

# **Cash and Conflict:**

## **Evidence from the Indian Banknote Demonetization**

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### **Abstract:**

This paper studies the causal effect of India's 2016 ban on high-denomination bills ('demonetization') on conflict levels, based on data from the ACLED conflict database. It examines how the severity of the demonetization shock in a district affects post-demonetization conflict events, and tests for spillover effects of conflict into districts less exposed to demonetization. Estimates using a 2SLS model show that districts more exposed to demonetization experience slightly fewer conflicts in the short-term, however this effect dissipates within six months of the policy announcement. This short-term reduction is largely driven by small-scale conflicts. One potential explanation of these results is that while demonetization does provide an immediate shock/disruption to the financing capabilities of terrorist/mafia groups, these groups are more sophisticated than initially assumed and quickly find ways of bypassing these new cash constraints.

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# 1 Introduction

In recent times, there have been increasing calls among some economists and policymakers to transition towards a cashless society. For instance, in his book ‘The Curse of Cash’, Kenneth Rogoff presents numerous arguments as to why paper money is a major cause of many pressing economic issues today (Rogoff, 2017). Specifically, he argues that large-denomination bills result in significant tax evasion, constrain the potential effectiveness of monetary policy (in particular at the zero lower bound), and aid in the facilitation and execution of various forms of crime and conflict (Rogoff, 2017).

This paper focuses on the effect of high-denomination bill availability on conflict instigated by terrorist and mafia groups. An empirically rigorous answer to this question has proven elusive thus far, for three related reasons. First, causally identifying the effect of cash on conflict using cross-country data is challenging due to the fact that there are many external factors influencing both cash availability and conflict that are unobservable and/or difficult to control for across countries. Second, policy announcements regarding adjustments in cash availability and legitimacy are often planned in advance and specifically target regions/groups that use cash to illicitly finance conflict, thus leading to anticipation effects and biased coefficients of interest. Third, until very recently, granular datasets documenting more minor conflicts within a particular country were not widely available.

In order to overcome these three challenges, this paper utilizes a rich, geocoded dataset from ACLED that includes even the smallest conflicts (kidnappings, stoning of politicians’ houses etc.), and exploits a unique policy implemented by the Indian government. On November 8<sup>th</sup> 2016, India’s Prime Minister, Narendra Modi, announced without any prior warning that all 500 and 1000 rupee notes would no longer be accepted as legal tender. Residents were given a 50-day period (until December 30<sup>th</sup>) to deposit old notes at a commercial bank branch and/or exchange them for new 500 and 2000 rupee notes. Since the

old notes made up approximately 86% of cash in circulation (Marthinsen, 2017), this policy delivered a considerable monetary shock to the Indian economy. The intended effects of the policy included incentivizing citizens to place their savings into official bank accounts, reducing tax evasion, and reducing the funding capabilities of terrorist and mafia groups, who often use cash as an untraceable means to finance their illicit activities (Marthinsen, 2017).

This paper exploits the exogenous, sudden nature of this policy announcement to explore the **causal effect of demonetization on the incidence of conflict in India**. Firstly, this paper aggregates the number of conflict events by district (the second smallest administrative unit in India) and month, and runs a panel regression to determine how the ‘severity’ of the demonetization shock in a district affects the number of conflicts three months, six months, and one year after the policy announcement. The demonetization shock severity is measured using Reserve Bank of India data on the number of commercial bank branches in a district pre-demonetization (based on Bhavnani and Copelovitch 2018). This regression also tests for spillover effects of conflict, by including a term capturing the average number of bank branches in a neighbouring district. Additionally, a district’s eligibility for a government bank branch expansion policy between 1979 and 1982 is used as an instrumental variable to account for issues of endogeneity in the panel regressions (based on Bhavnani and Copelovitch 2018, and Kochar 2011).

This paper finds that districts that experienced more severe demonetization shocks had a statistically significant decrease in conflict in the short-term (3 months after the policy was introduced). However, these effects were small in magnitude and dissipated 6 months after the policy announcement. Moreover, there appear to be minimal spillover effects of conflict into neighbouring districts following demonetization, and no heterogeneity in effects across different types of terrorist/mafia groups.

The remainder of this paper proceeds as follows. Section 2 provides a review of the existing literature, Section 3 provides a detailed description of the data and empirical strategy, and Section 4 presents the results. Section 5 concludes and discusses future directions for further analysis.

## 2 Literature Review

This paper adds to a nascent body of literature studying the impact of demonetization on various facets of the Indian economy. Bhavnani & Copelovitch (2018) analyze the effect of demonetization on district-level electoral and investment outcomes. Their main specification regresses investment activity and the % of votes received by the BJP (the ruling party who implemented demonetization) on an interaction term (number of bank branches\*post-demonetization dummy variable), controlling for time and district fixed effects. They also use a dummy for whether districts were subject to ‘branch licensing policies’ between 1979-1982 as an instrument for the number of bank branches. They find that sufficiently banked districts had investment activity drop 8% less than underbanked districts, and that the post-demonetization BJP vote share increased more in underbanked districts.

This paper leverages the Reserve Bank of India (RBI) bank branch dataset Bhavnani & Copelovitch (2018) use to proxy for the district-level demonetization shock, and follows their IV strategy, however the outcome of interest (conflict) is different. Additionally, this paper tests for spillover effects of demonetization shocks on conflict in neighboring districts, and for heterogeneity of effects across regions where certain types of terrorist/mafia groups are more prominent.

Chodorow-Reich et al. (2019) develop a model of how demonetization shocks affect short-run economic outcomes. Their dataset consists of confidential daily cash flow records from approximately 4000 RBI currency chests throughout India that send and receive notes

from commercial branches. The demonetization shock in a district is defined as the total amount of non-demonetized/new currency in the closest chest to that district, divided by the total value of currency pre-demonetization in this chest. They find that districts that experienced more severe demonetization shocks saw reductions in ATM withdrawals, employment and real economic activity, and more adoption of digital payment methods.

Building upon this work, Chanda & Cook (2019) examine the effects of demonetization on economic activity in the medium-term (1.5 years post-announcement). They use three main measures of the district-level demonetization shock (DS): the percent change in total outstanding bank deposits between Q3 and Q4 of 2016, rural population shares, and agricultural labor shares. They then run a standard panel regression of night light intensity on the interaction between DS and a ‘during demonetization’ dummy, and the interaction between DS and a ‘post-demonetization’ dummy. After controlling for various time and district-varying characteristics, and including fixed effects, they find that irrespective of the chosen DS measure, districts with higher growth in deposits experience a contraction in economic activity during the demonetization period, but a relative expansion post-demonetization.

This paper uses measures employed by Chanda and Cook (2019) and Chodorow-Reich et al. (2019) as robustness checks for the main DS measure (number of bank branches). However, while those papers only use panel regressions with fixed effects, this paper also incorporates an instrumental variables methodology and tests for potential spillover effects. Moreover, instead of studying the direct financial implications of demonetization, this paper studies effects on conflict levels.

