Economic Inquiry

ANALYZING THE IMPACT OF THE WORLD'S LARGEST PUBLIC WORKS PROJECT ON CRIME

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India started the implementation of a rural public works program in 2006, covering all districts of the country within 3 years. The program guarantees 100 days of employment per year at minimum wage to each rural household, with the goal of reducing joblessness and poverty. We exploit the design and implementation of this program to investigate its employment impact on various types of crimes to provide rare evidence on the employment–crime relationship in a developing country context. We find that employment generated by the program has a negative impact on both property crime and violent crime. The same conclusion is reached when we analyze the impact of the program using its staggered rollout structure as the source of identification in a difference-in-difference analysis. Although crime elasticities with respect to employment are small, this finding represents another dimension of the social benefit generated by the program. (*JEL* K4, E24, H54)

I. INTRODUCTION

The Mahatma Gandhi National Rural Employment Guarantee Act of India (henceforth MGNREGA) was enacted in August 2005 and implemented in three phases starting in 2006, covering all districts of the country within 3 years. The program guarantees 100 days of employment per year at minimum wage to each rural household with the goal of providing relief for joblessness and poverty. MGNREGA is the largest public works program in the world with annual outlays of about $10 billion per year, generating more than 2.5 billion person days of employment each year. On average 55 million households are provided employment through the program, which is about one-third of the 167 million rural households in the country. In this paper we exploit the design and implementation of this program to investigate its employment impact on various types of crimes, ranging from burglary to kidnapping.

The relationship between legal labor market conditions and crime is well-determined theoretically, based on the seminal works of Gary Becker and Isaac Ehrlich (Becker 1968; Ehrlich 1973), and their recent extensions (e.g., Lochner 2004; Mocan, Billups, and Overland 2005). A large body of empirical work has analyzed aggregate data sets and reported a positive relationship between unemployment and crime (Altindag 2012; Lin 2008; Mocan and Bali 2010; Öster and Agell 2007; Raphael and Winter-Ebmer 2001), and a negative relationship between legal market wages and crime (Corman and Mocan 2005; Gould, Weinberg, and Mustard 2002; Machin and Meghir 2004), confirming theoretical predictions.[1] Additional details and references to these

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1. Identifying the causal effect of an individual's own joblessness or own wages on his/her criminal propensity is arguably more challenging using micro data. Issues surrounding the endogeneity of wages and employment status, and reverse causality from criminal activity to labor market opportunity are hard challenges to resolve. Nevertheless, studies that employed microdata proposed instrumental variables or used reduced form specifications to tackle these issues (Grogger 1998; Mocan and Unel 2017).

ABBREVIATIONS

CI: Crime in India
MGNREGA: Mahatma Gandhi National Rural Employment Guarantee Act of India
MoRD: Ministry of Rural Development

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two strands of literature can be found in Draca and Machin (2015). Also, recent research has shown that the variation in earning opportunities in the legal sector (Axbard 2016) as well as in the criminal sector (Draca, Koutmeridis, and Machin 2019) influences criminal activity.

The overwhelming majority of the literature analyzing the relationship between crime and economic opportunity has focused on developed countries, although there are exceptions. For example, Fafchamps and Minten (2006) analyzed the impact of poverty on crop theft in Madagascar. Dube and Vargas (2013) investigated the impact of income shocks on armed conflict in Colombia. Prasad (2012) analyzed the impact of trade liberalization on violent crime in India.

The goal of our paper is to identify the impact on crime of the MGNREGA program of India, which provides employment to rural workers on demand. India consists of 28 states, and each state is divided into districts, yielding a total of 641 districts. There is a federal governance structure with separate legislatures for the states and the central government.[2] The central government and the state governments have separate budgets and tax revenues. The implementation and management of the MGNREGA program are at the local level, but the funding of the program is provided by the central government. This feature of the program is important as it provides a setting where the implementation of the program is not endogenous to local conditions such as the fiscal health of the state or that of the district. We provide the details of the program and the governance structure in Section II, but an important feature of the program is that it does not provide permanent employment; rather, it is designed much like an insurance scheme to provide some employment security to rural workers whose job opportunities are subject to fluctuation because of (mainly weather-related) shocks in agricultural production.

While the impact of employment on crime at the individual level is theoretically unambiguous, the predicted impact of this nationwide program on crime is unclear. The program can reduce crime by directly decreasing joblessness and raising income in rural areas. Various public works projects implemented under the program help improve rural infrastructure, and this could increase agricultural productivity, economic activity, and wages (Bhargava 2014), leading to a decrease in criminal activity.[3] However, the magnitude of each of these effects is uncertain, and therefore the aggregate impact on crime is ambiguous. Furthermore, it is also possible that such massive public sector hiring may crowd-out private sector work, including self-employment, or it can crowd out public sector investment and employment (Imbert and Papp 2015).[4] Thus, it is unclear the extent to which the program would impact aggregate criminal activity, and which types of crimes would be most impacted.

Several recent papers have reported a positive impact of the MGNREGA program on women's welfare on such domains as nutrition, health, women's voice in household decisions, and participation in local governance (Das 2012; Holmes, Sadana, and Rath 2010; Jandu 2008; Nayak and Khera 2009) It has also been shown that the program had a significant impact on women's employment and wages while the impact on male employment and wages has been negligible (Azam 2012). If the impact of the program is concentrated on women, its influence on crime may be limited by the fact that males have a higher propensity to participate in illegal activities.[5] On the other hand, an increase in female employment will have a positive impact on total household income and a negative impact on poverty (Klonner and Oldiges 2014), thereby negatively influencing men's propensity for criminal activity. Khanna and Zimmermann (2017) and Dasgupta et al. (2017) have investigated the effect of the MGNREGA program on Maoist insurgency in India. The former paper found an increase in insurgency-related violence and attacks after the program, while the latter reported a large decline in violence related to

2. The central legislature is the Parliament, the members of which are elected every 5 years through a national election. Each state has its own legislature which is elected every 5 years.

3. The program can also have a general equilibrium effect by raising rural wages because labor demand and overall employment can be increased through the program (Imbert and Papp 2015). This potential wage effect would also help reduce crime.

4. We will discuss in Section VI that while there exists evidence for some crowding out in private sector employment, there is no evidence for significant public sector crowding out, nor is there any evidence of an impact on federal government financing and taxation, at least during the period of analysis.

5. Fallesen et al. (2014) find that mandatory participation in active labor market programs (job-search courses, training, etc.) reduces the propensity to commit crime among young unemployed welfare recipients in Denmark. MGNREGA's potential impact on crime through "incapacitation" of potential perpetrators while they are employed by the program may be limited to the extent that the program influences women's employment more than that of men's.

Maoist conflict, concentrated in districts with large state capacity.

