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Author: Giridaran Subramaniam

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ESSAYS IN MACROECONOMICS AND DEVELOPMENT

Giridaran Subramaniam

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Giridaran Subramaniam

Advisors:

Prof. Ryan Chahrour (Co-Chair)

Prof. Fabio Schiantarelli (Co-Chair)

Prof. Anant Nyshadham

Abstract

This dissertation consists of three chapters.

The first chapter, *"The Supply-Side Effects of India's Demonetization"*, investigates the supply-side effects of a unique monetary shock – the 2016 Indian *demonetization* – that made 86% of currency in circulation illegal overnight. Exploiting cross-sectional variation in firm and industry characteristics that correlate with cash usage and exposure to the informal sector, I find that firms that use cash more and obtain larger shares of labor or material inputs from the informal sector, experienced declines in their labor and material shares after demonetization. I also show that casual laborers were more likely to report being unemployed in the months following demonetization. These findings document a supply channel for demonetization and also show that cash plays an essential role in India's informal sector. Crucially, given that India's formal sector is highly dependent on the informal sector for labor and materials, any shock to the supply of cash is likely to have affected the economy as a whole.

In the second chapter, *"Directed Lending and Misallocation: Evidence from India"*, joint with Deeksha Kale, we leverage a natural experiment to study whether targeted credit policy can help reduce misallocation. In 2006, the Government of India modified the definition of small firms thereby expanding eligibility to a directed credit program. We show that the credit policy changed eligible firms' input wedges and thereby reduced misallocation. For firms with

initially higher MRPK, the policy resulted in relatively larger increases in physical capital and decreased the MRPK. This policy moderately reduced *within-industry* dispersion of MRPK and increased aggregate productivity.

Finally, in the third chapter, *"Victims of Consequence: Evidence on Child Outcomes using Microdata from a Civil War"*, joint with Sajala Pandey, we study the short-run impacts of violent events on child time allocation, curative health-care, and education. Exploiting spatial and temporal variation in exposure to local-level armed conflict, we find that an increase in violent events: (i) leads to an increase in contemporaneous hours worked by children, with the effect being substantial for agricultural work; (ii) decreases the likelihood of parents taking their children to visit a health-care facility to seek curative care; and (iii) results in a reduced likelihood of attending school, along with a decline in years of education. Overall, the results indicate that the war affected schooling and time allocation of boys whereas girls were less likely to get curative health-care.

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CHAPTER 1

THE SUPPLY-SIDE EFFECTS OF INDIA'S DEMONETIZATION

1.1 Introduction

Paper currency is still widely used in both, developed and developing countries alike. Despite repeated calls for moving away from cash (Rogoff, 2015) and having made tremendous technological advances in payment technologies, cash facilitates easy exchange by overcoming financial barriers such as access to banking services. Businesses, even, may hold cash for precautionary motives or transactional purposes in order to pay for certain inputs that are easier to pay for in cash. The latter function of cash is reminiscent of countries with a large informal or unorganized production sector[1] where the formal and informal sector are interdependent. Firms rely on informal employment[2] by hiring temporary workers, usually without a formal contract, involve them in casual yet full-time labor, and typically pay these workers their wages in cash because they do not have or use bank accounts. In such environments, cash plays an essential role in overcoming the transactional friction.

In this chapter, I study the importance of cash to firms in India by exploiting industry reliance on cash and, exposure to informal employment and the informal sector. In order to do this, I analyze a unique unanticipated shock

[1] The "informal sector" or "unorganized sector" comprises of firms that are not registered. The exact definition varies from country to country.

[2] Informal employment is a job-based concept and is defined in terms of the employer-employee contractual relationship and basic protections that are included with the job of the worker (ILO, 2018). Typically, these "casual" workers get their wages in cash, work without a formal contract, and are not covered by regulatory protections (RBI, 2017).

to the supply of existing currency in circulation. On November 8th, 2016, the Government of India announced that the two largest denomination currency notes would cease to be legal tender. This was termed as *demonetization* and the policy amounted to rendering 86% of currency-in-circulation illegal tender overnight. Due to additional constraints on printing and distributing new notes to replace the demonetized currency, the policy resulted in a large and abrupt decline in the supply of cash (see Figure 1.B.1) in the months that followed. I exploit the unanticipated nature of the episode as a natural experiment to test whether firms in industries that were more reliant on informal employment and more exposed to the informal sector for material inputs, were disproportionately affected by the shock. Unable to pay informal workers their wages and materials suppliers the cost of goods in cash, these firms were forced to lay off part of their work force and procure fewer materials, respectively, in the period immediately after the demonetization announcement.

My analysis proceeds in two steps. *First*, I construct measures of cash usage, reliance on informal employment, and exposure to the informal sector, using a survey of workers, a census of manufacturing, and a survey of informal enterprises. It is key that these measures are taken from data prior to the demonetization episode so that I can identify from the cross-section of industries. I then merge these to a database of quarterly financial statements of firms and estimate the near-term effect of the demonetization shock using a difference-in-differences approach. The sudden and unanticipated nature of the announcement renders itself useful for and provides credence to the identification strategy. Additionally, cross-sectional heterogeneity in industry and firm exposure to informality helps unpack the causal effects of the shock by naturally producing firms that were treated with different intensity. *Second*, I use a household panel to verify whether casual-type workers were more likely to report being unemployed in the months after demonetization relative to formal-type workers.

I find three main results. *First*, I find that firms in industries characterized by greater cash usage hired fewer workers and purchased fewer materials following the demonetization shock. I show this by documenting a relative decline in firms' labor share and materials share in value added for industries with greater cash usage in the quarters during and after the sudden announcement. Firms that hire more informal workers and inputs from the informal sector need to hold more cash. Hence, I measure cash usage in two ways: one, the industry share of cash in current assets, and two, the industry share of cash in total spending on labor and materials. A one standard deviation increase in cash usage, by either measure, translates to a 1.5 percentage point decline in labor share, and a 1.6 percentage point decline in materials share in value added.

Second, I construct measures of industry dependence on informal employment and a measure of exposure to the informal sector for materials. For labor I measure informal employment by the fraction of informal workers in total workforce in an industry, and the fraction of casual-type payments made to workers in an industry. I find that a one standard deviation increase in informal employment, by either measure, is associated with a 0.5 percentage point decline in labor share in value added. For materials, I first construct a firm-level measure of exposure to the informal sector. This measure uses product-level purchase value of materials by firms and the extent of informality at the respective product-mapped industry using value added by informal enterprises vis-à-vis that of total (formal and informal) enterprises. I find that firms that are more exposed to the informal sector by this measure experienced a significant decline in their materials share in value added in the quarter immediately after demonetization.

Third, using a household panel I show that casual or temporary laborers (such as, wage laborers, hawkers, support staff etc.), relative to salaried workers in formal employment (such as, businessmen, organized farmers, industrial workers, white collar clerical employees etc.) were more likely to report becoming unemployed in the months after demonetization. These worker-level findings thus verify my findings from the firm-level analysis that document a decline in the labor share.

My findings highlight that there were significant supply-side effects caused by the large and unanticipated contraction in currency in circulation. Cash plays an essential role in India's informal sector and given that India's formal sector is highly dependent on the informal sector, a large shock to the supply of cash is likely to have affected the economy as a whole. The identification strategy based on the cross-section of industry dependence and exposure to informally-sourced inputs is limited in documenting near term impacts of the episode. My results also indicate that these negative effects are relatively smaller even dissipate in the quarter after the demonetization announcement. Given that available data exclude the informal sector, which was presumably most hurt by the shock, my findings are a conservative lower bound of the total effects.

Aggregate data do not describe the true effects of demonetization for two reasons. One, output from the informal sector is not measured but estimated as a fixed proportion of formal sector, and that factor is updated every few years by conducting a survey. Hence, measured GDP will understate the effects of demonetization. Second, as there was a shift away from cash to electronic means of payment, many under-the-table transactions were shifted to the measured economy; any assumption that official and black market GDP move together is probably invalid in that moment. Hence, I study the consequences of demonetization in the cross-section of industries and firms.

The rest of this chapter is organized as follows: In Section 1.2, I discuss the contributions of this chapter and provide an overview of the related literature. In Section 2.2, I present a background of the main events surrounding the demonetization episode, provide an account of the events that followed the shock, and describe the legislative framework concerning labor regulation in India that is relevant to my study. In Section 1.4, I sketch a model of how firms react to the demonetization and summarize main results from the model. I then present the data sources I use in Section 1.5 and discuss the empirical strategy to test the model implications using the data in Section 3.5. I present results in Section 2.5 and conclude in Section 1.8.

1.2 Related Literature

This chapter contributes to the existing literature in three broad areas. First, the findings in this chapter add to the growing list of studies that attempt to unpack the effects of demonetization on the Indian economy. Second, viewing the demonetization episode as a natural experiment, specifically, as a large and an unexpected monetary shock to the economy, the empirical findings in this chapter provide support to the literature on identifying the real effects of nominal disturbances. Third, my findings also highlight the link between the formal and informal sectors in developing economies like India where there is a prevalent use of informal employment and a heavy reliance on the informal sector by firms in the formal sector in India.

An evolving number of papers attempt to identify demand-side impacts of demonetization on the real economy[3]. Chodorow-Reich et al. (2018) exploit

[3]These are in the spirit of Velde (2009) who unpacks the effects of three overnight diminu-

the geographic distribution of demonetized and new notes in order to identify the impact of the currency supply shock on real economic activity, deposits, credit, and alternative forms of payment technology. This chapter also sheds light on the aggregate effects of the demonetization episode. While their paper focuses purely on identifying the demand-side effects from geographical heterogeneity, I am able to identify significant supply-side effects by exploiting cross-sectional heterogeneity across industries. On the household-side, Karmakar and Narayanan (2019) provide additional evidence of households without bank accounts witnessing declines in income and expenditure, and of smoothing behavior by way of increased leverage from informal sources. On the firm-side, Banerjee and Kala (2017) find from surveys that wholesalers and retailers reported 40% lower sales in December and January.

Viewing the demonetization episode as an aggregate coordination device and focusing on adoption dynamics by retailers, Crouzet et al. (2019) document that the episode led to a permanent shift to electronic payments even though the shock was transitory. Focusing on agricultural markets, Aggarwal and Narayanan (2017) estimate the impact of demonetization on arrivals and prices of agricultural commodities, and find reduced trade, arrivals and lower prices in government regulated markets (or *mandis*) in the short-run. This decline in prices appears to have recovered over a period of three months. Taken together, the last two studies provide some evidence for significant supply-side effects along with demand-side effects of demonetization.

Second, this chapter also relates to the literature that attempts to identify the real effects of surprise nominal shocks to the economy. Many different approaches have been followed in order to identify these real effects from plausibly exogenous variation in monetary policy (Christiano et al., 2005; Naka-

tions of gold and silver coins in eighteenth century France using a narrative approach.

mura and Steinsson, 2018). Some approaches include narrative studies such as Romer and Romer (1989) who peruse historical records and select episodes where there were large disturbances in monetary policy that were not driven by the real sector. They then test whether output is unusually low (high) following the negative (positive) shocks of this kind. The Indian demonetization is a well-suited episode for this kind of analysis, in that at least the policy was completely unanticipated and plausibly unrelated to the state of the real economy, in addition to the shock banning 86% of currency-in-circulation. Nakamura and Steinsson (2018) point out that in order to use the controlled experiment method of identification in monetary policy, identification may come from either "natural experiments" where the change in policy is large relative to potential confounding factors, that may be controlled for, or, an approach that focuses on large policy actions for which it can be plausibly argued that potentially confounding factors are drowned out.

This chapter also relates to an older strand of literature in monetary theory that asks whether money can be thought of as an input in the production function of firms[4]. Fischer (1974) provides two theoretical arguments that allow for money to be treated as a factor of production, while also admitting that treating money as an input in production is more for the convenience it offers[5]. In his paper, he calls for a deeper explanation of the demand for money by firms. My claim in this study is that if at least one type of input needs to be paid in cash, this creates a need to firms for holding cash. Nadiri (1969) assumes a model of a firm that minimizes costs subject to a production function which includes cash as an input with an aim to estimate the determinants of real money bal-

[4]This was also pointed out in the Economic Survey 2016-17 as a thought exercise in order to understand the aggregate supply side effects of demonetization (GOI, 2017).

[5]This is similar to the argument provided by Feenstra (1986), that there is a functional equivalence between treating money as an argument in the utility function and as an input that lower liquidity costs.

ances in the U.S. manufacturing sector. This chapter can be thought of as an improvement over this approach in terms of identification.

My understanding of this strand of literature is that thinking about money as an input in production provides for a good thought exercise that helps unpack the supply side effects of monetary shocks such as demonetization. Considering money as an input in the production function may proxy for the various uses of money to firms, insofar as the neoclassical production function is itself a supposedly reduced form of an engineering relationship between various inputs and output. In this study, the function of money to firms is for transaction purposes – firms need to pay for certain inputs in cash, and this is the reason why they hold real money balances. Tax evasion and the speculative motive may very well be other reasons for which firms need to hold cash.

Lastly, my findings highlight the link between informal and formal activity in India, specifically the use of informal employment by formal sector firms. Formal sector firms recruit half of their labor force via informal employment[6] (Narayanan, 2015). Typically, these workers get their wages in cash and are not covered by regulatory provisions (RBI, 2017). Firms hire informal workers to avoid providing for job benefits that come with a contract, and for workers lack of opportunities in the formal economy may make informal employment lucrative. Substituting permanent workers with workers on temporary, or even no formal contract – a term coined as 'flexibilization' of labor – is a global trend, and has increased especially in developing countries (Saha et al., 2013). Strict regulatory provisions and open-ended contracts typically contribute to an increased use of temporary workers (Balakrishnan et al., 2010). A report by the ILO (2018) states that while more than 60% of the world's employed population earn their livelihoods from the informal economy, about 11% of informal

[6] Informal employment as a share of formal sector employment increased from about 38% in 1999-2000 to more than 50% in 2011-12.

workers are in the formal sector. Demonetization was expected to have disrupted the informal economy disproportionately more than the formal economy, as the former is more cash-intensive. However, given the link between informal and formal activity in India, demonetization must have affected firms that were more exposed to the informal economy. Informal economic activity is measured using surveys from time to time, and is estimated using indicators and proxies from the formal economy[7]. To the extent that we have data only for the formal economy, my results will be an underestimate of the true effect of demonetization.

Additionally, given the setting of a removal of currency in a cash-intensive economy, my findings also hint at some of the potential costs of abolishing cash and provide additional support to the the provisions that need to be made by the government before such policies are implemented. For instance, Rogoff (2015) advises that access to free basic debit accounts and basic smartphones must be in place before making the gradual transition to a cashless economy.

1.3 Background

In this section, I first describe the main events that took place following the announcement of demonetization that document the shortage of cash faced by the economy afterwards. Next, I provide accounts from newspaper reports and anecdotal evidence pertaining to the cash shortage faced by firms focusing on worker layoffs and job losses. Finally, I provide some background of the legislative framework pertaining to labor in India that lead firms to hire temporary, casual, and contract workers.

[7]For instance, manufacturing in India is proxied using the Index of Industrial Production (IIP), which includes mostly large establishments. As noted in the Economic Survey 2016-17, the effect of demonetization on informal economic activity will be underestimated (GOI, 2017).

1.3.1 Demonetization

On November 8th, 2016, the Prime Minister of India announced via an unexpected nationally televised address, that currency notes of the two largest denominations, the ₹500 and ₹1,000 notes (worth about \$7.5 and \$15, respectively), would be stripped of their status as legal tender (RBI, 2016). The stated goals of the policy were to eliminate fake currency and impose losses on those who held black money in the form of unaccounted earnings and bribes. These objectives were justified by stating that fake currency was increasingly being used to finance terrorism and that the policy would eventually reduce corruption. In order to achieve the policy's objectives, the policy, including its announcement, had to be kept a secret and very few high-ranking government officials knew about it prior to the televised address.

During the same address, the introduction of new ₹500 and ₹2,000 banknotes with improved security features to replace the old ones was also announced. Holders of the old notes could either deposit them at banks in exchange for lower denomination or newer notes newer notes but could not use them in transactions with effect from November 9th. Withdrawal and deposit limits were placed on individuals and businesses in order to avoid excessive currency withdrawal due to public frenzy or fear and to monitor large deposits, respectively. The deadline to return the old notes was set at December 31st, 2016.

The demonetized notes accounted for about 86% of currency-in-circulation (CIC) in value terms, which was nearly 12% of GDP in 2015-16. Effectively the policy resulted in a sharp decline in total currency in circulation from a pre-demonetization peak of about ₹15,205.65 billions to a post-demonetization trough of ₹7,832.57 billions – which amounted to an actual decline of about 50% . Figure 1.B.1 plots total currency with the public and notes in circulation from fortnightly measures of money stock provided by the central bank. It is worth pointing out that this data includes, both, the demonetization and remonetization phase of the policy. The slow replacement of old notes with new ones resulted in a sharp decline in CIC.

First, the new notes were not printed or distributed prior to the policy announcement. The amount of cash that needed to be printed was several magnitudes higher relative to the usual printing activity undertaken by the printing press. This slow process caused additional delays during the printing and distribution process. Second, owing to the extreme secrecy of the operation, retail banks were not informed before the announcement was made and were thereby left unprepared with their capacities not updated to smoothly implement the replacement phase of the policy. One example is during the remonetization process, since the new currency notes differed in size compared to the old ones, in order to put them into circulation ATM machines needed replacing which resulted in delays and slowed down the remonetization process. Third, the general process of introducing new currency into the economy was subject to the existing capacity of the central bank's infrastructure, which could not have been vastly updated prior to the announcement.

Holders of the old notes were forced to turn in their cash, either depositing them in their banks accounts, or exchanging them for lower denomination currency. In addition to the inadequacy of currency supply, various limits were placed on exchange and withdrawal of currency due to the cash shortage. Initially, the exchange of old notes was capped to ₹4,000 ($60) per person per day, cash withdrawals from bank accounts at ₹10,000 ($150) per day and ₹20,000 per week ($300), and ATM-withdrawals were initially capped at ₹2,000 ($30) per day per card. Exceptions were made in the case of purchasing airline or train tickets, paying for utility bills, where old notes could be used. For small-businesses, withdrawals were restricted at ₹50,000 per week. Chodorow-Reich et al. (2018) point out that withdrawal of new notes in an area was determined by the supply of new notes to the same area. This provides evidence of the economy being cash constrained in the short term.

1.3.2 Evidence of Cash Crunch faced by Firms and Job Losses

India is a heavily cash-based economy[8]. The months following the announcement were filled with newspaper accounts of shortages of cash by households and even by firms[9]. These included shortage of liquidity and working capital, leading to worker layoffs, retrenchments, shutdowns, etc. Firms, and contractors employed to hire workers on behalf of firms, who pay their workers in cash reported shortage of liquidity to pay employees their wages (Bhowmick, 2016). Such workers typically do not have bank accounts and are hence paid daily wages in cash[10]. Facing cash flow issues contractors in labor-intensive industries, such as garment manufacturing, were forced to let workers go and some even shut down. Another article reported that the cash shortage had disrupted the supply chain – trucks were left stranded with no money for fuel and goods were not loaded because workers were not being paid (Choudhury and Singh, 2016).

Supply chains even at medium and larger companies broke down, providing evidence of how much the organized corporate sector relied on cash to

[8]In 2012, 87% of transactions in India were cash based and typically even households with access to formal banking carry a lot of cash, especially in high denomination bills, with them (Mazzotta et al., 2014). Cash to GDP ratio was at 12.04% in 2013. To get a sense of this magnitude, this ratio for comparable countries was 3.93% for Brazil, 5.32% for Mexico, and 3.72% for South Africa.

[9]In Mazzotta et al. (2014), the section on 'Reasons and Attitudes to using cash in India' states that "... more than half (55%) of those who use cash alone are either women engaged in unpaid household work and casual laborers who do not have any regular source of income".

[10]Due to the withdrawal limits placed on households, some laborers who had bank accounts were unwilling to start accepting direct payment out of fear that they may lose their below poverty line status (Pattanayak, 2016).

conduct day-to-day operations. Business owners reported that the legally imposed withdrawal limit of ₹50,000 per week was not sufficient to cover expenses, and that payment of wages to workers and transportation costs were the major problem. Pattanayak (2016) reports that some industry executives demanded a tenfold hike in the cash withdrawal limit of ₹50,000 per week in order to be able to conduct certain necessary business transactions. Formal sector firms that relied on informal, cash-based channels were affected the most, specifically firms in labor-intensive industries such as construction and building materials sectors, where contractors sill pay workers in cash. Dey (2016) reports how an employer at a footwear manufacturing unit asked nearly 150 workers to go on unpaid leave for a month, citing his inability to pay their wages at the time. Likewise contractors and landlords were unable to cover wage expenses and were forced to let workers go, at least until they could lay their hands on the new notes that were meant to remonetize the economy. In the near-term they were severely cash constrained.

The RBI provided some relief by announcing, a month and a half later on December 29th, that banks may provide an 'additional working capital limit' to micro, small, and medium enterprises (MSME) borrowers in order to overcome any cash flow difficulties. This was an ad hoc one-time measure up to March 31st, 2017, after which working capital limits would revert to normalcy.

A study conducted by All India Manufacturers Organisation (AIMO) assessing the impact of first fifty days of demonetization found considerable declines in jobs in the manufacturing sector (Janardhanan, 2017). AIMO found that medium scale industries with a staff strength of 300 to 700 suffered 3% job losses and 7% loss in revenue. Large-scale industries, with 2,000 to 3,000 employees, experienced 2% job losses and 3% loss in revenue. Indeed, the worst-hit sectors were those dominated by unorganized labor. The Center for Monitoring Indian Economy (CMIE), a private organization that collects and analyzes business and economic data, reported that about one and a half million jobs were lost from January, 2017 through April, 2017 (Vyas, 2017). This includes organized and unorganized sectors, and agricultural and non-agricultural sectors. Despite November being the festive season, labor force participation rate (LFPR) fell to a new low of 44.8% (from 46.4% in the previous month), recovering slightly to 45.2% in the following two months. At the same time, the unemployment rate fell from 6.8% (September-December, 2016) to 4.7% (January-April, 2017). This is most likely due to the increase in working age population (persons greater than 14 years of age) while the number of employed in fact shrank. The recovery in LFPR was only moderate as evidenced from the drop in its average: from 46.9% (January-October, 2016) to 44.3% (January-April, 2017). These findings of long lasting effects are in line with the recently released annual report by the government, the Periodic Labour Force Survey (PLFS), that states that unemployment rate based on *usual status* stood at 6.1% and *current weekly statu*s at 8.9% in 2017-18[11].

1.3.3 Labor Regulations and Temporary Workers in India

A vast majority of India's labor force comprises of informal workers. Informal employment, including agricultural employment, accounts for 88.2% of total employment (ILO, 2018). Excluding agriculture, more than 12% of these informal workers reside in the formal sector. The ILO (2016) finds that the share of informal workers in the organized sector has increased significantly because of a greater use of contract and other forms of casual labor[12]. Hsieh and Klenow

[11]Estimates based on *usual status* consider an individual's principal status as well as and subsidiary status in employment. While estimates based on *currency weekly status* provide a picture of unemployment in a short period of seven days during the survey period.

[12]In 2011-12, 79% of non-agricultural wage workers had no written contract and only about 24% were eligible for social security benefits.

(2014) point out that while nearly 70% of manufacturing output is in the formal sector, a majority of manufacturing employment, nearly 80%, is in the informal sector.

Labor regulation, specifically the Industrial Disputes Act (IDA) of 1947, and its amendments, has been named as one of the primary causes for making firms reliant on contract, temporary, or casual workers (Bertrand et al., 2015; Chaurey, 2015)[13]. The IDA lays out rules and regulations that also govern layoffs, retrenchments, strikes, and lockouts, and resolves labor-related disputes by setting up special bodies to arbitrate them, thereby raising the cost of hiring and firing workers, particularly for large firms. For instance, Section V-A of the IDA states that retrenched workers are entitled to compensation equaling 15 days' average wages for each year of service. A laid off worker is eligible for 50% of wages in addition to a dearness allowance per day (for a maximum of 45 days). The more severe Section V-B of the IDA calls for firms to obtain government permission to lay-off or retrench a single worker[14]. Taken together, these laws make it immensely difficult and costly for firms to hire permanent workers.

However, the IDA does not cover workers hired through contractors, temporary hires without formal contracts, or casual labor[15]. Hence this allows firms to circumvent the law allowing them to expand their workforce by employing these types of non-permanent workers. Firms also hire temporary workers who work full-time because they can afford to pay them a lower wage and can be hired and fired at will[16]. Figure 1.B.2 plots the share of temporary

[13]The ILO (2018) also finds that in countries characterized by pervasive labor regulation, formal sector firms rely heavily on informal employment.

[14]Regulations in Section V-A apply for establishments with 50 or more workers and regulations in Section V-B apply for establishments with 100 or more worker (Malik, 1997). These types of firms would potentially be included in the sample I study in this paper.

[15]I use the terms – temporary workers, workers without a formal contract, and casual workers – interchangeably in this paper.