This paper also relates to an established literature studying the effects of economic shocks on conflict. Bazzi & Blattman (2014) use panel data from all developing countries between 1957-2007 to study the effects of commodity price shocks on several categories of conflict. They argue that price shocks are exogenous to conflict after including country and

time fixed effects and omitting countries that significantly influence global commodity prices, and use panel regressions to find that although price shocks have no significant impact on the instigation of new conflicts, rising prices lead to shorter, less deadly wars. Dube & Vargas (2013) follow a very similar methodology, however restrict their attention to Colombian municipalities between 1988-2005, finding that sharp falls in coffee prices lead to more incidences of conflict in municipalities that cultivate more coffee.

This paper is broadly similar to Bazzi & Blattman (2014) as it explores the effect of a macroeconomic shock on conflict, and is similar to Dube & Vargas (2013) because it studies effects within a country. However, this paper does not rely on running cross-country regressions, instead using a rich within-country dataset combined with a robust IV strategy to recover causal estimates. Moreover, the unique nature of India's demonetization policy allows this paper to study how important illicit financing is as a mechanism for instigating conflict. In contrast, the current literature focuses more on reduced form estimates of the causal effects economic shocks on conflict.

In summary, the current literature on demonetization focuses exclusively on its financial, economic and political implications while the literature on economic shocks and conflict remains broad and inconclusive. Hence, this paper adds meaningfully to the existing literature by using new empirical approaches (namely, instrumental variables) to study a previously unexplored consequence of demonetization, and by providing causal estimates on how important the illicit financing channel is when studying the effects of economic shocks on conflict.

## **3 Data and Empirical Strategy**

### **3.1 Data and Variables**

The final district-level dataset used in this paper is a balanced panel of 389 districts in each month between January 2016 and November 2017. Because the ACLED conflict dataset is at the event-level, this balanced panel is constructed by assuming that if a district does not appear in the dataset during a given month, then this district had no conflicts during this month. Given the comprehensiveness of the ACLED dataset and the variety of news sources used in constructing it, this imputation of zero values is likely valid.

Additionally, the main district-level dataset used excludes the state of Jammu & Kashmir. This is because during the period of analysis, Jammu & Kashmir was an immensely volatile region with conflicts occurring due to a plethora of complex socio-political and religious factors. Additionally, much of the conflict in the region also involved disputes over territory between nations (namely, India and Pakistan), and conflict instigated by international, sophisticated terrorist organizations. In short, the region has complexities and unobservable factors that make the reasons for conflict instigation difficult to disentangle, and thus it would be difficult to ascertain an accurate causal effect of demonetization by keeping this region in the sample. For the sake of transparency, results including Jammu & Kashmir are shown in section 4.2.3.

#### **3.1.1 Outcome Variable: Number of Conflicts**

The outcome variable utilized in the district-level regression specifications is the number of conflicts in a particular district during a particular month. This variable is constructed from the ACLED conflict database, which documents geocoded event-level data on conflicts and crimes of all sizes (from kidnappings to large-scale terrorist operations) in India from January 2016 to September 2020. The data is filtered to exclude events instigated by the government, military

and police, as well as events categorized as non-violent protests, in order to only consider mafia/terrorist-initiated conflicts. A shapefile of districts in India (derived from the 2011 Indian census) is used to assign the event locations to districts and then the conflicts are aggregated by district-month to transform the data into panel format.

### **3.1.2 Explanatory Variables: Severity/Intensity of Demonetization Shock**

The primary measure of the demonetization shock severity in a district is the number of commercial bank branches in this district in 2015, obtained from the Reserve Bank of India (RBI) Basic Statistical Returns dataset. This variable is a strong proxy for the demonetization shock intensity in a district because the main constraints individuals faced when trying to exchange or deposit their old notes were the incredibly long lines at many bank branches, and the fact that certain branches would frequently face shortages of new currency. Consequently, in a district with more branches, it is more likely that mafia/terrorist groups can find some way to legitimately exchange their illicit cash into legal tender (for instance, through asking/threatening an intermediary to exchange the cash on their behalf, as was reported by Deka 2017). Conversely, in a district with very few bank branches, it becomes exceedingly difficult to exchange illicit cash and thus finance conflict. *Thus, it would be intuitively expected that a district with fewer bank branches would experience a greater decrease in conflict post-demonetization.*

As robustness checks, three other measures of demonetization shocks are also employed. The first two alternative measures: the rural population share in a district, and the share of agricultural workers in a district, are derived from the 2011 Indian Census. The final alternative measure is the % growth of deposits in the bank branches of a district between Q3 and Q4 2016 from the RBI Basic Statistical Returns (based on Chanda and Cook 2019).

### **3.1.3 Auxiliary Variable of Interest: Spillover Effect**

In this context, the spillover effect is the effect that the demonetization shock in one particular district has on conflict in its neighbouring districts. This effect is captured through a variable measuring the average number of commercial bank branches in one of the adjacent districts to a particular district. I construct this variable using the aforementioned RBI bank branch dataset and matching by district name onto a shapefile of Indian districts. Intuitively, this measure should capture the spillover effect, because if one of a district's neighbours has a significantly lower number of branches, then it is plausible that instigators of conflict will temporarily 'migrate' to that district in order to exchange their notes, which could increase conflict events in that district. In this case, we would expect the coefficient on this variable to be negative. On the other hand, it is also possible that conflict locations are pre-determined and thus instigators return to incite violence in their 'home' district despite exchanging cash elsewhere. In this case, the coefficient on this variable would be positive.

### **3.1.4 Instrumental Variable**

To improve causal identification, this paper uses an instrumental variables (IV) approach. Specifically, a dummy variable for whether a district was exposed to the 1979 and 1981 Indian Government Branch Licensing Policies (BLPs) serves as an instrument for the current number of bank branches in a district. This dummy variable is constructed by using Amazon Textract to extract population and bank branch data from 1981 Census documents. The dummy takes a value of 1 if the 1981 population-to-bank branch ratio is less than 17,000, and 0 otherwise. The historical context and validity of this IV is discussed in Section 3.3.

### **3.1.5 Control Variables**

The control variables for the district-level analysis include: the share of Hindus in a district, the share of Muslims in a district, the share of young men (ages 19-30) in a district, the shares of females in a district, the total population of a district, the proportion of illiterate residents in a district and the average luminosity at night in a district during a particular month. The religious variables are included as violence and conflict in India is often incited due to tensions between Hindus and Muslims, especially in the recent political environment. The share of young men is included because many media sources report this demographic as the most likely to be hired by mafia/terror groups to carry out their illicit activities (Jain, 2018).

### **3.1.6 Summary Statistics**

Table 1 below presents the number of observations, mean, standard deviation, median, min and max of the variables used for the analysis.

