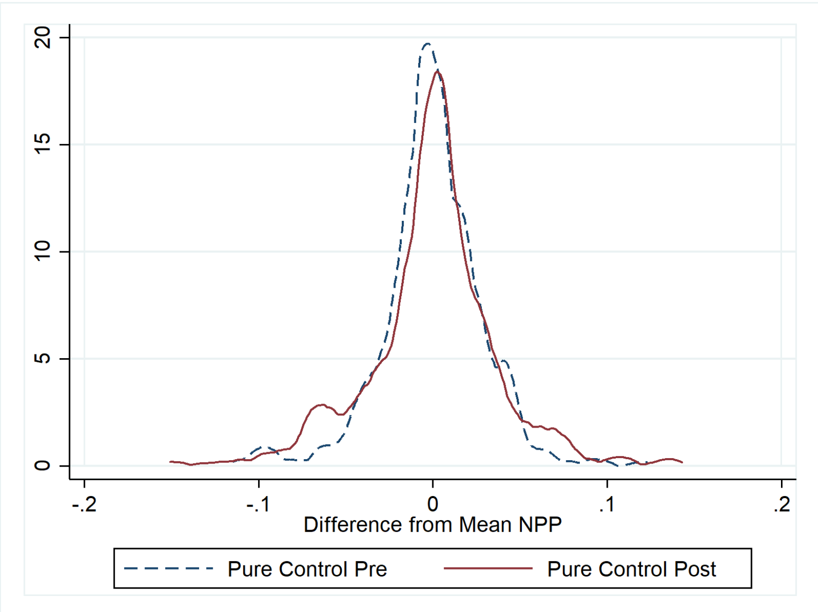

Notes: The graph plots the total cultivated area of all major *Kharif* season crops for Toba Tek Singh and Nankana Sahbi – two districts that experienced an increase in pollution after 2009. Cotton, Rice, and Sugarcane are water-intensive crops whereas Fodder and Maize required relatively less water.

Figure A17: Net Primary Productivity – Treatment




The graph plots difference of Net Primary Productivity (NPP) from year-distributary specific mean for treatment (contaminated) areas, both before and after the 2009.

Figure A18: Net Primary Productivity – Control




The graph plots difference of Net Primary Productivity (NPP) from year-distributary specific mean for control (non-contaminated) areas, both before and after the 2009.

# B Additional Tables

Table A1: Village-level Correlates of Water Theft

| | (1) | (3) | (4) |
|---|---|---|---|
| | | count of theft | |
| head | 0.056** | | |
| | (0.027) | | |
| any market | | -0.044*** | |
| | | (0.016) | |
| any institution | | | -0.031 |
| | | | (0.020) |
| No. of Disty-Village Groups | 747 | 729 | 729 |
| Observations | 65,824 | 63,888 | 63,888 |

Notes: Each column represent a separate regression that was estimated using distributary-village-week level data. The variable “any market” and “any institution” are from 2008 Mouzza Census. The market could either be livestock, grain, fruit or vegetable. All regressions include week fixed effects and standard errors are clustered at distributary level.

Table A2: Distributary-level Correlates of Water Theft

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | actual to authorized tail discharge | | | |
| cca (size) | -0.019*** | | | |
| | (0.003) | | | |
| any market | | 0.028 | | |
| | | (0.022) | | |
| any institution | | | 0.016** | |
| | | | (0.008) | |
| downstream position | | | | 0.004*** |
| | | | | (0.001) |
| No. of Disty-Village Groups | 405 | 303 | 430 | 430 |
| Observations | 75,812 | 74,822 | 74,822 | 76,604 |

Notes: Each column represent a separate regression that was estimated using distributary-week level data. The variable "any market" and "any institution" are from 2008 Mouzza Census. The market could either be livestock, grain, fruit or vegetable. The downstream position calculates how far each distributary is from the source of branch canal. All regressions include week fixed effects and column (2) and (3) also control for size of a distributary. Standard errors are clustered at the distributary level.

Table A3: Groundwater Pollution and Proximity to Drains

| | Pollution | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| close to a drain x post | 0.134** | 0.176** | 0.585*** | 0.714*** |
| | (0.0629) | (0.0853) | (0.176) | (0.201) |
| Observations | 4332 | 2931 | 1393 | 944 |
| Sample | All Water Wells | Downstream Only | Excl. Ind. Areas | Excl. Ind. and Rivers |

*Notes:* The standard errors are clustered at the water-well level. "Close to drain" variable assign 1 to all those wells that are within 5km of a wastewater drain, and 0 otherwise. "Post" is 1 after 2009. The second column only considers wells that are downstream from the industrial area. Column (3) excludes the industrial area in Faisalabad district. Last column also excludes the well that are with 10km of a river.

Table A4: Unexpected and Short-term Scarcity – Village-level Estimates

| | =1 if a theft took place | |
|---|---|---|
| | (1) | (2) |
| Negative Rainfall Shock | 0.00490* | 0.00532* |
| | (0.00248) | (0.00293) |
| Observations | 61600 | 52008 |
| Dependent Variable mean | 0.0370 | 0.0370 |
| Controls | No | Yes |

*Notes:* All regressions are estimated using village-week level data. The controls include mean temperature, variation in rainfall, a dummy for reform time period and a dummy to indicate the weeks when head discharge was zero. Clustered standard errors are clustered at both distributary-level.

Table A5: Unexpected and Short-term Scarcity

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | tail discharge relative to allocated OLS | | | | | |
| Rainy days less than average | -0.009*** | -0.008*** | -0.008*** | -0.007*** | -0.008*** | -0.007*** |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Rainy days less than average (L-1) | | | | | -0.002 | -0.001 |
| | | | | | (0.002) | (0.002) |
| Rainy days less than average (L-2) | | | | | 0.001 | 0.002 |
| | | | | | (0.002) | (0.002) |
| Mean of dep var | | | 0.72 | | | |
| Number of Distributaries | | | 392 | | | |
| Observations | 77,616 | 77,616 | 59,972 | 59,972 | 59,334 | 59,334 |
| Controls | No | No | Yes | Yes | Yes | Yes |
| Disty specific time trend | No | Yes | No | Yes | No | Yes |

All regressions are estimated using distributary-week level data. A day is considered rainy if the rainfall is more than 0.1 mm. The long-term average is calculated from the data ranging from 2001-2016. The decrease in number of observations from column (3) onwards is due to the limited availability of groundwater control variables. All regressions control for distributary and week fixed effects. Standard errors are clustered at the distributary level.