[16]Table 1.B.2 presents the number and share of permanent and full-time workers involved

workers as a share of total workforce for firms, surveyed in the World Bank Enterprise Survey, 2014, that report employing any temporary workers. While large firms typically hire fewer casual worker, the use of casual labor is widely prevalent across the firm-size distribution.

The IDA is legislated by the central government and then amended by the state governments as India follows a federal system of government. These amendments have resulted in some states establishing "pro-worker" or "pro-employer" labor regimes. Many studies have exploited this heterogeneity across states in order to identify the effect of labor market regulation on formal and informal manufacturing (Besley and Burgess, 2004), employment responses to shocks (Adhvaryu et al., 2013), and on contract labor use (Chaurey, 2015).

Apart from geographical variation, there also is considerable heterogeneity across industries in their use of casual labor. Figure 1.B.3 plots the share of casual labor employed in the total workforce against the share of labor in value-added for industries as classified in the KLEMS India 2015-16 database. While the two are not tightly linked, there is in general a positive relationship between the degree of labor-intensiveness in production and the share of casual labor used. Possibly labor-intensive industries (such as textiles or manufacturing of wooden products) face the brunt of "pro-worker" labor regulation more, as compared to relatively less-labor intensive industries (such as manufacturing of rubber and plastic products, or manufacturing of transportation equipment) and hence must resort to hiring more casual laborers in order to realize the economies of scale while minimizing firing costs.

I will exploit various measures of cross-sectional heterogeneity across industries as proxies for industry-exposure to the informal sector and informal

in production for all firms. On average, temporary workers are paid lower wages and work fewer hours. Their share in production stands at about 15-17%.

employment (for instance, industry use of casual labor) in order to identify the effects of demonetization on firms. What is key is that these measures are from before the demonetization episode, hence do not vary with time, but vary only across industries.

1.4 Theory: The Effect of Demonetization on Firms

The goal of this section is to present a simple model of how firms may behave after a demonetization shock that will help motivate the empirical tests that I present later in section 3.5. To this end I sketch a static model of a production environment characterized by heterogeneous firms that face a need for cash in order to finance some factors of production.

The model consists of firms that use three inputs – capital, labor, and materials, denoted by K, L, and M, respectively. There are two types of labor and materials: formal-type labor (L_F) and formal-type materials (M_F), and casual-type labor (L_C) and casual-type materials (M_C) – in order to produce an industry-specific output (Y). Assume that firms need to pay a sunk cost Ψ before production begins, for instance, installation of machines and setting up factories. Once this sunk cost is incurred, the production function for each firm i in industry s is given by a constant-returns-to-scale technology as follows:

$$Y_{si} = K_{si}^{\alpha_s}(L_{F,si}^{\gamma_s} \cdot L_{C,si}^{1-\gamma_s})^{\beta_s}(M_{F,si}^{\delta_s} \cdot M_{C,si}^{1-\delta_s})^{1-\alpha_s-\beta_s} \tag{1.1}$$

The model is set in partial equilibrium and all factors are supplied inelastically at their given prices. Note that the shares of these factors are allowed to be different across industries. Assume that inputs of the casual-type may only be paid in cash. I do not explicitly model firms' need for cash. Possibly one could imagine that these workers do not have bank accounts or contractors who hire these workers only accept cash as it eliminates some type of friction. Further assume that firms may enjoy some flexibility in hiring and firing casual-type labor, possibly because they work without a formal contract, and hence firms find a need for this specific type of input.

Timing. The model is completely static but within each period firms act according to the following timeline:

(i) Firms withdraw (or set aside) some cash in order to pay for the casual-type inputs.

(ii) Firms decide how much capital to employ, labor to hire, and materials to procure.

- In normal times, when currency is in adequate supply, firms raise as much cash as their first-best choice of L_C and M_C dictates and the cash constraint will not matter for the optimal solution. In other words, firms' choices mimics the friction-less benchmark.
- After a demonetization shock, firms are now "cash-constrained" in the sense that it is now difficult for them to obtain cash, although they may have funds in less liquid forms[17]. Firms are now forced to hire L_C and procure M_C that are lower than their first-best levels.

(iii) Firms produce and sell their output.

Wage and expense payments to formal-type laborers and formal-type materials suppliers, respectively, can be made using relatively more "sophisticated" payment technology, such as direct deposit or checks[18]. Since casual-type laborers only accept cash[19], firms face the following cash-in-advance constraint

[17]My definition of being cash constrained here differs from the usual definition in that it refers to the inability to access liquidity rather than a firm having cash at the bank. In this regard, my definition is similar to that of Karmakar and Narayanan (2019) who define liquidity constrained households as those with access to bank accounts during demonetization.

[18]Credit may be used in which case, in this static framework, the interest rate on short-term credit would be subsumed under the price of each factor.

[19]This is similar to Banerjee and Duflo (2014) who, in their model of credit constraints, dis-

for casual-type inputs:

$$w_{C,si}L_{C,si} + p_{C,si}M_{C,si} \leqslant \mathcal{C} \tag{1.2}$$

Here, $w_{C,si}$ stands for the wage rate paid to casual labor and $p_{C,si}$ stands for the price of casual-type materials expenses. $\mathcal{C}$ stands for the amount of cash firms set aside to fulfill their casual wage-bill. Demonetization in this framework would result in a decline in the supply or value of cash available to firms $\mathcal{C}^d < \mathcal{C}$.

I assume that firms minimize costs subject to (1.1) and (1.2). This cost-minimization problem simplifies to:

$$\begin{aligned}\min \mathcal{L} = w_{F,si}L_{F,si} + w_{C,si}L_{C,si} + p_{F,si}M_{F,si} + p_{C,si}M_{C,si} + rK_{si} \\ + \lambda[Y_{si} - K_{si}^{\alpha_s}(L_{F,si}^{\gamma_s} \cdot L_{C,si}^{1-\gamma_s})^{\beta_s}(M_{F,si}^{\delta_s} \cdot M_{C,si}^{1-\delta_s})^{1-\alpha_s-\beta_s}] \\ + \varphi(w_{C,si}L_{C,si} + p_{C,si}M_{C,si} - \mathcal{C})\end{aligned} \tag{1.3}$$

This leads to a simple solution that says that the ratio of wages of the two types of labor must be proportional to the ratio of its respective marginal products. That is, for a constrained firm (when $\varphi \neq 0$):

$$\frac{w_{F,si}}{(1+\varphi)w_{C,si}} = \frac{\gamma_s}{1-\gamma_s} \cdot \frac{L_{C,si}}{L_{F,si}} \equiv \frac{MPL_{F,si}}{MPL_{C,si}}$$

Similarly for materials, the optimal solution for a constrained firm is:

$$\frac{p_{F,si}}{(1+\varphi)p_{C,si}} = \frac{\delta_s}{1-\delta_s} \cdot \frac{M_{C,si}}{M_{F,si}} \equiv \frac{MPM_{F,si}}{MPM_{C,si}}$$

The only difference between the unconstrained and the constrained solution is that in the latter, the presence of the Lagrange multiplier on the cash

tinguish between inputs that are paid using working capital, that comes from bank credit and market borrowing, and inputs that can be financed using trade credit.

constraint, φ, distorts the first order condition for casual-type labor casual-type materials, and introduces a wedge between the optimal choices of the formal-type input vis-à-vis the informal-type input. This multiplier is the shadow value of cash to the firm and comes into play only when the cash constraint binds (periods in which $\mathcal{C}^d < \mathcal{C}$).

This framework provides me with a way of thinking about how firms may react after a demonetization shock. I summarize the intuition provided by the model below. I then test these results using firm-level data with cross-sectional variation across industries, and worker-level data for casual-type workers.

Result 1. When a firm is constrained on the supply of cash following demonetization, relative to its unconstrained first-best choice of casual-type labor and materials, the constrained firm now employs fewer casual-type workers and purchases fewer casual-type material inputs. Following demonetization, firms in industries that employed more casual-type inputs (characterized by relatively low values for γ_s and δ_s) employed fewer of these inputs.

Assume the wage rate for casual-type labor is lower than that of formal-type labor, so that the MPL-per-rupee for casual-type worker is greater than the MPL-per-rupee for formal-type worker[20]. Assume also that formal-type labor is a fixed factor at some constant optimal level, $L_{F,si} = \overline{L_{F,si}}$, given that firms face significant costs in hiring and firing formal workers. In this case, the shadow value of cash is positive and following a demonetization shock, cash-constrained firms hire fewer casual-type workers.

$$\text{If} \quad \frac{MPL_{C,si}}{w_{C,si}} > \frac{MPL_{F,si}}{w_{F,si}}, \quad \text{then} \quad \Rightarrow \quad \varphi > 0$$

[20]Indeed, this result requires that nominal wages are rigid downwards, an assumption that is consistent with the evidence found in Kaur (forthcoming) for wages in India due to which equilibrium employment can be less than inelastically supplied labor. For a dynamic model of demonetization with downward nominal wage rigidity, see (Chodorow-Reich et al., 2018).

Similarly, for materials, assume the price[21] for casual-type materials is lower than the price for formal-type materials, so that the MPM-per-rupee for casual-type materials is greater than the MPM-per-rupee for formal-type materials. Once again, the multiplier φ is positive, and cash-constrained firms purchase fewer materials for production.

So far I have assumed that the upper bound on the cash constraint is fixed and homogeneous across industry, at some arbitrary level $\mathcal{C}$. However, this need not be the case. If firms were indeed financing part of their wage-bill and materials expenses using cash, their cash holdings prior demonetization may provide information of their use of informal-type inputs

Result 2. Under the assumption that cash holdings may vary across industries depending on the intensity of use of informal-type inputs, firms in industries characterized by greater cash usage (cash relative to cost of inputs, for instance) experienced more severe demonetization.

$$\frac{dL_{C,si}}{d\varphi_s} < 0 \quad \text{and} \quad \frac{dM_{C,si}}{d\varphi_s} < 0$$

This result states that firms in industries that experienced more severe demonetization will have sharper declines in casual-type inputs.

Given the static nature of the model, I do not explicitly allow for credit to be an option in the case that a firm is cash constrained. However, in reality short-term credit and sundry credit lines may help a firm smooth a liquidity shock such as demonetization, by providing an alternate source of liquidity that is not cash. However, it may be difficult to open such credit lines in the short term that were not open before demonetization[22].

[21] As noted by Chodorow-Reich et al. (2018), there was no discernible change in trend in consumer price inflation and rural wage inflation (the only high frequency wage series available), with both remaining positive, which is consistent with wage and therefore price rigidity.

[22] Typically, short-term working capital provided by banks are essentially credit lines with

This simple model, under the plausible assumptions mentioned above, provides me with testable implications that I verify using firm-level and worker-level data.

1.5 Data and Summary Statistics

1.5.1 Data

I combine data from a few different sources: quarterly data on firm financial statements(income and expenses, and balance sheet items), an employment survey, a census of manufacturing establishments, a survey of informal enterprises, and household panel with information on employment status, occupation, and demographics. All these sources including variables used from each database are summarized in Table 1.C.1.

Firm-Level Panel Data

I use firm-level data from the *Prowess* database maintained by the Centre for Monitoring Indian Economy (CMIE) from income-expenditure statements and balance sheets[23]. This database covers publicly listed firms in the organized sector that consists of registered companies that submit quarterly financial statements. Although this database may not render a representative sample of Indian firms, it has three main advantages. First, *Prowess* contains detailed information on items in the financial statements at a quarterly frequency, as publicly-listed firms are mandated to report their quarterly financial statements. The availability of data at a high frequency make them well-suited for

a pre-specified limit and an interest rate that is slightly higher than the prime rate (Banerjee and Duflo, 2014). In addition to this, due to mandated lending laws such as "priority sector lending", banks require to lend at least 40% of their net credit to the "priority sector" which includes agriculture, agricultural processing, transport industry, and small scale industries.

[23] The Prowess database has been used in many other studies, such as Asker et al. (2014), Bertrand et al. (2002), and Alfaro and Chari (2014), to name a few. Companies in *Prowess* together account for more than 70% of industrial output, 75% of corporate taxes, and more than 95% of excise duty collected by the Government of India (Shah et al., 2008).

a study that aims to identify the immediate or near-term effects of the demonetization episode. Second, the availability of data on various items from a firm's financial statement allows for an detailed analysis of the short-run effects of demonetization on firms. Lastly, disclosure requirements for listed firms imply that these data are reliable and comprehensive. The main disadvantage of using this data is that the sample of firms is skewed towards medium and large firms[24]. While the ideal data for a study of firms may be the Annual Survey of Industries (ASI), a census of registered manufacturing plants, these data are available only at an annual frequency and hence render themselves unsuitable for any short-term analysis. *Prowess* classifies firms by 5-digit industry codes according to the National Industrial Classification (NIC-2008) code, the Indian equivalent to the Standard Industrial Classification used in the US and UK. Using this I merge firm-level data with variables from industry-level surveys I describe below.

Industry-Level Data: Measures for Use of Informal Labor

I construct industry-level measures of cash usage, reliance on informal employment, and a indirect measure of firm-level exposure to the informal sector, using data from an employment survey, a census of manufacturing plants, and a survey of informal manufacturing enterprises. These data are all prior to the demonetization episode.

Employment Survey Data. In order to obtain data on workers, the types of jobs they work at, and the way in which they get paid, I use the 2011-12 Employment and Unemployment Survey (EUS), conducted by the National Sample Survey Organization (NSSO) in India every five years. This survey collects information on individual characteristics, the nature of job, conditions of the

[24] I test for the effects of firm size in all my specifications using fixed effects based on size deciles provided in *Prowess* and size fixed effects based on plant and machinery. I discuss other shortcomings of this dataset in detail in Appendix 1.A.

workplace, and social security benefits. The survey also contains the sector of employment for each working individual according to the NIC-2008 classification. The 2011-12 EUS included 101,724 households that consisted of 456,999 individuals. The data are representative at the level of regions as defined by the NSSO. I construct two measures of informal employment using the nature of employment (formal or casual) by looking at the worker's principal activity status, and the nature of payment of wages to these workers (cash versus non-cash).

Manufacturing Survey Data. In order to observe the use of cash by firms across different industries, I use the Annual Survey of Industries (ASI) 2014-15, which is a cross-sectional survey and census of manufacturing establishments that is conducted by the Central Statistical Organization of India (CSO). These data also contain information on the sector in which a factory belongs using the NIC-2008 code. The combined ASI census and survey are representative of all factories in India and are repeatedly used to estimate the performance of the industrial sector, both regionally, as well as nationally. I construct two measures of cash usage: one, the industry share of cash in current assets, and two, the industry share of cash in total spending on labor and materials. The first measure captures the extent of cash holdings as a fraction of liquid assets of the firm, and the second measure captures cash used as a fraction of flow of expenses.

Informal Sector Manufacturing Data. I compute net value added by informal enterprises at the 5-digit industy level, for which I use the Survey of Unincorporated Enterprises (Excluding Agriculture) from the 67$^{\text{th}}$ round of NSSO's enterprise survey. I match products used by firms to their respective industries, as explained in Section 3.5, in order to distinguish between informally-sourced and formally-sourced raw materials.

Employment Status

Household Panel Data. In order to verify my firm-level findings, I use worker-level survey data from Consumer Pyramids (CP), also maintained by CMIE, for the period starting from May, 2016 through April, 2017. In recent times, the CP has been widely used in the study of employment conditions in India, a few papers include Chodorow-Reich et al. (2018); Crouzet et al. (2019); Karmakar and Narayanan (2019), since India does not have an official monthly household survey or a survey of establishments conducted by the government until very recently in April 2018 with data going back to September 2017. Abraham and Shrivastava (2019) show that the CP unemployment survey and the NSS Employment rounds are comparable in terms of individual employment status.

The unemployment module in the CP survey closely resembles the questions asked in the Current Population Survey (CPS) in the United States with regard to employment status. An individual is counted as employed if, on the day of or the day prior to the survey, the individual: (i) did any paid work, (ii) was on paid or unpaid leave, (iii) was not working because his/her workplace was temporarily shut down for maintenance or labor dispute but expected to resume work within fifteen days, (iv) owned a business in operation, or (v) assisted in a family business. The survey covers nearly 110,000 adults (persons aged 15 and above) per month. The module also contains information on an individual's primary occupation which is defined as "the occupation which is undertaken for maximum time during the day by a member". Although the scope of its definition is quite wide to ensure that everyone is associated with an occupation, it contains some information on the nature of work undertaken by the individual. I use this variable to define workers as casual-type workers and formal-type workers. See Appendix 1.A for further information on how this variable is coded. The data have very few individuals who change their occupation during the period of study. I exclude individuals in the self-employed category.

1.5.2 Summary Statistics

Table 1.C.2 reports summary statistics for the sample covering the pre-demonetization period, that is 2015Q1 to 2016Q2. Labor's share in value added, as measures by net sales, is 10% for the average firm, while materials share is close to 60%. Only about 4% of net sales is the value of cash balances in the bank while sundry credit stands at a little above 50%. Since the Prowess sample is skewed towards medium and large firms, these firms maintain very little cash in hand. The large standard deviation possibly suggests that firms in some industries need to hold more cash in order to conduct transactions. Possibly, most firms maintain as much cash as is needed to conduct transactions and may decide to withdraw if more is needed.

1.6 Empirical Strategy

The empirical analysis in this section derives directly from the simple model of heterogeneous firms outlined in Section 1.4 where some production inputs need to be paid for using cash. The basic intuition is that following a demonetization shock, firms are unable to conduct cash-based transactions, due to the reduced aggregate supply of currency and their inability to immediately substitute with credit, and are hence unable to hire and acquire labor and materials, respectively. Firms in industries that typically hire more casual-type labor and use inputs predominantly from the informal sector are more dependent on informal employment and are more exposed to the informal sector. Following demonetization, firms in these industries were treated with greater intensity and were plausibly more cash constrained.

Exploiting cross-sectional heterogeneity of industry-exposure to informal employment and the informal sector, I uncover the causal short-run effects of demonetization. The unanticipated nature of the announcement provides more credence to this method of identification. To the extent that these industry and firm characteristics were uncorrelated with other shocks to the economy during the period surrounding the episode, I can uncover the causal impact of demonetization. Using representative data from worker-level and firm-level surveys, I construct proxies of industry dependence on informal employment and firm exposure to the informal sector. In the context of my empirical analysis, I use a difference-in-differences approach to causally identify the effect of this inability to transact on two margins – the share of labor and the share of material expenses in production. I focus on the financial years 2015 and 2016 in order to uncover the short-term effects. Specifically, the sample begins on April 1$^{\text{st}}$, 2015, and ends on March 31$^{\text{st}}$, 2017, thereby avoiding the implementation of a new sales tax collection system from April 2017, another major policy enactment.

1.6.1 Firm-level regressions

Labor. Exploiting variation in industry-dependence on casual labor and on workers without formal contracts, I investigate whether being heavily reliant on informal employment causes firms to hire fewer workers following demonetization by looking at firms' labor share in value added. To do this I regress the firm's quarterly wage bill as a share of net sales for a firm i in industry j on quarter dummies t for the periods following demonetization interacted with measures that proxy for industry-dependence on informal employment given by z_j. For outcome y_{ijt}, the baseline specification is[25]:

$$y_{ijt} = \beta_0 + \beta_1(\text{During}_t \times z_j) + \beta_2(\text{Post}_t \times z_j) + \gamma z_j + X'_{it}\Gamma + \mu_i + \lambda_t + \varepsilon_{ijt} \quad (1.4)$$

where z_j is either the fraction of workers with no formal contract or the fraction of casual workers in industry j, During_t is an indicator for the quarter during which demonetization was announced (2016Q3) and Post_t is an indicator for the quarter immediately after demonetization was announced (2016Q4), λ_t are quarter dummies, μ_i are firm fixed effects. Other controls include quarter fixed effects, firm-level controls for size, demand and profitability, and firm fixed effects. Robust standard errors are clustered at the industry level.

The dependent variable is the firm's wage bill as a share of net sales. The use of this dependent variable warrants some explanation. First, for labor input I use the firm's wage bill because firms in Prowess do not report employment in their financial statements. The wage bill is arguably a better measure for labor as it contains information on hours worked and human capital. Furthermore, with respect to my analysis on informal employment, firms may underreport the number of employees to evade labor regulation but this is less likely to be the case with the wage bill. I take care to appropriately deflate the wage bill and net sales measures using 2-digit industry deflators. Second, revenue-based measures for labor share are used in other studies such as Hsieh and Klenow (2009) that study misallocation by looking at wedges in the first order conditions of firms. Third, Asker et al. (2014) uses net sales as a proxy for value added using the Prowess dataset.

[25]The identification procedure followed here is similar to Rajan and Zingales (1998), who study the effect of financial development on growth by looking at whether industrial sectors that were relatively more in need of external finance developed faster in countries with more-developed financial markets.

The coefficients of interest, β_1 and β_2, uncover the short-term effects of exposure to the informal sector on the firm's labor share due to the demonetization shock. The specification also helps control for other confounding shocks or policies that may have impacted demand and supply similar to the argument provided by Chodorow-Reich et al. (2018). This method relates well with other empirical studies in the literature that use similar *pre* and *post* treatment periods and implement a difference-in-differences (DID) estimation strategy to uncover the impact of a policy across treatment groups. I also follow the DID estimation literature and cluster standard errors at the level of treatment as suggested by Bertrand et al. (2004), which in this case would be at the industry level j. I also estimate heteroskedasticity-robust standard errors and find similar results.

Materials. Most firms use a combination of intermediate inputs in production. Each of these inputs are sourced from various different industries, some are sourced from the informal sector and some from the formal sector. Firms in Prowess provide information on their material inputs expenditure annually. I exploit this information and compute firms' exposure to the informal/formal sector sector for a given mix of materials used in production. In order to do this, I proceed in three steps:

1. First, I compute the share of each material input l in a firm's material expenditure bill. Prowess contains information on specific products used by firms and the expense incurred thereof. These data are from firms' annual financial statements from 2015. Later, for robustness, I calculate averages of these shares for five years prior. Material expense incurred by a firm i in industry j using an input l that is produced by industry j' is denoted by $m^l_{ij:j'}$. For any firm i, the share of this intermediate input in

total expenditure on intermediate inputs is given by:

$$\forall l \quad \frac{m^{l}_{ij:j'}}{\sum_{l=1}^{L} m^{l}_{ij:j'}}$$

where $l = 1, 2, ..., L$.

2. Next, for each input I map product codes (for material inputs) to their relevant 5-digit NIC industry codes. For each of the J industries, I compute the share of value added by the informal sector in that industry over total value added (sum of formal and informal sectors).

$$\frac{y_{C,j'}}{y_{C,j'} + y_{F,j'}}$$

where $y_{F,j'}$ ($y_{C,j'}$) denotes total value added by the formal (informal) sector and $j' = 1, 2, ..., J$.

3. Lastly, I multiply the firm i's material input share (calculated in point 1) by the respective industry's informal sector share in production (in point 2) for each product used by the firm, and sum across all L materials used. This measure gives me a measure of the firm's *total exposure to the informal sector* for a given mix of materials used in production. For firm i in industry j, I define:

$$\text{Exposure}_{ij} = \sum_{l=1}^{L} \left(\frac{m^{l}_{ij:j'}}{\sum_{l=1}^{L} m^{l}_{ij:j'}} \cdot \frac{y_{C,j'}}{y_{C,j'} + y_{F,j'}} \right) \tag{1.5}$$

where $j, j' = 1, 2, ..., J$.

Using this as a source of variation at the firm-level, I test whether differences in greater exposure to the informal sector for intermediate inputs caused firms to procure fewer inputs in the periods after demonetization. Similar to the specification in equation (2.3), I regress the firm's quarterly total materials expenditure as a share of value added for a firm i in industry j on quarter dummies t for the periods following demonetization interacted with Exposure_{ij} as defined in point 3. The dependent variable is the materials expenditure share in value added (using net sales as a proxy for value added). For outcome y_{ijt}, the baseline specification is:

$$\begin{aligned} y_{ijt} = \beta_0 + \beta_1(\text{During}_t \times \text{Exposure}_{ij}) + \beta_2(\text{Post}_t \times \text{Exposure}_{ij}) + \gamma \text{Exposure}_{ij} \\ + X'_{it}\Gamma + \mu_i + \lambda_t + \varepsilon_{ijt} \end{aligned} \tag{1.6}$$

where all variables are as defined in the specification in equation (2.3) and Exposure_{ij} is as defined in (1.5). Other controls include quarter fixed effects, firm-level controls for size, demand and profitability, and firm fixed effects. Robust standard errors are clustered at the industry level.

1.6.2 Worker-level regressions

In order to investigate whether casual-type workers were relatively more likely to become unemployed following demonetization, I estimate the following specification:

$$\begin{aligned} u_{idt} = \beta_0 + \beta_1(\text{Post}_t \times \text{Casual Worker}_i) + \gamma(\text{Casual Worker}_i) \\ + X'_{it}\Gamma + \theta_d + \lambda_t + \varepsilon_{idt} \end{aligned} \tag{1.7}$$

where u_{idt} takes on a value 1 if individual i in district d is unemployed on date t of the survey. The indicator variable CasualWorker_i equals 1 for individuals who are either casual or temporary workers, and zero otherwise[26]. Post_t equals 1 for the months following demonetization. The sample runs from May, 2016 through April, 2017 – six months before and after demonetization. Individual-level controls X'_{it} include age, age-squared, education, literacy and caste. θ_d stands for district fixed effects and λ_t stands for month fixed effects. The coefficient of interest is β_1. Owing to the specification and the structure of the survey I cannot distinguish between contemporaneous and lagged effects.