Table 1: **Summary Statistics**

	Observations	Median	Mean	SD	Min	Max
No. of Conflicts in District during one month	8976	0.00	0.32	0.91	0.00	16.00
Proportion of Males between 15-30, 2011	8976	0.16	0.16	0.02	0.11	0.34
Proportion of Hindus, 2011	8976	0.87	0.78	0.24	0.02	0.99
Proportion of Muslims, 2011	8976	0.07	0.10	0.11	0.00	0.79
Proportion of Pop. who are illiterate, 2011	8976	0.39	0.38	0.10	0.11	0.71
Proportion of Primary School Graduates, 2011	8976	0.15	0.15	0.02	0.09	0.22
Proportion of Secondary School Graduates, 2011	8976	0.08	0.09	0.04	0.01	0.19
Total Population (thousands), 2011	8976	1704.83	2104.86	1567.03	21.17	11060.15
Proportion of Females, 2011	8976	0.49	0.48	0.02	0.35	0.53
Rural Population Share, 2011	8976	0.79	0.74	0.19	0.00	0.97
Proportion of Workforce in Agriculture, 2011	8976	0.17	0.17	0.07	0.00	0.36
No. of Commercial Bank Branches, 2015	8976	1.56	2.40	3.43	0.01	35.97
Avg no. of Branches in Neighboring District	8952	1.78	2.32	1.79	0.43	19.17
Percent Change in Deposits between Q3 and Q4 2016	8976	-0.44	-0.30	6.09	-22.00	38.64
No. of Commercial Bank Branches, 1981	8928	66.00	89.24	95.49	2.00	860.00
Average Luminosity in District (nW/cm <sup>2</sup> /sr)	8976	0.75	1.62	5.15	-0.10	76.61

## 3.2 Empirical Models

### 3.2.1 Panel Regression Model

The causal equation to be estimated is:

$$Y_{it} = \alpha + \delta_1 TREAT_i + \delta_2 ADJ_i + \sum_{k \in K} \gamma_k \mathbb{1}\{t = k\} + \sum_{k \in K} \delta_k (\mathbb{1}\{t = k\} * ADJ)_{it} + \sum_{k \in K} \beta_k (\mathbb{1}\{t = k\} * TREAT)_{it} + \mathbf{X}_{it}^T + \lambda_t + \eta_i + \zeta_s + \epsilon_{it}$$

where  $i$  denotes a district,  $s$  denotes a state,  $t$  denotes the number of months before/after demonetization (0 corresponds to November 2016), and  $K$  denotes the set of months {-12,-6,-3,3,6,12}.  $Y_{it}$  is the number of conflicts in district  $i$  during month  $t$  and  $TREAT_i$  is the number of bank branches in district  $i$  in 2015.  $ADJ_i$  is the maximum number of bank branches in a district bordering district  $i$ ,  $POST_t$  is a dummy variable taking value 1 if  $t > -1$  (November 2016 and beyond),  $\lambda_t$  are time fixed effects,  $\eta_i$  are district fixed effects and  $\zeta_s$  are state fixed effects.  $\mathbf{X}_{it}^T$  is a vector of controls (described in section 3.1.5), and  $\epsilon_{it}$  is an error term. All specifications use robust standard errors clustered at the district level.

### 3.2.2 Instrumental Variables (IV) Model

To address potential omitted variable bias and/or reverse causality concerns , I utilize a 2SLS model. The first-stage equations are:

$$TREAT_i = \alpha + \beta_1 Z_i + \mathbf{X}'_{it} + \epsilon_{it}$$

and

$$C(k)_{it} = (\mathbb{1}\{t = k\} * TREAT)_{it} = \alpha + \beta_2 (\mathbb{1}\{t = k\} * Z)_{it} + \mathbf{X}'_{it} + \epsilon_{it}$$

where  $k \in K$ .

The second-stage equation is:

$$Y_{it} = \alpha + \delta_1 \hat{TREAT}_i + \delta_2 ADJ_i + \sum_{k \in K} \gamma_k \mathbb{1}\{t = k\} + \sum_{k \in K} \delta_k (\mathbb{1}\{t = k\} * ADJ)_{it} + \sum_{k \in K} \beta_k \hat{C}(k)_{it} + \mathbf{X}_{it}^T + \lambda_t + \eta_i + \zeta_s + \epsilon_{it}$$

The reduced form equation is:

$$Y_{it} = \alpha + \delta_1 Z_i + \delta_2 ADJ_i + \sum_{k \in K} \gamma_k \mathbb{1}\{t = k\} + \sum_{k \in K} \delta_k (\mathbb{1}\{t = k\} * ADJ)_{it} + \sum_{k \in K} \beta_k (\mathbb{1}\{t = k\} * Z)_{it} + \mathbf{X}_{it}^T + \lambda_t + \eta_i + \zeta_s + \epsilon_{it}$$

where  $Z_i$  is the number of bank branches opened in rural/unbanked areas during the government's branch licensing policy (BLP) period of 1979-1982, and all other variables are defined as in section 3.2.1. Note that since the instrumental variable is for the current number of bank branches, but our coefficient of interest is the interaction term between the number of bank branches and the dummy variables for 3,6 and 12 months pre and post-demonetization, we must construct seven instrumental variables,  $Z_i, (Z * \mathbb{I}\{t = k\})_{it}$ , where  $k \in K$ , as per Wooldridge (2010).

### 3.3 Empirical Strategy

#### 3.3.1 2SLS Regressions

There may be sources of omitted variable bias and reverse causality that affect causal identification in the panel regressions. For example, it is plausible that in areas where there are higher conflicts, there are also fewer bank branches, as there is a higher risk of a security breach. This would lead to biased estimators, which affects the interpretation of coefficients. To address these concerns, this paper employs a 2SLS model. Following the work of Bhavnani and Copelovitch (2018) and Kochhar (2011), the proposed instrumental variable is a dummy for whether a district was included in India's Branch Licensing Programs (BLPs).

Between 1979 and 1990, the Indian government implemented several waves of what were known as 'Branch Licensing Programs' (BLPs), the first of which occurred between 1979 and 1981 (Kochhar, 2011). During this program, the government authorized the construction of many new bank branches in approximately 30,000 previously unbanked and rural areas (Burgess and Pande, 2005). Inclusion into the programs was strictly rule-based, with only

districts that had fewer than 17,000 residents per branch receiving additional bank office openings. Intuitively, since the BLPs explicitly involved the construction of new bank branches, one would expect this instrument to be a relevant predictor of the current number of bank branches. As can be seen in Table 6 in the Appendix, the F-Stat for all of the first stage interaction terms exceeds 10, suggesting that there is no weak IV problem.

The exclusion restriction holds due to the rule-based implementation of the policy as well as India's rapidly changing population and political dynamics. One concern regarding the exclusion restriction could be that inclusion into the BLP implies a lack of wealth/economic development in a region in 1981, which could persist until today, thus providing an alternative channel through which the IV could affect post-demonetization conflict. However, this concern is invalid since in fact the main objective of the policy was to 'narrow regional disparities in the availability of banks' and thus precisely avoid any persistence of wealth disparities (Kochhar 2011). Additionally, India has experienced rapid economic liberalization since the 1990s, which has significantly changed population and wealth dynamics across districts due to factors such as remittance payments from urban workers to their rural households and the industrialization of previously rural areas.