We create a district level panel between 2002 and 2012 for various crimes. We control for a number of time-varying district characteristics, although omitting these characteristics has no meaningful impact on the results. We have information not only on the timing of the program implementation, but also on the number of households that received jobs under the program, as well as on the worker-days of employment generated by the program.[6] By exploiting the variation in the rollout and the intensity of implementation of the program, we identify the impact of employment generated by the program on crime. We also implement a difference-in-difference analysis that utilizes only the introduction of the program in different districts in different years.

The paper contributes to the literature in a number of ways. First, it identifies the relationship between crime and economic opportunity for the poor using a quasi-natural experiment and therefore it arguably avoids standard endogeneity issues. More specifically, as explained below, the specific purpose of the MGNREGA program is to reduce rural unemployment and poverty, but criminal activity *has not* been a concern of the program.[7] Second, the paper makes a contribution to the literature that investigates the relationship between employment and crime in a developing country setting. This is important because it allows for a comparison of the relationship between employment and crime in developing versus developed countries. More generally, to the extent that criminal activity has an impact on the formation of both legal and criminal human capital (Bayer, Hjalmarsson, and Pozen 2009; Mocan and Bali 2010; Mocan, Billups, and Overland 2005), and that legal human capital is a vital ingredient of economic development (Hanushek and Kimko 2000), investigation of the determinants of crime is important in developing countries. This is because, participation in the criminal sector, which could be prompted, among other factors, by an unfavorable economic environment represented by low wages and high unemployment rate, would create path-dependence in crime and this would further hinder the development of marketable skills (Mocan and Bali 2010; Mocan, Billups, and Overland 2005). Thus, high rates of engagement in criminal activity can hurt economic development as criminal participation could be a detriment to the formation of legal human capital which is needed for economic development.

Data from India allow us to analyze crimes that are not typical or prevalent in developed countries such as kidnapping, and unlawful assembly and riots.[8] Finally, the results can provide insights into a positive externality (reduction in crime) that can be generated by public works projects in developing countries.

Our results show that employment generated by the program has a negative impact on both property and violent crimes.[9] Crime elasticities with respect to employment are small, which is not surprising because, as mentioned earlier, the program provides not permanent, but temporary and limited employment as a safety net against negative shocks in agricultural production.

The paper is organized as follows: Section II describes the MGNREGA program and its implementation, Section III explains the data, Section IV presents the empirical specification, and Section V discusses the analysis of preprogram trends. Section VI presents the results and the extensions, and Section VII is the conclusion.

II. THE DETAILS OF MGNREGA PROGRAM

The MGNREGA is a rural employment guarantee act, enacted by the Indian Parliament in August 2005. The implementation started in 2006. The program provides legal guarantee of 100 days of work to any member of a rural household at minimum wage. The program is demand driven, and there is no capacity constraint. That is, any adult in rural India is entitled to obtain work under the program. Anyone who is 18 or older can join, leave, and re-join the program at any time during the year provided that the total number of days worked by all members of his/her household does not exceed 100 per year. The type of the work is of casual labor and there are no minimum qualifications or training requirements. The typical jobs are digging ponds and wells, digging irrigation canals, paving of roads, and so on.

6. The program has no capacity constraint; that is, anyone who seeks employment obtains employment on demand with the proviso that total number of days of employment cannot exceed 100 for the households.

7. We show in that preprogram crime trends are unrelated to program implementation.

8. The only existing research on economics determinants of kidnapping is Detotto, McCannon, and Vannini (2015) who analyzed the impact of sanctions on kidnapping in Italy.

9. We cannot identify particular mechanisms through which the program impacts crime.

The only conditions that apply to an adult who wants to work under the program are: they must live in a rural area and they must be willing to undertake unskilled manual work for which they will receive the minimum wage. The minimum wage varies between states but remains the same across districts in a particular state. Typically, the minimum wage is equivalent to between two to three dollars per day.[10] Job seekers need to furnish their name, age, and address to the village council (Gram Panchayat), which issues a job card to each household containing details of adult members of the household. Applicants who are provided employment are informed by a letter which is mailed to the address mentioned on the job card, and a public notice of employment is displayed at the Panchayat office. By law, the work has to be provided within 10 km of the home of the job seeker. If that is not possible, then the work must be within the Block (a subdivision of a district) of the residence of the job seeker and an extra 10% of the wage of the worker must be paid for travel expenses. Wages must be paid within 15 days of the completion of the work. If an applicant is not provided a job within 15 days upon the receipt of an application then the applicant is eligible for unemployment allowance for each day after the 15 days when he/she is not employed until the state finds work for him/her. This unemployment allowance cannot be less than one-fourth of the wage rate in the first 30 days after the expiration of the 15 day deadline and three-fourth of the wage rate in the remaining period until he/she is provided a job. Local government is obligated to provide employment upon the request of the applicants, but the full funding of program is provided by the central government. In addition, the central government covers three-fourth of the cost of materials while the rest is funded the state governments. This is important because it indicates that other outlays of the state government, such as expenditures on police, are not impacted by the MGNREGA spending.

The program has been implemented nationally since 2006. Implementation was rolled out in three phases, starting in 2006 with 200 districts of Phase I. In 2007, the program was extended to include another 130 districts (Phase II). In 2008 the program covered all rural districts of the country. Districts of India vary in their size and population density. The largest district is Kachh, with an area of over 45,000 km^2, which is twice the size of the state of New Jersey. Population density of districts varies from a few dozen to 4,500 per km^2.[11]

In the phased implementation of the program, economically poorer districts were chosen to participate in the earlier phases. In India each district of the country is assigned an index of "backwardness" by the central government, which specifies the lack of economic development in the district.[12] In Phase I of the program the number of district chosen from each state was determined by the overall economic condition of the state and its population size, where poorer and larger states contributed more districts.[13] Once the number of districts from a state is determined, the decision to choose specific districts from that state is made by ranking the districts by their backwardness index: poorer districts were given priority. All these decisions were made by the central government. The same procedure was followed in Phase II; and the program was extended to all districts in Phase III.[14] We show below that entry of the districts to the program was

10. To monitor the progress of the program social audits are carried out by independent nongovernment organizations.

11. The most densely populated districts are those that are completely urban, such as Kolkata. The population density in such districts can be as high as 20,000 people per km^2. These districts are excluded from our analysis because they are fully urban and the MGNREGA program provides employment in rural areas only.

12. The backwardness index is the sum of three subindices measuring agricultural output, agricultural wages and proportion of the population that belongs to the Scheduled Tribe/Scheduled Caste groups (official designations given to various historically disadvantaged indigenous people) groups. Indian government ranks districts based on the value of the index. The lower the value, the more underdeveloped or backward a district is.

13. Although the Planning Commission, the body which was responsible for planning the program, did not explicitly mention the algorithm used to choose the districts, it can be assumed that they used the same algorithm which had been used in roll-out of previous government programs (Zimmermann 2015). According to the algorithm, the number of districts to be chosen from each state depends on the percentage of population below poverty line in a state and the population size of the state; then the districts from a given state is chosen according to the economic condition of the districts with poorer districts being given priority.