Table A6: Unexpected and Short-term Scarcity – Robustness Check

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | tail discharge relative to allocated OLS | | | |
| Rainy days less than average | -0.009*** | -0.009*** | -0.010*** | -0.009*** |
| | (0.003) | (0.003) | (0.002) | (0.002) |
| Mean of dep var | | 0.72 | | |
| Observations | 66,745 | 51,575 | 64,548 | 48,708 |
| Controls | No | Yes | No | Yes |

All regressions are estimated using distributary-week level data. A day is considered rainy if the rainfall is more than 0.1 mm. The long-term average is calculated from the data ranging from 2001-2016. The first two column exclude weeks with very high rainfall (top decile). The next two columns exclude those areas that never saw a reform. All standard errors are clustered at the distributary level.

Table A7: Unexpected and Short-term Scarcity – Excluding High Rainfall Weeks

| | =1 if a theft took place | |
|---|---|---|
| | (1) | (2) |
| Negative Rainfall Shock | 0.00638*** | 0.00788*** |
| | (0.00230) | (0.00265) |
| Observations | 53998 | 45562 |
| Dependent Variable mean | 0.0380 | 0.0380 |
| Controls | No | Yes |

*Notes:* All regressions are estimated using village-week level data. The controls include mean temperature, variation in rainfall, a dummy for reform time period and a dummy to indicate the weeks when head discharge was zero. These regressions trim the lower docile of the independent variable. Clustered standard errors are clustered at both distributary-level.

Table A8: Unexpected and Short-term Scarcity – Alternate Measure

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | tail discharge relative to allocated OLS | | | | | |
| Negative Rain Shock | -0.010*** | -0.009*** | -0.013*** | -0.010*** | -0.013*** | -0.010*** |
| | (0.003) | (0.003) | (0.004) | (0.003) | (0.004) | (0.003) |
| Negative Rain Shock (t-1) | | | | | -0.002 | 0.001 |
| | | | | | (0.004) | (0.003) |
| Negative Rain Shock (t-2) | | | | | 0.004 | 0.006* |
| | | | | | (0.004) | (0.003) |
| Mean of dep var | 0.72 | | | | | |
| Disty-village group | 392 | | | | | |
| Observations | 77,616 | 77,616 | 59,972 | 59,972 | 59,334 | 59,334 |
| Controls | No | No | Yes | Yes | Yes | Yes |
| Disty specific time trend | No | Yes | No | Yes | No | Yes |

All regressions are estimated using distributary-week level data. The negative rain shock takes three values; 1 if rainfall is below 20th percentile, -1 if it is above 80th percentile, and zero for rest. The long-term distributary specific distribution is calculated from the data ranging from 2001-2016. The decrease in number of observations from column (3) onwards is due to the limited availability of groundwater control variables. All regressions control for distributary and week fixed effects. Standard errors are clustered at the distributary level.

Table A9: Unexpected and Short-term Scarcity – High Water Demand

| | =1 if a theft took place (1) | (2) |
|---|---|---|
| Negative Rainfall Shock | 0.00511** | 0.00518** |
| | (0.00234) | (0.00243) |
| Observations | 47600 | 40188 |
| Dependent Variable mean | 0.0390 | 0.0390 |
| Controls | No | Yes |

*Notes:* All regressions are estimated using village-week level data. The controls include mean temperature, variation in rainfall, a dummy for reform time period and a dummy to indicate the weeks when head discharge was zero. These regressions only use data from following 4 months: May, June, July and August. These months were indicated by the farmers as the time period when the demand for water is highest during the *Kharif* season. Clustered standard errors are clustered at both distributary-level.

Table A10: Unexpected and Short-term Scarcity – Temporal Dependence

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | tail discharge relative to allocated OLS | | | |
| Rainy days less than average | -0.008*** | -0.007*** | -0.009*** | -0.008*** |
| | (0.002) | (0.002) | (0.003) | (0.003) |
| Rainy days less than average (F-1) | -0.004** | -0.003* | -0.005** | -0.005* |
| | (0.002) | (0.002) | (0.003) | (0.003) |
| Rainy days less than average (F-2) | 0.004* | 0.005** | 0.001 | 0.001 |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Mean of dep var | 0.72 | | | |
| GW Quality Control | No | No | Yes | Yes |
| Observations | 58,946 | 58,946 | 42,202 | 42,202 |
| Controls | Yes | Yes | Yes | Yes |
| Disty specific time trend | No | Yes | No | Yes |

All regressions are estimated using distributary-week level data. A day is considered rainy if the rainfall is more than 0.1 mm. The long-term average is calculated from the data ranging from 2001-2016. The last two specification also control for lagged dependent variable. All specifications distributary and week fixed effects. Standard errors are clustered at the distributary level.

Table A11: Unexpected and Short-term Scarcity – Alternate Measure of Inter-village Cooperation

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | tail height relative to allocated<br>OLS | | | |
| Rainy days less than average | -0.007*** | -0.007*** | -0.007*** | -0.006*** |
| | (0.002) | (0.002) | (0.002) | (0.002) |
| Mean of dep var | 0.73 | | | |
| Observations | 77,418 | 77,418 | 59,796 | 59,796 |
| Controls | No | No | Yes | Yes |
| Disty specific time trend | No | Yes | No | Yes |

All regressions are estimated using distributary-week level data. A day is considered rainy if the rainfall is more than 0.1 mm. The long-term average is calculated from the data ranging from 2001-2016. The dependent variable is now constructed using the data on water height rather than discharge. All specifications distributary and week fixed effects. Standard errors are clustered at the distributary level.