Another concern is that the political party implementing the BLPs (the Congress party), could have favoured certain districts, and these districts continue to be aligned with the party today. Current political affiliation could then affect post-demonetization conflict outcomes. This is also not a plausible concern because of the clear population-to-branch ratio that was used in determining program inclusion. Hence, there does not seem to be another channel/mechanism through which the IV can affect the outcome variable.

## 4 Results

### 4.1 Main Results

#### 4.1.1 Full Sample, District-Level

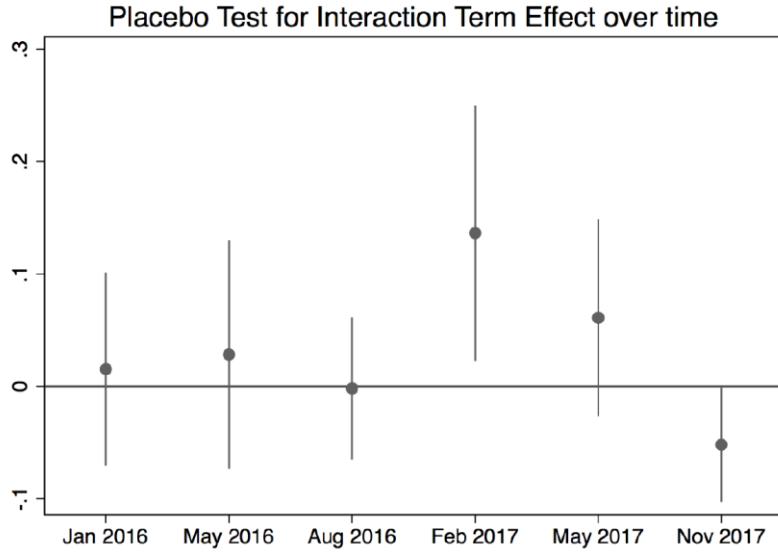
The table and figure in this section present the results for the panel regression and IV 2SLS regression specifications described in Section 3.2. Table 2 reports the results for the full sample of 389 districts from January 2016 – November 2017. Figure 1 depicts the results of a placebo test/pre-trends analysis, with the null results on the coefficients before November 2016 suggesting that the pre-trends assumption holds and that any significant increase or decrease in conflict observed after November 2016 can be causally attributable to the demonetization policy itself. The preferred results are the results in Column (3) of Table 2, which uses the IV 2SLS approach described in Section 3.3.1 to attain more plausibly causal estimates. A weak IV does not seem to be a concern due to the SW F-stat being significantly above 10 in all cases (see Appendix, Table 6).

Table 2: Full Sample

	Panel (1)	Panel (2)	IV 2SLS (3)
No. of Bank Branches (Hundreds) * 3 Months After	.042 (.027)	.047* (.027)	.136** (.058)
No. of Bank Branches (Hundreds) * 6 Months After	.033 (.022)	.030 (.023)	.061 (.045)
No. of Bank Branches (Hundreds) * 1 Year After	-.044*** (.012)	-.044*** (.012)	-.052** (.026)
Avg. Branches in Neighboring District (Hundreds) * 3 Months After	-.031 (.020)	-.032 (.021)	-.033 (.022)
Mean Conflict Count (per district month)	.32	.32	.32
Implied Treatment Effect	-.07	-.08	-.23
Adjusted R-squared	.08	.32	.32
Number of Observations	8952	8952	8952

Regressions at the district-month level, and the window of time under consideration is restricted to one year before and after the demonetization announcement. Outcome: number of conflicts in a particular district during a particular month. 'No. of bank branches (hundreds)' = the number of commercial bank branches (in hundreds) in a district in 2015, as per RBI data. 'Avg. branches in neighboring district' = the average number of branches in a neighboring district. 'post-demonetization' = 1 if date is November 2016 or beyond. '3 Months After' = 1 if date is Feb 2017. '6 Months After' = 1 if date is May 2017. '1 Year After' = 1 if date is November 2017. Columns 2 and 3 include district, state and month fixed effects. All specifications include additional socioeconomic controls. Controls at the district level include share of young males between 18-30, share of illiterate individuals, share of Muslims, share of Hindus, share of females, total population and average luminosity (measured using satellite data). Standard errors are robust and clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Figure 1: Placebo Test - Full Sample



Examining the interaction term coefficients from the IV 2SLS approach in Table 2, it can be seen that on average, the severity of the demonetization shock in a district does have a statistically significant effect on conflict three months following the policy announcement. Specifically, the coefficient estimate on the three-month interaction term suggests that a district with 100 fewer branches (i.e. a district more exposed to the demonetization shock) will experience on average 0.14 fewer conflicts three months after demonetization. However, this statistically significant reduction dissipates six months post-demonetization, as can be seen by the statistically insignificant coefficient value on the six-month interaction term. In fact, as time progresses, there seems to be a reversal in the policy effect, with the coefficient estimate one year after the policy introduction suggesting that districts more exposed to demonetization have a slightly higher number of conflicts on average.

These results indicate that more exposure to the demonetization shock does lead to a very short-term decrease in conflict, however conflict levels quickly recover in more severely affected districts within six months. Intuitively, this suggests that demonetization did lead to immediate financing constraints for terrorist/mafia groups, who found themselves unable to easily fund their operations in the short-term due to the cash shortage. However, over time,

these conflict-instigating actors seem to have found alternative means of financing, through which they could circumvent these new cash constraints and return to their previous levels of activity. These alternative means could include more sophisticated digital currencies, or could be attributable to the (anecdotally documented) fact that many local mafia groups were able to exchange their illegitimate cash for the new legal 2000 rupee notes through intermediaries.

The negative coefficient on the one year interaction term could be the result of some sort of ‘overcompensation effect’, whereby local criminal groups compensate for their inability to instigate conflicts earlier in the 2017, by increasing conflicts in more severely demonetized districts later in the year, after financially recovering. However, this negative coefficient could also be the result of other events that occurred through 2017 in India such as the introduction of the GST and numerous local and state elections, all of which might be confounding factors that affect the treatment effect measured one year after the policy announcement.

Moreover, while there is a statistically significant short-term reduction in conflict, the actual magnitude of this reduction is very small. The ‘implied treatment effect’ in Table 2 calculates the average difference in conflict three months after demonetization between a district with a number of branches equal to the nationwide 25<sup>th</sup> percentile, and a district with a number of branches equal to the nationwide 75<sup>th</sup> percentile. The implied treatment effect in Column 3 suggests that suggests that a district at the 25<sup>th</sup> percentile of branches only experiences 0.23 fewer conflicts three months after demonetization, relative to a district at the 75<sup>th</sup> percentile of branches, which is an economically insignificant amount. Thus, the short-term reduction in conflict in more ‘exposed’ districts is not very meaningful in reality.