14. Fraud and corruption in implementation can result in disparities between official data and the situation on the ground. Although the program has a built-in auditing system based on independent private auditors, incidences of corruption, poor implementation by local governments, capturing of the benefits of the program by less deprived households through political manipulation and incorrect targeting of the program such that the benefits do not reach the poorest households has proven to be widespread (Dutta et al. 2012; IBN Live 2013; Jha et al. 2009; Shariff 2009, etc.). But despite that, extensive surveys have shown that the program has had a sizable impact on poverty and income (Hindustan Times 2013).

not related to their crime rates in the preceding years.

We exclude some districts from the analysis because the MGNREGA program is not relevant for these districts. For example, because the program targets the rural poor, districts which are entirely urban (such as Kolkata) are not part of the program, and are omitted from the analysis. Similarly, we exclude all Northeastern states except Assam because these states receive special grants from the central government under various schemes, and therefore will effectively be different from the other states in many aspects. We also exclude the Union Territories as they are directly under central administration and have different mechanism of governance. We exclude Maharashtra because it has its own rural employment guarantee scheme since 1977 which is similar to the MGNREGA and therefore will not register the same impact as other states. We exclude the state of Jammu and Kashmir because this state has historically faced insurgency which escalated in the 1990s; thus the military has a big presence there which would influence the crime rate in that state. Lastly, we also drop districts which have been divided or newly created between 2001 and 2012.[15]

In the universe of all 624 districts that are covered by the program, 200 districts entered in Phase I, which is 32% of all districts. One hundred thirty districts entered in Phase II (21%), and the remaining 46% entered in Phase III. Because we dropped some districts due to the reasons mentioned above, our sample contains 417 districts from 18 states. Of these 417 districts, 150 entered the program in Phase I (36%), 92 districts entered in Phase II (22%), and 175 districts (42%) entered in Phase III.[16] Of the 100 backward districts identified by the Planning Commission of the central government, we have 75 in our sample. We cover 130 million rural households of the 165 million reported in the 2010–2011 census. The number of worker-days created by the program in 2010 is about 2.5 billion days, and our sample covers over 2.1 billion days.

III. DATA

A. Crime Data

The data on crime are collected from the annual reports of the National Crime Records Bureau, called Crime in India (CI) from 2002 to 2012. The CI provides the total number of reported crimes committed in a year under various categories in each district. We analyze murder, kidnapping and abduction, robbery, burglary, theft, and unlawful assembly and rioting.[17] These latter events usually take place due to political and religious reasons.[18] We also group these crimes as violent (the sum of murder, kidnapping, and robbery) and property (the sum of theft and burglary). Crime rates are calculated per 100,000 residents, using district populations. Complete definitions of crimes (according to the Indian Penal Code) are provided in the Supporting information (Appendix A) and the time-series behavior of violent and property crimes in all districts is displayed in Figure 1. Descriptive statistics are provided in Table 1. Although it is problematic to compare crime rates across countries because of differences in classification and reporting, murder is one particular crime which is recorded presumably accurately in most countries (Soares 2004). The murder rate in India is about 3 per 100,000 population and there are substantial differences in murder rates between countries. For example, the murder rate is 9.2 in Russia, 4.7 in the United States, and 1.2 in France. Among Asian countries the murder rate ranges from 9.0 in Kazakhstan to 0.2 in Singapore.[19] The kidnapping rate in India is about 2.5, which is one of the highest in the world (UNODC). There are about 5.5 incidents of unlawful assembly and rioting per 100,000 population. Most crimes in India, however, are subject to underreporting

15. Coincidentally, by excluding districts which have undergone divisions between 2001 and 2012 we also exclude the districts that are most affected by the Maoist insurgency. Only 29% of deaths caused by Maoist violence took place in the districts we analyze (South Asian Terrorism Portal—www.satp.org).

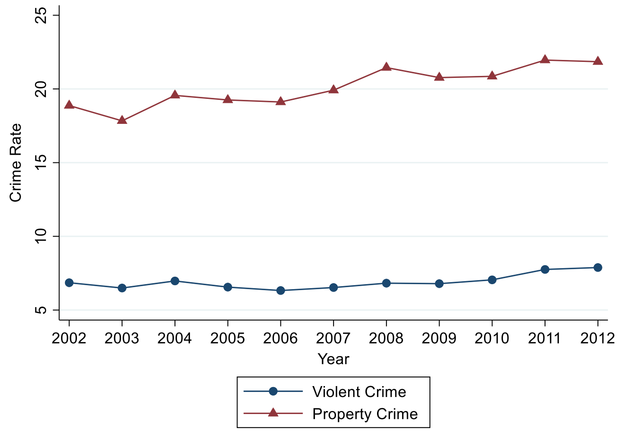
16. Of the 206 districts that we lose, 125 are dropped because we have to eliminate the state with which they are affiliated for the reasons mentioned above, while 81 districts are lost due to re-division.

17. When multiple charges are filed against a single perpetrator, only the most serious offense is reported in the data. For example if a victim is murdered while being robbed, then the crime records will only show murder and not robbery.

18. There are other crime types, including banditry, criminal breach of trust, cheating, counterfeiting, arson, hurt, dowry death, molestation, sexual harassment, cruelty by husband and relatives, importation of girls, causing death by negligence. We do not analyze these crimes primarily because reporting is negligible.

19. Data from U.S. Bureau of Justice Statistics, and United Nations Office on Drugs and Crime.

FIGURE 1
Time-Series Behavior of the *Violent* and *Property* Crime Rates of all Districts



(Dreze and Khera 2000)[20] but following the standard approach we use the logarithms of crimes as dependent variables. Under the assumption that reported crime rates are proportional to true crimes rates, the logarithm of the reported crimes are good proxies of the actual crime rates (Ehrlich 1996). Furthermore, the time period of the analysis is short enough that the reporting rates are not expected to have changed systematically; nor are they expected to be impacted by the MGNREGA program.