Table A12: Unexpected and Short-term Scarcity – Further Controls

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | tail discharge relative to allocated OLS | | | | | |
| Rainy days less than average | -0.011*** | -0.009*** | -0.009*** | -0.008*** | -0.009*** | -0.008*** |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| Rainy days less than average (L-1) | | | | | -0.001 | -0.000 |
| | | | | | (0.003) | (0.003) |
| Rainy days less than average (L-2) | | | | | -0.002 | -0.002 |
| | | | | | (0.003) | (0.003) |
| Mean of dep var | 0.72 | | | | | |
| Disty-village group | 392 | | | | | |
| Observations | 50,952 | 50,952 | 42,834 | 42,834 | 42,202 | 42,202 |
| Other Controls | No | No | Yes | Yes | Yes | Yes |
| Disty specific time trend | No | Yes | No | Yes | No | Yes |

All regressions are estimated using distributary-week level data. A day is considered rainy if the rainfall is more than 0.1 mm. The long-term average is calculated from the data ranging from 2001-2016. All specifications in this table control for groundwater quality, distributary and week fixed effects. Standard errors are clustered at the distributary level.

Table A13: Unexpected and Short-term Scarcity – Event Study

| | =1 if a theft took place | |
|---|---|---|
| | (1) | (2) |
| Negative Rainfall Shock | 0.00485** | |
| | (0.00227) | |
| L.Negative Rainfall Shock | 0.00478** | |
| | (0.00234) | |
| L2.Negative Rainfall Shock | 0.000754 | |
| | (0.00180) | |
| L3.Negative Rainfall Shock | 0.000301 | |
| | (0.00158) | |
| F.Negative Rainfall Shock | 0.00427* | |
| | (0.00221) | |
| F2.Negative Rainfall Shock | 0.00246 | |
| | (0.00243) | |
| F3.Negative Rainfall Shock | -0.00175 | |
| | (0.00225) | |
| Rainy days less than average | | 0.00320** |
| | | (0.00159) |
| L.Rainy days less than average | | 0.00335* |
| | | (0.00202) |
| L2.Rainy days less than average | | 0.00141 |
| | | (0.00158) |
| L3.Rainy days less than average | | 0.000562 |
| | | (0.00133) |
| F.Rainy days less than average | | 0.0000442 |
| | | (0.00178) |
| F2.Rainy days less than average | | 0.00163 |
| | | (0.00171) |
| F3.Rainy days less than average | | -0.000138 |
| | | (0.00171) |
| Observations | 57400 | 57400 |
| Dependent Variable mean | 0.0370 | 0.0370 |
| Controls | No | No |

*Notes:* All regressions are estimated using village-week level data. The controls include mean temperature, variation in rainfall, a dummy for reform time period and a dummy to indicate the weeks when head discharge was zero. Clustered standard errors are clustered at both distributary-level.

Table A14: Unexpected and Short-term Scarcity – Lagged Dependent Variable

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | tail discharge relative to allocated | | | | | |
| | OLS | | | | | |
| Rainy days less than average | -0.007*** | -0.007*** | -0.006*** | -0.005*** | -0.006*** | -0.006*** |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) |
| Rainy days less than average (L-1) | | | | | 0.002 | 0.002 |
| | | | | | (0.002) | (0.002) |
| Rainy days less than average (L-2) | | | | | 0.002 | 0.002 |
| | | | | | (0.002) | (0.002) |
| Mean of dep var | 0.72 | | | | | |
| Disty-village group | 392 | | | | | |
| Observations | 0.564 | 0.893 | 0.563 | 0.890 | 0.563 | 0.890 |
| Controls | No | No | Yes | Yes | Yes | Yes |
| Disty specific time trend | No | Yes | No | Yes | No | Yes |

All regressions are estimated using distributary-week level data. A day is considered rainy if the rainfall is more than 0.1 mm. The long-term average is calculated from the data ranging from 2001-2016. All specification also control for lagged dependent variable, distributary and week fixed effects. Standard errors are clustered at the distributary level.

Table A15: Unexpected and Short-term Scarcity – Spatial Correlation Adjustment

| | =1 if a theft took place | |
|---|---|---|
| | (1) | (2) |
| Negative Rainfall Shock | 0.00490** | 0.00532** |
| | (0.00199) | (0.00227) |
| Observations | 61600 | 52008 |
| Dependent Variable mean | 0.0370 | 0.0370 |
| Distance Cut-off | 20 | 20 |
| Lag Cut-off | 2 | 2 |
| Controls | No | Yes |

*Notes:* All regressions are estimated using distributary-village-week level data. The controls include mean temperature, variation in rainfall, a dummy for reform time period and a dummy to indicate the weeks when head discharge was zero. Clustered standard errors adjust for spatial and serial correlation, with respective cut-offs given at the bottom of the table.

Table A16: Unexpected and Short-term Scarcity – Two-way Clustering

| | =1 if a theft took place | |
|---|---|---|
| | (1) | (2) |
| Negative Rainfall Shock | 0.00490* | 0.00532* |
| | (0.00248) | (0.00293) |
| Observations | 61600 | 52008 |
| Dependent Variable mean | 0.0370 | 0.0370 |
| Controls | No | Yes |

*Notes:* All regressions are estimated using distributary-village-week level data. The controls include mean temperature, variation in rainfall, a dummy for reform time period and a dummy to indicate the weeks when head discharge was zero. Clustered standard errors are clustered at both distributary-level and week of the year.