1.7 Results

In this section, I first present the results for firm-level regressions for labor and materials as specified by equations (2.3) and (1.6), respectively. Then I present results from the household panel for worker-level regressions as specified by equation (1.7). I present the firm-level regressions with and without firm fixed effects, with firm-level controls measured contemporaneously with the outcome, as well as one year lagged controls also with and without firm fixed effects in order to account for variation that is unobserved at the firm-level. Contemporaneous controls are unfortunately also contaminated by the shock and to this end I use the previous year's variables as controls. I also control for quarter fixed effects in all specifications.

1.7.1 Labor and Informal Employment

Use of Cash: I begin by testing the premise that firms that used more cash were disproportionately affected by demonetization in their ability to pay for labor. The mechanism at play is the following: after the demonetization shock firms that typically make more cash payments were left cash-constrained and were

[26]See Appendix for 1.A for how this variable is coded.

hence unable to pay wages to informal workers who would only accept payment in the form of cash. To test whether this was the case I examine whether firms in industries with a greater share of cash in current assets and total costs[27], prior demonetization, witnessed relatively larger declines in their labor share in net sales in the periods immediately after demonetization. The results of these regressions are reported in Tables 1.C.3 and 1.C.4. We see a decline in the labor share of firms in industries with greater cash usage in the quarter of and after the shock that is consistent across all specification. Columns 1 and 2 in Table Tables 1.C.3 capture the main effects of the shock on firms' labor share, and I control for firm fixed effects in the latter. When I control for firm size, demand, and profitability using contemporaneous controls, in columns 3 and 5, the size of the coefficients of interest decline sharply. Using lagged controls in columns 4 and 6 increases the magnitude of the effect and this is statistically significant in both quarters. In Table 1.C.4, the magnitude of the coefficients of interest remain fairly stable. A one standard deviation increase in share of cash in current assets is associated with a 3% decline in the mean of labor's share in net sales. This translates to a 0.3 percentage point decline in the labor share.

Dependence on Informal Employment: In order to verify the mechanism, I examine whether firms that are more reliant on informal employment were disproportionately affected by demonetization. I find large declines in firms' labor share in the quarters after the shock that were relatively more dependent on informal employment, suggesting that these firms were possibly cash constrained, hence unable to pay for temporary labor in cash, and were thereby forced to let go of their temporary workforce. Using firm-level surveys conducted prior to demonetization, I construct proxies for informal employment such as the share of casual workers in total workforce in an industry and the

[27]Total costs here is defined as the cost of inputs involved in production, namely wage payments and raw materials expenses.

fraction of casual-type wage payments made to workers in an industry. The construction of these variables are relegated to the appendix. Table 1.C.5 reports results when informal employment is measured as the fraction of casual workers in total workforce in an industry j. The magnitude of the coefficient of interest is larger for the quarter after relative to the quarter during demonetization. This is unsurprising as the policy was enacted in November, 2016, which falls right in the middle of 2016Q3, hence only about half of Q3 was "treated". Table 1.C.6 reports results when informal employment is proxied by the fraction of casual-type payments made to workers in an industry j. Here the coefficient of interest is significant at the 10% level only in 2016Q4, while the coefficient for 2016Q3 is only significant for the specifications that include contemporaneous controls, it is consistently negative and smaller in magnitude. In line with previous results for cash usage, a one standard deviation increase in exposure to informal employment is associated with nearly a 3% decline in the mean labor share in the quarter of the shock and a 3.5% decline in the quarter immediately after the shock.

1.7.2 Materials and the Informal Sector

Use of Cash: Next, I examine whether firms that used cash to purchase intermediate inputs were disproportionately affected in their ability to pay for these inputs following demonetization. The mechanism is similar to that for labor, in that firms may be paying for some inputs in cash, and due to the shortage of cash, were left cash-constrained and could not pay for these inputs. Similar to the procedure implemented for labor and informal employment, I examine whether firms in industries with larger shares of cash in current assets and total costs, prior demonetization, faced relative reductions in their materials expenses after the shock. The results of these regressions are reported in Tables 1.C.7 and 1.C.8. The main effects presented in columns 1 and 2 in Table 1.C.7 are negative but not statistically significant, while in columns 3-6 the coefficient on the interaction term for 2016Q3 is stable and significant at the 10% level. Similarly in Table 1.C.8, the coefficient is negative and significant at the 10% level, and is stable across columns 1 through 6. A one standard deviation increase in share of cash in current assets is associated with a 2.6% decline in the mean of materials share in net sales which translates to a 1.6 percentage point decline in the materials share in net sales. The stability of the coefficient's magnitude to the inclusion of fixed effects and lagged controls suggests that while firms did face some reduction in their materials expenses, these were not as large as that of labor. This may potentially be a result of the fact that firms purchase a variety of intermediate inputs from suppliers not all of which are paid for using cash. This suggests an approach that takes into account the different types of products that a firm purchases and the degree of informality of the industry that supplies the respective products. I present the results for this exercise below.

Exposure to the Informal Sector: Exploiting the availability of information on intermediate inputs purchased by firms, I construct a measure of indirect exposure to the informal sector (unregistered enterprises) at the level of the firm by merging product codes of inputs to their respective industries, both, in the formal and informal sector. This *Exposure* measure takes into account firms' input mix and the extent of value added by informal enterprises vis-à-vis total (formal and informal enterprises) value added as outlined in Section 1.6.1 in equation (1.5). Using this I test whether firms that were more dependent on the informal sector for intermediate inputs were hurt more by demonetization. More informally-produced inputs are likely to be paid for in cash and we would expect firms that purchase more of these inputs to witness relatively larger declines in their materials share in value added, as they may have been unable to pay for them. The results of these regressions are reported in Table 1.C.9. In columns 1 and 2, the coefficient for the interaction term for 2016Q3 is statistically significant at the 10% level and is stable to the inclusion of firm fixed effects. In specifications that include contemporaneous controls that are potentially also treated, columns 3 and 5, the coefficient of interest is negative but not significant. In columns 4 and 6, the specifications that include lagged controls for firm-level shocks and characteristics, the coefficient for the interaction term for 2016Q3 is statistically significant. A one standard deviation increase in *Exposure* is associated with a 0.4 percentage point decline in materials share in net sales.

1.7.3 Effects on Employment by Worker Type

I verify my results from the firm-side with worker-level regressions using the CP survey data. In order to test my findings, I look at whether casual and temporary workers were more likely to be unemployed in the months following demonetization. Table 1.C.10 presents results from the regression in Equation (1.7). The sample consists of observations running from May, 2016 to April, 2017, thus providing for six months before and after demonetization. In column 1, I control for month fixed effects, and in column 2 I control for district fixed effects. Additional controls in columns 3 and 4 include age, age-squared, education, caste, and literacy. Column 4 also controls for the individual's lagged employment status. In all specifications, I uncover a positive and statistically significant coefficient for β_1, the coefficient of interest on the interaction term. I find that in the months following demonetization, the unemployment rate for casual or temporary workers was 0.20% higher than formal or full-time workers. These results, taken together with the firm-level results, complete the story that the cash contraction had sharp negative effects on employment possibly because firms were unable to pay their workers in cash. It seems plausible that it may be virtually costless for the firm to let these workers go in the face of an adverse shock and hire temporary workers back again later.

1.8 Conclusion

This study documents the supply-side effects of a large unanticipated currency contraction. I exploit the 2016 Indian demonetization that rendered 86% of currency in circulation illegal overnight as a natural experiment to identify the real effects of a nominal liquidity shock. Due to the high use of cash in India, the presence of a large informal sector and the prevalence of informal employment, even among formal sector firms, the Indian environment provides a good experimental setting for this study. Using cross-sectional variation in usage of cash, exposure to the informal sector and reliance on informal employment I study whether firms in industries that were more exposed were disproportionately affected by the shock. These industry measures proxy for the extent of informal employment (casual/temporary workers), the use of cash, and exposure to the informal sector in the cross-section of industries. The sudden and unanticipated nature of the announcement provides credence to this identification strategy and helps unpack the causal effects of the demonetization shock by naturally producing firms that were treated with different intensities.

I find significant supply-side effects of demonetization in the period immediately after demonetization. Firms that use cash more and were more exposed to the informal sector witnessed significant declines in their labor share and materials share in value added after the unanticipated shock. In order to identify this effect, I construct various measures of cash usage, reliance on informal employment, and exposure to the informal sector using surveys of workers, industries, and informal enterprises. On the worker-side, I find that casual-type workers were more likely to report being unemployed in the months after demonetization relative to formal-type workers. Taken together these findings highlight the near term effects of demonetization. Similar to the approach followed by Chodorow-Reich et al. (2018), my identification strategy based on the cross-section of industries and firms best serves for near term analysis.

Indeed my findings were to some extent anticipated as documented in the preliminary macroeconomic assessment of demonetization by the Reserve Bank of India (RBI). RBI (2017) points out that due to the limited access of currency following the announcement, workers who get paid wages in cash experienced temporary loss of work[28]. More specifically, the report mentions that labor-intensive sectors that engage casual/migrant workers relatively more must have been disproportionately adversely affected. This essentially sums up the identification strategy that I adopt in this study: some industries were treated more by the shock relative to others as they face a greater need to transact in cash. Furthermore, the Economic Survey 2016-17, produced by the Government of India (GOI, 2017), points out that one of the three broad channels through which demonetization affects the economy was as an aggregate *supply* shock, to the extent that economic activity utilizes cash as an input (for example, agricultural labor is traditionally paid in cash; some companies may pay their employees' salary in cash). The report also admits that in India the informal and formal economies are *"inextricably entwined, so that problems in one inevitably affect the other"*, providing the example that many firms in the formal economy depend on suppliers from the informal economy (including labor).

My results also indicate that these negative effects are relatively smaller or

[28]See pp. 2-3 from the cited report.

even dissipate in the quarter after the demonetization announcement. However, given that available data exclude the informal sector, which was presumably most hurt by the shock, these findings are a conservative lower bound of the total effects. There may be eventual gains from moving towards a cashless economy, however my study focuses on the immediate real impacts felt by firms, suppliers and workers due to the liquidity crunch.

APPENDIX

1.A Data

1.A.1 Main Data Sources

Consumer Pyramids. Coding of Casual Type Worker:

- Individuals with the following occupations are classified as "casual or temporary workers": Agricultural Labourer, Small Farmer, Small Trader / Hawker / Businessman without Fixed Premises, Support Staff, and Wage Labourer.
- Individuals with the following occupations are classified as non-casual or non-temporary workers: Businessman, Industrial Workers, Legislator / Social Worker / Activists, Manager, Non-Industrial Technical Employee, Organised Farmer, White Collar Clerical Employees, and White-Collar Professional Employees and Other Employees.
- Individuals with the following occupations are not included in either classification: Home Maker, Home-based Worker, Self Employed Entrepreneur, Self employed professional, Self Employed Entrepreneur, Unoccupied, Student, NonSchooling Child, and Qualified Self Employed Professionals.

Prowess. The *Prowess* database has been used in many other studies, such as Asker et al. (2014), Bertrand et al. (2002), and Alfaro and Chari (2014), to name a few. Companies in *Prowess* together account for more than 70% of industrial output, 75% of corporate taxes, and more than 95% of excise duty collected by the Government of India (Shah et al., 2008).. While *Prowess* is not, strictly speaking, a panel, since data may be missing for certain time periods for various reasons, I construct a non-missing panel of firms by dropping firms that have missing observations for the years 2015 and 2016. Additionally, the sample of firms in Prowess is skewed towards medium to large firms. Figure 1.B.1 compares the firm size distribution in the sample of firms from Prowess that I use in this study with that from the Annual Survey of Industries. Prowess has virtually no micro enterprises. Restricting the firm size distribution to small, medium, and large firms, it is clear that the sample is skewed towards large firms. To the extent that small and medium sized firms faced a greater inability to smooth the demonetization shock, my results in this study are an underestimate of the true effects representative of the entire firm size distribution.

1.A.2 Supplementary Data Sources

The supplementary data used in this paper come from a range of sources. The main data sources used in the empirical analysis are presented in 1.5. In addition to those I use the India KLEMS Database 2015-16 maintained by the World KLEMS Initiative in order to create some of the figures. I map industries in the KLEMS database to their 2-digit counterparts using the RBI's Data Manual.

I also use wholesale price indices to deflate all nominal variables. The data on wholesale prices are obtained from the Ministry of Commerce Industry website maintained by the Department for Promotion of Industry and Internal Trade (DPIIT). I map industries in the WPI data to the two-digit industry codes from the NIC-2008 classification.

1.B Figures

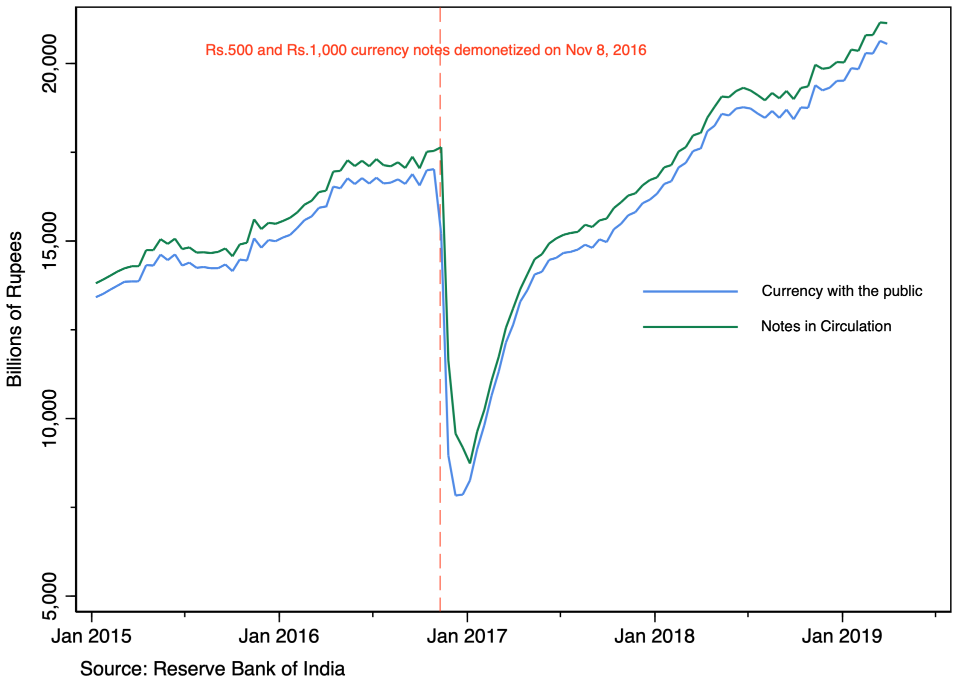


Figure 1.B.1: Measures of Currency Supply

Source: Database of Indian Economy, Reserve Bank of india. Notes: Dashed line indicates the date of announcement of demonetization.

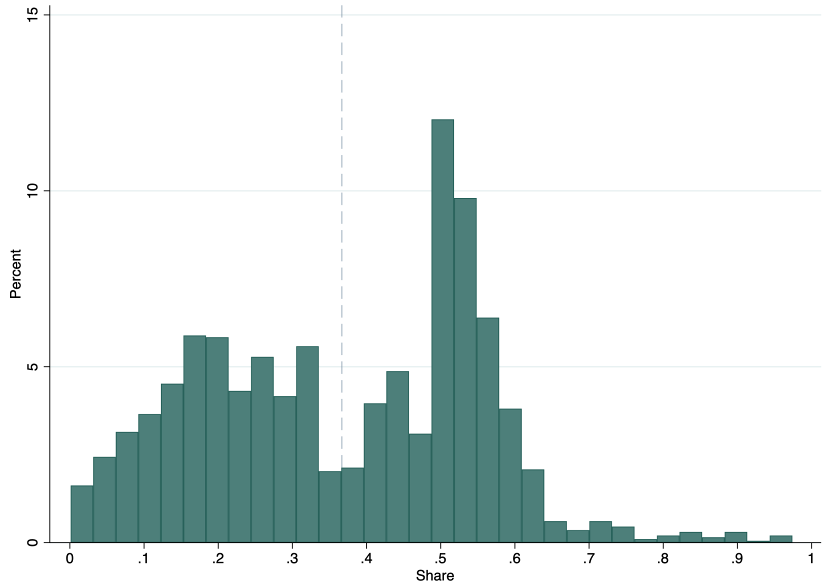


Figure 1.B.2: Full-Time Temporary Workers as a share of Total Workforce

Source: Author's calculation using World Bank Enterprise Survey 2014.
Notes: For firms that report a non-zero share of temporary workers.
Dashed line indicates mean value.

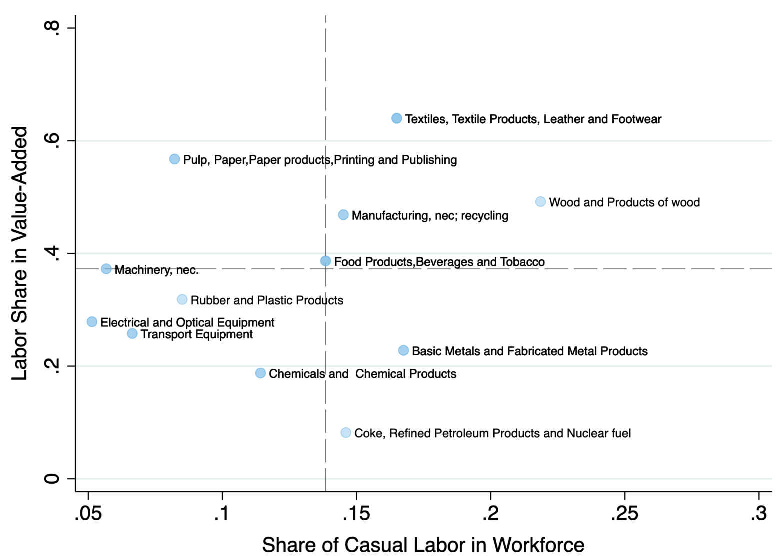
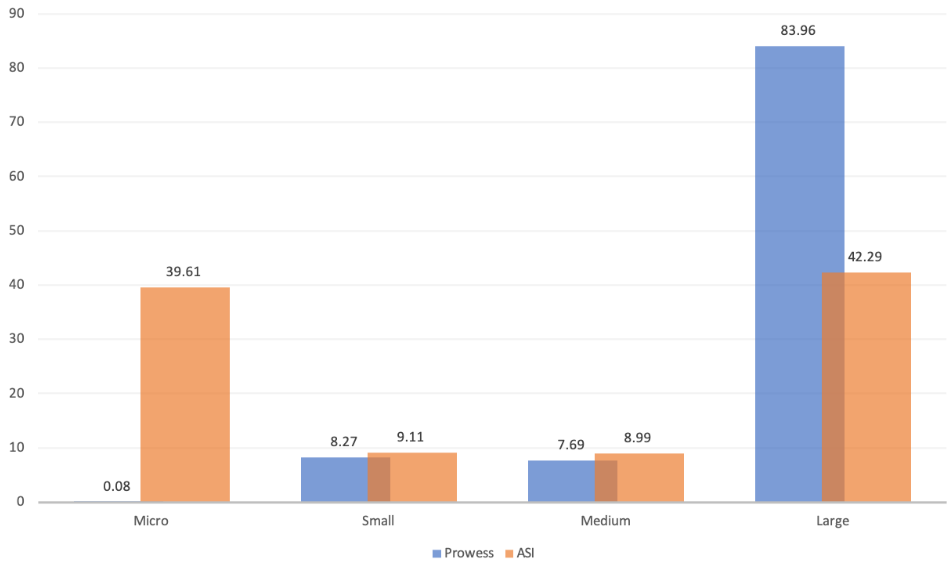
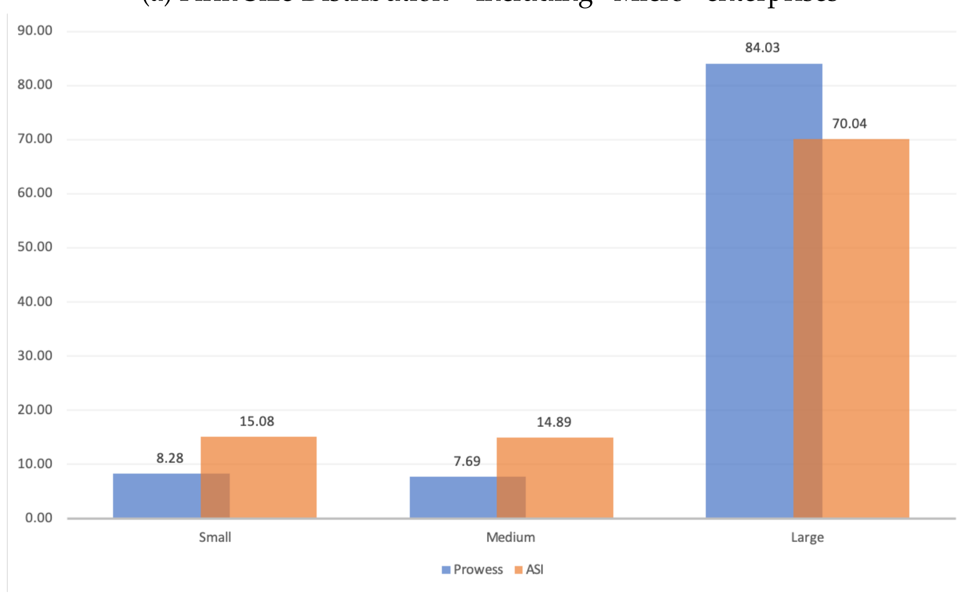


Figure 1.B.3: Labor Share in Value-Added and Casual Labor

Notes: Dashed lines indicate median values. Source: Author's calculations
using KLEMS India Database 2011-12.



(a) Firm Size Distribution – Including "Micro" enterprises



(b) Firm Size Distribution – Excluding "Micro" enterprises

Figure 1.B.1: Sample Composition

Source: Author's calculation using Prowess and Annual Survey of Industries 2014-15. Notes: This definition of firm size is based on the value of plant and machinery for manufacturing enterprises as per the Micro, Small & Medium Enterprises Development Act, 2006 (MSMED Act).

1.C Tables

Table 1.C.1: Data Sources

Variables	Source	Name
Standalone interim quarterly financial statements for 2015-17	Centre for Monitoring the Indian Economy (CMIE)	Prowess
Balance sheet items (cash holdings, current assets, cost of production) of formal sector firms within 5-digit industries	Central Statistical Organization (CSO) of India	Annual Survey of Industries (ASI), 2015-16
Share of informal employment in total workforce within 5-digit industries	National Sample Survey Organization of India	Employment and Unemployment Survey, 2011-12
Products and by-products manufactured by 5-digit industries in the informal sector	National Sample Survey Organization of India	Unincorporated Non-Agricultural Enterprises, 2010-11
Employment status of working-age members from a household panel for 2016-2017	Centre for Monitoring the Indian Economy (CMIE)	Consumer Pyramids

Table 1.C.2: Summary Statistics

Variable	Mean	Std. Dev.	Median	N
Share of Wage Bill in Value Added	0.10	0.09	0.08	2,250
Share of Materials in Value Added	0.62	0.84	0.61	2,230
Net Sales	4,887	37,138	558	2,250
Wage Bill	263	1,110	41	2,250
Operating Expenses	4,161	30,008	519	2,250
Operating Income	4,161	30,008	572	2,250
Plant and Machinery (A)	8,289	54,572	672	2,165
Sundry Creditors (A)	2,578	18,232	229	2,150
Cash Balances at Bank (A)	20	177	1	1,998
Cash in Hand (A)	3	12	1	7,950

Notes: Data correspond to the pre-demonetization period (2015Q1 to 2016Q2). Data are in millions of rupees, apart from the share variables. Variables denoted by "A" in parentheses indicate that those variable are from firms' annual financial statements. All other variables are at a quarterly frequency from the firms' quarterly financial statements.

Table 1.C.3: Labor Share and Industry Share of Cash in Current Assets

	(1)	(2)	(3)	(4)	(5)	(6)
2016Q3 × Industry *j*'s share of Cash in Current Assets	-0.0848***	-0.0848***	-0.0616**	-0.0994***	-0.0555**	-0.1004***
	(0.0291)	(0.0291)	(0.0293)	(0.0328)	(0.0266)	(0.0324)
2016Q4 × Industry *j*'s share of Cash in Current Assets	-0.0897**	-0.0897**	-0.0504	-0.1007**	-0.0689	-0.1035**
	(0.0449)	(0.0449)	(0.0446)	(0.0480)	(0.0431)	(0.0482)
Industry *j*'s share of Cash in Current Assets	0.5417***		0.4431***	0.5162***		
	(0.1357)		(0.1260)	(0.1317)		
Observations	10,264	10,264	9,593	9,409	9,593	9,409
Clusters	248	248	247	246	247	246
Mean of Dependent Variable	0.1008	0.1008	0.1009	0.1011	0.1009	0.1011
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	No	Yes	Yes
Contemporaneous Firm Controls	No	No	Yes	No	Yes	No
Lagged Firm Controls	No	No	No	Yes	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, reported in parentheses, are clustered at the industry level. This table reports the impact of demonetization on labor's share in the quarter *during* (2016Q3) and the quarter *after* (2016Q4) the announcement of the policy corresponding to the specification in Equation (2.3). Controls for firm size (measured by fixed assets), demand, and profitability include log(capital), log(sales), and PBIT/Capital ratio, respectively. The wage-bill is deflated using the WPI and and net sales are deflated using industry-specific deflators.