Crime data are available at the district level, but they are not categorized as rural versus urban crime, while the MGNREGA program is implemented in rural areas. This is not an issue regarding big metropolitan cities which form their own separate districts (such as Kolkata), or big cities that contain multiple urban districts within them (such as Mumbai). These metropolitan cities are not part of the MGNREGA program and therefore they are not included in the analysis. On the other hand, almost all districts, even those that are highly rural, contain some urban areas (such as small cities and district towns). Because crimes are recorded at the district level, our crime data contain offenses committed both in urban and in the rural areas of a district. Urban areas are different from rural areas in many dimensions including population density and income, and districts that have bigger urban areas are expected to have more crime per capita. Such districts are also less intensely impacted by the MGNREGA program because of their smaller rural populations, thereby weakening the estimated impact of MGNREGA on crime.[21] To account for urbanization differences between districts, in some empirical models we control for the percentage of

TABLE 1
Summary Statistics of District-Level Crimes

Crime Rates Per 100,000 People	**2002–2005**	**2006–2012**	**2002–2012**
Murder	3.10	2.84	2.93
	(1.62)	(1.38)	(1.47)
Kidnapping	1.97	2.81	2.50
	(1.59)	(2.19)	(2.04)
Robbery	1.51	1.63	1.59
	(1.37)	(1.63)	(1.54)
Riot	5.70	5.43	5.52
	(5.81)	(8.62)	(7.72)
Burglary	8.28	7.59	7.85
	(7.18)	(6.35)	(6.67)
Theft	17.19	19.51	18.67
	(15.03)	(18.20)	(17.15)
Violent	6.59	7.27	7.02
	(3.43)	(3.74)	(3.65)
Property crime	26.98	28.73	28.10
	(21.22)	(23.82)	(22.92)
N	1,664	2,911	4,575

Note: Standard deviations are in parentheses.

20. This is especially the case for crimes against women, for petty crimes, and for crimes which are difficult to prove such as “cheating or breach of trust.” Police stations will often discourage complainants from filing a complaint in case of small crimes just to decrease their work-load.

21. It is also the case that the location of a crime may not coincide with the residence of the perpetrator, and some of the crimes committed in urban localities may be perpetrated by a transient from a rural area.

urban population in each district. Importantly, we also derive the relationship between the impact of the program on district-level crime (which we estimate econometrically) and the impact of the program on district's *rural* crime. Using this relationship we are able to provide bounds of the program's impact on rural crime. This procedure is explained in the Supporting information (Appendix C).

B. Data from the Census of India

The data on district level variables such as demographics, social, and economic indicators are collected from the Census of India. We use data from the censuses of 2001 and 2011. District level demographic variables include total population, total number of households, number of rural households, population density, percentage of Scheduled Tribal people in total population, percentage of Scheduled Caste in total population, and percentage of urban population. Scheduled Castes and Scheduled Tribes are official designations assigned to various historically disadvantaged indigenous people in India as per the constitution of India. According to the Census of 2010–2011 the Scheduled Castes and Scheduled Tribes constituted about 16.6% and 8.6% of India's population, respectively.

The social indicator variables obtained at the district level are the literacy rate and the sex ratio. The economic indicators at the district level are the percentage of houses that have been classified to be in "good condition" by census data collectors, the percentage of households that use electricity as the main source of lighting, the percentage of households that own a television, and the percentage of households that own a motorcycle or scooter. We also collected data on the percentage of agricultural workers in the total working age population. Because the census data are only available for the 2 years 2001 and 2011, we interpolate the data for the other years.[22] Summary statistics of these data are presented in the Supporting information (Table B-1). The difference in the mean values of the variables between the three phases is statistically significant for a number of variables as such urbanization, literacy, population density, proportion of workers involved in agricultural work, and so on. This is expected because poorer districts were prioritized in the implementation of the program and Phases I and II consist of poorer districts compared to Phase III. Nevertheless, we show that the parallel trend assumption is not violated despite this nonrandom selection of districts. We also show that the results are insensitive to the inclusion or exclusion of the control variables.

C. MGNREGA Data

The data on the employment intensity of the MGNREGA program in each district are obtained from the Ministry of Rural Development (MoRD). Each district reports the annual number of jobs in worker-days generated under the program to the MoRD. We deflate the number of total worker-days generated in each district in a year by the number of rural residents in that district in that year to obtain the intensity of the program for a particular year. Deflating employment by the number of rural households provided the same inference. Panel A of Table 2 shows that the program generated an average of 3.4 worker-days per rural resident per year in Phase I districts. As described in Section II, this group by design contains the poorest districts of the country. The average worker-days employed per rural household is about 2 in Phase II districts, and it is 1.6 in Phase III districts. Panel B of Table 2 displays average annual worker-days of employment generated by the program per rural household. On average 15.4 days of work has been generated per rural household under the program and the same pattern exists: more employment is created in Phase I districts, followed by Phase II and Phase III districts. Thus, Table 2 indicates that, not surprisingly, households and residents of poorer districts have worked at a higher rate under the program.

IV. EMPIRICAL IMPLEMENTATION

We employ a district level panel spanning the years 2002–2012 to identify the impact of employment, generated by the MGNREGA program, on crime. There is variation between districts in the year of introduction of the program, and there are differences in the intensity of implementation of the program across districts. More specifically, we exploit the fact that

22. We restrict our analysis to the period 2002–2012 as we use the Census of 2001 and 2011 to identify the geographical boundaries of the districts. Indian districts undergo changes in boundaries periodically and if we extended our sample before 2001 we would had to depend on the Census of 1991 to identify the districts. This would have led to the loss of a large number of districts from our sample due to boundary changes. We exclude 2001 as the district level crime data for 2001 is based on the district demarcation defined in Census of 1991.

TABLE 2
Intensity of the MGNREGA Program

Panel A: Annual worker-days of employment per rural individual	
Phase I Districts (2006–2012)	3.41
Phase II Districts (2007–2012)	2.12
Phase III Districts (2008–2012)	1.57
All Districts after their introduction of the program.	2.47
All Districts in the sample period (2002–2012)	1.34
Panel B: Annual worker-days of employment per rural household	
Phase I Districts (2006–2012)	19.81
Phase II Districts (2007–2012)	12.98
Phase III Districts (2008–2012)	11.57
All Districts after their introduction of the program.	15.37
All Districts in the sample period (2002–2012)	8.30

Note: Phase I, Phase II, and Phase III identify the districts which were selected for the implementation of the program in 2006, 2007, and 2008 respectively.

districts are enrolled in the program in three consecutive years, and that employment intensity of the program has varied both between districts and within districts over time.[23]

We estimate versions of Equation (1)

$$y_{itc} = \alpha_c + \beta_c(MGNREGA_{it}) + X'_{it}\psi_c + \delta_{ic} + \mu_{stc} + \varepsilon_{itc}, \tag{1}$$

where y_{ict} is the logarithm of crime type c per 100,000 people in district i in the year t, where c stands for crime type such as murder, burglary, kidnapping, riots, and so on. $MGNREGA_{it}$ represents the intensity of the MGNREGA program in district i in year t. It is measured as the number of worker-days generated under the MGNREGA program per rural resident in district i and year t.[24] X_{it} is a vector of district-specific demographic and socioeconomic controls such as the literacy rate, sex ratio, population density, urbanization, percentage of Scheduled Caste in district population, percentage of Scheduled Tribe in district population, and district-specific controls for economic condition such as electricity usage, housing conditions, percentage of houses with a television set, percentage of households having two wheelers (motorcycles or scooters), and percentage of workers involved in agriculture in the district.