Table A17: Long-term Scarcity – Reduced-from Estimates

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | tail discharge relative to authorized | | | | | |
| treat x post | 0.084*** | 0.097*** | 0.098*** | | | |
| | (0.023) | (0.024) | (0.024) | | | |
| treat x 2008 | | | | 0.013 | 0.026 | 0.027 |
| | | | | (0.035) | (0.036) | (0.036) |
| treat x 2010 | | | | 0.006 | 0.032 | 0.031 |
| | | | | (0.020) | (0.022) | (0.022) |
| treat x 2011 | | | | 0.026 | 0.067** | 0.068** |
| | | | | (0.027) | (0.030) | (0.030) |
| treat x 2012 | | | | 0.118*** | 0.145*** | 0.146*** |
| | | | | (0.033) | (0.034) | (0.034) |
| treat x 2013 | | | | 0.103*** | 0.115*** | 0.116*** |
| | | | | (0.035) | (0.034) | (0.033) |
| treat x 2014 | | | | 0.127*** | 0.143*** | 0.146*** |
| | | | | (0.029) | (0.031) | (0.030) |
| treat x 2015 | | | | 0.103*** | 0.118*** | 0.122*** |
| | | | | (0.033) | (0.034) | (0.034) |
| treat x 2016 | | | | 0.155*** | 0.168*** | 0.171*** |
| | | | | (0.028) | (0.030) | (0.030) |
| Mean of dep var | 0.71 | | | | | |
| Disty-village group | 392 | | | | | |
| Observations | 3,483 | 3,483 | 3,483 | 3,483 | 3,483 | 3,483 |
| Controls | No | Yes | Yes | No | Yes | Yes |
| Circle-specific time trends | No | No | Yes | No | No | Yes |
| Disty and Year FE | Yes | Yes | Yes | Yes | Yes | Yes |

All regressions were estimated using distributary-week level data. The controls include rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, and a dummy for areas that were flooded in 2010. Standard errors are clustered at the distributary level.

Table A18: Reduced-from Estimates — Sensitivity to Distance Cut-off

| | tail discharge relative to allocated | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | >30 | >35 | >40 | >45 | >50 | >55 | >60 | >65 |
| treat x 2008 | 0.049 | 0.041 | 0.048 | 0.041 | 0.020 | -0.012 | -0.022 | -0.030 |
| | (0.032) | (0.031) | (0.031) | (0.031) | (0.031) | (0.033) | (0.033) | (0.035) |
| treat x 2010 | -0.001 | -0.007 | -0.003 | 0.004 | 0.008 | -0.008 | -0.001 | -0.006 |
| | (0.022) | (0.021) | (0.020) | (0.020) | (0.019) | (0.020) | (0.020) | (0.021) |
| treat x 2011 | 0.010 | 0.004 | -0.007 | 0.000 | 0.008 | -0.012 | -0.005 | -0.021 |
| | (0.023) | (0.023) | (0.023) | (0.023) | (0.024) | (0.026) | (0.027) | (0.028) |
| treat x 2012 | 0.079*** | 0.071** | 0.080*** | 0.090*** | 0.099*** | 0.073** | 0.055* | 0.038 |
| | (0.029) | (0.028) | (0.028) | (0.028) | (0.029) | (0.031) | (0.032) | (0.033) |
| treat x 2013 | 0.065* | 0.061* | 0.068** | 0.069** | 0.081*** | 0.051 | 0.037 | 0.008 |
| | (0.033) | (0.032) | (0.031) | (0.031) | (0.031) | (0.032) | (0.033) | (0.034) |
| treat x 2014 | 0.082*** | 0.076*** | 0.088*** | 0.095*** | 0.117*** | 0.080*** | 0.064** | 0.038 |
| | (0.026) | (0.026) | (0.026) | (0.026) | (0.027) | (0.028) | (0.029) | (0.029) |
| treat x 2015 | 0.105*** | 0.081*** | 0.077** | 0.071** | 0.082*** | 0.048 | 0.030 | 0.001 |
| | (0.030) | (0.030) | (0.030) | (0.030) | (0.031) | (0.032) | (0.034) | (0.035) |
| treat x 2016 | 0.136*** | 0.130*** | 0.139*** | 0.139*** | 0.156*** | 0.136*** | 0.126*** | 0.099** |
| | (0.030) | (0.030) | (0.032) | (0.033) | (0.035) | (0.038) | (0.041) | (0.044) |
| Observations | 3,815 | 3,815 | 3,815 | 3,815 | 3,815 | 3,815 | 3,815 | 3,815 |

Notes: All regressions were estimated using distributary-week level data and include distributary and year fixed effects. The regressions also include liner time trend for each of the two circles. The Standard errors are clustered at the distributary level.

Table A19: Changes in Population

| | Population | |
|---|---|---|
| | (1) | (2) |
| treat=1 × post=1 | -0.229*** | -0.203*** |
| | (0.0787) | (0.0781) |
| Observations | 9317644 | 9317644 |
| Sample mean | 6.385 | 6.385 |
| Fixed Effects | Distributary | Cell |

*Notes:* The table shows results from a reduced form regression where dependent variable is population in a grid-cell. The data includes two time period; 2010 and 2015. Due to data limitation, the 2010 is considered the base time period and 2015 as post. Both regressions include time fixed effect and either a distributary fixed effect or a grid-cell fixed effect. The treat is equals to 1 if a grid-cell is far away from the pollution source as well as rivers. The standard errors are clustered at at the distributary-level.

Table A20: Long-term Scarcity – Placebo Check

| | (1) | | (2) |
|---|---|---|---|
| | tail discharge relative to allocated | | |
| | 2SLS | | 2SLS |
| Pollution | -0.029 | | 0.001 |
| Mean of dependet variable | | 0.466 | |
| No of Distributaries | | 387 | |
| KP F-Stat | 19.95 | | 17.21 |
| Observations | 2,322 | | 2,322 |
| Controls | No | | Yes |
| Distributary and Year FE | Yes | | Yes |

Notes: All regressions were estimated using distributary-week level data. The controls include rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, and a dummy for areas that were flooded in 2010. Standard errors are clustered at the distributary level. The data only include Rabbi crop season.