Table 1.C.4: Labor Share and Industry Share of Cash in Cost of Production

	(1)	(2)	(3)	(4)	(5)	(6)
2016Q3 × Industry *j*'s share of Cash in Costs	-0.0001**	-0.0001**	-0.0001**	-0.0002***	-0.0001	-0.0002***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
2016Q4 × Industry *j*'s share of Cash in Costs	-0.0002**	-0.0002**	-0.0003***	-0.0003***	-0.0000	-0.0002***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Industry *j*'s share of Cash in Costs	-0.0014***		-0.0009***	-0.0011***		
	(0.0003)		(0.0004)	(0.0003)		
Observations	10,264	10,264	9,593	9,409	9,593	9,409
Clusters	248	248	247	246	247	246
Mean of Dependent Variable	0.1008	0.1008	0.1009	0.1011	0.1009	0.1011
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	No	Yes	Yes
Contemporaneous Firm Controls	No	No	Yes	No	Yes	No
Lagged Firm Controls	No	No	No	Yes	No	Yes

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors, reported in parentheses, are clustered at the industry level. This table reports the impact of demonetization on labor's share in the quarter *during* (2016Q3) and the quarter *after* (2016Q4) the announcement of the policy corresponding to the specification in Equation (2.3). Controls for firm size (measured by fixed assets), demand, and profitability include log(capital), log(sales), and PBIT/Capital ratio, respectively. The wage-bill is deflated using the WPI and and net sales are deflated using industry-specific deflators.

Table 1.C.5: Labor Share and Industry Share of Share of Casual Workers in Workforce

	(1)	(2)	(3)	(4)	(5)	(6)
2016Q3 × Share of Casual Workers in Industry *j*	-0.0134*	-0.0134*	-0.0111*	-0.0200***	-0.0112*	-0.0192**
	(0.0076)	(0.0076)	(0.0062)	(0.0075)	(0.0062)	(0.0075)
2016Q4 × Share of Casual Workers in Industry *j*	-0.0237*	-0.0237*	-0.0176*	-0.0235*	-0.0207**	-0.0233*
	(0.0131)	(0.0131)	(0.0100)	(0.0128)	(0.0090)	(0.0129)
Share of Casual Workers in Industry *j*	0.0096		-0.0295	-0.0012		
	(0.0389)		(0.0388)	(0.0378)		
Observations	7,752	7,752	7,261	7,156	7,261	7,156
Clusters	135	135	134	134	134	134
Mean of Dependent Variable	0.0979	0.0979	0.0984	0.0980	0.0984	0.0980
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	No	Yes	Yes
Contemporaneous Firm Controls	No	No	Yes	No	Yes	No
Lagged Firm Controls	No	No	No	Yes	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, reported in parentheses, are clustered at the industry level. This table reports the impact of demonetization on labor's share in the quarter *during* (2016Q3) and the quarter *after* (2016Q4) the announcement of the policy corresponding to the specification in Equation (2.3). Controls for firm size (measured by fixed assets), demand, and profitability include log(capital), log(sales), and PBIT/Capital ratio, respectively. The wage-bill is deflated using the WPI and and net sales are deflated using industry-specific deflators.

Table 1.C.6: Labor Share and Industry Share of Fraction of Casual-Type Payments made

	(1)	(2)	(3)	(4)	(5)	(6)
2016Q3 × Fraction of Casual-Type Payments made in Industry *j*	-0.0071	-0.0071	-0.0102*	-0.0093	-0.0117**	-0.0088
	(0.0065)	(0.0065)	(0.0053)	(0.0070)	(0.0051)	(0.0071)
2016Q4 × Fraction of Casual-Type Payments made in Industry *j*	-0.0167**	-0.0167*	-0.0138*	-0.0167*	-0.0182**	-0.0165*
	(0.0085)	(0.0085)	(0.0076)	(0.0087)	(0.0072)	(0.0089)
Fraction of Casual-Type Payments made in Industry *j*	0.0147		-0.0062	0.0097		
	(0.0292)		(0.0283)	(0.0286)		
Observations	7,848	7,848	7,331	7,203	7,331	7,203
Clusters	142	142	141	140	141	140
Mean of Dependent Variable	0.0977	0.0977	0.0979	0.0978	0.0979	0.0978
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	No	Yes	Yes
Contemporaneous Firm Controls	No	No	Yes	No	Yes	No
Lagged Firm Controls	No	No	No	Yes	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, reported in parentheses, are clustered at the industry level. This table reports the impact of demonetization on labor's share in the quarter *during* (2016Q3) and the quarter *after* (2016Q4) the announcement of the policy corresponding to the specification in Equation (2.3). Controls for firm size (measured by fixed assets), demand, and profitability include log(capital), log(sales), and PBIT/Capital ratio, respectively. The wage-bill is deflated using the WPI and and net sales are deflated using industry-specific deflators.

Table 1.C.7: Materials Share and Industry Share of Cash in Current Assets

	(1)	(2)	(3)	(4)	(5)	(6)
2016Q3 × Industry *j*'s share of Cash in Current Assets	-0.5339	-0.5339	-0.6478*	-0.5377*	-0.6281*	-0.5218*
	(0.3388)	(0.3387)	(0.3708)	(0.3008)	(0.3712)	(0.3061)
2016Q4 × Industry *j*'s share of Cash in Current Assets	-0.6930	-0.6930	-0.7208	-0.6710	-0.6975	-0.6398
	(0.5222)	(0.5221)	(0.5257)	(0.5191)	(0.5301)	(0.5126)
Industry *j*'s share of Cash in Current Assets	0.4847		0.3929	0.3366		
	(0.3543)		(0.3061)	(0.2997)		
Observations	9,936	9,936	9,298	9,138	9,298	9,138
Clusters	245	245	244	243	244	243
Mean of Dependent Variable	0.6091	0.6091	0.6096	0.6109	0.6096	0.6109
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	No	Yes	Yes
Contemporaneous Firm Controls	No	No	Yes	No	Yes	No
Lagged Firm Controls	No	No	No	Yes	No	Yes

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors, reported in parentheses, are clustered at the industry level. This table reports the impact of demonetization on labor's share in the quarter *during* (2016Q3) and the quarter *after* (2016Q4) the announcement of the policy corresponding to the specification in Equation (2.3). Controls for firm size (measured by fixed assets), demand, and profitability include log(capital), log(sales), and PBIT/Capital ratio, respectively. The wage-bill is deflated using the WPI and and net sales are deflated using industry-specific deflators.

Table 1.C.8: Materials Share and Industry Share of Cash in Cost of Production

	(1)	(2)	(3)	(4)	(5)	(6)
2016Q3 × Industry j's share of Cash in Costs	0.0006	0.0006	0.0004	0.0006*	0.0004	0.0006
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
2016Q4 × Industry j's share of Cash in Costs	-0.0010*	-0.0010*	-0.0010*	-0.0011*	-0.0009	-0.0013**
	(0.0005)	(0.0005)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Industry j's share of Cash in Costs	-0.0126***		-0.0116***	-0.0119***		
	(0.0008)		(0.0010)	(0.0009)		
Observations	9,936	9,936	9,298	9,138	9,298	9,138
Clusters	245	245	244	243	244	243
Mean of Dependent Variable	0.6091	0.6091	0.6096	0.6109	0.6096	0.6109
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	No	Yes	Yes
Contemporaneous Firm Controls	No	No	Yes	No	Yes	No
Lagged Firm Controls	No	No	No	Yes	No	Yes

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors, reported in parentheses, are clustered at the industry level. This table reports the impact of demonetization on labor's share in the quarter *during* (2016Q3) and the quarter *after* (2016Q4) the announcement of the policy corresponding to the specification in Equation (2.3). Controls for firm size (measured by fixed assets), demand, and profitability include log(capital), log(sales), and PBIT/Capital ratio, respectively. The wage-bill is deflated using the WPI and and net sales are deflated using industry-specific deflators.

Table 1.C.9: Materials Share and Exposure to Informal Sector

	(1)	(2)	(3)	(4)	(5)	(6)
2016Q3 × Exposure of Firm's Materials to Informal Sector	-0.0155*	-0.0155*	-0.0125	-0.0179*	-0.0130	-0.0175*
	(0.0086)	(0.0086)	(0.0092)	(0.0095)	(0.0097)	(0.0097)
2016Q4 × Exposure of Firm's Materials to Informal Sector	-0.0177	-0.0177	-0.0178	-0.0176	-0.0173	-0.0188
	(0.0130)	(0.0130)	(0.0130)	(0.0130)	(0.0130)	(0.0134)
Exposure of Firm's Materials to Informal Sector	-0.0459**		-0.0425**	-0.0405**		
	(0.0192)		(0.0187)	(0.0183)		
Observations	7,656	7,656	7,331	7,246	7,331	7,246
Clusters	237	237	237	236	237	236
Mean of Dependent Variable	0.6017	0.6017	0.6013	0.6037	0.6013	0.6037
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	No	Yes	No	No	Yes	Yes
Contemporaneous Firm Controls	No	No	Yes	No	Yes	No
Lagged Firm Controls	No	No	No	Yes	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors, reported in parentheses, are clustered at the industry level. This table reports the impact of demonetization on materials expenses in the quarter *during* (2016Q3) and the quarter *after* (2016Q4) the announcement of the policy corresponding to the specification in Equation (1.6). Exposure to the informal sector is proxied by the measure constructed in Equation (1.5). Controls for firm size (measured by fixed assets), demand, and profitability include log(capital), log(sales), and PBIT/Capital ratio, respectively. Materials expenses are deflated using the WPI and and net sales are deflated using industry-specific deflators.

Table 1.C.10: Employment Status and Worker Type

	(1)	(2)	(3)	(4)
Post × Casual Worker	0.0019***	0.0019***	0.0020***	0.0018***
	(0.0006)	(0.0006)	(0.0007)	(0.0007)
Casual Worker	0.0006*	0.0005	-0.0000	0.0002
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Observations	461,620	461,620	461,620	331,400
Clusters	421	421	421	419
Mean of Dependent Variable	0.0032	0.0032	0.0032	0.0026
Month FE	Yes	Yes	Yes	Yes
District FE	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes
Lagged Employment Status	No	No	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the individual's labor market status at the time of the survey, zero if currently employed and one if unemployed. *Post* is a dummy variable that equals one for all the months after demonetization was announced. Robust standard errors are clustered at the district level. This table reports the impact of demonetization on an individual's employment status by worker type in the months following the demonetization announcement corresponding to the specification in Equation (1.7). Controls include Age, Age^2, Education, Caste, and Literacy. The sample consists of observations running from May, 2016 to April, 2017 (six months before and after demonetization). Casual Worker is a dummy that equals one for workers whose occupation can be categorized as temporary or casual workers. See Appendix 1.A for how this variable is coded.

CHAPTER 2

DIRECTED LENDING AND MISALLOCATION: EVIDENCE FROM INDIA

[WITH DEEKSHA KALE]

2.1 Introduction

Access to finance remains a crucial barrier for the growth of small and young firms in many countries (León-Ledesma and Christopoulos, 2016). Governments typically use targeted lending programs or subsidized credit policies to help small and young firms become more efficient and unlock larger scales of production. By eliminiating credit barriers, these programs intend to level the playing field among firms of different sizes thus helping them expand their capital and scale up production. Two natural questions a policy maker would like to have answers for are: Do such policies channel funds efficiently to credit constrained firms? And at a macro level, can targeted credit policies lead to improvements in aggregate productivity?

In this chapter, we use a policy change that affected the flow of credit by expanding eligibility of firms to a nationwide directed credit policy. Small firms that were previously excluded were brought under the purview of the priority sector lending program. We first show that credit constrained firms used this credit to expand their physical assets. We show how the credit policy affects aggregate productivity via a decline in misallocation by reducing the dispersion of marginal revenue products across firms within an industry. Then we quantify the extent of misallocation resulting from the policy, specifically, we provide a lower bound of the magnitude.

The analysis proceeds in two steps. *First*, we construct measures of marginal revenue products of capital (MRPK) for firms from the period before eligibility to the lending policy was modified. We use these to classify firms as constrained and unconstrained by comparing their ex ante levels of MRPK vis-à-vis industry median MRPK using within-industry variation in MRPK. We then implement a tripled-differences empirical strategy to estimate the effects of the expansion of eligibility for directed credit on misallocation of capital across firms. The approach is in the spirit of (Hsieh and Klenow, 2009) in identifying gaps in the marginal products of capital of firms within narrowly defined industries. *Second*, we combine these reduced-form estimates with a newly developed theoretical results, stemming from the work by (Baqaee and Farhi, 2019), to quantify the effect of the policy on aggregate productivity. Specifically, we estimate a lower bound of the true effect of the policy on manufacturing productivity.

We find three main results. *First*, we find that the directed credit program changed eligible firms' input wedges by demonstrating that for firms with ex ante higher MRPK, the policy resulted in relatively larger increases in installed physical capital. Firms with relatively higher MRPK also saw declines in their MRPK in the period after the program was initiated. Physical capital increased by 7% and owing to the expansion capital constrained firms witnessed a decline in their MRPK by about 8%. *Second*, we find that industries with a greater fraction of firms that were eligible for directed credit and with high ex ante MRPK witnessed a statistically significant decline in dispersion of MRPK as measured by the interdecile range of *within-industry* MRPK. A one standard deviation increase in the industry-level treatment variable results in a 30% decline in the mean value of the interdecile range. However, using other measures of dispersion, like the standard deviation and interquartile range of MRPK, we do not observe a statistically significant decline. *Third*, using these reduced-form estimates as well as parameters estimated from firms' production function, we calculate the lower bound of the effect of the policy on aggregate manufacturing productivity using changes in the Solow residual to be 16%.

This chapter relates to two main strands in the literature. First, this chapter relates to the literature that aims to evaluate the role of credit on the performance and growth of small firms. Second, this chapter also relates to the more recent literature on identifying the sources of misallocation across firms, and quantifying the effects of specific policies in either reducing or increasing misallocation.

With respect to the literature on the role of credit on the performance of firms, numerous studies have looked at how access to finance, alleviating credit constraints, the different types of credit, have significant consequences for the growth of young and small firms. Beck and Demirguc-Kunt (2006) document that small and medium enterprises (SMEs) face larger barriers to growth and lack adequate access to finance, thereby contributing less to economic growth. The paper highlights that financial and institutional development can help alleviate SMEs growth constraints thereby leveling the playing field between firms of all sizes. This is especially relevant in developing economies that are characterized by a large but unproductive SME sector and a few large firms that have well-established formal sources of finance. Banerjee and Duflo (2014) identify severe credit constraints using Indian data and leveraging a directed lending policy change similar to the one we consider in this chapter.

Cross-country evidence from Galindo, Schiantarelli, and Weiss (2007) points out that financial liberalization has increased the efficiency with which funds are allocated across firms. However the authors point out there is little evidence of the effect of relaxing financial constraints on the efficiency of resource allocation for small firms. We do know that even in developed economies like the US, information constraints can severely reduce access to credit for small firms (Berger et al., 2005). Perhaps León-Ledesma and Christopoulos (2016) is the most comprehensive study that tests the role of access to finance and private versus public credit in determining misallocation. They use cross-country data and find that access-to-finance obstacles increase the dispersion of distortions in inputs across firms leading to a fall in total factor productivity. Their indirect calculations also point to capital misallocation.

The literature on misallocation has focused on measuring the degree of and identifying all possible sources of misallocation by using firm-level data and pointing to the dispersion in marginal products of inputs. Hsieh and Klenow (2009) identify large distortions that lead to dispersion of TFP across firms in narrowly-defined industries in India and China relative to the US. They take the approach of modeling wedges in the allocation of inputs that arise due to government policies that distort firms' allocation of resources. Economists have attempted to identify such policies and market distortions that generate misallocation. Alfaro and Chari (2014) suggest that deregulation of industries can affect resource misallocation and the firm-size distribution by increasing entry of small firms. David, Hopenhayn, and Venkateswaran (2016) point to imperfect information in the allocation of firms' inputs as a source of misallocation. Using a quantitative model, Jo and Senga (2019) analyzes the aggregate effects of government policies that attempt to alleviate credit constraints for small and young firms and finds that while credit subsidies resolve resource misallocation and improve aggregate productivity, increased factor prices, in equilibrium, leads to fewer firms in production, which decreases aggregate productivity. These general equilibrium effects depend on the underlying distribution of firms and their financial status.

To the best of our knowledge the misallocation literature has not empirically identified the effect of providing access to finance to small firms on misallocation and therefore on aggregate productivity. Some studies that identify a causal link between external funding and economic growth, although not through the misallocation channel, include Rajan and Zingales (1996), Demirgüç-Kunt and Maksimovic (1998) and Ayyagari, Demirgüç-Kunt, and Maksimovic (2008). Our approach identifies changes in these wedges due to the credit policy that provided access to finance after accounting for other unobserved firm and time effects that are uncorrelated with the policy. In equilibrium there may be other consequences of directed lending programs such as the costs of crowding out smaller and younger firms as pointed out by Kale (2017) who finds that smaller firms were crowded out by relatively larger firms that becamse eligible for the directed lending program in India. There could also be unintended negative real effects where firms give up growth in order to maintain access to finance. Bhue, Prabhala, and Tantri (2019) show that firms that are closer to the upper bound of the eligibility threshold grew slower than firms that were further away for the same directed lending program[1]. Other costs that are not the focus of this study include the cost subsidizing credit for firms through taxes paid for by households, of lending to less creditworthy borrowers, to name a few, that may be of consequence for aggregate welfare. We focus only on the misallocation channel which is affected by a discontinuity in the credit policy that in turn affects aggregate productivity. In terms of

[1]The underlying finding is that small business lending could prove to be a disincentive for growth, so firms try to remain small so as to maintain eligibility, as otherwise the firm loses access to credit and the bank loses a potentially creditworthy borrower. Although this could be the reality it could also be that firms closer to the eligibility threshold have low marginal products of capital to begin with, which explains their slow relatively rate of growth of investment.

methodology, this study is closest to Bau and Matray (2020) in which where the authors document the effect of providing firms access to foreign capital on misallocation and aggregate productivity. Their study exploits variation coming from staggered industry liberalization policy to provide reduced form estimates that they translate to measuring the lower bound effect of the policy reform on manufacturing productivity.

The rest of this chapter is organized as follows: In Section 2.2, we provide a brief background of the directed lending policy in India and the 2006 policy change. In Section 2.3, we sketch the framework for estimating the reduced-form and the quantitative effects of misallocation. In Section 3.5, we discuss the data that we use and the main empirical strategy, and in Section 2.5, we present the results. We then conclude in Section 2.6.

2.2 Background

Directed Lending in India. Small and young firms form a large part of the firm-size distribution in both developed[2] and developing countries. These policies typically take the form of mandating lending, loan guarantees, and interest rate caps. The premise behind these targeted programs are that: (i) small business are engines of economic growth, and (ii) market failures impede their growth, thereby justifying government intervention (Beck and Demirguc-Kunt, 2006). The primary goals of these policies are to provide access to credit and ease credit constraints for specific sectors or firms that are otherwise financially constrained.

Directed lending in India takes shape via the Priority Sector Lending (PSL) program. As part of this program, commercial banks are mandated to lend

[2]For instance, the US Small Business Administration (SBA) provides loan guarantees for small business entrepreneurs in order to strengthen access to capital.

40% of their Adjusted Net Bank Credit or Credit Equivalent Amount of Off-Balance Sheet Exposure, whichever is higher, to specific *priority sectors* such as agriculture, and micro and small enterprises, students for education, and low income groups and weaker sections of society. Over time the Reserve Bank of India (RBI) has expanded the set of sectors that are designated as priority sectors. Banks are required to meet the overall 40% target as well as other internal sub-targets, and these are communicated via Master Circulars[3] and updates to the specified guidelines issued therein. These guidelines do not specify a rate of interest for priority sector loans.

India's PSL policy is based on size as defined by investment in plant and machinery[4]. Banks can lend to micro and small enterprises belonging to any industry in order to meet the priority sector targets. Banks that do not meet the specified targets are penalized by the RBI. Banks are required to lend their shortfalls from the specified targets to Rural Development Bonds at a very low interest rate that is decided by the RBI. If they repeatedly fall short of meeting the targets, banks can be disallowed from opening new branches across the country.

Policy Change in 2006. In October 2006, the Government of India enacted the *Micro, Small and Medium Enterprises Development Act (MSMED), 2006*, to "facilitate the promotion and development" of small and young firms. The MSMED Act covered various aspects of support including employees' skill development and training, marketing assistance, strengthening backward and forward linkages, as well as credit support.

As part of credit support, the Act also created an expansion in in the pool of

[3]These Master Circulars can be found on the RBI's website: see here.

[4]While there is no universal definition for a small business, many countries use number of employees as cutoffs and these cutoffs vary across countries and sometimes across industries within a country (Ayyagari, Demirguc-Kunt, and Maksimovic, 2011).

firms that were eligible for directed credit. Previously, small firms that whose value of plant and machinery was between ₹2.5 and ₹10 million were eligible for directed credit. After the MSMED Act was enacted, this investment threshold was expanded to include firms whose investment in plant and machinery does not exceed ₹50 million[5]. Previously eligible firms continued to remain eligible for directed lending. The banking sector as a whole was also encouraged to increase their lending to SMEs by directing banks to achieve a 20% year-on-year growth in loans made to SMEs, and eventually doubling the lending amount over five years.

SME size limits have been modified prior to this instance. In 1998, the limit was changed from ₹6.5 million to ₹30 million, and again in 2000 it was reversed downwards to ₹10 million. Banerjee and Duflo (2014) study both these policy changes using loan-level data from a single large bank. They first identify severe credit constraints and show that newly eligible borrowers use this credit to expand production – as evidenced by increased rate of growth in sales and profits – for the 1998 policy change and decreased production in the 2000 policy reversal. Small firms are characterized by credit constraints that play a key role in lending outcomes and firm growth, and looking at (relative) marginal products of capital is one way to analyze their effects. In the next section we sketch a framework that helps in analyzing the effects of the directed lending policy for constrained vis-à-vis unconstrained firms.

[5] I exclude enterprises belonging to the service sector as they faced a few different set of guidelines. The analysis in this study focuses on a sample of manufacturing firms.

2.3 Misallocation: Reduced-Form to Aggregate Effects

In this section, we first sketch a simple framework that shows how policy affects misallocation for constrained vis-à-vis unconstrained firms. Then we provide the equation that used to quantify aggregate effects of these changes in misallocation.

2.3.1 Framework for Reduced-Form Estimates

In this section we present a simple model of how misallocation distorts firms' allocation of inputs due to the presence of wedges that act as taxes on the prices of inputs. This is the standard approach in the literature. Suppose the price paid by a firm i for an input x is $(1 + \tilde{\tau}_i^x)P^x$, where P^x denotes the price of input $x \in \{K, L, M\}$, where K, L, and M denote capital, labor, and materials, and $\tilde{\tau}_i^x$ is the additional wedge that the firm needs to pay over the market price ($\tau_i^x < 0$ indicates a subsidy, and $\tau_i^x > 0$ indicates a tax). The firm's profits can be written as:

$$\Pi_i = P_i F_i(K_i, L_i, M_i) - \sum_{x \in \{K,L,M\}} (1 + \tilde{\tau}_i^x) P^x x_i$$

where $F_i(\cdot)$ denotes the firm's production function, which exhibits diminishing marginal returns in each input.

Cost-minimization implies that the firm will equate marginal returns for each input with marginal cost:

$$P_i \cdot \frac{\partial F_i(K_i, L_i, M_i)}{\partial x_i} = \mu_i (1 + \tilde{\tau}_i^x) P^x$$

where P_i denotes the firm's products market price and μ_i denotes the markup. Define the combined wedge as $1 + \tau_i^x = \mu_i(1 + \tilde{\tau}_i^x)$. For any input x_i, its marginal revenue product (MRPX) is directly proportional to its combined wedge. Firms that have higher input wedges for a certain input will have higher marginal revenue products on that input.

A decline in misallocation will have several effects on inputs and MRPX. For firms with a high wedge on input x, a decline in misallocation of input x will reduce its wedge τ_i^x relative to other firms. This implies MRPX will also fall and usage of input x will increase. The increase in in one input may have spillover effects on other inputs by increasing the marginal revenue products for those inputs and hence demand for those inputs. Using more inputs will allow these firms to produce more and earn more revenue. Hence, for firms with high capital wedges (say), we expect the policy to cause firms to increase capital, labor, and sales, and decrease MRPK. In the data, we would expect these effects to be differentially stronger for constrained (high MRPK) firms relative to less constrained firms.