The inclusion of these variables is justified based on economic theory of crime.[25] Inclusion or exclusion of these variables had trivial effects on the results.

In Equation (1) δ_i stands for a vector of district dummies, and ε_{it} is the error term. District dummies control for time-invariant factors that may affect crimes at the district level, such as institutions and culture which do not change in short periods of time. Some districts may have higher propensity for criminal activity because of a variety of reasons. For example, the efficiency of public institutions may vary between districts. District dummies also control for potentially differential tendency of crime reporting; μ_{stc} are state-by-year fixed effects. Standard errors are clustered at the treatment-district level. The models are estimated both with and without weighting by district population, which provided very similar results.

Deterrence indicators such as the arrest rate or the police force are available at the state level, not at the district level. However, the size of the police force is not related to the MGNREGA program, because as mentioned earlier, MGNREGA program is funded by the central government. Therefore, program spending has no impact on the state budget; hence it cannot influence state spending on police, and time invariant differences in law enforcement are absorbed by the fixed effects.

The program provides employment only in rural areas of a district, without impacting the urban areas of the same district. Thus, if the program has an impact on crime, it would influence rural crime, but it should have little or no

23. If all districts had entered the program in the same year (being treated at the same time), it would have been a more challenging task to attribute the impact of the treatment to the program because other nation-wide factors, coincident with the program, could have been responsible for the treatment effect. Staggered treatment reduces this concern.

24. In alternative specifications we used employment generated by MGNREGA per rural household, and obtained the same inference.

25. For example, we control for the literacy rate at the district level, because human capital, approximated by average education of the district, is expected to impact crime (Lochner and Enrico 2004; Machin, Marie, and Vujic 2011). Housing conditions, ownership of motorcycles and scooters, having a TV set in household are indicators of household wealth in India, and they are expected to be related to crime through various channels. For instance, to the extent that these items are indicators of wealth, they should be negatively related to crime. On the other hand, availability or TV sets, motorcycles and scooters may increase the opportunities for theft of these items. We also control for the sex ratio because male-biased sex ratio is shown to be an indicator of systemic cultural bias against women (Carranza 2014; Dyson and Moore 1983; Sen 1990) and such cultural traits may be correlated with other harmful behavior, including the propensity for delinquency.

impact on urban crime.[26] The dependent variable y_{ict}, however, measures the crime rate (for type c crime) in the entire district i because crime data are not broken down by urban versus rural crime. This means that the estimated coefficient β_c in Equation (1) captures the impact of the program on district's total crime (committed in both urban and rural locations) for that crime category c. Nevertheless, as explained in the Supporting information (Appendix C), we can place bounds on the impact of the program on rural crime.

A. *Difference-in-Difference Specification*

It can be argued that the specification depicted by Equation (1) may generate biased estimates of the impact of employment generated by MGNREGA if the capability of local government is correlated with both employment produced by the program and criminal activity. For example, government corruption of the district may impact the reported worker-days and it may also be related to crime. Although the model shown in Equation (1) includes district fixed effects to absorb such unobservables, we nevertheless estimate an alternative specification that identifies the impact of intention-to-treat. Here we use district assignment as the variable of interest. In this specification, shown in Equation (2), $Treatment_{it}$ is a dichotomous variable to indicate whether the district i was enrolled in the program in year t; ζ_{ic} stands for district fixed effects for crime c, and λ_{stc} represents state-by-year fixed effects. Following the design of Imbert and Papp (2015), Phase III districts, which are enrolled in the last stage of the program, are used as controls, and the model is estimated using data until 2008. The difference-in-differences estimate is given by τ.

$$(2) \qquad y_{itc} = \pi_c + \tau_c(Treatment_{it}) + X'_{it}\Omega_c + \xi_{ic} + \lambda_{stc} + u_{itc}.$$

V. ANALYSIS OF PREPROGRAM TRENDS

The analysis of the program's impact on crime is problematic if districts are given priority to enter the program based on their existing crime rates. That is, it is possible that the crime rates were rising (or declining more slowly) in poorer districts in comparison to other districts. In that case, differential trends in crime rates between poor and nonpoor districts may have prompted the central government to enroll poorer districts earlier. In other words, it is possible that the poor districts which entered the program in Phase I (rather than in Phase II or in Phase III) did so because their crime rates were moving differently in comparison to late-entering districts. Relatedly, if districts which were selected in the earlier phases of the program had falling crime rates (relative to the districts that were enrolled later) even before the policy had been implemented, at least some of the effect of this trend would be attributed to the policy.

In the Supporting information (Appendix D) we present graphs and discuss the trends in crime rates in districts before and after their entry to the program. We group the districts by their phase of entry (Phase I, II, or III) and calculate the crime rates for each group in each year. We combine individual crime categories into violent (the sum of murder, kidnapping, and robbery) and property (burglary and theft), although we obtained the same inference by analyzing individual crimes categories separately. The graphical evidence presented in the Supporting information (Figures 3–13) do not indicate that the districts are chosen by the central government to enter the program because their crime rates were rising.[27] The graphs are consistent with the hypothesis that the districts which were chosen to enter the program in earlier years would have had similar changes in crime rates in comparison to districts that entered the program in later years. When we test econometrically whether the crime rates were diverging before the entry into the program, we find no evidence for differential pre-trends in the crime rates before the districts enter the program. This is discussed later in the paper.

VI. RESULTS

The results of the specification for various crimes are presented in Table 3. These specifications do not include control variables. The estimated coefficient of worker-days per rural resident generated by the MGNREGA program is negative for all crimes. The magnitudes imply

26. Urban crime can be impacted if the MGNREGA program affects migration from rural to urban areas. Research, however, finds no evidence of such an impact on migration (Ahuja, Chauhan, and Chaudhary 2011; Kumar and Maruthi 2011; Singh 2013).

27. In Figure 12 in the Supporting information, the assumption of parallel pre-trends of violent crime rates in high intensity and low intensity Phase 1 districts before 2005 appears to be violated, although this is due to the jump in violent crime in low intensity Phase 1 districts in 2004.