Table A21: Long-term Scarcity – Further Checks on Second Stage Results

| | (1) | | (2) |
|---|---|---|---|
| | tail discharge relative to allocated | | |
| | 2SLS | | 2SLS |
| Pollution | 0.180* | | 0.240** |
| | (0.095) | | (0.110) |
| Mean of dependet variable | | 0.706 | |
| No of Distributaries | | 387 | |
| KP F-Stat | 10.83 | | 10.536 |
| Observations | 2,322 | | 2,322 |
| Controls | No | | Yes |
| Distributary and Year FE | Yes | | Yes |

Notes: All regressions were estimated using distributary-week level data. The controls include rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, and a dummy for areas that were flooded in 2010. Standard errors are clustered at the distributary level. All specification also control for circle specific linear time trend and Electrical Conductivity.

Table A22: Long-term Scarcity – Sub-division Specific Time Trends

| | (1) | | (2) |
|---|---|---|---|
| | tail discharge relative to allocated | | |
| | 2SLS | | 2SLS |
| Pollution | 0.219* | | 0.193* |
| Mean of dependet variable | | 0.706 | |
| No of Distributaries | | 387 | |
| KP F-Stat | 9.84 | | 13 |
| Observations | 2,016 | | 2,016 |
| Controls | No | | Yes |
| Distributary and Year FE | Yes | | Yes |

Notes: All regressions were estimated using distributary-week level data. The controls include rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, and a dummy for areas that were flooded in 2010. Standard errors are clustered at the distributary level. All specification include sub-division specific linear time trends.

Table A23: Long-term Scarcity – Robustness

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | tail discharge relative to allocated | | | |
| | 2SLS | 2SLS | 2SLS | 2SLS |
| Pollution | 0.258* | 0.257** | 0.097** | 0.125** |
| | (0.145) | (0.129) | (0.048) | (0.049) |
| Area Included | Only in the close Proximity | | Excluding Industrial Site | |
| Mean of dependet variable | 0.73 | | 0.66 | |
| No of Distributaries | 281 | | 355 | |
| KP F-Stat | 7.59 | 9.15 | 24.74 | 28.74 |
| Observations | 1,686 | 1,686 | 2,130 | 2,130 |
| Controls | No | Yes | No | Yes |
| Distributary and Year FE | Yes | Yes | Yes | Yes |

Notes: All regressions were estimated using distributary-week level data. The controls include rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, and a dummy for areas that were flooded in 2010. Standard errors are clustered at the distributary level.

Table A24: Long-term Scarcity – Placebo Check in the Upstream Areas

| | (1) | (2) |
|---|---|---|
| | tail discharge relative to allocated | |
| | Downstream | Upstream |
| Pollution (dummy) | 0.499** | 0.222 |
| | (0.197) | (0.565) |
| Mean of dependet variable | 0.769 | 0.619 |
| No of Distributaries | 229 | 165 |
| KP F-Stat | 19.12 | 0.863 |
| Observations | 1,344 | 978 |
| Controls | No | No |
| Distributary and Year FE | Yes | Yes |

Notes: All regressions were estimated using distributary-week level data and also include a linear time trends for each of the two circles. Standard errors are clustered at the distributary level. Both regressions report the second stage results. In the first stage, pollution was instrumented using variable $Treat$: that is a dummy variable and takes into account the distance to both industrial area as well as rivers.

Table A25: Inter-village Cooperation and Proximity to Drains

| | tail dischrage relative to allocated | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Pollution | 0.119** | 0.155*** | 0.189** | 0.195** | | |
| | (0.0526) | (0.0552) | (0.0948) | (0.0942) | | |
| Pollution (Dummy) | | | | | 0.209*** | 0.227*** |
| | | | | | (0.0807) | (0.0753) |
| Observations | 2322 | 2322 | 2322 | 2322 | 3483 | 3483 |
| Sample mean | 0.712 | 0.712 | 0.712 | 0.712 | 0.712 | 0.712 |
| Instruments | All three | All three | Drains | Drains | All three | All three |
| Controls | No | Yes | No | Yes | No | Yes |
| KP F-Stat | 10.56 | 11.00 | 12.68 | 12.53 | 31.00 | 38.75 |

*Notes:* The standard errors are clustered at the distributary level. The instrument "Drains" is defined as "close to drain × post" where post is 1 after 2009 and close to drain is defined as distributary where average distance of the villages from a nearby drain is less than 5km. The "close to drain" is also equals to 0 for areas that are inside the industrial district or are close to the rivers. The instruments "All three" are defined as: "distance to rivers × post", "distance to industrial areas × post", and "close to drain × post".

Table A26: Long-term Scarcity – Political Patronage

| | (1) | (2) |
|---|---|---|
| | tail discharge relative to allocated | |
| | 2SLS | 2SLS |
| Pollution | 0.148* | 0.210*** |
| | (0.082) | (0.079) |
| Mean of dependet variable | 0.69 | |
| No of Distributaries | 326 | |
| KP F-Stat | 10.05 | 15.26 |
| Observations | 1,956 | 1,956 |
| Controls | No | Yes |
| Distributary and Year FE | Yes | Yes |

Notes: All regressions were estimated using distributary-week level data. The controls include rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, and a dummy for areas that were flooded in 2010. Standard errors are clustered at the distributary level. All specification include party FE of the Member of Provincial Assembly over two election tenure.

Table A27: Long-term Scarcity – Dummy Variable for Contamination

| | (1) | | (2) |
|---|---|---|---|
| | tail discharge relative to allocated | | |
| | 2SLS | | 2SLS |
| $SAR > \overline{SAR}$ | 0.310** | | 0.374*** |
| | (0.142) | | (0.144) |
| Mean of dependet variable | | 0.706 | |
| No of Distributaries | | 387 | |
| KP F-Stat | 20.5 | | 23.44 |
| Observations | 2,322 | | 2,322 |
| Controls | No | | Yes |
| Distributary and Year FE | Yes | | Yes |

Notes: All regressions were estimated using distributary-week level data. The controls include rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, and a dummy for areas that were flooded in 2010. Standard errors are clustered at the distributary level. The $\overline{SAR}$ is the value of SAR above which the groundwater is likely to negatively affect crop growth.