2.3.2 Aggregate Effects using the Solow Residual

In order to estimate the aggregate effects of a decline in misallocation, we quantify changes in the Solow residual, which measures net output growth minus net input growth, using the first order approximation from Baqaee and Farhi (2019) of industry I over time as follows:

$$\Delta Solow_{I,t} \approx \sum_{i \in I} \lambda_i \cdot \Delta \log A_i \quad + \sum_{\substack{i \in I \\ x \in \{K,L,M\}}} \lambda_i \cdot \alpha_i^x \cdot \tau_i^x \cdot \Delta \log x_i \qquad (2.1)$$

where λ_i is firm i's share of sales in manufacturing industry's net output, α_i^x is the output elasticity of i with respect to input x, τ_i^x is firm i's input wedge on input x, and $\Delta \log A_i$ is the firm-specific change in total factor productivity. This expression converts firm-level effects using reduced-form estimates into aggregate effects[6]. This expression allows me to estimate the Solow residual of industry I due to the policy by estimating the components of the right hand side of equation (2.1). In subsection 2.5.3 we explain how these numbers are estimated from firm-level data.

2.4 Data and Empirical Strategy

2.4.1 Data Sources

In this section, we first describe the data sources used in this study. In the second subsection, we explain how firms in the sample are classified as high or low MRPK firms. And in the third subsection, we describe the empirical strategy.

***Prowess* database.** The main source of data used in this study comes from firm financial statements from the Prowess database maintained by the Centre for Monitoring the Indian Economy (CMIE). The Prowess database consists of a firm-level panel[7] that comprises of income-expenditure statements and balance sheets of firms. The data covers all publicly-listed firms and many private firms as well, and it has been used widely in many other studies[8]. The data are representative of medium and large firms in Indian manufacturing. Some of the key variables we use include firms' sales, capital stock (measured by gross fixed assets), wagebill, raw materials expenses, and profits. Prowess

[6] Refer to the appendix in Bau and Matray (2020) for a derivation of this expression.

[7] We construct a panel that consists of non-missing firm-year observations for the period between 2004 and 2009.

[8] The Annual Survey of Industries, which is a plant-level census of Indian manufacturing firms, is the other commonly used dataset and is not a panel.

classifies firms using a 5-digit industry-level code according to the National Industrial Classification (NIC-2008) code, the Indian equivalent to the Standard Industrial Classification used in the US and UK. Table 2.1 provides summary statistics for all manufacturing firms in the Prowess database between 2004 and 2006.

Other data sources. The firm-level variables are in revenue terms, not in quantities, and hence need to be deflated. We use the wholesale price index[9] to deflate all variables using 2-digit industry-level deflators that are merged with the firm-level data.

In order to compute λ_i, the share of firm i's sales in industry sales that is not re-used by the manufacturing industry, from equation (2.1), we use data from India's input-output table drawn from the World Input-Output Database[10]. We calculate the share of output that is re-used by manufacturing as inputs and scale total industry sales by 1 minus this share. λ_i is then equal to firm i's sales over this number.

2.4.2 Classifying Firms as High or Low MRPK

In this subsection, we describe how firms are classified as having high or low MRPK. In order to assess whether the policy increased or reduced misallocation, following the logic outlined in subsection 2.3.1, we test whether the reform had a differential effect on firms with high vis-à-vis low MRPK. We use the Cobb-Douglas production function, as is commonplace in the production function estimation literature, where we assume that firms operate using the

[9]These data come from the Ministry of Commerce and Industry, Government of India, and can be found here: `https://eaindustry.nic.in/download_data_0405.asp`.

[10]See `http://www.wiod.org/home`

following production function:

$$Y_{ijt} = A_{ijt} K_{ijt}^{\alpha_j^K} L_{ijt}^{\alpha_j^L} M_{ijt}^{\alpha_j^M} \tag{2.2}$$

where i stands for a firm, j for a 2-digit industry, and t for a year. Y_{ijt}, K_{ijt}, l_{ijt}, M_{ijt}, are (revenue) measures of output, assets, the wage-bill, and materials, and A_{ijt} denotes firm-specific unobserved productivity. Output is proxied with sales. As the data come from firm financial statements that do not contain information on quantities, this production function is in revenue terms. We deflate the variables appropriately using wholesale price indices.

Estimating MRPK. We estimate the marginal revenue product of capital by using the fact that with a revenue-based Cobb-Douglas production function,

$$MRPK = \frac{\partial Y_{ijt}}{\partial K_{ijt}} = \alpha_j^K \frac{Y_{ijt}}{K_{ijt}}$$

and owing to the assumption that firms in an industry have a constant α_j^K, the output-capital ratio, $\frac{Y_{ijt}}{K_{ijt}}$, provides a within-industry measure of MRPK. The advantage of this measure is that it imposes very few data requirements which make it possible to use the largest amount of data.

Following Bau and Matray (2020), in order to determine whether firms had a high or low MRPK prior to the policy reform, we proceed in two steps. First, we construct an average measure of a firm's MRPK over three years of the pre-reform period from 2004 to 2006. Then, we classify a firm as capital constrained (or high MRPK) if its average is above the 4-digit level industry median. Figure 2.1 shows the distribution of log(MRPK) as measured by the methodology described above. This measure is confounded by measurement error. However, there seems to be a considerable amount of capital misallocation prior to this lending policy.

2.4.3 Econometric Specification

Firm-level regressions. In order to asses the effect of the policy on the reallocation of resources within industries, our baseline regression specification is:

$$\begin{aligned} y_{ijt} = \beta_0 + \beta_1 \left(\mathbb{1}_i^{RE} \times \mathbb{1}_{ij}^{HighMRPK} \times \text{After}_t \right) + \beta_2 \left(\mathbb{1}_i^{RE} \times t \right) + \beta_3 \cdot \mathbb{1}_i^{RE} \\ + \mathbf{X}_{it}'\Gamma + \mathbf{I}_{jt}'\Phi + \mu_i + \lambda_t + \varepsilon_{ijt} \end{aligned} \tag{2.3}$$

where i stands for a firm, j for an industry, t for a year, and y_{ijt} is the outcome variable of interest that consists of the logs of physical capital, MRPK, the wagebill, and revenue[11]. $\mathbb{1}_i^{RE}$ is an indicator that equals one if firm i became "Recently Eligible" (i.e., plant and machinery valued at ₹10 million to ₹50 million) due to the policy change. $\mathbb{1}_{ij}^{HighMRPK}$ equals one if the firm was classified as having high MRPK (constrained) during the pre-policy period. The indicator After_t equals one for all the years after the year of the policy change (2007-09) and zero otherwise.

The control group consists of firms that are ineligible for the directed lending policy (i.e., firms with plant and machinery valued over and above ₹50 million). We drop firms that were always eligible, both, before and after the policy change (i.e., firms with plant and machinery valued between ₹2.5 million and ₹10 million). We consider a three-year estimation window before and after the policy change. This is because in 2010 two regulatory changes – deregulation of the interest rate regime and the introduction of a credit guarantee scheme targeted at small firms – were implemented, and these are likely to bias the

[11]Revenue-based measures are used as proxies for quantity-based measured in other studies such as Asker et al. (2014) and Hsieh and Klenow (2009), among others, that study misallocation by looking at wedges in the first order conditions of firms.

results.

The coefficient of interest β_1 uncovers the relative differential effect of the policy on newly eligible firms that have ex ante high MRPK relative to low MRPK firms. If $\beta_1 > 0$, then the policy caused a relative increase in y_{ijt} to constrained (high MRPK) firms that became eligible following the policy change relative to unconstrained firms. $\mathbf{X}_{it}$ include firm-level controls for profitability and size that capture, respectively, the effects of demand and firm-size (and age). Industry-level controls, contained in $\mathbf{I}_{jt}$, such as the growth rate of industry output and total industry output, capture the effects of industry-specific shocks to productivity and demand. We include firm fixed effects to control for time-invariant firm characteristics and year fixed effects to account for aggregate national-level shocks that are not specific to any firms or industries. We also control for linear time trends that may potentially affect the dependent variable for the treated group of firms. And finally, standard errors are clustered at the firm level. Treatment is at the firm level[12] i, hence the focus is on *within-firm* changes in distortions before and after the policy.

Industry-level regressions. To evaluate how the level of misallocation changed at the industry level as a result of the policy, we test the following empirical specification:

$$\begin{aligned}\sigma_{jt}^{mrpk} = \beta_0 + \beta_1 \left(\text{Frac}_j^{RE,HighMRPK} \times \text{After}_t\right) + \beta_2 \cdot \text{Frac}_j^{RE,HighMRPK} \\ + \mathbf{I}_{jt}^{'}\Phi + \theta_j + \lambda_t + \varepsilon_{jt}\end{aligned} \tag{2.4}$$

where the dependent variable is a measure of dispersion (standard deviation, interquartile range, or the interdecile range) of MRPK within industry j in year t. The variable $\text{Frac}_j^{RE,HighMRPK}$ is the fraction of recently eligible firms with

[12] A set of firms in any industry j become eligible for the credit program based on their value of plant and machinery.

high MRPK over the total of eligible firms and ineligible firms within a 2-digit industry j, thus measuring the intensity of treatment at the industry level. For any industry j, this fraction can be written as:

$$\text{Frac}_j^{RE,HighMRPK} = \frac{\sum_i \left(\mathbb{1}_{ij}^{RE} \times \mathbb{1}_{ij}^{HighMRPK}\right)}{\sum_i \left(\mathbb{1}_{ij}^{RE} + \mathbb{1}_{ij}^{NE}\right)}$$

Industry controls such as the total industry output and industry fixed assets are contained in $\mathbf{I}_{jt}$. These capture the effects of industry-specific shocks on changes to misallocation. Unobserved time-invariant heterogeneity across industries are absorbed by industry fixed effects θ_j. These capture the effects of industry-specific policies enacted by the government to boost productivity. Year fixed effects λ_t capture any macro-level fluctuations that impact all firms in the sample during the period of this study.

The coefficient of interest β_1 measures the differential effect of the policy on the degree of within-industry misallocation on industries that have a greater fraction of "treated" firms (eligible with high MRPK) relative to industries that were treated with less intensity.

2.5 Results

2.5.1 Firm-level Outcomes

Table 2.2 reports the estimates of the differential effects of the policy with respect to the baseline specification, equation (2.3). The table presents estimates of β_1, our coefficient of interest on the interaction term, for the four main outcome variables – capital, MRPK, wagebill, and income. The sample consists of firms that were "recently eligible" for the credit program and firms that were ineligible (much larger firms[13]).

In the three-year period after the eligibility to directed credit was modified, high MRPK (or capital constrained) firms invested more and increased physical capital (row 1 of Table 2.2). Physical capital increased by 7% and along with this expansion, capital constrained firms witnessed a decline in their MRPK (row 2) by about 8%. While these changes are strongly statistically significant, there seem to be no significant change in the wagebill[14] and revenues (rows 3 and 4, respectively). It is worth pointing out that the signs on these coefficients point in right direction as theory would suggest.

These results are robust to the inclusion of firm controls to proxy for firm-level shocks to productivity, demand, and profitability, as well as industry-level controls that proxy for industry size and growth. All specifications control include year and firm fixed effects to control for aggregate shocks and unobserved firm-level time-invariant shocks, respectively.

2.5.2 Industry-level Outcomes

Next, we present the effects of the policy on the level of misallocation at the 2-digit industry level. We measure the degree of misallocation using a measure of dispersion of industry-level MRPK. Table 2.3 shows the estimates of β_1, the coefficient on the interaction term from equation (2.4), for three different measures of dispersion: the standard deviation, the 75$^{\text{th}}$ minus the 25$^{\text{th}}$ percentile (interquartile range), and the 90$^{\text{th}}$ minus the 10$^{\text{th}}$ percentile (interdecile

[13]We control for variations in firm size within the sample using fixed effects and firm-level controls.

[14]The Prowess data do not report the number of employees but only the total wagebill. This is problematic for the results because we cannot distinguish between a change in the wagebill that is due to a change in: (i) the number of employees, (ii) wage rates, or (iii) the skill-composition of firms' labor force. It is likely that changes in all of these components of the wagebill may confound the results.

range)[15].

The sample consists of firms that became eligible and those that remained ineligible throughout the estimation period. The greater the fraction of "recently eligible" firms with high MRPK in any industry j, the greater is the treatment intensity of the credit policy. We test this hypothesis by interacting the variable $\text{Frac}_j^{RE,HighMRPK}$, as defined in subsection 2.4.3, with the post-period dummy After_t. If relaxing credit constraints allows capital constrained firms to invest more then industries with a higher fraction of treated firms will experience significantly larger relative reductions in the dispersion of MRPK.

In the period after the policy was changed, industries with a greater fraction of "recently eligible" firms with high MRPK witnessed a statistically significant decline in dispersion of MRPK as measured by the interdecile range (row 3, Table 2.3). A one standard deviation increase in the treatment variable results in a 30% decline in the mean value of the interdecile range. In line with the firm-level results, the gains from reallocation were not large enough to result in statistically significant decline in the standard deviation and the interquartile range. It is worth pointing out, however, that the signs of the coefficients are all consistently negative. We control for total sales and fixed assets as proxies for production and industry size. Industry fixed effects capture all unobserved time-invariant heterogeneity that

2.5.3 Aggregate Effects

Our results so far have established that capital constrained firms that became eligible to the credit policy expanded by investing more in physical assets, and this results in a decline in misallocation, although only for one measure of dispersion of MRPK – the interdecile range. In this subsection, we quantify the

[15]We normalize the interquartile range and the interdecile range by dividing them by 100 to make the coefficients comparable.

effects of this reduction on aggregate productivity for the eligible firms using equation (2.1).

Identification. Our goal is to estimate the parameters in equation (2.1) using the reduced-form regressions. The equation can be split into two parts: (i) a change in firm-level productivity, and (ii) and change in firm-level inputs. The first part, within-firm productivity, is given by $\Delta \log A_i$, and since we do not observe any significant changes of the policy on firm-level productivity, we set $\Delta \log A_i = 0$. The components of the second part of equation (2.1) can be obtained from data or estimated from firm-level data using the policy change as a natural experiment.

We estimate λ_i as the share of firms' total sales in industry net output that is not re-used as manufacturing inputs[16]. The values for α_i^x are obtained directly from the estimates of the production function. For each of the inputs, capital, labor, and materials, we estimate $\Delta \log x_i$ from the average treatment effects using a simple triple differences strategy as follows:

$$\log x_{ijt} = \beta_1 \left(\mathbb{1}_i^{RE} \times \mathbb{1}_{ij}^{HighMRPK} \times \text{After}_t \right) + \beta_2 \cdot \mathbb{1}_i^{RE} + \mathbf{X}_{it}^{'}\Gamma + \mathbf{I}_{jt}^{'}\Phi + \mu_i + \lambda_t + \varepsilon_{ijt} \tag{2.5}$$

where $\log x_{ijt}$ is one of the three inputs. This provides an estimate of the change in inputs used between the pre- and post-period due to the policy change and we estimate the change in inputs due to the policy, $\widehat{\log x_{ijt}}$, as $\widehat{\beta_1} \left(\mathbb{1}_i^{RE} \times \mathbb{1}_{ij}^{HighMRPX} \times \text{After}_t \right) + \widehat{\beta_2} \cdot \mathbb{1}_i^{RE}$.

Estimating firm-level wedges. All that remains in order to estimate equation (2.1) is the estimation of τ_i^x, the firm-level input wedges prior to the policy. We

[16] Using the World Input-Output Database, we calculate the share of output that is re-used by manufacturing as inputs and scale total industry sales by 1 minus this share. λ_i is then equal to firm i's sales over this number.

follow the procedure adopted in Bau and Matray (2020) and estimate a lower bound of the effects of the policy on the Solow residual since measurement error in inputs or marginal products greatly affects the dispersion in wedges.

We need to make two assumptions to estimate a lower bound on the wedges. First, we assume that the policy did not subsidize treated firms in the sense of making their wedges negative (the policy does not increase misallocation). Second, we assume that firms that were ineligible to the credit policy did not see any significant change in their wedges. This simply means that ineligible firms form the control group in equation (2.5). Defining the post-policy wedge for a firm as $\tau^x_{Post} = \tau^x_{Pre} + \Delta\tau^x$, where $\Delta\tau^x$ is the change in the wedge of input x due to the policy, and assuming that $\tau^x_{Post} \geqslant 0$, the least value for the pre-treatment wedge is $-\Delta\tau^x$. This is due to the assumption that after the policy there are no wedges remaining in any of the inputs that are due to misallocation but could still remain due to mismeasurement. Estimating $\Delta\tau^x$ provides a lower bound of the pre-treatment wedge that we then use to estimate equation (2.1).

Assume that true marginal revenue product of input x for firm i, denoted by $mrpx_{it}$, is observed with measurement error. Measured marginal revenue product is given by $\log(MRPX_{it}) = \log(mrpx_{it}) + \mu_i + \lambda_t + \varepsilon_{it}$, where μ_i is a firm-specific, time-invariant shock, λ_t is a year-specific shock, and ε_{it} is an idiosyncratic error term. The true marginal revenue product for input x before the policy has taken effect is given by $\log(mrpx_{it} = \log(1 + \tau^x_{it})) + \log(p^x_t)$. After the policy has taken effect (when $\text{After}_t = 1$) for recently eligible firms (RE=1), due to the assumption that treated firms have zero post-policy wedges, $\log(mrpx_{it}) = \log(p^x_t)$, and

$$\log(MRPX_{it}) = \log(p^x_t) + \mu_i + \lambda_t + \varepsilon_{it}$$

Owing to the assumption that the policy does not change the wedges of untreated, or ineligible, firms (RE=0):

$$\log(MRPX)_{it} = \log(1+\tau_{it}^{x}) + \log(p_t^x) + \mu_i + \lambda_t + \varepsilon_{it}$$

Consider the triple differences regression:

$$\log(MRPX_{ijt}) = \beta_1 \left(\mathbb{1}_i^{RE} \times \mathbb{1}_{ij}^{HighMRPX} \times \text{After}_t\right) + \beta_2 \cdot \mathbb{1}_i^{RE} + \mathbf{X}_{it}^{'}\Gamma + \mathbf{I}_{jt}^{'}\Phi + \mu_i + \lambda_t + \varepsilon_{ijt} \tag{2.6}$$

The firm-specific shock will be absorbed by firm fixed effects μ_i. The time-specific shock along with changes in $\log(p_t^x)$ will be absorbed by the year fixed effects λ_t. The identifying assumption here is that ε_{it} is orthogonal to treatment so that. $\mathbb{E}[\varepsilon_{it}|\mathbf{X}_{it}^{'}, \mathbf{I}_{jt}^{'}] = 0$. Then estimates from the triple-differences regression will allow us to predict $\log(1+\tau_i^x)$. Using this we can back out the input wedges by computing $\widehat{\tau}_i^x = \exp\left(\widehat{\log(1+\tau_i^x)}\right) - 1$.

We estimate the following triple-differences regression for the three inputs' marginal revenue products:

$$\log(MRPX_{ijt}) = \beta_1 \left(\mathbb{1}_i^{RE} \times \mathbb{1}_{ij}^{HighMRPX} \times \text{After}_t\right) + \beta_2 \cdot \mathbb{1}_i^{RE} + \mathbf{X}_{it}^{'}\Gamma + \mathbf{I}_{jt}^{'}\Phi + \mu_i + \lambda_t + \varepsilon_{ijt}$$

Using the estimates from this regression, we can estimate the wedges as follows:

$$\log\widehat{(1+\tau_i^x)} = \widehat{\beta_1}\left(\mathbb{1}_i^{RE} \times \mathbb{1}_{ij}^{HighMRPX} \times \text{After}_t\right) + \widehat{\beta_2} \cdot \mathbb{1}_i^{RE}$$

Using estimates for all the components of equation (2.1), we can now calculate the effect of the policy on the Solow residual and we find that the lower bound effect is a 16% increase between the pre- and post- period of the policy change.

2.6 Conclusion

In this chapter we study whether targeted lending programs can affect the misallocation of capital by channeling credit to firms with high marginal products of capital. We leverage a policy experiment that expanded the eligibility of firms to a nationwide directed credit program. The results document the link between targeted credit policy and misallocation for small firms.

The directed lending policy identifies a large pool of firms that became eligible for loans. We exploit cross-sectional heterogeneity in marginal revenue products of capital (MRPK) across firms within industries to classify firms as credit constrained (high MRPK) vis-à-vis unconstrained using the value of their MRPKs relative to the industry median prior to the policy. We then combine these reduced-form estimates and estimates from the production function to quantify the effect of the policy on aggregate productivity.

If new investment projects were financed from credit that flowed to firms with relatively high MRPK, this would cause a decline in the dispersion of MRPK and therefore reduce *within-industry* misallocation. We identify that for firms with initially higher MRPK, the policy resulted in relatively larger increases in physical capital in the period after the credit policy was modified. We also find that as capital increases for constrained firms, these firms exhibit a decline in their MRPK relative to unconstrained firms. However, we find no statistically significant change in the wagebill and revenues. At the industry level, the number of newly eligible firms varies quite a bit. This would suggest that within-industry misallocation reduced in industries with relatively more "treated firms" – newly eligible firms with high MRPK. We exploit heterogeneity in treatment intensity across industries and find that industries with a larger fraction of eligible firms with high MRPK relative to the total number of firms witnessed a decline in dispersion of MRPK as measured by the interdecile range. We then quantify the aggregate effects of this reduction in misallocation on aggregate productivity using changes in the Solow residual as a proxy. Using the reduced-form estimates and estimating parameters from firms' production functions, we find that the policy increased the manufacturing industry's Solow residual by at least 16%. Since measures of misallocation that use changes in firms' input wedges can be greatly influenced by measurement error, we focus on estimating a lower bound that accounts for this error.

This study is important for two reasons. First, this study documents how targeted credit policy can lead to improvements in productivity by reducing misallocation across firms. This relates to the vast literature on the importance of small and young firms, and policies that aim to enhance their productivity. Second, this study quantifies the effect of the policy on aggregate productivity. These findings are important for the policy maker to help in designing efficient credit policies and evaluating their effects on firms. Since targeted lending is prevalent across many developing and developed countries, it is important to study their effectiveness and evaluate whether they succeed in leveling the playing field for firms.

2.7 Figures

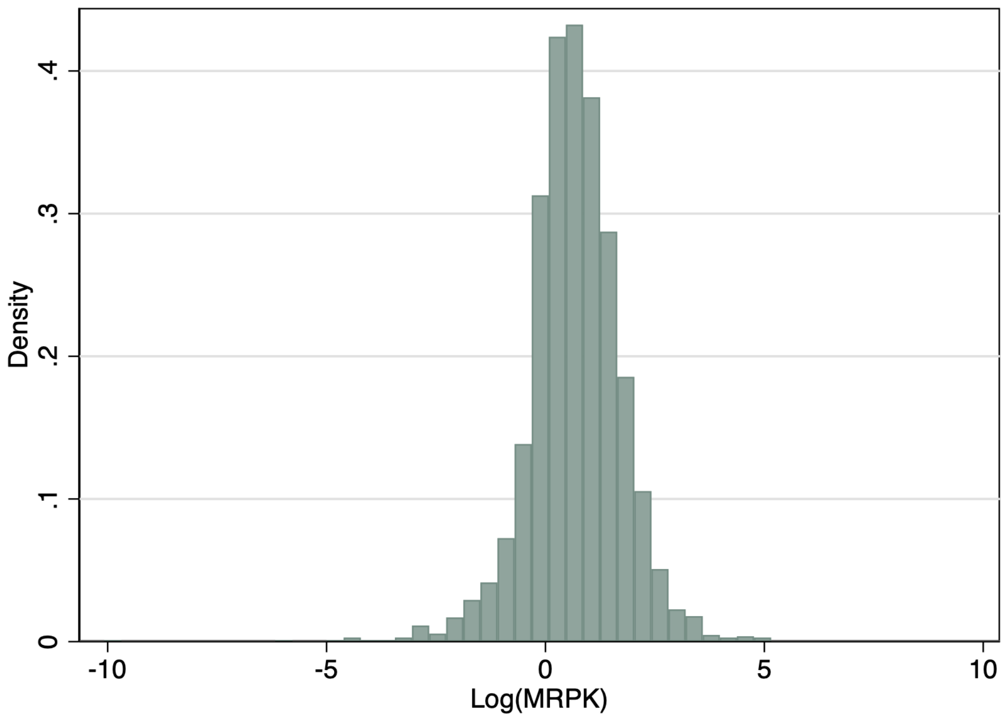


Figure 2.1: Distribution of Log(MRPK), 2004-2006

Source: CMIE Prowess Database, Author's calculations. Notes: This figure shows the distribution of log(MRPK) for manufacturing firms in the Prowess data for 2004-06.

2.8 Tables

Table 2.1: Summary Statistics

Variable	Mean	Std. Dev.	Median	N
Capital	1,266	7,525	164	8,154
Wagebill	102	695	16	8,115
Sales	2,083	10,118	382	8,154
Income	2,166	11,014	387	8,154
PBIT	286	2,375	22	8,154

Notes: Data correspond to the period before the policy change (2004-06). Data are in millions of rupees, apart from the share variables.