TABLE 3
The Impact of Employment Generated by MGNREGA on Crime. Without Control Variables

	Murder	Kidnapping	Robbery	Riot
Worker-days per rural resident	−0.0051** (0.0024)	−0.0014 (0.0043)	−0.0255*** (0.0064)	−0.0097 (0.0074)
District fixed effects	Yes	Yes	Yes	Yes
State-by-year fixed effects	Yes	Yes	Yes	Yes
N	4,567	4,538	4,485	4,350
	Theft	**Burglary**	**Violent**	**Property**
Worker-days per rural resident	−0.0063* (0.0036)	−0.0084* (0.0049)	−0.0122*** (0.0027)	−0.0085** (0.0033)
District fixed effects	Yes	Yes	Yes	Yes
State-by-year fixed effects	Yes	Yes	Yes	Yes
N	4,575	4,570	4,570	4,575

Notes: The dependent variables are natural logarithms of crimes per 100,000 people in a district in a year. Violent = murder + kidnapping + robbery; property = theft + burglary. Standard errors are clustered at the treatment district level. *$p<.1$, **$p<.05$, ***$p<.01$.

that one additional worker-day of employment per rural resident generated by the program reduces murders by 0.5%, robberies by 2.5%, thefts by 0.6%, and burglaries by 0.8%. When these offenses are grouped as violent and property crimes, the estimated impacts are also negative and significantly different from zero, and they reveal that an extra worker-day of employment per capita reduces violent crimes by 1.2% and property crimes by about 0.9%. Adding all control variables to the model had no meaningful impact on the estimated coefficients of MGNREGA, although the coefficients of theft and burglary became −0.004 and −0.006, respectively, and lost their statistical significance at conventional levels. An increase in per capita employment by 1 day represents a rise by almost 75%. Thus, the estimated impact of the program on crime is modest.

Fraud and corruption in implementation can result in disparities between official data and the situation on the ground. Although the program has an built-in auditing system based on independent private auditors, incidences of corruption, poor implementation by local governments, capturing of the benefits of the program by less deprived households through political manipulation and incorrect targeting of the program such that the benefits do not reach the poorest households has proven to be widespread (Dutta et al. 2012; IBN Live 2013; Jha et al. 2009, Shariff 2009, etc.).[28] This indicates that actual employment created by the program may differ from reported employment, and that some beneficiaries of the program may be different from the target population, which in turn implies that the effects we identify may be underestimated.

It should be noted that "the job cards" that allow individuals to work under the program are distributed by the local *Panchayat* (village council) office to families who are residents of the village. To qualify as a resident, a person must be registered as a voter or must have some other government documents which proves the same. Therefore, it is unlikely that the program will induce migration from nontreatment/low-intensity districts to treatment/high intensity districts. In fact, the opposite seems to be the case. The MGNREGA program has been found to reduce seasonal migration (Ahuja, Chauhan, and Chaudhary 2011; Das 2015; Dodd et al. 2018; Parida 2016). Most migration takes place from rural areas to big urban cities, and such areas are excluded from our analysis. To the extent that the program curtailed migration, potential migrants, who are poor and arguably more crime-prone, remained in rural areas instead of migrating to big cities. Under this scenario, the true impact of the program on crime reduction would be underestimated.

A. *Potential Impact of Rainfall*

If rainfall is negatively correlated with joblessness and poverty in India, this would imply that a decline in rainfall would generate a higher demand for jobs under the MGNREGA program. Put differently, to the extent that the program aims to provide insurance against joblessness (at least temporarily) and given that jobs under the program is acquired on demand, it could be that adequate rainfall in a district would increase crop production and diminish the demand for the program in that district. We ran a regression where district-level annual job creation per rural resident under the MGNREGA program was regressed on the same set of district attributes as described in Equation (1), as well as the logarithm of total rainfall in that district in that year. The results revealed that rainfall was negatively related to worker-days of employment, but that the impact was statistically insignificant.

28. Despite these issues, extensive surveys have shown that the program has had a sizable impact on poverty and income (Hindustan Times 2013).

This was true in models with or without control variables.

Sekhri and Storeygard (2014) analyzed district level data and reported that rainfall had a negative impact on dowry deaths (murder of a bride for bringing in insufficient dowry) in India. Iyer and Topalova (2014) showed that consumption spending was influenced by rainfall. They also ran district-level property crime and violent crime regressions and found that an increase in rainfall had a negative impact on crime. We ran the similar crime models as shown in Table 3, replacing the variable that measures district-level employment generated by MGNREGA (*worker-days per rural resident*) with the logarithm of total rainfall in the district.[29] The results showed that rainfall is not a significant determinant of crime. Half of the estimated coefficient were positive, the other half were negative, and only one of them was significant (burglary—at the 10% level).

In summary, testing the premise that rainfall can have an influence on joblessness in rural areas, we find that rainfall has no statistically significant impact on the demand for jobs under the MGNREGA program. Similarly, we find that rainfall has no significant direct impact on crime.

Finally, adding rainfall to the crime equations had no impact on the estimated coefficients of MGNREGA, with the exception of the coefficient in the burglary equation which became smaller and statistically insignificant (−0.0056, SE = 0.005).

B. Elasticities

The results in Table 3 indicate that if employment generated by the program goes up by 1 day per *rural resident*, this reduces violent crimes *in the district* by 1.2%, and property crimes by 0.9%. Because the sample mean of worker-days of employment per rural resident is 1.34, this implies crime elasticities of employment between 0.01 and 0.02. As discussed in the Supporting information (Appendix C), the coefficients of *worker-days per rural resident* reported in Table 3 are the estimates of $\widehat{\beta}_T$, which represent the impact of the program on total district crime. To recover $\widehat{\beta}_R$ (the impact of the program on rural crime), we use Equation (4A) of the Supporting information (Appendix C). In our sample 22.5% of the population lives in urban areas. Thus, we set $\theta_U = 0.225$ in Equation (4A). Under the assumption that the urban crime rate is the same as the total crime rate ($CR_T = CR_U$), Equation (4A) implies that $\widehat{\beta}_R = 1.29\widehat{\beta}_T$ which provides the lower bound for $\widehat{\beta}_R$. To obtain the upper-bound of $\widehat{\beta}_R$, we use the crime rates in 37 cities in India as an estimate for the urban crime rate in all district (CR_U).[30] Crime rates in these big cities are higher than the crime rates in towns and small cities; thus attributing big city crime rates to CR_U in Equation (4A) provides an upper bound for $\widehat{\beta}_R$. Using this procedure, we generate bounds for $\widehat{\beta}_R$. For example, in case of robbery we can bound $\widehat{\beta}_R$ between −0.03 and −0.1, which in turn provides the elasticity of rural robbery with respect to employment generated by the program in the range of −0.045 to −0.131. Elasticities of all crime categories are calculated similarly and reported in Table 4.

TABLE 4
Elasticity of the Crime Rates with Respect to Worker-Days Generated by. The MGNREGA Program

	Murder	**Kidnapping**	**Robbery**	**Riot**
Upper bound	−0.010	−0.003	−0.131	−0.019
Lower bound	−0.010	−0.002	−0.045	−0.017
	Theft	**Burglary**	**Violent**	**Property**
Upper bound	−0.147	−0.019	−0.035	−0.046
Lower bound	−0.010	−0.014	−0.021	−0.015

Notes: Crimes are defined as the number of offenses per 100,000 people in a district in a year.