Finally, migration of conflict to neighbouring districts with more branches does not seem to be a mechanism/channel through which conflict in a district is reduced in the short-term. This is because the coefficient on the ‘average number of branches in neighbouring districts’ interaction term (final row of Table 2) is statistically insignificant at the 10% level.

#### **4.1.2 Sample Restricted to Maoist/Leftist-Instigated Conflicts, District-Level**

One potential concern when running the regressions on the full sample of conflicts is that there could be a high degree of heterogeneity in the sophistication of terrorist and mafia groups across India, with certain groups being more technologically advanced regarding their financing channels, and others relying heavily on cash. To see if this heterogeneity exists and if there are more sizable and persistent effects on cash-reliant groups, the same specifications are run on a sample restricted to conflicts initiated by the ‘Maoists’ / ‘Leftists’, a well-known left-wing extremist group. This particular group is chosen because it is widely documented that they predominantly financed operations using 500 and 1000 rupee notes prior to demonetization (Jain, 2018), and thus in theory should have suffered significant reductions in their operational capabilities post-demonetization.

As seen by the IV 2SLS results in Table 3 and Figure 2 below, there seems to be no statistically significant effect of the severity of the demonetization shock on Maoist-instigated conflicts, in the long or short-term. Additionally, there are no statistically significant coefficients on the spillover effect term, indicating that despite being heavily reliant on cash, Maoist groups did not choose to ‘migrate’ their operations to another district in the short-term, where there was a greater likelihood of them exchanging their illegal tender for new cash.

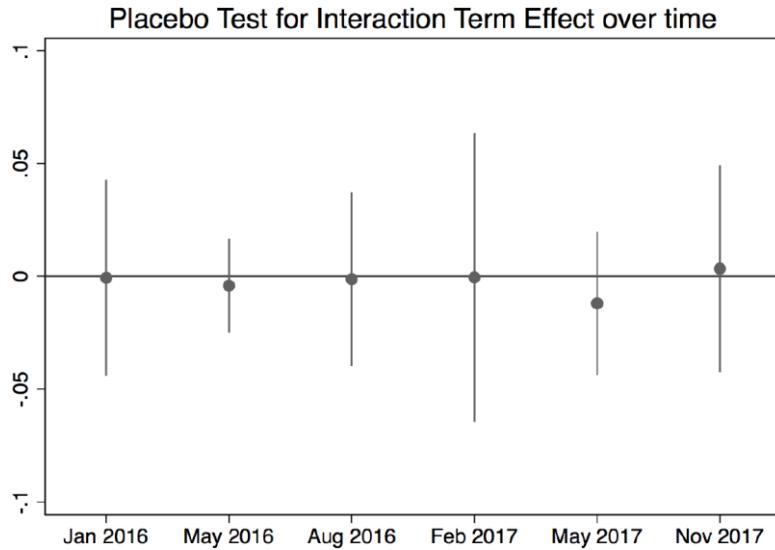
While these results are certainly counterintuitive, there are some potential explanations. For instance, it is possible that despite having few branches in their ‘home’ district, Maoist groups still found ways to exchange their illegal tender for legal tender without migrating to another district. There is anecdotal evidence of this second channel, with certain media sources reporting that terror groups found a way to overcome demonetization by ‘exchanging currency via villagers and contractors’ or through other backdoor means (Deka, 2017).

Table 3: **Leftist Conflicts Only**

	Panel (1)	Panel (2)	IV 2SLS (3)
No. of Bank Branches (Hundreds) * 3 Months After	-.014 (.013)	-.010 (.012)	-.000 (.033)
No. of Bank Branches (Hundreds) * 6 Months After	-.002 (.006)	-.004 (.006)	-.012 (.016)
No. of Bank Branches (Hundreds) * 1 Year After	-.002 (.009)	-.003 (.010)	.003 (.023)
Avg. Branches in Neighboring District (Hundreds) * 3 Months After	-.033 (.021)	-.032 (.022)	-.031 (.021)
Mean Conflict Count (per district month)	.29	.29	.29
Implied Treatment Effect	.02	.02	.00
Adjusted R-squared	.29	.60	.60
Number of Observations	1848	1848	1848

Regressions at the district-month level, and the window of time under consideration is restricted to one year before and after the demonetization announcement. Outcome: number of conflicts incited by Maoist/Leftist groups in a particular district during a particular month. 'No. of bank branches (hundreds)' = the number of commercial bank branches (in hundreds) in a district in 2015, as per RBI data. 'Avg. branches in neighboring district' = the average number of branches in a neighboring district. 'post-demonetization' = 1 if date is November 2016 or beyond. '3 Months After' = 1 if date is Feb 2017. '6 Months After' = 1 if date is May 2017. '1 Year After' = 1 if date is November 2017. Columns 2 and 3 include district, state and month fixed effects. All specifications include additional socioeconomic controls. Controls at the district level include share of young males between 18-30, share of illiterate individuals, share of Muslims, share of Hindus, share of females, total population and average luminosity (measured using satellite data). Standard errors are robust and clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Figure 2: **Placebo Test - Leftist Conflicts Only**



#### 4.1.3 Sample Restricted to Small Scale Conflicts, District-Level

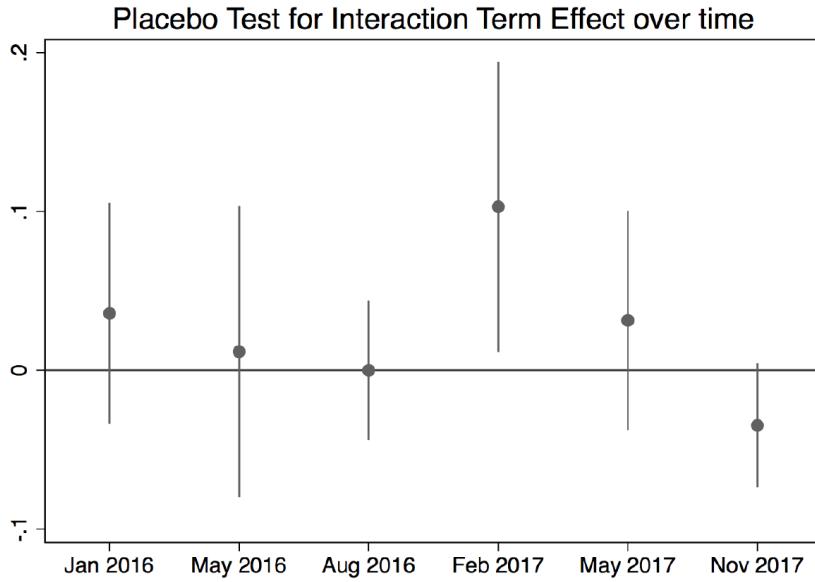
Another potential source of heterogeneity in the effect of demonetization could be the ‘scale’ of the conflict under consideration. Specifically, small-scale conflicts such as kidnapping, stoning and violence against small groups could experience a more noticeable short-term (and/or long-term) reduction in response to a demonetization shock, because such conflicts require fewer actors and less organizational sophistication, and are thus more likely to use cash as a means of payment. Conversely, larger conflicts carried out by national/international syndicates and terrorist groups may be less likely to use cash, due to the sophistication of the group’s operations. In order to test for this heterogeneity, the sample size is restricted to only small-scale conflicts, which is achieved by using the event type and sub-type categories provided by the ACLED database.