Table 2.2: Baseline Firm-Level Specification

Dependent Variable	(1)	(2)	(3)
Capital	0.0692***	0.0731***	0.0711***
	(0.0267)	(0.0235)	(0.0235)
MRPK	-0.0834*	-0.0841***	-0.0823***
	(0.0474)	(0.0273)	(0.0273)
Wagebill	0.0302	0.0318	0.0318
	(0.0326)	(0.0269)	(0.0269)
Income	0.0118	0.0071	0.0059
	(0.0414)	(0.0121)	(0.0120)
Observations	15,438	15,438	15,437
Clusters	2,638	2,638	2,638
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Firm Controls	No	Yes	Yes
Industry Controls	No	No	Yes

Notes: This table reports the impact of the PSL program by firms' ex-ante capital constraints (proxied by their MRPK) as specified by Equation (2.3). This table reports estimates of β_1 for all the dependent variables of interest. The respective dependent variable is as specified in column 1. Controls include proxies for firm-level profitability and size, and industry-level averages of total sales and growth rate of sales. Robust standard errors, reported in parentheses, are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.3: Industry-Level Misallocation

Dependent Variable	(1)	(2)	(3)
S.D. (MRPK)	-0.3196	-0.7028	-0.3767
	(1.4310)	(1.4654)	(1.5299)
Disp. (75 – 25)	-0.0794	-0.1156	-0.1086
	(0.0894)	(0.0986)	(0.0949)
Disp. (90 – 10)	–0.2515*	-0.3253**	-0.2880**
	(0.1494)	(0.1347)	(0.1229)
Observations	126	126	126
Clusters	21	21	21
Year FE	Yes	Yes	Yes
Industry Controls	No	Yes	Yes
Industry FE	No	No	Yes

Notes: This table reports the impact of the PSL program on a *within-industry* measure of dispersion as specified by Equation (2.4). This table reports estimates of β_1 for all the dependent variables of interest. The dependent variables are measures of dispersion as listed in column 1. Controls include industry-level averages of sales and capital. Robust standard errors, reported in parentheses, are clustered at the 2-digit industry level. *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX

2.A Tables

Table 2.A.1: Baseline Specification, Log(Capital)

	(1)	(2)	(3)
$\mathbf{1}^{\text{RE}} \times \text{After}_t \times \mathbf{1}^{\text{High MRPK}}$	0.0692***	0.0731***	0.0711***
	(0.0267)	(0.0235)	(0.0235)
Recently Eligible $\times$ t	-0.0611***	-0.0533***	-0.0527***
	(0.0069)	(0.0063)	(0.0063)
Observations	15,438	15,438	15,437
Clusters	2638	2638	2638
Mean of Dep. Var.	5.52	5.52	5.52
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Firm Controls	No	Yes	Yes
Industry Controls	No	No	Yes

Notes: This table reports the impact of the PSL program by firms' ex-ante capital contsraints (proxied by their MRPK) as specified by Equation (2.3). The dependent variable is the firms' Log(Capital) . Controls include proxies for firm-level profitability and size, and industry-level averages of total sales and growth rate of sales. Robust standard errors, reported in parentheses, are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.2: Baseline Specification, Log(MRPK)

	(1)	(2)	(3)
$\mathbf{1}^{\text{RE}} \times \text{After}_t \times \mathbf{1}^{\text{High MRPK}}$	-0.0834*	-0.0841***	-0.0823***
	(0.0474)	(0.0273)	(0.0273)
Recently Eligible $\times$ t	0.0381***	0.0566***	0.0560***
	(0.0142)	(0.0076)	(0.0076)
Observations	15,438	15,438	15,437
Clusters	2638	2638	2638
Mean of Dep. Var.	0.62	0.62	0.62
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Firm Controls	No	Yes	Yes
Industry Controls	No	No	Yes

Notes: This table reports the impact of the PSL program by firms' ex-ante capital contsraints (proxied by their MRPK) as specified by Equation (2.3). The dependent variable is the firms' Log(MRPK) . Controls include proxies for firm-level profitability and size, and industry-level averages of total sales and growth rate of sales. Robust standard errors, reported in parentheses, are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.3: Baseline Specification, Log(Wagebill)

	(1)	(2)	(3)
$\mathbf{1}^{\text{RE}} \times \text{After}_t \times \mathbf{1}^{\text{High MRPK}}$	0.0302	0.0318	0.0318
	(0.0326)	(0.0269)	(0.0269)
Recently Eligible $\times$ t	-0.0405***	-0.0289***	-0.0289***
	(0.0094)	(0.0076)	(0.0076)
Observations	15,386	15,386	15,385
Clusters	2637	2637	2637
Mean of Dep. Var.	3.09	3.09	3.09
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Firm Controls	No	Yes	Yes
Industry Controls	No	No	Yes

Notes: This table reports the impact of the PSL program by firms' ex-ante capital contsraints (proxied by their MRPK) as specified by Equation (2.3). The dependent variable is the firms' Log(Wagebill) . Controls include proxies for firm-level profitability and size, and industry-level averages of total sales and growth rate of sales. Robust standard errors, reported in parentheses, are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.4: Baseline Specification, Log(Income)

	(1)	(2)	(3)
$\mathbf{1}^{\text{RE}} \times \text{After}_t \times \mathbf{1}^{\text{High MRPK}}$	0.0118	0.0071	0.0059
	(0.0414)	(0.0121)	(0.0120)
Recently Eligible $\times$ t	-0.0292**	-0.0039	-0.0037
	(0.0118)	(0.0040)	(0.0040)
Observations	15,438	15,438	15,437
Clusters	2638	2638	2638
Mean of Dep. Var.	6.14	6.14	6.14
Year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Firm Controls	No	Yes	Yes
Industry Controls	No	No	Yes

Notes: This table reports the impact of the PSL program by firms' ex-ante capital constraints (proxied by their MRPK) as specified by Equation (2.3). The dependent variable is the firms' Log(Income) . Controls include proxies for firm-level profitability and size, and industry-level averages of total sales and growth rate of sales. Robust standard errors, reported in parentheses, are clustered at the firm level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.5: Industry-Level Misallocation, S.D. (MRPK)

	(1)	(2)	(3)
$\text{Frac}_j^{\text{RE,High MRPK}} \times \text{After}_t$	-0.3196	-0.7028	-0.3767
	(1.4310)	(1.4654)	(1.5299)
$\text{Frac}_j^{\text{RE,High MRPK}}$	1.4197	1.3870	1.8926
	(1.2237)	(1.2496)	(1.4823)
Observations	126	126	126
Clusters	21	21	21
Mean of Dep. Var.	1.48	1.48	1.48
Year FE	Yes	Yes	Yes
Industry Controls	No	Yes	Yes
Industry FE	No	No	Yes

Notes: This table reports the impact of the PSL program on a *within-industry* measure of dispersion as specified by Equation (2.4). The dependent variable is the *within-industry* S.D. (MRPK) (normalized). Controls include industry-level averages of sales and capital. Robust standard errors, reported in parentheses, are clustered at the 2-digit industry level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.6: Industry-Level Misallocation, Disp. (75 – 25)

	(1)	(2)	(3)
$\text{Frac}_j^{\text{RE,High MRPK}} \times \text{After}_t$	-0.0794	-0.1156	-0.1086
	(0.0894)	(0.0986)	(0.0949)
$\text{Frac}_j^{\text{RE,High MRPK}}$	0.0864**	0.1047**	0.0743*
	(0.0401)	(0.0532)	(0.0368)
Observations	126	126	126
Clusters	21	21	21
Mean of Dep. Var.	0.03	0.03	0.03
Year FE	Yes	Yes	Yes
Industry Controls	No	Yes	Yes
Industry FE	No	No	Yes

Notes: This table reports the impact of the PSL program on a *within-industry* measure of dispersion as specified by Equation (2.4). The dependent variable is the *within-industry* Disp. (75 – 25) (normalized). Controls include industry-level averages of sales and capital. Robust standard errors, reported in parentheses, are clustered at the 2-digit industry level. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.A.7: Industry-Level Misallocation, Disp. (90 – 10)

	(1)	(2)	(3)
$\text{Frac}_j^{\text{RE,High MRPK}} \times \text{After}_t$	-0.2515*	-0.3253**	-0.2880**
	(0.1494)	(0.1347)	(0.1229)
$\text{Frac}_j^{\text{RE,High MRPK}}$	0.1426	0.1449	0.1154
	(0.1715)	(0.1782)	(0.1247)
Observations	126	126	126
Clusters	21	21	21
Mean of Dep. Var.	0.08	0.08	0.08
Year FE	Yes	Yes	Yes
Industry Controls	No	Yes	Yes
Industry FE	No	No	Yes

Notes: This table reports the impact of the PSL program on a *within-industry* measure of dispersion as specified by Equation (2.4). The dependent variable is the *within-industry* Disp. (90 – 10) (normalized). Controls include industry-level averages of sales and capital. Robust standard errors, reported in parentheses, are clustered at the 2-digit industry level. *** p<0.01, ** p<0.05, * p<0.1.

CHAPTER 3

VICTIMS OF CONSEQUENCE: EVIDENCE ON CHILD OUTCOMES USING MICRODATA FROM A CIVIL WAR

[WITH SAJALA PANDEY]

3.1 Introduction

Exposure to an armed conflict has detrimental effects on infants and children. Existing literature in economics has documented the negative effects of violent events on child health (Akresh et al., 2012), child birthweight (Mansour and Rees, 2012), height and cognitive skills (Duque, 2016), and long-run educational achievements (Akresh, 2008). However, most studies seek to estimate the causal effects of violent conflict either on the long-run educational outcomes or short-run health outcomes like height and weight. Still, relatively little is known about how short-run child time-allocation decisions, schooling outcomes, and contemporaneous health-seeking behaviors are distorted due to persistent violence.

In this chapter, we study the causal effects of violence on child time allocation, curative health care, and education. In particular, we examine how periods of heightened conflict during the Nepalese Civil War (1996-2206) affect contemporaneous child labor and health care utilization decisions along with short-run educational outcomes. As violence increases, parents are less likely to send their children to school or take them to health facilities and more likely to keep them home. Less time in school might translate to more time spent at work (paid or unpaid) for these children. Coping with violence, therefore, disrupts education and curative health care of young children which can be potentially damaging to their later-life outcomes.

Additionally, armed conflicts can have differential impacts on boys and girls. Shemyakina (2011) finds that girls (but not boys) who were exposed to Tajik armed conflict were less likely to complete mandatory schooling. Likewise, Chamarbagwala and Morán (2011) find that Guatemala's 36-year-long civil war affected the long-run education outcomes of Mayan females more adversely than males. However, evidence on the gender disparity in the effects of armed conflict is mixed. Akresh (2008) finds strong negative effects of the Rwandan genocide on the completed education of boys. In terms of health outcomes, Akresh et al. (2012) find that both boys and girls who were born during the 1998–2000 Eritrea–Ethiopia experienced similar negative impacts on height-for-age Z-scores. There is also evidence of how conflict affects children of certain age groups more than the others, even though the research on this dimension remains limited. In two separate studies, Shemyakina (2011) (for Tajik conflict) and Rodriguez and Sanchez (2012) (for Colombian conflict) find that violent events affected schooling outcomes of children aged 12 and older.

The salient horrors of violence are obvious and anecdotal evidence on them is widely available. However, the causal effect of armed-conflict on contemporaneous outcomes of children is difficult to measure as data and information arrive haphazardly. The reason being that violent events prevent survey takers from doing their jobs. In this situation of an "informational black hole", the Nepalese Civil War (1996-2006) provides an excellent setting to examine the research questions. Firstly, this armed conflict did not deter ongoing surveys, hence, allowing us to examine contemporaneous outcomes. Secondly, the nature of the war itself, where different parts of the country experienced a varying level of violent events overtime, provides us with the required geographical and temporal variation to address our questions. The prolonged armed conflict between the Maoist and the government of Nepal initially began in the Western part of the country as a small-scale anti-government protests. However, over time the conflict engulfed the entire country killing more than 13,000 people.

Our data on armed conflict comes from a unique database of victims from the civil war which includes the date and the location of every war-related event. We then merge this data with a nationally representative household survey to access information on contemporaneous child-level outcomes during the conflict. For identification, we exploit the spatial and temporal variation in exposure to violence at local administrative levels.[1] This gives us a setting for a quasi-natural experiment to answer our research question.

We show that an increase in conflict-related violent events in the past 30 days increases the total hours worked by children aged 5 to 16. A one standard deviation increase in exposure to conflict in a given month increases total hours worked by 4% of the sample mean. The estimated effects are especially substantial for the time allocated to agricultural work: a one standard deviation in violence exposure increases hours worked in agriculture by roughly 8% of the sample mean. The impact of violence on time allocated to work is largely driven by the younger cohort (age 5-11) and boys.

As for health-seeking behavior, we find that violence does not make people sicker but reduces the probability of visiting a health care facility for children less than 16 years of age. This effect is non-negligible: a one standard deviation increase in violent events in the past 30 days, decreases the likelihood of taking a child to a health facility by roughly 6% of the sample mean. Unlike the neg-

[1] Local administrative levels include Village Development (VDCs) and Municipalities which are the second-lowest administrative units in Nepal.

ative impact of conflict on time-allocation, the drop in curative care-seeking behavior is driven by girls. The potential connection between child's (labor) productivity and sound health can be a reason why we do not observe a decrease in curative care for boys. Next, we focus on the educational outcomes of children during the war. Exposure to violent events reduces the likelihood of currently attending school and decreases years of education, in the short-run, by approximately 1.5 months. The negative impact of violence on educational achievements is significant for boys.

One of the potential mechanisms driving our contemporaneous outcomes is that an increase in violent events changes parents' perception of risk, hence, altering their decisions related to children. This causal channel aligns with the theoretical model provided by Estevan and Baland (2007). Their model shows that when there is an increase in child mortality risk and parents are not very altruistic, child labor increases whereas schooling decreases. An increase in mortality risk hinders parents from truly internalizing the impact of child labor on their children's welfare. Therefore, they prefer immediate transfers from their children in the form of child labor rather than risky investments like education. The need for immediate transfer can also explain why violent events negatively impact boys than girls. A larger portion of a child's time allocation is on a physically strenuous and arduous task like agricultural work where the returns might be higher from boys. On health-seeking behavior, our findings echo the results from Molina (2016). She finds that local violence in the Philippines reduced curative care utilization for children by their parents due to avoidance behavior. Violence increases the risk of being victimized which then translates to higher non-monetary costs of seeking health care.

Related Literature and Contribution

This study is closely related to the literature in economics that studies the effect of violence on child-level outcomes. Our findings relate well with those of Akresh (2008) and Shemyakina (2011) who find negative effects of genocide and armed conflict on educational achievements in Rwanda and Tajikistan, respectively. We add to this literature by studying the contemporaneous effect of violence on child time allocation and health-seeking behavior along with schooling outcomes. As per our knowledge, Rodriguez and Sanchez (2012) and Di Maio and Nandi (2013) are the only other two studies analyzing the effect of conflict on child-labor. Valente (2013) studies the causal effect of conflict on education in Nepal but relies on district-level analysis. During the time of conflict, Nepal had 75 districts and 3,915 villages within those districts. We, on the other hand, exploit temporal variation in conflict across these villages around the date of the survey and identify the effects of local-level violence.

The second strand of literature this study contributes to is the studies on the relationship between violence and risk. Some experimental studies document that exposure to violence can change one's risk preferences (Brown et al., 2017; Callen et al., 2014; Voors et al., 2012). Households also adjust their production, savings and labor supply decisions as a response to income risk caused by an increase in violence (Bundervoet et al., 2006; Fernández et al., 2011). We add to this literature by providing suggestive evidence for risk associated with fear of victimization and mortality in the context of Nepal's civil war.

The rest of the chapter is organized as follows. In Section 3.2, we elaborate on the Nepalese Civil War. In Section 3.3 we discuss the main data sources that we use in the chapter. Section 3.5 lays out the empirical strategy. Section 3.6 presents results for the effect of violence on child time use, education, and health-seeking behavior. Section 3.7 discusses potential channels and Section

3.8 concludes.

3.2 Background

The Nepalese Civil War

On February 13, 1996, the Communist Party of Nepal (Maoist) (CPN-M) formally launched a rebellion against the government termed as the "People's War". This resulted in a prolonged armed conflict between the CPN-M forces and the government of Nepal that lasted until the Comprehensive Peace Accord was signed in 2006. During this period over 13,000 people were killed and about 1,300 went missing (UN, 2012).[2]

Historically, Nepal was governed as an absolute monarchy. During the early 1990s, Nepal transitioned to a constitutional monarchy, following a pro-democracy movement – the *Jana Andolan* ("People's Movement") – that witnessed the unification of various political parties towards the establishment of a constitutional framework. The "People's Movement" led to the establishment of multiparty democracy and voting rights, and in November 1990, the new constitution was drafted. This raised expectations of social progress, and some historians believe that this was one of the factors that contributed to the onset of internal conflict in 1996.

Shortly before the formal announcement of the "People's War", the CPN-M submitted a 40-point demand to the Nepali Government that covered many socioeconomic and political issues, and warned of a militant struggle that would follow if those demands were not met. Over the course of the next

[2]Different sources provide different estimates for this figure. The government claims that a total of 12,686 individuals were killed; although, since the State was actively involved in killings during this period, the government has an incentive to under-report. While the National Geographic Magazine also reports a similar figure as the government's (Bendiksen and Douglas, 2005), we identify 13,247 killings from our microdata.

ten years, acts of violence and destruction, human rights abuses, and mass killings by both, government forces and the CPN-M forces, were committed across Nepal's 75 districts.[3] Appendix 3.A shows the general time-line of key events during this civil war.

The CPN-M militia served under the leadership of a Chairman, who was also the Supreme Commander of the People's Liberation Army (PLA), which was formed in September 2001. According to a UN (2012) report, the Maoist militia had between 5,000 to 10,000 active combatants throughout the period of conflict and towards the end had expanded to multiple divisions across the country that was organized under three commands that were under the authority of the Supreme Commands and four Deputy Commanders. The PLA's playbook included guerrilla attacks, and sabotage and propaganda actions, such as random destruction and seizure of property (Shrestha, 1997). The hilly terrains of Nepal allowed the PLA to easily carry out guerrilla type warfare. Rural areas were more likely to be affected, at least during the initial phases of the war.

Apart from the CPN-M militia, the government's forces were also actively involved during the conflict period to fight against the PLA. Initially, since the conflict was seen as a minor threat, the Nepal Police (NP) was mobilized in order to contain the insurgency. In 2001, the Armed Police Force of Nepal, a paramilitary force, was set up in order to fight the insurgents due to the growing power of the Maoist forces. The Royal Nepalese Army (RNA) was not deployed by the government until late 2001 citing that the insurgency was a law and order problem that was to be addressed by the Nepal Police. Although the government's forces were to combat the insurgents, numerous acts of vi-

[3]With the exception of two districts – Manang and Mustang – all other districts witnessed conflict-related killings. Manang and Mustangs districts are both high-altitude trans-Himalayan regions and very sparsely populated.

olence and unlawful killings were committed as a result of collateral damage and chance encounters (UN, 2012). In particular the targets included those who were alleged informants or perceived as sympathizers for a particular side. Our microdata covers victims of violence from both sides of the conflict.

3.3 Data

Our data primarily comes from three independent sources: (i) the Informal Sector Service Center (INSEC) microdata on victims from conflict, (ii) the Nepal Living Standards Survey, and (iii) the National Population Census.

3.3.1 Microdata on Civil War

The data on victims from the Nepalese Civil War was collected by the Informal Sector Service Center (INSEC), a non-governmental organization based in Nepal that works on human rights issues.[4] This data was compiled from qualitative records from investigations of international human rights violations and international humanitarian law violations during the ten-year insurgency and are cross-referenced in the United Nations Human Rights Office of the High Commissioner's Nepal Conflict Archive.[5] The data maintained by INSEC has been used in previous studies, like those by Do and Iyer (2010)[6] and Shrestha (2017), and is, as per our knowledge, the most reliable and impartial database on conflict intensity during the civil war. It is also unique in nature since it is a census of a known population of victims from the war. The unit of observation

[4]This database is unique in that it is a census of victims from domestic conflict and is compiled by an impartial entity–Informal Sector Service Center (INSEC). This is important since both the Nepali government and the Maoist forces were actively involved in killings and acts of violence throughout this period. It is only reasonable to suspect that if the government were to build a similar database, it would necessarily try to underplay its role in the civil war.

[5]http://nepalconflictreport.ohchr.org

[6]I use geographically granular data on conflict than this study.

is an individual victim. The data also provides information on whether the victim was killed, injured, or disappeared along with information on the location and the exact date of the violent event.

The INSEC data reports 14,959 fatalities, of which 13,247 were killings, 932 were disappearances, and 780 were instances when the victim sustained disability inducing injuries.[7] By construction the data excludes acts of violence where people were not killed or injured, or did not disappear. For instance, if a building was torched and nobody was affected, our data would not record such an incident. To this extent, our data includes only victimization from conflict and not a broader set of threat to property and life due to conflict. Due to the rich temporal and spatial information in the data, we are able exploit variation along these dimensions in order to identify the effect of risk due to violence on economic decisions.

Figures 3.1 depicts the three major outcomes of violent events (killed, disappeared, or injured). More than half the number of deaths were caused by the government's forces. Figure 3.2(a) summarizes the distribution of deaths throughout the conflict by the perpetrator (State, Maoist, and Others). This data also captures the delayed involvement of the Nepali government's forces in the civil war. Specifically, after 2001, once the army was deployed, the number of deaths due to the State were strikingly higher than those caused by the Maoists. Figure 3.2(b) depicts the causes of these deaths as recorded in the INSEC database. Out of all the deaths that occurred during this war, 30% were due to combat fighting whereas the remaining of the victims died in a non-combat setting. The majority of the deaths in the non-combat setting was due to extra-judicial killing perpetrated by the state.

[7] Authors' calculation.

Spatial and Temporal Variation in Conflict

Figure 3.3 shows district-level spatial and temporal variation in conflict-related events from the start of this war to its end. Violent events first started in districts like Rukum and Rolpa and slowly started spreading in neighboring districts with varying and greater intensity (as it goes from lighter to darker shades). Here, for each individual map, we define intensity as conflict-related events (deaths, killing or disappearances) per 1000 population within a district for the given time frame. Although this district-level breakdown of conflict intensity provides information on the geographical spread of conflict across time, our analysis actually uses Village Development Committees (VDCs) and municipality level analysis. VDCs and municipalities are the second-lowest administrative units and collections of these VDCs and few municipalities make up a district. Focusing on smaller administrative units allows conflict to be local enough to influence decision making. Figure 3.B.1 in Appendix 3.B shows all the villages marked in red for which some kind of conflict-related event has been recorded in the INSEC data. Out of 3915 Village Development Committees (VDCs) and municipalities[8], the conflict data records some violent events for 2427 of them. This spatial and temporal variation in conflict across local level administrative units is what we intend to exploit in this study.

In order to illustrate the variation in conflict intensity across time and space, we plot the standard deviation of the number of violent events per thousand population across time and space (district level). Figure 3.4 plots the cross-sectional standard deviation for all districts across time (1996-2006) and Figure 3.5 plots the standard deviation for each district across time. As can be seen in Figure 3.4, with reference to the time-line of events in Appendix 3.A, the period immediately after the army was employed in November 2001 until the second

[8]VDCs (rural) and municipalities (urban)are the second-lowest administrative units in Nepal. Collection of VDCs and municipalities make up a district

round of peace talks began in January 2003, witnessed an increased number of killings across the country. There were however regions that witnessed relatively low levels of violence even during the peak of the war. Taken together, these two figures show the variation that we exploit in order to identify he causal effects of violence on household decisions on issues related to children.[9]

3.3.2 Nepal Living Standards Survey

To study the impact of violence on the household's economic decisions such as education, labor and time allocation, and health care of children, we use data from the Living Standards Measurement Study (LSMS), otherwise known as the Nepal Living Standards Survey (NLSS). This is a multi-topic representative household survey conducted by the Central Bureau of Statistics (CBS) and is available for the periods of 1995-96, 2003-04, and 2010-11. The survey is a cross-section that covers a broad range of household-level topics including consumption, income, labor markets, education, and health. Our outcome variables and bulk of control variables are taken from the NLSS, 2003-04 as the time of the survey falls within the conflict period allowing us to assess contemporaneous decision making during the time of armed conflict.

3.3.3 Population Census of Nepal

To weight our victim-level data by village-level population, we use the National Population Census of 2001. In order to distinguish between densely populated villages with a lot of violent events and sparsely populated regions with few deaths, our weighting technique is crucial in understanding the differential effect of conflict intensity across time and space.

[9] While doing so we are obviously restricted by the time-line of other surveys and the nature of questions in those surveys.