Using state panels from the United States Raphael and Winter-Ebmer (2001) report that the elasticity of property crime with respect to the unemployment rate is about 0.14. Corman and Mocan (2005) report that in New York City the elasticity of burglaries with respect of unemployment rate is about 0.17, and it is about 0.14 in case of motor vehicle thefts. Öster and

29. The rainfall data are obtained from the University of Delaware website which compiles monthly terrestrial rainfall data obtained from weather stations across the globe. The rainfall is measured in millimeters and is available for every latitudinal and longitudinal grid of 0.5° by 0.5°. We use a GIS map to identify the centroid of each district and find its latitudes and longitudes. Then we match the latitudes and longitudes of each district centroid with the nearest rainfall database grid. Following Iyer and Topalova (2014) we define our measure of rainfall as the logarithm of total annual rainfall for each district.

30. The crime rates in India's biggest 37 cities are as follows: murder: 2.82, kidnapping: 4.99, robbery: 5.22, burglary: 15.54, theft: 78.55, riots: 7.74, violent crime: 16.62, and property crime: 94.09.

TABLE 5
The Impact of Enrollment in the MGNREGA Program (2002–2007), Difference-in-Difference Models

	Murder	**Kidnapping**	**Robbery**	**Riot**	**Theft**	**Burglary**	**Violent**	**Property**
Treatment	−0.0161	−0.0234	−0.0827*	−0.0063	−0.0519*	−0.0271	−0.0364*	−0.0574**
	(0.0206)	(0.0311)	(0.0493)	(0.0467)	(0.0269)	(0.0445)	(0.0200)	(0.0252)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,494	2,472	2,458	2,388	2,496	2,493	2,495	2,496

Notes: The dependent variables are natural logarithms of crimes per 100,000 people in a district in a year. *Treatment* is a dichotomous indicator of a district's enrollment in the MGNREGA program (=1 for all years during enrolment). Violent = murder + kidnapping + robbery; property = theft + burglary. Standard errors are clustered at the treatment-district level.
$^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

Agell (2007) find that in Sweden the elasticity of burglary with respect to unemployment rate is 0.25, and it is 0.35 in case of auto theft. The results of Buonanno and Montolio (2008) imply a property crime elasticity with respect to youth unemployment of 0.13 in Spain. Altindag (2012) employs panel data from 33 European countries and finds that the property crime elasticity of the unemployment rate is in the range of 0.20–0.32. The elasticities we report (displayed in Table 4) are significantly smaller than those reported in developed countries, but it should be noted that they are not directly comparable to unemployment elasticities because our elasticities pertain to an increase in short-lived employment at minimum wage.

C. Difference-in-Difference Estimates

As an alternative specification we estimated the model depicted by Equation (2). Here we use the MGNREGA assignment as the key explanatory variable, rather than worker-days. More specifically, in Equation (2) *Treatment* is a dummy variable that takes the value of one of it the MGNREGA program is in existence in a district in a year, and zero otherwise As discussed earlier, poorer districts are enrolled earlier than less-poor districts by design. Nevertheless, we employ the specification shown in Equation (2), which identifies the difference in differences between districts that are enrolled in the first two phases (poorer districts), and compere them to districts that are not enrolled, to provides additional evidence on the impact of the program. The results, displayed in Table 5, are consistent with the ones reported earlier. These "intent to treat" effects show that enrollment in the program has no statistically significant impact on the murder rate. On the other hand, robberies, thefts, and aggregate property and violent crimes are negatively impacted, as was the case in previous regressions. Specifically, the last two columns of Table 5 show that the violent crime rate and the property crime rate went down by about 4% and almost 6%, respectively after a district has enrolled in the program.[31]

D. Crowding Out, Poverty, and Other Extensions

One issue which can affect the interpretation of our results is a potential crowding-out effect of the MGNREGA program. If the Indian government has eliminated other employment or welfare programs to finance the MGNREGA, this would imply that employment generated by MGNREGA simply replaced other programs, rather than having created new employment. The MGNREGA has in fact replaced the Food for Work Program of India, which has been discontinued since the inception of the MGNREGA. The Food for Work Program was a centrally funded program, implemented in the rural sector since 1977. In the 1999–2000 fiscal year that program generated 500 million person days of work which was the highest since its inception (Bhalla 2011). In comparison, the MGNREGA generated on average 2.5 billion person days of work in 2012. Thus, even though the introduction of the MGNREGA allowed discontinuation of another welfare program, MGNREGA is five times larger than the program it replaced.

Alternatively, the program could have tightened the labor market by absorbing workers who otherwise would have worked in the private sector, thereby increasing the cost of labor

31. Adding control variables had almost no impact on the estimated coefficients or their standard errors. The coefficient (standard error) of robbery was −0.101 (0.050). They were −0.059 (0.027) in theft, −0.043 (0.020) in violent crime, and −0.059 (0.025) in property crime.

and crowding-out private investment in the rural economy. Zimmermann (2015) found that the program had no impact on private sector wages or private sector employment. Imbert and Papp (2015), on the other hand, reported a rise in private sector wages after the implementation of the program. They also reported a decline in the employment of unskilled workers in the private sector by the same amount as the rise in the employment in the MGNREGA program, leaving the unemployment rate unchanged. The implication is that, although wages may have risen, this has not led to a rise in unemployment; thus generating an increase in the welfare of unskilled workers. In summary, although there is no evidence of a substantial curtailment of other government programs due to the implementation of MGNREGA, crowding-out of private employment is likely.[32] However, the net effect of the program is an increase in the welfare of unskilled workers in rural areas because the program seems to have had considerable positive effects on rural poverty with no tangible impact on the unemployment rate (Imbert and Papp 2015; Klonner and Oldiges 2014).

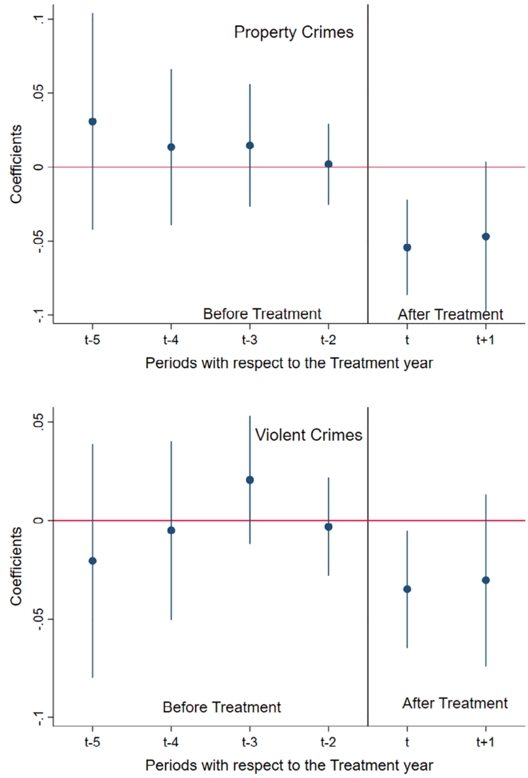
Although the graphical analyses presented in the Supporting information (Figures 3–13) do not reveal any indication of differential pre-trends between treatment and control districts, we performed formal tests to investigate the behavior of crime rates before the initiation of the program. Specifically, we estimated the specification depicted by Equation (3).