Table A28: Long-term Scarcity – Symmetric Winsorization

| | (1) | | (2) |
|---|---|---|---|
| | tail discharge relative to allocated | | |
| | 2SLS | | 2SLS |
| Pollution | 0.129** | | 0.158** |
| | (0.060) | | (0.062) |
| Mean of dependet variable | | 0.706 | |
| No of Distributaries | | 326 | |
| KP F-Stat | 19.93 | | 21.61 |
| Observations | 2,322 | | 2,322 |
| Controls | No | | Yes |
| Distributary and Year FE | Yes | | Yes |

Notes: All regressions were estimated using distributary-week level data. The controls include rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, and a dummy for areas that were flooded in 2010. Standard errors are clustered at the distributary level.

Table A29: Informal Institutions – Survey Data

| | =1 if use informal institutions | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| pollution (dummy) | 0.0489 | | 0.0888** | |
| | (0.0400) | | (0.0426) | |
| pollution (self-reported) | | 0.0380 | | 0.00500 |
| | | (0.0372) | | (0.0440) |
| Observations | 782 | 782 | 595 | 595 |
| Sample mean | 0.252 | 0.252 | 0.261 | 0.261 |
| Sample | All | All | Tail | Tail |
| Controls | Yes | Yes | Yes | Yes |

*Notes:* The tables shows results from weighted regressions that also control for position of the village on a distributary, tehsil FE, ground water, price, location of a distributary, and total cultivable area of a distributary. Standard errors clustered by distributary.

Table A30: Devolution Reforms

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | tail discharge relative to allocated | | | | | | | |
| | OLS | OLS | 2SLS | 2SLS | OLS | OLS | 2SLS | 2SLS |
| Devolved 1 | -0.182*** | -0.175*** | -0.258*** | -0.230*** | -0.213*** | -0.200*** | -0.265*** | -0.237*** |
| | (0.040) | (0.039) | (0.090) | (0.087) | (0.056) | (0.055) | (0.091) | (0.090) |
| Devolved 2 | -0.012 | -0.017 | -0.019 | -0.021 | -0.021 | -0.027 | -0.021 | -0.024 |
| | (0.019) | (0.019) | (0.044) | (0.043) | (0.026) | (0.026) | (0.044) | (0.045) |
| Devolved 1 x SAR | 0.010*** | 0.010*** | 0.018** | 0.016* | | | | |
| | (0.003) | (0.003) | (0.009) | (0.008) | | | | |
| Devolved 2 x SAR | -0.000 | 0.000 | 0.001 | 0.001 | | | | |
| | (0.001) | (0.001) | (0.004) | (0.004) | | | | |
| Devolved 1 x EC | | | | | 0.054*** | 0.051*** | 0.078** | 0.068* |
| | | | | | (0.019) | (0.018) | (0.036) | (0.036) |
| Devolved 2 x EC | | | | | 0.003 | 0.006 | 0.003 | 0.004 |
| | | | | | (0.011) | (0.010) | (0.021) | (0.021) |
| Mean of dep var | 0.726 | | | | | | | |
| Wald F-Stat | N/A | N/A | 51.894 | 45.733 | N/A | N/A | 140.052 | 130.263 |
| Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| N | 3,483 | 3,483 | 3,483 | 3,483 | 3,483 | 3,483 | 3,483 | 3,483 |

Notes: All regressions were estimated using distributary-week level data. The controls include rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, and a dummy for areas that were flooded in 2010. Standard errors are clustered at the Farmer Organization level.

Table A31: Devolution Reforms – Alternate Specification

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | tail discharge relative to allocated | | | | | | | |
| | OLS | OLS | 2SLS | 2SLS | OLS | OLS | 2SLS | 2SLS |
| First Tenure | -0.169*** | -0.161*** | -0.220*** | -0.197*** | -0.191*** | -0.182*** | -0.201*** | -0.181*** |
| | (0.037) | (0.037) | (0.074) | (0.072) | (0.047) | (0.046) | (0.064) | (0.062) |
| Second Tenure | -0.010 | -0.014 | -0.016 | -0.020 | -0.009 | -0.013 | -0.019 | -0.022 |
| | (0.017) | (0.017) | (0.033) | (0.033) | (0.019) | (0.019) | (0.028) | (0.028) |
| First Tenure x SAR | 0.145*** | 0.141*** | 0.244** | 0.211* | | | | |
| | (0.040) | (0.039) | (0.121) | (0.117) | | | | |
| Second Tenure x SAR | -0.008 | -0.004 | 0.007 | 0.011 | | | | |
| | (0.021) | (0.021) | (0.062) | (0.060) | | | | |
| First Tenure x EC | | | | | 0.150*** | 0.145*** | 0.166** | 0.143* |
| | | | | | (0.048) | (0.047) | (0.079) | (0.077) |
| Second Tenure x EC | | | | | -0.014 | -0.009 | 0.006 | 0.008 |
| | | | | | (0.023) | (0.022) | (0.042) | (0.042) |
| Mean of dep var | 0.726 | | | | | | | |
| Wald F-Stat | N/A | N/A | 76.792 | 67.335 | N/A | N/A | 144.316 | 131.79 |
| Controls | No | Yes | No | Yes | No | Yes | No | Yes |
| N | 3,483 | 3,483 | 3,483 | 3,483 | 3,483 | 3,483 | 3,483 | 3,483 |

Notes: Notes: All regressions were estimated using distributary-week level data. The controls include rainfall, temperature, interaction of rainfall and temperature, a dummy variable for reform, and a dummy for areas that were flooded in 2010. Standard errors are clustered at the distributary level.