3.4 Summary Statistics

Table 3.1 reports summary statistics on our variables of interest and controls. Panel A of the table provides the descriptive statistics for children's time spent in work each week. On average, children of ages between 5 to 16 worked for 10.8 hours per week. This involves working in wage/non-wage market activities within or outside the household, agricultural work, or domestic chores. The variation in total hours worked is also high at 17.6 hours per week. A larger amount of time is allocated to agricultural work where children spent an average of 6.3 hours per week. The NLSS also provides information on how many hours these children worked in a year. The mean hours worked in activities outside schooling per year is equal to 426.3 hours.

Though the decision to obtain an education is a dynamic process, the NLSS has information on only final education outcomes observed at the time of the survey. Unlike time allocation in other activities, this survey does not report the number of hours dedicated to schooling per week. For education, we focus on ages 6 and above as Nepal's Education Act of 1971 [10] suggested the minimum age for primary school enrollment to be 6 years. Panel B of Table 3.1 reports the mean and standard deviation of educational outcomes of children aged 6 to 16 during the time of the survey. 80 percent of children reported having been ever enrolled and currently attending school. A large number of children are not in appropriate grade for their age. On average, 70 percent of current school-going children are over-age for the grades they are attending. Finally, the mean number of years of education for this age group is at 5.2 years.

Panel C of 3.1 reports summary statistics on health-seeking behavior for

[10] The first amendment of this Act was in July 2003. However, NLSS survey had already begun by then and the school year had already started for the amendment to have an immediate impact.

children of ages between 0 to 16. Only 8 percent of the children were taken to health services in the past 30 days to seek any curative health care. Fifty percent of children are female and eighty percent of them are from rural locations. The average number of years of education of household head is 3.9 years whereas that of the mother is of 1.8 years only.

3.5 Empirical Strategy

3.5.1 Specification

Since we are interested in contemporaneous and short-run outcomes, we exploit conflict around the date of the NLSS survey for each individual. We estimate the following specification: for outcome y_{ivt} of child i, living in village/municipality v and surveyed at date t[11], we have

$$y_{ivt} = \beta_0 + \beta_1 Intensity_{ivt} + \mathbf{X'_{iv}\Gamma} + \delta_m + \delta_y + \alpha_{ps} + \varepsilon_{ivt} \tag{3.1}$$

where $Intensity_{ivt}$ is a measure of conflict intensity that child i was exposed to in village v up to the date of the survey t. The construction of conflict intensity is outcome variable specific. For contemporaneous outcomes like time allocation and curative health care seeking, our goal is to understand if local-level conflict around the time of the survey has any causal impact. In this case, $Intensity_{ivt}$ is calculated as the total number of conflict-related events in the past 30 days up to the date of the survey per 1000 population in a village or a municipality. As mentioned earlier, unlike time allocation outcomes, the NLSS provides information only on the final educational outcomes of the children reported at the time of the survey. Therefore, for impacts on short-run educational outcomes, $Intensity_{ivt}$ is measured as the sum of the total number of

[11]Date includes day d, of month m of year y

violent events per 1000 population a child was exposed to since her birth (if born after 1996) or after the start of the civil war in 1996 (for those born before 1996) up to the date of the survey.

The NLSS survey spans over a period of more than one year. However, there is a limited variation in the date of survey of households within the same village or municipality and inclusion of local level fixed effects will drive away all the variation that we intend to exploit. Therefore, we include district-specific stratum fixed effects denoted by α_{ps}. District is a larger administrative area which includes several villages and municipalities. At the time of the survey, Nepal had 75 districts and 3,915 villages or municipalities within those districts. Stratum here takes care of the ecological and topographical division of villages and municipalities. X_{iv} are a set of individual, household and village level controls such as the age of the child, gender, mother's education, household head's education, household size, household wealth, village-level population, and total time taken to primary school or nearest health facility. We also control for ethnicity fixed effects. δ_m and δ_y are month and year of survey fixed effects. The coefficient of interest, β_1, measures the effect of a unit increase in conflict exposure on the outcome variable of interest. ϵ_{ivt} is the error term of the regression model. Finally, we estimate all regressions using Ordinary Least Squares (OLS) and cluster standard errors at the village or municipality level.

3.6 Results

3.6.1 Child Time Allocation and Labor

When the number of violent events in a locality increases, parents might decide to keep their children home and involve them in household work or other activities like agriculture. Spending more time in economic work or domestic chores by children due to ongoing conflict translates to a reduction in time allocated to studying or leisure.

Our source for the time use data is the NLSS-2003 survey which records hours per week or per year dedicated by children (aged 5-16) to various activities outside schooling. This allows us to analyze if local-level conflict in the past 30 days or 12 months from the date of the survey (for the latter measure of time use) had any contemporaneous effect on a child's time allocation and labor supply. *Our measure of conflict intensity ($Intensity_{ivt}$) variable as shown in equation 3.1 is calculated as the total number of conflict-related events in the past 30 days (or 12 months) up to the date of survey per 1000 population in a village or a municipality.* The first two rows of Table 3.2 present summary statistics for our measure of conflict intensity used to analyze impacts on time allocation. The mean of conflict intensity in the past 30 days and is 0.02 with a standard deviation of 0.1 whereas the average conflict intensity in the past 12 months is 0.2 with a standard deviation of 0.6.

Table 3.3 reports the coefficient estimates from equation 3.1 for time use of children in past 7 days. Column 1 of Panel A shows the results for the entire sample of children aged 5-16. We find that a one standard deviation increase in conflict intensity in the past 30 days, which is 0.1 (Table 3.2), increases total hours worked in a week by $4.205 \times 0.1 = 0.4205$ hours which is roughly 4% of the sample mean. This effect is significant and largely driven by an increase of approximately 7.4% of the sample mean in the total hours worked by younger children aged 5 to 11 (Column 2 of Panel A). Panels B and C of Columns 1 and 2 present results for effects on total hours worked for female and male samples, respectively. The increase in total hours for all children aged 5 to 16 is driven by an increase in working hours of boys belonging to both younger (age 5 to 11) and older (age 12 to 16) age-cohorts. For girls, there is a significant increase in total hours worked only for the younger cohort.

Columns 4 to 6 of Table 3.3 present the results for the contemporaneous effect of exposure to violence on time allocated to agricultural work. A standard deviation increase in conflict intensity in the past month increases time dedicated to agriculture for children of ages 5 to 16 by $5.340 * 0.1 = 0.534$ hours which is roughly 8.4% of the sample mean. The magnitude of the effect on time dedicated to agriculture is larger for the younger cohort (approximately 15% of the sample mean). As seen in Columns 4-6 of Panel C, these results are driven by an increase in the time allocated by boys belonging to both younger and older cohorts. Whereas for girls (Columns 4-6 of Panel B) a significant increase in agricultural work due to recent violence is observed only for the younger cohort of ages 5 to 11. The coefficients hours spent in domestic activities are negative but very small in magnitude with no statistical significance. [12]

3.6.2 Health

Curative Health Seeking Behavior

Our next goal is to understand if conflict-related events in the past 30 days affect parents' decision to seek any health care for their children less than 16 years of age. We focus on health-seeking behavior because a lack of proper health care, especially in developing countries, can be detrimental to children, the effect of which can last into adulthood. Access to proper health care can also improve school attendance and performance. Additionally, a larger por-

[12] Table 3.C.1 reports estimates for 3.1 when the outcome variable is hours worked in past 12 months. *For this estimation, our measure of conflict intensity ($Intensity_{ivt}$) is the total number of violent events per thousand population in a village or a municipality in the past 12 months of the date of the survey.* An increase in conflict intensity does not have a significant impact on total hours worked. The only exception is the older male cohort for which the effect is significant and positive. However, we do find a significant effect on hours worked in agricultural work. A one standard deviation increase in conflict in the past 12 months, which is 0.6 (Table 3.2) increases yearly time allocated to agricultural work by $56.329 \times 0.6 = 33.8$ hours which is roughly 14.4% of the mean. This impact is significant (in comparison to the sample mean) for both boys and girls belonging to the younger cohort.

tion of a child's time allocation is on arduous agricultural work, collecting water or firewood. In this case, parents might have a greater incentive to seeking curative health care of children to improve their productivity in physically strenuous activities.

Table 3.4 presents estimates for equation 3.1 where the outcome variable is a dummy that takes value 1 if the survey respondent reported that any health care was sought for the child in past 30 days. As seen in Column 1 of Panel A, conflict intensity is negatively associated with health care. A one standard deviation increase in conflict in the past 30 days, decreases the likelihood of taking a child of age between 0-16 to a health facility by $-0.052 \times 0.1 = 0.0052$ points which is 6.11 % of the sample mean. This drop is significant and negative for children of age cohort 5 to 16 by $-0.034 \times 0.1 = 0.0034$ points which is roughly 6.3% of the sample mean. The drop in the probability of seeking curative care is driven by the negative impact of recent conflict on health-seeking for female children as observed in Panel B. A standard deviation increase in conflict intensity reduces the probability of going to a health facility for girls $-0.080 \times 0.1 = 0.0080$ points which is roughly 16% of the sample mean. Girls of age group 5 to 11 are also significantly impacted by recent violence. We do not find evidence for any significant effect of conflict on curative health care seeking of boys.

The NLSS survey records the answer to whether a child was taken to a health facility if only she was reported to be ill in the past 30 days. Following Molina (2016), we assigned a zero to those children who were not reported to be sick while creating the outcome variable in Table 3.4. This is consistent with this variable being an indicator of curative care-seeking and not health-care utilization after being sick. To check if conflict affects sick children differently, we run the estimation only for sick children. The results of this estimation are presented in Table 3.5. Before we describe the results in detail, we limit this analysis to sick children belonging to ages 16 or below. Conducting sub-sample analysis of different age cohorts reduces our sample size by a lot resulting in issues with the power of the test.

As seen in Column 1 of Table 3.5, an increase in conflict in the past 30 days negatively impacts curative health care seeking but we fail to find any statistical significance. However, when we analyze female and male samples separately, we find a negative association of conflict with curative health care seeking for sick girls. A standard deviation increase in conflict intensity decreases the probability of being taken to health facility for sick girls by $-0.863 \times 0.1 = 0.0863$ or 8.63 percentage points. This decrease in the probability is roughly 13.4% of the sample mean. The result for sick boys is, however, the complete opposite. As conflict increases by one standard deviation, curative health care seeking for male children increases by 0.0493 or 4.93 percentage points, which is 7% of the sample mean. Parents prefer to take sick boys to health facilities during times of conflict because these periods are also marked by an increase in their labor supply. The potential connection between a child's productivity and sound health might be a motivating factor for this increase in the likelihood of health care utilization for sick male children.

Does conflict make children sick?

However, the effect of conflict can be two-fold: 1) as discussed above, it can prevent parents' from seeking health-care for their children, which in turn might deteriorate their health, 2) conflict can itself make children sicker. Therefore, to understand the effect of violence on health-seeking behavior, it is crucial to know if conflict itself is making children sicker. NLSS survey asks respondents questions on the exact date of when they last fell sick and for each village, we have information on the number of conflict-related events for each day from 1996-2006. Using this information we create an artificial unbalanced village-level panel. To do this, for each reported date of sickness, we sum up all respondents of the same village who fell sick on that particular day. We then estimate the following panel regression:

$$S_{jt} = \beta_1 + \beta_2 Intensity_{jt} + \delta_j + \alpha_t + \epsilon_{jt} \tag{3.2}$$

where S_{jt} is the number of children (age 0-16) in a village j who reported being sick on date t. $Intensity_{jt}$ is is a measure of conflict intensity calculated by adding the total number of conflict-related events in the past 30 days up to the date of reported illness weighted by the population of the village times 1000. We also include village and date of illness fixed effects, δ_j and α_t, respectively and cluster the standard errors are at the village level.

Table 3.6 shows the results for the panel estimation as seen in equation 3.2. Conflict intensity in the past 30 days of the date of reported illness has no significant effect on the number of children reported being sick. The statistical insignificance of the coefficient estimates remains when we run the specification for boys and girls separately. Therefore, conflict does not make children sick, rather the channel it operates by is affecting healthcare-seeking behavior.

3.6.3 Education

An increase in hours worked on other activities immediately after a period of violent conflict raises the question of whether this leads to a decrease in the time allocated to schooling. Unfortunately, the NLSS does not have any information on hours spent in schooling but only on final educational outcomes observed at the time of the survey. *As a result of which, we summed up the total number of violent events a child was exposed to since her birth (if born after 1996) or after the start of the civil war in 1996 (for those born before 1996) up to the date of* *the survey to calculate the number of violent events per thousand population (conflict intensity, $Intensity_{ivdmy}$).* The summary statistics for this measure of conflict intensity can be found in the third row of Table 3.2. On average, children were exposed to 0.6 violent events per thousand population with a standard deviation of 2.3. The maximum exposure is at 41.8 events per 1000 population.

As mentioned earlier, the minimum age for primary schooling in 2003 was 6 years old. Therefore, the only cohort born after the civil war that we can include in our analysis is those who were born in the year 1997. Since, children aged 7-16 at the time of the survey were born in 1996 or before, there is very little variation in conflict intensity across birth year cohorts within the same village. Therefore, we follow our baseline specification of equation 3.1 to include only year and month of the survey, and stratum varying district fixed effects.

Enrollment

Violent events due to armed conflict would have affected parents' decisions to enroll their children in primary schools. The age group whose enrollment was affected by the war includes children who were of 6 years or below in 1996 (13 years and below in 2003) or those who were born after 1996. We do not observe these children after 2003 and hence do not know their long term school enrollment outcome. However, we can assess whether exposure to conflict impacted their likelihood of ever being enrolled in primary school. This effect on enrollment can be considered as the short-run effects of conflict. Because the official age to start primary school was 6 years in Nepal, we study the effect of conflict in the probability of ever being enrolled during the wartime of students aged 6 to 13 in 2003. Column 1 of Table 3.7 presents results for this estimation. The outcome variable takes value 1 if a child was ever enrolled in school. Though the sign of the coefficient on Panel A of Column 1 is negative, it is very small in magnitude and we fail to find any significant effect on enrollment.

Currently Attending

The enrollment variable above does not tell us whether the child was out of school at the time of the survey or had to drop out. A child might have been enrolled in school at some point in time but might not be currently attending due to a violent situation. Columns 2-5 of Table 3.7 present estimates for an outcome variable “Currently Attending" which takes value 1 if a child is reported to be attending school at the time of the survey. Panel A of column 1 shows that exposure to the conflict has a negative and significant effect on the probability of currently attending school for children who are of ages 6 to 16. However, the magnitude of this effect on education is smaller compared to the effects on labor hours. A one standard deviation increase in conflict intensity, which is 2.3 (Table 3.2), leads to a decrease in the likelihood of currently attending school by $0.005 \times 2.3 = 0.0115$ or 1.15 percentage points - approximately 1.45 percent of the sample mean. For a highly exposed child (conflict intensity of 41.8), this drop is nearly 27% of the sample mean. This result is largely driven by the older cohort of ages 12 to 16, as seen in Column 4 of Panel A.

Columns 2-4 of Panel C present results for this estimation for boys. As observed in Column 2, the drop in the likelihood of currently attending school due to being exposed to conflict is negative and highly significant for boys. A standard deviation increase in conflict decreases this probability by $0.006 \times 2.3 = 0.0138$ or 1.38 percentage points, which is nearly 1.67% of the sample mean. We observe a significant drop in the likelihood of attending school for both younger (age 6 to 11) and older (age 12 to 16) boys. This result is in line with the effect of conflict on child time allocation and labor hours worked (Section 3.6.1) where results were largely driven by an increase in hours worked by male children.

Over-age

Conflict increases the likelihood of delay enrollment in primary school, absenteeism in school, missed exams, and closure of schools leading to grade repetition. Even though we do not have direct measures for these variables, we can study whether conflict affects the probability of students being in the appropriate grade for their age. We limit our sample to students who are of 6-16 years of age in 2003 and currently attending school. Columns 5 to 7 of Table 3.7 report the results for equation 3.1 where the outcome is an indicator for a child currently being below the grade she is supposed to be for her age - that is, if her age is greater than the nationally determined age for that grade. As seen in Panel B, for the entire sample of children, an increase in conflict intensity increases the probability of being overage for older (age 12 to 16) children.

However, as seen in, Columns 5-7 of Panel C, when we run the specification for boys the results are positive in sign and highly significant. A standard deviation increase in conflict intensity increases the likelihood of being over-age for boys (Column 5) by $0.007 \times 2.3 = 0.0161$ or 1.61 percentage points, which is nearly 2.25% of the sample mean. Though we observe this increase for both younger and older cohorts, the magnitude of the effect is larger for the former group. For younger boys (Column 6), a standard deviation increase in conflict increases the probability of being over-age by $0.010 \times 2.3 = 0.023$ which is roughly 4% of the sample mean for this cohort.

Surprisingly, for girls of ages 6 to 16 (Column 5 of Panel B), an increase in conflict intensity reduces their probability of being over-age and this effect is driven by the younger girls.

Years of education

Next, we study whether conflict affected the number of years of education of children. We study children below the age of 16 at the time of the survey and hence, can only analyze the short-run effects of conflict on educational achievement. Table 3.8 reports estimates for years of education. As observed in Panel A, a one standard deviation increase in conflict intensity decreases years of education of exposed children by $0.031 \times 2.3 = 0.0713$ years or approximately by a month. This estimate is only 0.6% of the sample mean. This result is largely driven by the drop in the years of education of boys, as observed in Panel C. A standard deviation increase in conflict intensity reduces years of education of boys by $0.055 \times 2.3 = 0.1265$, i.e., roughly by almost 1.5 months.

3.6.4 Potential Threat to Identification

Exogeneity of Violent Events

Numerous studies put conflict on the left-hand side of an equation that consists of demographic characteristics, resources, geographic and political conditions on the right-hand side in an attempt to provide explanations for when and why conflict arises in a particular setting, between two or more groups. This literature has explored and found various determinants of conflict such as, the presence of natural resources and 'lootable wealth' (Weinstein, 2006; Ross, 2004, 2006; Adhvaryu et al., 2018), international aid (De Ree and Nillesen, 2009; Nunn and Qian, 2014; Crost et al., 2014), arbitrary national boundaries (Michalopoulos and Papaioannou, 2013), inequality and ethnic cleavages (Cederman et al., 2013, Montalvo and Reynal-Querol, 2005, Esteban et al., 2012), the opportunity cost of conflict (Miguel et al., 2011; Dube and Vargas, 2013), the lack of political accountability and democracy (Skrede Gleditsch and Ruggeri, 2010) and other types of exploitative institutions (Richards, 1996; Wood, 2003).

While conflict, in and of itself, may never be fully exogenous, we test for valid concerns that may prove as a threat to our identification strategy.

As a first pass, we run the following specification to test for consistent month, year, district effects:

$$Intensity_{jt} = \beta_0 + \beta_1 \mathbb{1}(month_t) + \beta_2 \mathbb{1}(year) + \beta_1 \mathbb{1}(district_j) + \varepsilon_{jt} \tag{3.3}$$

where we test for the presence of consistent month, year, and district-specific effects. We then plot the residuals from this regression along with the number of victims due to conflict across time in Figure 3.6 and we see that these residuals almost perfectly align with the number of victims from conflict. This shows that unobservable time and district level characteristics are not affecting our results.

3.7 Mechanism

One of the potential mechanisms driving our contemporaneous outcomes is that an increase in violent events changes parents' perception of risk, hence, altering their decisions related to children. This causal channel aligns with the theoretical model provided by Estevan and Baland (2007). Their model shows that when there is an increase in child mortality risk and parents are not very altruistic, child labor increases whereas schooling decreases. An increase in mortality risk hinders parents from truly internalizing the impact of child labor on their children's welfare. Therefore, they prefer immediate transfers from their children in the form of child labor rather than risky investments like education. The need for immediate transfer can also explain why violent events negatively impact boys than girls. Larger portion of a child's time allocation is on a physically strenuous and arduous task like agricultural work where the returns might be higher from boys. On health-seeking behavior, our findings echo the results from Molina (2016). She finds that local violence in the Philippines reduced curative care utilization for children by their parents due to avoidance behavior. Violence increases the risk of being victimized which then translates to higher non-monetary costs of seeking health care.

Valente (2013) reports finding positive effects of Nepal's civil war on the education of girls with no significant impact on the educational achievements of boys. She argues that the positive effect on female educational attainment might be due to change in societal attitude toward female schooling and the Maoist policy[13] of coercing parents to send their daughters to school. Though our results do not find a significant impact of conflict on the educational achievements of girls, we do find that girls are more likely to be in the grade appropriate to their age. Maoist's motto of a more equitable society might be one of the potential channels driving our results on education.

3.8 Conclusion

This paper intends to document the effects of violent events on short-run economic decisions and outcomes. We focus on aspects of human capital accumulation, such as education, and health, and child's time use. Using micro-data from a unique database of violent events we find that during the periods of heightened violence, parents are more likely to involve their children, especially boys, in work-related activities. Conflict-related events also hamper education of boys. However, increase in violence reduces curative-care seeking for girls rather than boys. The potential connection between a boy's productivity at arduous tasks like agricultural work and sound health might be a

[13] Although there are several anecdotal evidence of Maoist's policy disrupting schooling.

motivating factor in play here.

So far we have not been able to perfectly disentangle whether the forces of demand or supply are at work, but this is something we are interested in doing for future work. One could think of violence as an imposition of a tax to everyday economic activity and this tax could distort both the supply and demand side. Alternatively, in the long run, one could think of violence affecting preferences as well. To disentangle whether violence affects prices or preferences, we may need a structural model that poses some testable implications of the effects of an increase in risk.

Finally, whether these short-run effects have long-run consequences is a valid question. This is the second, and more interesting, question that we wish to answer in future work. Are individuals who have invested less in education due to the risk of violence worse off, and if so, do they have worse labor market outcomes in the future? Are children who have not been provided proper curative care in the early stages of life worse off on their later life outcomes? Some of these questions are answered in Pandey (2020) by analyzing the long-run implications of exposure to conflict in childhood. Others we leave for future research.

3.9 Figures

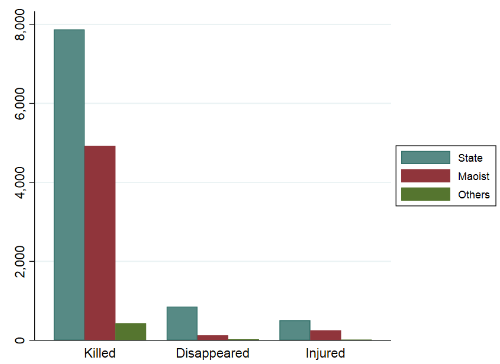
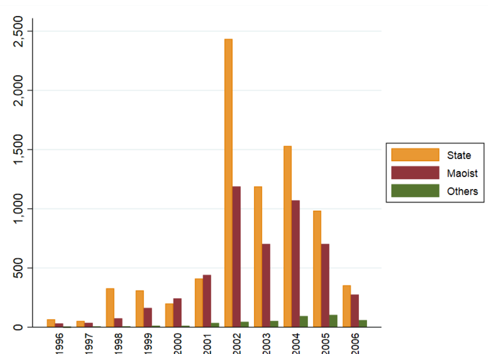
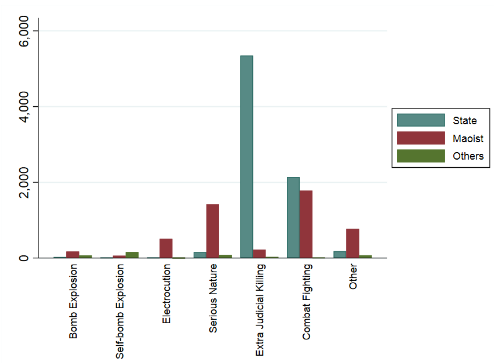


Figure 3.1: Outcome of incident by perpetrator

Notes: In the figure above, we plot the outcomes (killed, disappeared, and injured) of violent events by perpetrator of respective incidence.



(a) Number of Deaths by Perpetrator

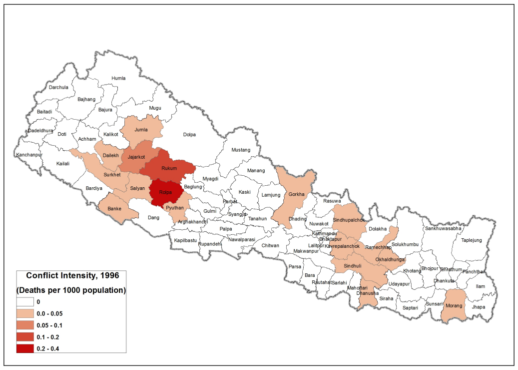


(b) Cause of Death

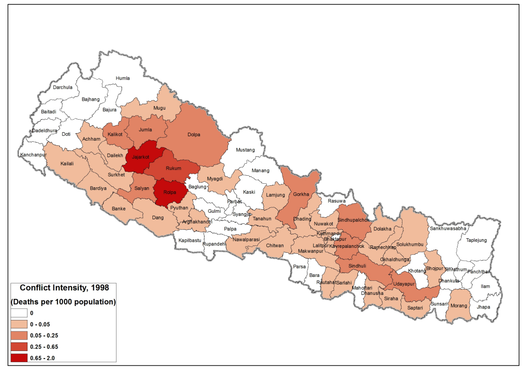
Figure 3.2: Number of Victims: by perpetrator and by cause of death

Notes: Figure (a) plots the total number of deaths each year by perpetrator. Figure (b) plots the cause of death by perpetrator. In this figure, serious nature deaths were caused by heinous killings that involved prolonged torture of the victim by the perpetrator.