$$(3) \qquad y_{it} = \alpha_i + \mu_{st} + \sum_{t=-5}^{-2} \pi_t (D_t * D_{Treat}) + \sum_{t=1}^{2} \varphi_t (D_t * D_{Treat}) + \varepsilon_{itc}$$

where D_{Treat} is a dummy for the treated group (Phase I and II districts), D_t is a dummy for time period (year). The coefficients π_t pertain to four preprogram periods ($t = -5, \ldots, -2$). Time period $t-1$ signifies the year before the entry in the program of district i, and it is the left-out category. Time periods 1 and 2 signify the two postenrollment years, and φ_t represents the coefficients of the postenrollment periods. Under the parallel re-trends assumption, π_t should not be different from zero. The results are presented in graphical form in Figure 2. The estimated coefficients (dots) are displayed along with their 95% confidence intervals. As Figure 2 reveals, the estimated impacts are not different form zero for periods before the entry to the MGNREGA program for either violent or property crimes, and they are negative after the entry, providing support for the parallel trends assumption.

32. The fiscal cost of the program does not seem to have triggered a change in the taxation scheme (Chakraborty 2007; Rukmini 2015).

FIGURE 2
Treatment Coefficients for Each Period Before and After the Treatment Year



Poverty is positively correlated with criminal propensity, and poverty should induce higher demand for jobs under the program. Given that districts' level of poverty cannot be measured fully with the available data, including the poorest (most backward) districts in the analysis should weaken the estimated impact of the program on crime. To investigate this conjecture, we removed the Phase I entrants, which include the most backward districts with higher employment creation (see Table 1), from the sample and re-ran the regressions using Phase II and Phase III districts only. The results are presented in the Supporting information (Table E-1). In comparison to those reported in Table 3, the estimates are larger in absolute value for almost all crime categories.

If poorer districts, which were selected to enter the program earlier, caught-up in terms of economic development with the more developed districts during the same period and if development decreases crime our results will be biased. To mitigate the issue we estimated the difference-in-difference specification, controlling for phase-specific time trends which did not alter the results. These results, reported in the Supporting information (Table E-2), show that the inference is not altered,

We also investigated the heterogeneity of the results by urban versus rural districts by dividing the sample into two groups: districts that rank above-median in the urbanization rate and those who are at the bottom 50%. The results, presented in the Supporting information (Table E-3), show that there is no systematic difference between highly urban and less urban districts regarding the impact on crime of an increase in employment generated by the program. While the impact of worker-days per rural resident is bigger in case of theft and aggregate property crime in less-urbanized districts, the impact on robbery and aggregate violent crime is bigger in more urban districts.

Finally, as discussed by Goodman-Bacon (2018) the difference-in-differences model in Equation (2), where different districts are treated in different time periods, produces an estimate that is a weighted average of all possible difference-in-differences estimators that compare groups to each other that differ in timing of the program entry. To provide some insight into the relative importance of different phases of entry, we estimated four separate difference-in-differences models, the results of which are provided in the Supporting information (Table E-4). The top panel employs Phase I and Phase II districts, whereas the former is the treatment group and the latter is the control group. The estimation period is 2002–2006. Phase I districts entered the program in 2006, while none of the Phase II districts were treated between 2002 and 2006. The results show that enrollment in the program of Phase I districts lowered thefts, violent crimes, as well as property crimes in comparison to Phase II districts. The second panel of Table E-4 uses the period 2002–2007 and considers Phase III districts as the control group as this group does not enroll in the program until 2008. The same inference is obtained: enrollment in the program reduces crime in Phase I districts in comparison to Phase III districts. The third panel of Table E-4 in the Supporting information uses the period 2002–2007 and performs the same exercise using Phase II districts as the treatment group (which entered the program in 2007) and Phase III districts as the control group (which are not enrolled between 2002 and 2007). The estimated coefficients are negative in all crime categories, but the magnitudes are smaller and none is statistically significant. Finally, the bottom panel uses the period 2006 and 2007 when the Phase II districts entered the program while the Phase I districts had already been in the program from the previous year. Thus, in this model Phase II is the treatment group and Phase I serves as the control. The sample is small in this specification as it spans only 2 years. The estimated coefficients are negative in all crime categories, but most are not statistically significant. Thus, Table E-4 in the Supporting information suggests that much of the impact of the program, as measured by the difference-in-difference coefficient, is due to its influence on Phase I districts, which entered the program first.

VII. SUMMARY AND CONCLUSION

Since 2006 India has been implementing a massive public works project titled the MGNREGA. The program aims to provide employment to rural households on demand with the proviso that each household is entitled to 100 days of work per year at minimum wage. MGNREGA is primarily designed to reduce poverty and joblessness that emerges because of decline in agricultural output. The program generates more than 2.5 billion person-days of employment to more than 55 million households each year. In this paper we investigate the impact on employment, generated by MGNREGA, on crime.

The program has been implemented in three phases, where at least one district from each state participated in each of the three phases. The first group of districts were enrolled in 2006, the second phase followed in 2007, and the entire country was covered in 2008. The decision about which districts from which states to enroll is made, and the funding of the program is provided by the central government of India, indicating that local governments had no influence on implementation. We show in the paper that pre-program crime trends were similar between the district that entered the program earlier and those who entered later. The same is true for very poor districts and other districts.

Using a district level panel spanning 2002–2012, and exploiting the heterogeneity in the timing and intensity of the program across districts, we identify the impact of employment generated by the program and various types of crime. In addition to standard crime categories such as burglary, robbery, and murder, we are able to analyze kidnapping and unlawful assembly and riots. The fact that the goal of the program is to provide temporary relief from poverty and that crime is not a concern of the program or its implementation provides a framework where standard endogeneity concerns are avoided, which also indicates that crime reduction is a positive externality of this public works project.

An increase in employment due to the program has a negative impact on crime, with elasticities in the range of −0.02 to −0.05 for property crimes, and in the range of −0.02 to −0.04 for violent crimes. Crimes such as kidnapping and riots are not impacted.[33]

We also estimate alternative models where we obtain difference-in-differences estimates based only on districts' year of enrollment in the program. The results from these models are consistent with the ones obtained from the analysis of program intensity. Specifically, these specifications reveal that enrollment in the program reduced violent crimes by 4% and property crimes by 6%. While the magnitude of the impact of the MGNREGA program on crime is small, this still constitutes an indirect benefit to this developing economy which should be taken into consideration in a full scale cost–benefit analysis.

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33. Kidnappings are rare events, but riots are not (see Table 2). Thus, although the lack of impact in case of kidnapping could be attributed to lack of variation, the same in not true for riots which are politically motivated events.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Appendix S1. Supporting Information