Table 4: Small Scale Conflicts Only

	Panel (1)	Panel (2)	IV 2SLS (3)
No. of Bank Branches (Hundreds) * 3 Months After	.025 (.020)	.030 (.021)	.103** (.047)
No. of Bank Branches (Hundreds) * 6 Months After	.015 (.012)	.012 (.013)	.031 (.035)
No. of Bank Branches (Hundreds) * 1 Year After	-.030*** (.008)	-.031*** (.009)	-.035* (.020)
Avg. Branches in Neighboring District (Hundreds) * 3 Months After	-.040*** (.015)	-.040*** (.015)	-.040** (.017)
Mean Conflict Count (per district month)	.18	.18	.18
Implied Treatment Effect	-.04	-.05	-.17
Adjusted R-squared	.08	.21	.20
Number of Observations	8544	8544	8544

Regressions at the district-month level, and the window of time under consideration is restricted to one year before and after the demonetization announcement. Outcome: number of small-scale conflicts (e.g. kidnapping, stoning, violence against small group) in a particular district during a particular month. ‘No. of bank branches (hundreds)’ = the number of commercial bank branches (in hundreds) in a district in 2015, as per RBI data. ‘Avg. branches in neighboring district’ = the average number of branches in a neighboring district. ‘post-demonetization’ = 1 if date is November 2016 or beyond. ‘3 Months After’ = 1 if date is Feb 2017. ‘6 Months After’ = 1 if date is May 2017. ‘1 Year After’ = 1 if date is November 2017. Columns 2 and 3 include district, state and month fixed effects. All specifications include additional socioeconomic controls. Controls at the district level include share of young males between 18-30, share of illiterate individuals, share of Muslims, share of Hindus, share of females, total population and average luminosity (measured using satellite data). Standard errors are robust and clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Figure 3: Placebo Test - Small Scale Conflicts Only



As seen in Table 4 above, there does seem to be a significant short-term effect of the severity of the demonetization shock on conflict. Specifically, the coefficient on the 3-month interaction term is statistically significant at the 5% level and suggests that a district with 100 fewer branches experiences 0.1 fewer conflicts 3 months after demonetization, on average. However, similarly to the results for the main sample in section 4.1.1, this statistically significant reduction disappears after 6 months. This bolsters the notion that while demonetization does lead to an immediate reduction in conflict in more affected areas, eventually terrorist/mafia groups find a way to bypass cash constraints and resume their illicit activities to the same extent as before.

Additionally, the fact that we observe similar patterns and magnitudes of the coefficients between Figures 1 and 2, suggests that the short-term reduction in conflict observed in the main sample was largely driven by a reduction in small scale conflicts. This aligns with the intuition described earlier, whereby one would expect smaller-scale conflicts to decline in the immediate aftermath of demonetization because these conflicts are instigated by less sophisticated groups that are more likely to finance their operations through cash.

Nevertheless, it is once again important to note that the magnitude of the reduction is small, with the implied treatment effect suggesting that a district ranking in the 25<sup>th</sup> percentile of branches only experienced (on average) 0.17 fewer conflicts three months after demonetization, relative to a district ranking in the 75<sup>th</sup> percentile in terms of the number of branches.

Finally, the coefficient on the ‘average number of branches in neighbouring districts’ interaction term (final row of Table 4) is statistically significant at the 5% level. This provides evidence that the mechanism behind the short-term reduction in small-scale conflicts was ‘migration’ of conflict to neighbouring districts. This is because the negative value of the coefficient suggests that the higher the average number of branches in neighbours of a particular ‘home’ district, the fewer the small-scale conflicts in that ‘home’ district.

#### **4.1.4 Sample Restricted to Conflict-Prone Conflicts, District-Level**

An aggregation of the data to the district level reveals large heterogeneity in the average number of conflicts per month across districts, with certain districts having below 0.5 monthly conflicts on average, and others exceeding 8-10 conflicts. Thus, another potential avenue of interest is determining whether the results observed in the main sample are driven by conflict reductions in areas that consistently experience conflicts in almost every month. In this paper, these ‘conflict-prone’ areas are defined as districts experiencing an average of more than one conflict per month.

As seen in Column 3, Table 5 and Figure 4 above, a conflict-prone district that is more severely exposed to the demonetization shock does not experience a significant short-term or long-term decline in conflicts post-demonetization. Intuitively, this result suggests that demonetization did not have any effect, short or long-term, in reducing conflict in districts where conflict is most consistently prevalent, i.e. in districts where conflict reduction is likely

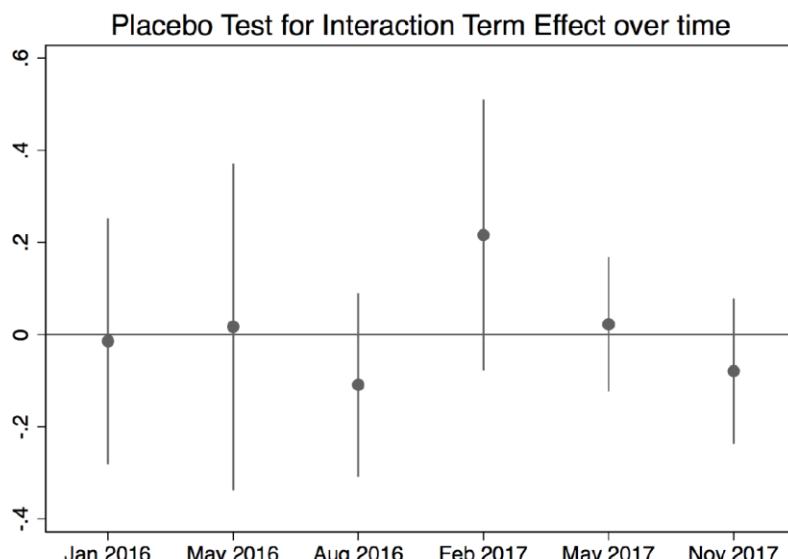
most beneficial. Instead, it seems that the short-term reduction seen in the main sample was driven by districts that experience sporadic conflicts month-to-month, but not consistently high levels of conflict. This finding can be potentially explained by the fact that conflict-prone areas are conflict-prone due to the sophistication of the terrorist/mafia groups operating within them. In other words, conflict-prone regions have more powerful terrorist groups who may have more sophisticated channels of financing their activities.