# C Community Survey Data Appendix

In summer 2018, I conducted a community survey in a sample of 800 villages. In each village, enumerators interviewed two respondents. The goal was to collect information that can help in understanding how farmers resolve inter- and within-village disputes related to surface water. The interviews were conducted with farmers that were frequently involved in dealing with surface water related disputes, and ethics approval was obtained from Humanities and Social Sciences Research Ethics Committee (HSSREC) based at Wariwck University. The questionnaire was further vetted by Irrigation Department and Home Department of Government of Punjab, Pakistan.[3]

To select the villages, I first conducted power calculations. The key outcome variable captures the presence of institutions that resolve inter-village disputes. However, there is no information available on this variable in the secondary datasets. Therefore, I used the data on the presence of village-level informal institutions from Mouzza (Village) Census 2008 to conduct the power calculations. Based on these estimates, I sampled 800 villages from 180 distributaries. This selection was stratified on three dimension; size of a distributary, proximity to the industrial district, and location of a village on a distributary. Since, the inter-village water disputes primarily affect tail-end villagers, these villages were over-sampled. In short, from each distributary, villages at the tail-end were twice as likely to get selected compared to head-end villages.

Some of the village have multiple settlements. In such cases, enumerators were asked to write down names of all the settlements that were connected to a listed distributary and then randomly select one. For respondent selection, enumerators recieved following instructions: first, they conducted one interview with *lambardar* —- an unofficial village representative who is usually the first point of contact for dispute resolution for most of the villagers. Then, enumerators asked *lambardar* to list down all the farmers that are usually involved in handling water related disputes. The second respondent was selected randomly from the list. In the final data, nearly 41% of the interviews were conducted with *lambardars*, 47.5% with village elders or leader of Jirga/Panchayat, and rest with other farmers.

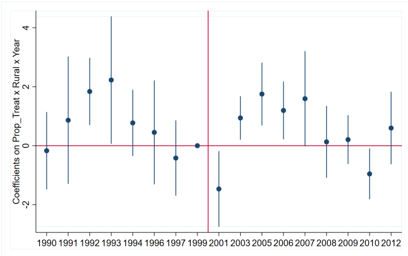
During the survey, enumerators found that mapping of villages to distributary was not perfect. They found a few villages that were not part of the sampled

[3]These two organizations advised me to exclude a couple of questions related to protests against water pollution, and therefore, I was not able to collect any such information.

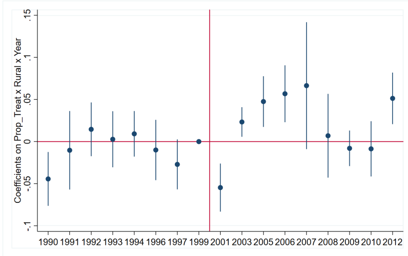
distribtuary. For such cases, I randomly selected another village from the same location on that distributary as a substitute.

# Appendix C (for Chapter 3)

## A Additional Figures



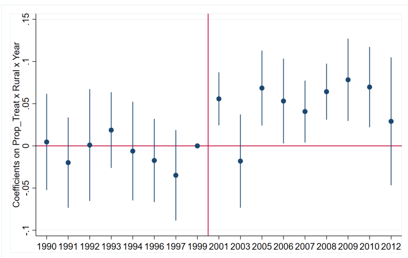

(a) Total Hours – Proportion Treated



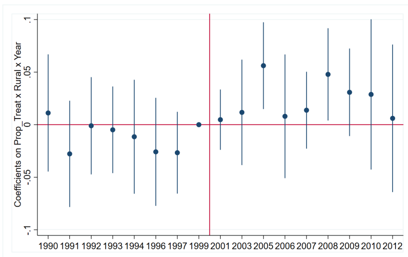

(b) Currently Working – Proportion Treated

*Notes:* Figures plot coefficients from triple difference specifications (equation (3.4)). Only data on the older cohort (16-60) is included. Standards errors are clustered by the district and 95% confidence intervals are shown. "Total Hours" is the total number of hours worked in the past week.

Figure C1: Older Cohorts – Triple Difference – Proportion Treated




(a) Currently Enrolled – Proportion Treated




(b) Education 5+ – Proportion Treated

*Notes:* Figures plot estimated coefficients of equation (3.4). Only data on the younger cohort (10-15) is included. Standards errors are clustered by the district and 95% confidence intervals are shown. "Education 5+" is more than or equal to 5 years of education.

Figure C2: Younger Cohorts – Triple Difference – Proportion Treated

# B Additional Tables

Table C1: Triple Difference Estimates

| | Older Cohort | | Younger Cohort | |
|---|---|---|---|---|
| | (1) work | (2) hours | (3) currently enrolled | (4) educ 5+ |
| **Panel A: Baseline Specification** | | | | |
| treat=1 × rural=1 × post=1 | 0.0382 | 0.0182 | 0.0676** | 0.0281 |
| | (0.0276) | (0.896) | (0.0280) | (0.0234) |
| Observations | 711682 | 711682 | 208696 | 208696 |
| Dependent Variable mean | 0.0960 | 3.757 | 0.533 | 0.370 |
| **Panel B: With Province × Year FE** | | | | |
| treat=1 × rural=1 × post=1 | 0.0392 | -0.114 | 0.0767*** | 0.0354 |
| | (0.0278) | (0.962) | (0.0276) | (0.0223) |
| Observations | 711682 | 711682 | 208696 | 208696 |
| Dependent Variable mean | 0.0960 | 3.757 | 0.533 | 0.370 |
| **Panel C: With Controls** | | | | |
| treat=1 × rural=1 × post=1 | 0.0385 | 0.0274 | 0.0672** | 0.0277 |
| | (0.0273) | (0.897) | (0.0279) | (0.0233) |
| Observations | 711682 | 711682 | 208696 | 208696 |
| Dependent Variable mean | 0.0960 | 3.757 | 0.533 | 0.370 |

*Notes:* ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Standard errors clustered by district in parentheses. All panels present estimates from equation (3.3). Older cohorts include women of age 16-50 and younger cohorts include women of age 10-15. For both set of cohorts, controls include total number of household members, proportion of female household members, and whether the household head is female. For older the older cohorts, we also control for marital status.