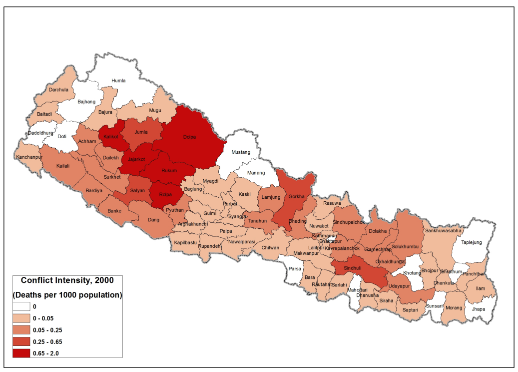
Figure 3.3: Conflict Intensity: 1996–2006



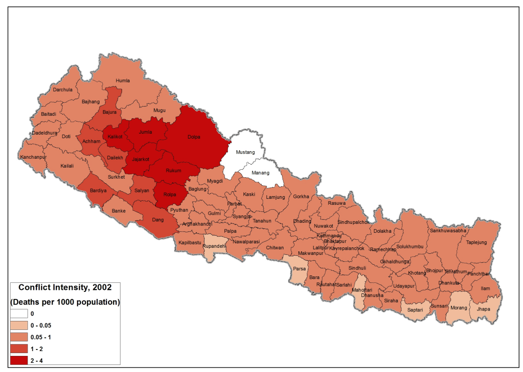
(a) Conflict Intensity (cumulated) by districts in 1996



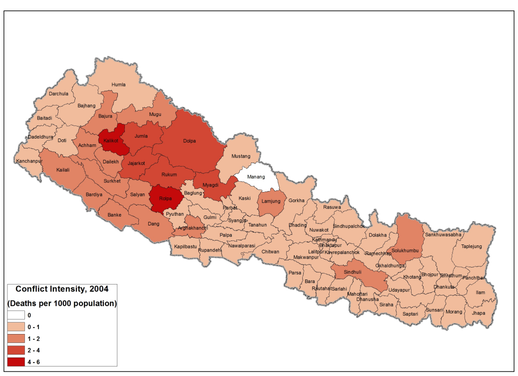
(b) Conflict Intensity (cumulated) by districts from 1996-98



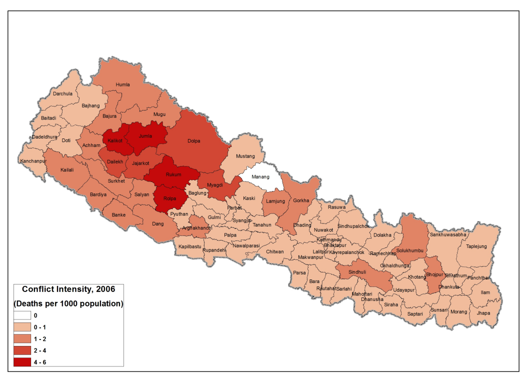
(c) Conflict Intensity (cumulated) by districts from 1996-00



(d) Conflict Intensity (cumulated) by districts from 1996-02



(e) Conflict Intensity (cumulated) by districts from 1996-04



(f) Conflict Intensity (cumulated) by districts from 1996-06

Notes: The maps above show the spread of Nepalese Civil War (in terms of conflict intensity) from 1996-2006. Conflict intensity is measured as the number of cumulated conflict related victims from 1996 to year on the figure per thousand population within a district. Although this district level breakdown of conflict intensity provides information on geographical spread of conflict across time, my analysis actually uses VDCs and municipalities (second lowest administrative unit) level analysis.

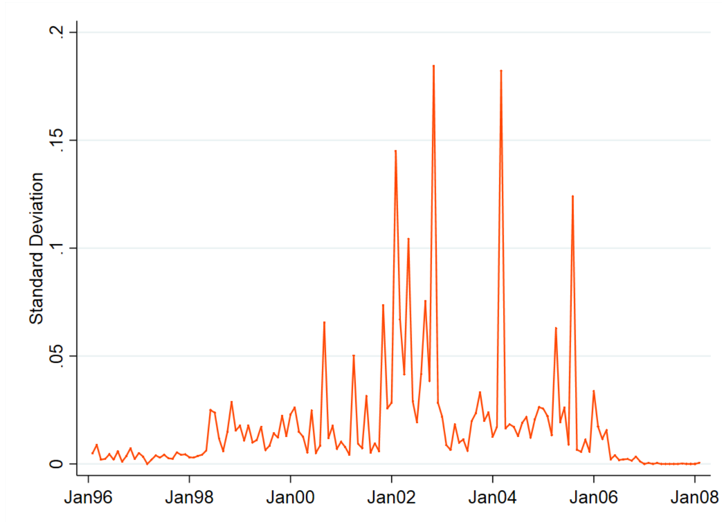


Figure 3.4: Spatial Variation in Conflict Intensity (across time)

Notes: In the graph above, for each time period, we provide the standard deviation in conflict exposure across districts. Conflict exposure (intensity) is measured as casualties/1000 population.

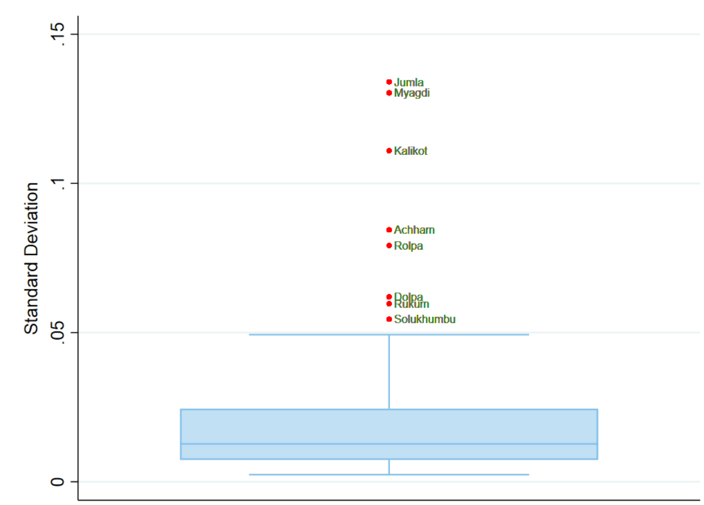


Figure 3.5: Temporal Variation in Conflict Intensity (across districts)

Notes: In the box plot above, we provide the standard deviation in conflict exposure across different time period. Conflict exposure (intensity) is measured as casualties/1000 population.

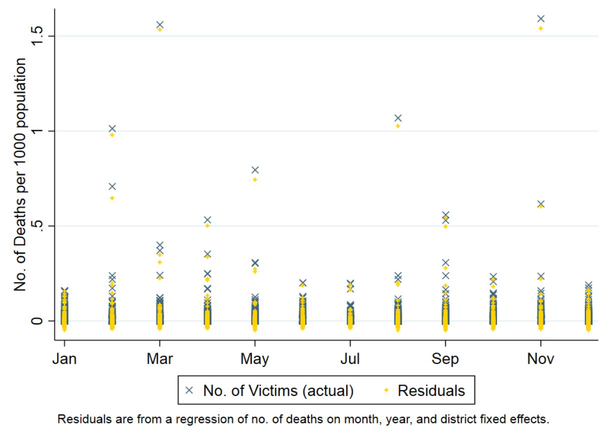


Figure 3.6: Test for Exogeneity of Conflict

Notes: In the figure above, we plot the residuals of the regression of conflict intensity on month, year, and district fixed effects.

3.10 Tables

Table 3.1: Summary Statistics

	Mean	SD	N
Panel A: Time Allocation Outcomes (Age 5-16)			
Total hrs worked in past 7 days in:			
all activities	10.8	17.6	7791
agriculture	6.3	13.2	7791
domestic work	3.6	8.5	7791
Total hrs worked in past 12 months in:			
all activities	426.3	724.0	7791
agriculture	234.7	500.1	7791
domestic work	159.4	379.9	7791
Panel B: Education Outcomes (Age 6-16)			
$\mathbb{1}$(Ever enrolled)	0.8	0.4	7106
$\mathbb{1}$(Currently attending school)	0.8	0.4	7104
$\mathbb{1}$(Over-age for the grade)	0.7	0.5	5479
Education (years)	5.2	3.4	7106
Time taken to primary school (minutes)	17.0	23.7	7106
Panel C: Health Care Outcomes (Age 0-16)			
$\mathbb{1}$(Any curative care)	0.08	0.3	10914
Time taken to health facility (minutes)	46.9	67.8	10914
Panel D: Other Variables (Age 5-16)			
Female	0.5	0.5	7791
Child's age	10.4	3.4	7791
Rural location	0.8	0.4	7791
HH head's education (years)	3.9	5.0	7791
Mother's education (years)	1.8	3.8	7790
Household's size	7.0	3.1	7791
Wealth	12.8	1.3	7791

Notes: The table above provides mean and standard deviation for NLSS (2003) data. Panel A reports the summary statistics for time allocation outcomes. Total hours is a continuous variable and it is the total sum of time spent in different activities outside schooling in the past 7 days. Panel B reports the summary statistics for educational outcomes. Ever enrolled is an indicator for whether the child has been ever enrolled in school upto the time of the survey. Panel C report the summary statistics for health-seeking behavior where Any curative care is an indicator for if the child was taken to a health service facility in the past 30 days. Wealth in Panel D is the log of total assets owned by the household.

Table 3.2: Summary Statistics - Conflict

	Mean	SD	Min	Max
Total no. of deaths/1000 population:				
in past 30 days	0.02	0.10	0	1.3
in past 12 months	0.2	0.6	0	7.9
from birth to survey date	0.6	2.3	0	41.8

Notes: The table above provides mean and standard deviation for conflict intensity. Conflict exposure denotes no. casualties/1000 population in a village/municipality in past 30 days, past 12 months, and from birth of a child to the date of the survey, respectively.

Table 3.3: Effect of exposure to conflict in past 30 days on child's time allocation and labor

	Total hours worked			Agricultural work			Domestic work		
	5 to 16 [1]	5 to 11 [2]	12 to 16 [3]	5 to 16 [4]	5 to 11 [5]	12 to 16 [6]	5 to 16 [7]	5 to 11 [8]	12 to 16 [9]
Panel A: All Sample									
Conflict	4.205**	3.939***	3.849	5.340***	4.334***	7.683**	-0.337	-0.125	-1.498
	(1.974)	(1.508)	(3.877)	(1.692)	(0.993)	(3.685)	(0.658)	(0.948)	(1.407)
Obs.	7790	4606	3184	7790	4606	3184	7790	4606	3184
Mean Outcome.	10.780	5.300	18.709	6.345	2.941	11.271	3.615	2.231	5.618
Panel B: Female Only									
Conflict	2.504	4.645*	-2.330	2.616	4.412***	-1.218	0.856	0.653	0.752
	(2.023)	(2.379)	(5.110)	(1.752)	(1.593)	(4.873)	(1.042)	(1.218)	(2.464)
Obs.	3822	2270	1552	3822	2270	1552	3822	2270	1552
Mean Outcome.	13.545	6.904	23.264	6.795	3.060	12.261	6.093	3.670	9.639
Panel C: Male Only									
Conflict	7.705***	4.261**	11.460**	9.711***	4.542**	17.623***	-0.849	-0.192	-2.472
	(2.860)	(2.037)	(5.586)	(2.719)	(2.231)	(5.707)	(0.775)	(0.762)	(1.804)
Obs.	3968	2336	1632	3968	2336	1632	3968	2336	1632
Mean Outcome.	8.115	6.904	14.377	5.912	2.825	10.329	1.228	0.832	1.794
Survey Year and Month, Ethnicity & District X Stratum Fixed Effects & Controls									

Notes: The table above reports the estimated coefficients (β_1) from specification 3.1. Conflict denotes casualties/1000 population in a village or a municipality in the past 30 days. Standard errors are clustered by VDC or municipality of birth. Each cell represents result from different regression. The results are estimated using OLS. Controls include: child's gender, and age, rural or urban region, education of the household health and mother, household size, wealth, and village level population. The outcome variables are total hours worked in specific activities in past 7 days. Domestic work also includes hours spent collecting water, and firewood. Panel A presents results for entire sample. Panel B & C show results for female and male sub-sample, respectively. Mean Outcome reports respective average value of the dependent variables in this sample. *** Significant at 1%, ** Significant at 5%, & * Significant at 10% level of significance.

Table 3.4: Effect of exposure to conflict on curative health care seeking

	Any curative health care				
	0 to 16	0 to 4	5 to 16	5 to 11	12 to 16
	[1]	[2]	[3]	[4]	[5]
Panel A: All Sample					
Conflict	-0.052**	-0.085	-0.034**	-0.047**	-0.014
	(0.022)	(0.063)	(0.013)	(0.021)	(0.031)
Obs.	10912	3120	7792	4607	3185
Mean Outcome.	0.085	0.161	0.054	0.061	0.045
Panel B: Female Only					
Conflict	-0.082*	-0.106	-0.080**	-0.072**	-0.103
	(0.042)	(0.104)	(0.034)	(0.036)	(0.067)
Obs.	5377	1554	3823	2271	1552
Mean Outcome.	0.076	0.140	0.049	0.055	0.042
Panel C: Male Only					
Conflict	-0.023	-0.090	0.018	-0.014	0.048
	(0.032)	(0.077)	(0.037)	(0.062)	(0.035)
Obs.	5535	1566	3969	2336	1633
Mean Outcome.	0.094	0.182	0.059	0.067	0.048
Survey Year and Month, Ethnicity & District X Stratum Fixed Effects & Controls					

Notes: The table above reports the estimated coefficients (β_1) from specification 3.1. Conflict denotes casualties/1000 population in a village or a municipality in the past 30 days. Standard errors are clustered by VDC or municipality of birth. Each cell represents result from different regression. The results are estimated using OLS. Controls include: child's gender, and age, rural or urban region, education of the household health and mother, household size, wealth, time taken to health service center and village level population. The outcome of interest is an indicator that takes value 1 if any curative care was sought for the child. Panel A presents results for entire sample. Panel B & C show results for female and male sub-sample, respectively. *** Significant at 1%, ** Significant at 5%, & * Significant at 10% level of significance.

Table 3.5: Effect of exposure to conflict on health care utilization

	Any curative health care (Sick only), Age 0-16		
	All Sample [1]	Female [2]	Male [3]
Conflict	-0.008	-0.863**	0.493***
	(0.181)	(0.396)	(0.177)
Obs.	1372	632	740
Mean Outcome.	0.676	0.644	0.703
Disease, Survey Year and Month, Ethnicity & District X Stratum Fixed Effects & Controls			

Notes: The table above reports the estimated coefficients (β_1) from specification 3.1. Conflict denotes casualties/1000 population in a village or a municipality in the past 30 days. Standard errors are clustered by VDC or municipality of birth. Each cell represents result from different regression. The results are estimated using OLS. Controls include: child's gender, and age, rural or urban region, education of the household health and mother, household size, wealth, time taken to health service center and village level population. The outcome of interest is an indicator that takes value 1 if any curative care was sought for the child. *** Significant at 1%, ** Significant at 5%, & * Significant at 10% level of significance.

Table 3.6: Effect of exposure to conflict on number of children reported sick

	Number of sick children aged 0-16		
	All Sample [1]	Female [2]	Male [3]
Conflict	0.024	0.014	0.029
	(0.022)	(0.026)	(0.032)
Obs.	4740	2422	2536
Mean Outcome.	1.101	1.052	1.053
Date and Location Fixed Effects			

Notes: The table above reports the estimated coefficients (β_1) from specification 3.2. Conflict denotes casualties/1000 population in a village or a municipality in the past 30 days of reported date of sickness. Standard errors are clustered by VDC or municipality of birth. Each cell represents result from different regression. The results are estimated using OLS. The outcome of interest is total number of sick children reported in a particular date in a village or municipality. *** Significant at 1%, ** Significant at 5%, & * Significant at 10% level of significance.

Table 3.7: Effect of exposure to conflict on educational status

	Ever Enrolled	Currently Attending			Over Age		
	6 to 13	6 to 16	6 to 11	12 to 16	6 to 16	6 to 11	12 to 16
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
Panel A: All Sample							
Conflict	-0.001	-0.005**	-0.003	-0.007***	-0.000	-0.001	0.003*
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
Obs.	5264	7103	3929	3174	5478	3205	2273
Mean Outcome.	0.841	0.776	0.818	0.725	0.707	0.607	0.846
Panel B: Female Only							
Conflict	0.000	-0.002	0.001	-0.023	-0.011***	-0.009*	0.000
	(0.003)	(0.003)	(0.003)	(0.016)	(0.004)	(0.005)	(0.017)
Obs.	2549	3467	1920	1547	2434	1458	976
Mean Outcome.	0.785	0.708	0.763	0.641	0.697	0.599	0.842
Panel C: Male Only							
Conflict	-0.002	-0.006***	-0.004*	-0.008***	0.007***	0.010**	0.006***
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.005)	(0.002)
Obs.	2715	3636	2009	1627	3044	1747	1297
Mean Outcome.	0.893	0.841	0.871	0.805	0.715	0.614	0.850
Survey Year and Month, Ethnicity & District X Stratum Fixed Effects & Controls							

Notes: The table above reports the estimated coefficients (β_1) from specification 3.1. Conflict denotes total number of deaths per 1000 population in the village or municipality of residence that the child is ever exposed to until the date of survey. Standard errors are clustered by VDC or municipality of birth. Each cell represents result from different regression. The results are estimated using OLS. Controls include: child's gender, and age, rural or urban region, education of the household health and mother, household size, wealth, time taken to primary school and village level population. Currently attending is an indicator of whether the child is attending school during the time of the survey. Over age take value 1 if the child's age is greater than the recommended age for the class s/he is currently attending. Panel A presents results for entire sample. Panel B & C show results for female and male sub-sample, respectively. *** Significant at 1%, ** Significant at 5%, & * Significant at 10% level of significance.

Table 3.8: Effect of exposure to conflict on years of education

	Years of Education		
	6 to 16	6 to 11	12 to 16
	[1]	[2]	[3]
Panel A: All Sample			
Conflict	-0.031***	-0.013	-0.064***
	(0.012)	(0.009)	(0.022)
Obs.	7104	3930	3174
Mean Outcome.	5.198	3.787	6.946
Panel B: Female Only			
Conflict	-0.004	0.019	-0.149
	(0.018)	(0.015)	(0.139)
Obs.	3468	1921	1547
Mean Outcome.	4.739	3.506	6.271
Panel C: Male Only			
Conflict	-0.055***	-0.047***	-0.073***
	(0.013)	(0.010)	(0.025)
Obs.	3636	2009	1627
Mean Outcome.	5.635	4.055	7.586
Survey Year and Month, Ethnicity & District X Stratum Fixed Effects & Controls			

Notes: The table above reports the estimated coefficients (β_1) from specification 3.1. Conflict denotes total number of deaths per 1000 population in the village or municipality of residence that the child is ever exposed to until the date of survey. Standard errors are clustered by VDC or municipality of birth. Each cell represents result from different regression. The results are estimated using OLS. Controls include: child's gender, and age, rural or urban region, education of the household health and mother, household size, wealth, time taken to primary school and village level population. The outcome of interest is total years of education. Panel A presents results for entire sample. Panel B & C show results for female and male sub-sample, respectively. *** Significant at 1%, ** Significant at 5%, & * Significant at 10% level of significance.

APPENDIX

3.A Timeline of Nepalese Civil War

1959	New constitution establishes parliamentary democracy (a "partyless" Panchayat system)
1990	"People's Movement" (*Jana Andolan*) ended 28 years of monarchical rule; established *panchayat* system of self-government
February, 1996	Formal announcement of the "Peoples War" by the Communist Party of Nepal (Maoist)
June, 2001	Ten royal family members are massacred in their palace, allegedly by Prince Dipendra
Aug–Nov, 2001	First round of peace talks begin (3 rounds held until November, 2001)
23rd November, 2001	Peace talks collapse
26th November, 2001	State of Emergency is declared and Nepal Army is sent in to attack the Maoists
January, 2003	A second ceasefire is established and a second set of peace talks begin
Apr–Aug, 2003	Three rounds of peace talks held
August, 2003	Maoists withdraw from the ceasefire
September, 2005	Maoists declare a three-month unilateral ceasefire to woo opposition political parties
January, 2006	Maoists decide not to extend the four-month ceasefire stating that the government had broken the ceasefire with numerous attacks on Maoist villages
May, 2006	Nepal's new cabinet declares a ceasefire. The cabinet also announces that the Maoist rebels will no longer be considered a terrorist group. Rebels are also encouraged to open peace talks.
November, 2006	Peace talks end with the signing of the Comprehensive Peace Accord between Prime Minister Koirala and Maoist leader Prachanda. The deal allows the Maoists to take part in government, and places their weapons under UN monitoring.

Notes: As can be seen from the table above, the Maoists repeatedly withdrew ceasefire following multiple rounds of peace talks and these were events that were typically followed by mass strikes of violence, unanticipated acts of violence, destruction of property etc., all as a propaganda for the Maoist cause.

3.B Figures

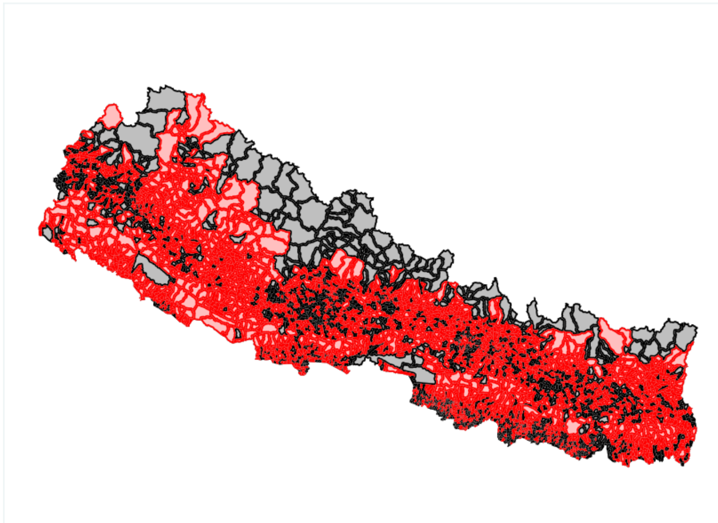


Figure 3.B.1: Village Development Committees (VDCs) & Municipalities of Nepal (in red) that experienced some conflict-related events from 1996-2006.

Notes: Out of 3,915 Village Development Committees (VDCs), the conflict data records some violent events for 2,427 villages.

3.C Tables

Table 3.C.1: Effect of exposure to conflict in past 12 months on child's yearly time allocation and labor

	Total hours worked			Agricultural work			Domestic work		
	5 to 16 [1]	5 to 11 [2]	12 to 16 [3]	5 to 16 [4]	5 to 11 [5]	12 to 16 [6]	5 to 16 [7]	5 to 11 [8]	12 to 16 [9]
Panel A: All Sample									
Conflict	59.091	54.335	74.752	56.329*	51.193*	81.949*	-5.389	-0.354	-15.774
	(43.670)	(44.285)	(58.830)	(29.613)	(26.587)	(46.997)	(16.633)	(26.913)	(23.330)
Obs.	7790	4606	3184	7790	4606	3184	7790	4606	3184
Mean Outcome.	426.252	204.343	747.336	234.722	105.778	421.294	159.367	93.850	254.164
Panel B: Female Only									
Conflict	63.902	73.892	38.969	58.160*	59.576*	62.590	0.050	11.592	-34.762
	(56.276)	(55.034)	(90.138)	(34.954)	(34.922)	(71.653)	(25.134)	(34.567)	(45.470)
Obs.	3822	2270	1552	3822	2270	1552	3822	2270	1552
Mean Outcome.	552.264	273.491	960.186	255.840	112.428	465.692	269.317	155.266	436.204
Panel C: Male Only									
Conflict	43.300	16.664	136.105**	48.112	36.645*	127.574*	-8.560	-23.145	14.016
	(35.789)	(35.510)	(66.493)	(31.097)	(21.158)	(65.914)	(15.480)	(21.766)	(27.710)
Obs.	3968	2336	1632	3968	2336	1632	3968	2336	1632
Mean Outcome.	304.844	137.119	544.920	214.375	99.312	379.072	53.435	34.144	81.048
Survey Year and Month, Ethnicity & District X Stratum Fixed Effects & Controls									

Notes: The table above reports the estimated coefficients (β_1) from specification 3.1. Conflict denotes casualties/1000 population in a village or a municipality in the past 30 days. Standard errors are clustered by VDC or municipality of birth. Each cell represents result from different regression. The results are estimated using OLS. Controls include: child's gender, and age, rural or urban region, education of the household health and mother, household size, wealth, and village level population. The outcome variables are total no. of days worked in specific activities in past 12 months. Domestic work includes days spent collecting water, and firewood. Panel A presents results for entire sample. Panel B & C show results for female and male sub-sample, respectively. Mean Outcome reports respective average value of the dependent variables in this sample. *** Significant at 1%, ** Significant at 5%, & * Significant at 10% level of significance.

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