Table 5: Conflict-Prone Districts Only

	Panel (1)	Panel (2)	IV 2SLS (3)
No. of Bank Branches (Hundreds) * 3 Months After	.033 (.031)	.046 (.033)	.216 (.150)
No. of Bank Branches (Hundreds) * 6 Months After	.035 (.026)	.028 (.026)	.022 (.074)
No. of Bank Branches (Hundreds) * 1 Year After	-.046* (.024)	-.047* (.024)	-.079 (.080)
Avg. Branches in Neighboring District (Hundreds) * 3 Months After	-.382 (.280)	-.375 (.290)	-.517 (.518)
Mean Conflict Count (per district month)	1.51	1.51	1.51
Implied Treatment Effect	-.28	-.40	-1.87
Adjusted R-squared	.15	.30	.27
Number of Observations	720	720	720

Regressions at the district-month level, and the window of time under consideration is restricted to one year before and after the demonetization announcement. The districts in this specification are very 'conflict-prone' in that they experience an average of  $\geq 1$  conflict per month prior to demonetization. Outcome: number of conflicts in a particular district during a particular month. 'No. of bank branches (hundreds)' = the number of commercial bank branches (in hundreds) in a district in 2015, as per RBI data. 'Max. branches in neighboring district' = the maximum number of branches in a neighboring district. 'post-demonetization dummy' = 1 if date is November 2016 or beyond. All specifications include district and month fixed effects. Columns 2 and 3 include additional socioeconomic controls. Controls at the district level include share of young males between 18-30, share of illiterate individuals, share of Muslims, share of Hindus, share of females and total population. Standard errors are robust and clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Figure 4: Placebo Test - Conflict-Prone Districts Only



## **4.2 Robustness Checks**

### **4.2.1 Limited Information Maximum Likelihood (LIML) Estimation**

As seen in Table 6 of the appendix, none of the IV 2SLS estimates suffer from a lack of ‘relevance’. For the sake of robustness, in Table 7 of the appendix, this paper verifies the results of the IV 2SLS specifications using an LIML estimator, which leads to more accurate estimation under concerns of weak instruments. Comparing the results of the standard 2SLS and the LIML estimators, we see almost identical results, further confirming the strength of the first stage in the IV methodology.

### **4.2.2 Alternative Demonetization Shock Measures**

This paper also tests if the main results in Section 4.1.1 are robust to using three alternative measures of the demonetization shock severity. These three measures are: the percent of the population in rural areas, the percent of population employed in agriculture, and the percentage change in bank deposits in a district between Q3 and Q4 2016. The logic behind using percent of agricultural workers and rural residents as demonetization shock proxies is that areas with many agricultural workers and/or rural residents are significantly more likely to be reliant on cash (Bhavnani and Copelovitch 2018), and thus are potentially more ‘exposed’ to the demonetization shock. The change in district-level deposits between Q3 and Q4 2016 is a good proxy for the demonetization shock because a district with a large increase in the number of deposits would be relatively unaffected by demonetization (as most old tender was deposited in a timely manner), whereas a district with a low change in the number of deposits could imply that there were difficulties in depositing and/or exchanging old cash (Chanda and Cook 2019). Tables 8-10 in the appendix present the results from using these alternative demonetization shock measures.

The results from Table 8 suggest that districts more exposed to the demonetization shock, i.e. districts with a higher rural population percentage, experience short-term reductions in conflict post-demonetization, which dissipate after 6 months. Likewise, Table 9 suggests that districts with a higher proportion of agricultural workers (i.e. districts more likely to rely on cash for transactions and payments for illicit activities) also exhibit a similar short-term decline in conflicts followed by a recovery to ‘normal’ levels beyond 6 months. Moreover, in both cases the coefficients on the 3-month interaction term variable are statistically significant (at the 10% and 5% level, respectively) and small in magnitude. The results from Table 10 suggest that districts with a larger inflow of deposits between Q3 and Q4 2016 (and thus more reliant on cash) do not seem to exhibit any statistically significant change in conflict post-demonetization, both in the short and long-term.

Overall, the results from these three alternative measures corroborate with the key patterns found in the main results in section 4.1.1 and indicate that these main results are generally robust.

#### **4.2.3 Results including Jammu & Kashmir**

As mentioned earlier in the paper, conflicts in the region of Jammu & Kashmir are excluded from the analysis due to the extreme volatility and socio-political complexities occurring in the region during this paper’s period of study. This complexity makes it very difficult to isolate the impact of demonetization on conflict, because there are many unobserved confounding variables specific to the state, and districts within the state. Additionally, the region is routinely involved in conflicts involving international entities such as overseas terrorist groups and other sovereign nations, again making it difficult to establish causality and account for all sources of omitted variable bias. Nevertheless, for the sake of transparency, the results on the full sample including conflicts in Jammu & Kashmir are shown in Table 11 and Figure 5 of the appendix.

As seen in column 3 of Table 11, the addition of Jammu and Kashmir conflicts into the sample leads to a dampening of the short-term effects seen in section 4.1.1. However, the magnitude of the three-month effect still remains similar (0.1 vs 0.14). Moreover, the panel regression in Columns (1) and (2) corroborate with the findings in section 4.1.1, since they show a small statistically significant decline in conflict in areas relatively more exposed to the demonetization shock. These findings indicate that the main results are somewhat (yet not entirely) robust to the inclusion of Jammu and Kashmir in the sample.

## **4.3 Possible Limitations**

### **4.3.1 Relevance of Control Variables**

One main concern with the district-level analysis is that many of the socio-economic and demographic controls used are sourced from the 2011 Census, five years prior to demonetization taking effect. As a result, the census data may not accurately capture the characteristics of districts in 2016 and beyond, thus affecting the causal interpretation of coefficients. This issue could be resolved in future work through the use of richer satellite data as a control, which has been recently used in conjunction with machine learning techniques to predict poverty at a granular level (see Jean et al. 2016).

### **4.3.2 Measurement of Conflict**

One of the downsides of the ACLED India conflict data is that conflicts are recorded beginning in January 2016, only 10 months prior to the demonetization announcement. As a result, it is possible that the data is not entirely comprehensive or accurate, especially during the first few months of collection. There is no clear solution for this issue, as the main alternative conflict dataset (UCDP Georeferenced Conflict Dataset) only captures larger conflict events, mostly instigated by the government, which are not useful for this paper.

#### **4.3.3 External Validity**

Since there could be a high degree of heterogeneity in the operations of terrorist and mafia groups across countries, it is possible that the results of this paper could be inapplicable in other contexts. I.e. it is possible that in another region of the world (e.g. a country in Sub-Saharan Africa) the introduction of a demonetization policy could lead to sizable and long-lasting reductions in conflict through a reduction of illicit terrorist financing. Reasons for this could be that mafia/terror groups in other regions are less formally organized, and more reliant on cash. In short, these results should not be used as a justification for not implementing demonetization in another nation, they must only be interpreted in the Indian context.