Table C2: Robustness – Migration

| | Older Cohort | | Younger Cohort | |
|---|---|---|---|---|
| | (1) work | (2) hours | (3) currently enrolled | (4) educ 5+ |
| **Panel A: Double Difference** | | | | |
| treat=1 × post=1 | 0.0699** | 1.494 | 0.0749*** | 0.0560*** |
| | (0.0298) | (1.123) | (0.0218) | (0.0153) |
| Observations | 290393 | 290393 | 94968 | 94968 |
| Dependent Variable mean | 0.176 | 7.479 | 0.479 | 0.304 |
| **Panel B: Double Difference – Proportion Treated** | | | | |
| post=1 × proportion treated | 0.0476*** | 0.881* | 0.0488*** | 0.0340*** |
| | (0.0146) | (0.460) | (0.0156) | (0.0120) |
| Observations | 290393 | 290393 | 94968 | 94968 |
| Dependent Variable mean | 0.176 | 7.479 | 0.479 | 0.304 |
| **Panel C: Triple Difference** | | | | |
| treat=1 × rural=1 × post=1 | 0.0512** | 0.736 | 0.0698** | 0.0471* |
| | (0.0226) | (0.897) | (0.0312) | (0.0252) |
| Observations | 476264 | 476264 | 158719 | 158719 |
| Dependent Variable mean | 0.140 | 5.862 | 0.601 | 0.400 |

*Notes:* ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Standard errors clustered by district in parentheses. Panel A and B present estimates from equation (3.1), whereas Panel C presents estimates from equation (3.3). Older cohorts include women of age 16-50 and younger cohorts include women of age 10-15. Panel A and B only use data on women living in the rural areas. “Proportion treated” is a ratio of state-owned land and total cultivable land in a district.

Table C3: Robustness – Excluding Lahore

| | Older Cohort | | Younger Cohort | |
|---|---|---|---|---|
| | (1) work | (2) hours | (3) currently enrolled | (4) educ 5+ |
| **Panel A: Double Difference** | | | | |
| treat=1 × post=1 | 0.0703* | 1.178 | 0.0319 | 0.0295** |
| | (0.0354) | (1.194) | (0.0226) | (0.0125) |
| Observations | 392644 | 392644 | 115594 | 115594 |
| Dependent Variable mean | 0.170 | 7.077 | 0.447 | 0.283 |
| **Panel B: Double Difference – Proportion Treated** | | | | |
| post=1 × proportion treated | 0.0438** | 0.201 | 0.0224* | 0.0244*** |
| | (0.0176) | (0.608) | (0.0128) | (0.00866) |
| Observations | 392644 | 392644 | 115594 | 115594 |
| Dependent Variable mean | 0.170 | 7.077 | 0.447 | 0.283 |
| **Panel C: Triple Difference** | | | | |
| treat=1 × rural=1 × post=1 | 0.0365 | -0.0891 | 0.0497** | 0.0106 |
| | (0.0251) | (0.860) | (0.0236) | (0.0226) |
| Observations | 676840 | 676840 | 198886 | 198886 |
| Dependent Variable mean | 0.131 | 5.416 | 0.576 | 0.384 |

*Notes:* ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Standard errors clustered by district in parentheses. Panel A and B present estimates from equation (3.1), whereas Panel C presents estimates from equation (3.3). Older cohorts include women of age 16-50 and younger cohorts include women of age 10-15. Panel A and B only use data on women living in the rural areas. "Proportion treated" is a ratio of state-owned land and total cultivable land in a district.

Table C4: Robustness – Neighboring Districts

| | Older Cohort | | Younger Cohort | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | work | hours | currently enrolled | educ 5+ |
| **Panel A: Double Difference** | | | | |
| neighbouring districts=1 × post=1 | -0.0241 | -0.430 | 0.00329 | 0.0276 |
| | (0.0337) | (1.219) | (0.0393) | (0.0277) |
| Observations | 332927 | 332927 | 98438 | 98438 |
| Dependent Variable mean | 0.147 | 6.320 | 0.436 | 0.273 |
| **Panel C: Triple Difference** | | | | |
| neighbouring districts=1 × rural=1 × post=1 | -0.0313 | -1.028 | 0.00450 | -0.00181 |
| | (0.0325) | (1.003) | (0.0366) | (0.0305) |
| Observations | 565805 | 565805 | 167259 | 167259 |
| Dependent Variable mean | 0.115 | 4.870 | 0.566 | 0.372 |

*Notes:* ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. Standard errors clustered by district in parentheses. Panel A and B present estimates from equation (3.1), whereas Panel C presents estimates from equation (3.3). Older cohorts include women of age 16-50 and younger cohorts include women of age 10-15. Panel A and B only use data on women living in the rural areas. "Proportion treated" is a ratio of state-owned land and total cultivable land in a district.

Table C5: Distance to School

| | (1) 0-2km | (2) 2-5km | (3) 5-10km | (4) 10-20km | (5) >20km |
|---|---|---|---|---|---|
| treat=1 × post=1 | 0.00402 | -0.00319 | -0.00444 | 0.00196 | 0.00164 |
| | (0.0225) | (0.0168) | (0.00522) | (0.00225) | (0.00200) |
| Observations | 148039 | 148039 | 148039 | 148039 | 148039 |
| Dependent Variable mean | 0.872 | 0.0920 | 0.0230 | 0.00800 | 0.00400 |

*Notes:* Standard errors are clustered at the district-level. Data of household members age 4-15 years taken from the Pakistan Social and Living Standards Measurement (PSLM) 1998, 2001, 2005, 2007, and 2011. All variables are dummy variable that are equal to 1 if round trip to a school is within a given range. "0-2km" is more than or equal to 0 and less than or equal to 2 km. "2-5km" is more than 2 and less than or equal to 5 km. "5-10km" is more than 5 and less than or equal to 10 km. "10-20km" is more than 10 and less than or equal to 20 km. ">20km" is more than 20 km.

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