## **5 Conclusion**

In conclusion, this paper examines the causal effect of India's November 2016 demonetization policy on the incidence of conflict at the district level. In doing so, this paper aims to determine the importance of illicit financing as a mechanism through which economic shocks can propagate conflict. Leveraging the exogenous nature of the demonetization policy announcement and using an instrumental variables empirical strategy, this paper finds that districts more exposed to demonetization exhibit a statistically significant decrease in conflict in the short-term, however this effect is small and dissipates after six months. It also finds spillover effects for small-scale conflicts, whereby terrorist/mafia groups choose to 'migrate' conflicts such as kidnappings, stonings and small-scale violence to areas that are less affected by demonetization (i.e. where there is a greater probability of depositing/exchanging their old currency). Moreover, there are no heterogeneous effects across different types of terrorist groups. I.e. groups more reliant on cash for daily operations (e.g. Maoists), do not seem to be

more hindered in their ability to instigate conflict. These results are robust to assuming a weak IV, and to utilizing varying demonetization shock measures.

These results suggest that in the Indian context, demonetization of high-denomination banknotes was somewhat effective in reducing conflict in the immediate aftermath of its implementation, as mafia and terrorist groups scrambled to find alternative means of financing their illicit activities. However, over the long-term, there is strong evidence to suggest that these groups financially recovered and were able to largely circumvent the cash constraints imposed upon them by this policy. It is important to note that these results should only be interpreted in relation to the Indian context, and not as conclusive evidence suggesting demonetization has no persistent impacts on conflict/crime in general. Avenues for future research could include utilizing a randomized control trial (RCT) or natural experiment in another developing country setting to see if these results are robust to changes in institutional, geographical and/or cultural settings.

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## 7 Appendix

Table 6: First Stage Results

	First Stage - 3 Months (1)	First Stage - 6 Months (2)	First Stage - 1 Year (3)
BLP Eligibility (IV) * 3 Months After	2.167*** (.414)		
BLP Eligibility (IV) * 6 Months After		2.186*** (.424)	
BLP Eligibility (IV) * 1 Year After			2.198*** (.429)
Sanderson-Windmeijer F-Stat	36.71	45.00	46.57
Adjusted R-squared	.42	.41	.41
Number Observations	8976	8976	8976

Table 7: LIML/Weak IV Robustness Check

	IV - LIML (1)
No. of Bank Branches (Hundreds) * 3 Months After	.136** (.058)
No. of Bank Branches (Hundreds) * 6 Months After	.061 (.045)
No. of Bank Branches (Hundreds) * 1 Year After	-.052** (.026)
Avg. Branches in Neighboring District (Hundreds) * 3 Months After	-.033 (.022)
Mean Conflict Count (per district month)	.32
Implied Treatment Effect	-.23
Adjusted R-squared	.32
Number Observations	8952

Regressions at the district-month level. Outcome: number of conflicts in a particular district during a particular month. 'No. of bank branches (hundreds)' = the number of commercial bank branches (in hundreds) in a district in 2015, as per RBI data. 'Max. branches in neighboring district' = the maximum number of branches in a neighboring district. 'post-demonetization dummy' = 1 if date is November 2016 or beyond. All specifications include district and month fixed effects and socioeconomic controls. Controls at the district level include share of young males between 18-30, share of illiterate individuals, share of Muslims, share of Hindus, share of females and total population. Standard errors are robust and clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Table 8: Alternative Measure - Rural Population (% of total)

	Panel (1)
Rural Population (% of total) * 3 Months After	-.007* (.004)
Rural Population (% of total) * 6 Months After	-.005* (.003)
Rural Population (% of total) * 1 Year After	.006*** (.002)
Mean Conflict Count (per district month)	.32
Implied Treatment Effect	.01
Adjusted R-squared	.32
Number of Observations	8976

Regression is at the district-month level. Outcome: number of conflicts in a particular district during a particular month. 'Rural Population (% of total)' = the share of a district's population living in rural areas. Specification includes district, month and state fixed effects, and socioeconomic controls. Standard errors are robust and clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Table 9: Alternative Measure - Proportion of Agricultural Workers (% of total)

	Panel (1)
Agricultural Workers (% of total) * 3 Months After	-.019** (.008)
Agricultural Workers (% of total) * 6 Months After	-.012* (.007)
Agricultural Workers (% of total) * 1 Year After	.012** (.005)
Mean Conflict Count (per district month)	.32
Implied Treatment Effect	.03
Adjusted R-squared	.32
Number of Observations	8976

Regression is at the district-month level. Outcome: number of conflicts in a particular district during a particular month. Agricultural Workers (% of total)' = the share of a district's workers working in agriculture. Specification includes district, month and state fixed effects, and socioeconomic controls. Standard errors are robust and clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Table 10: Alternative Measure - Percent Change in Deposits between Q3 and Q4, 2016

	Panel (1)
Percent Change in Deposits * 3 Months After	-.003 (.007)
Percent Change in Deposits * 6 Months After	.006 (.006)
Percent Change in Deposits * 1 Year After	.005 (.005)
Mean Conflict Count (per district month)	.32
Implied Treatment Effect	.01
Adjusted R-squared	.31
Number of Observations	8976

Regression is at the district-month level. Outcome: number of conflicts in a particular district during a particular month. Percent change in deposits' = the percentage point change in deposits in district commercial banks between Q3 and Q4 2016. Specification includes district, month and state fixed effects, and socioeconomic controls. Standard errors are robust and clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Table 11: Full Sample including Jammu & Kashmir

	Panel (1)	Panel (2)	IV 2SLS (3)
No. of Bank Branches (Hundreds) * 3 Months After	.052* (.028)	.053* (.028)	.100 (.065)
No. of Bank Branches (Hundreds) * 6 Months After	.029 (.021)	.028 (.022)	.088* (.050)
No. of Bank Branches (Hundreds) * 1 Year After	-.037*** (.011)	-.037*** (.012)	-.107** (.043)
Avg. Branches in Neighboring District (Hundreds) * 3 Months After	-.020 (.021)	-.021 (.022)	-.021 (.022)
Mean Conflict Count (per district month)	.41	.41	.41
Implied Treatment Effect	-.09	-.09	-.17
Adjusted R-squared	.14	.49	.48
Number of Observations	9312	9312	9312

Regressions at the district-month level, and the window of time under consideration is restricted to one year before and after the demonetization announcement. Outcome: number of conflicts in a particular district during a particular month. 'No. of bank branches (hundreds)' = the number of commercial bank branches (in hundreds) in a district in 2015, as per RBI data. 'Avg. branches in neighboring district' = the average number of branches in a neighboring district. 'post-demonetization' = 1 if date is November 2016 or beyond. '3 Months After' = 1 if date is Feb 2017. '6 Months After' = 1 if date is May 2017. '1 Year After' = 1 if date is November 2017. Columns 2 and 3 include district, state and month fixed effects. All specifications include additional socioeconomic controls. Controls at the district level include share of young males between 18-30, share of illiterate individuals, share of Muslims, share of Hindus, share of females, total population and average luminosity (measured using satellite data). Standard errors are robust and clustered at the district level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

Figure 5: Placebo Test - Full Sample including Jammu & Kashmir

