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## ESTIMATING TRANSFER MULTIPLIER USING SPENDING ON RURAL DEVELOPMENT PROGRAMS IN INDIA

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# Estimating Transfer Multiplier using Spending on Rural Development Programs in India

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2nd February 2017

## Abstract

Rural development programs in India are implemented for a variety of reasons. A key question is whether such transfer spending by the government is consequential for the local economic activity. This chapter estimates the multiplicative effects of rural transfer spending on state agricultural output using a novel dataset of state-wise expenditure on all major rural development programs that were operational between 1980-2010. Using government reports as narrative evidence we show that the principal motivation to introduce a new scheme is either (i) to replace old inefficient programs or (ii) to address a deep-rooted social or economic issue that has not been addressed by any existing program. Importantly, the introduction of a new scheme is largely independent of the current or prospective output fluctuations. Using this narrative evidence we isolate the “introductory variation” that occurs every time a new program is introduced as a measure of change in transfer spending that is exogenous to local output fluctuations. The results suggest that local variations in rural transfer spending can be quite consequential for the local economic activity in rural areas.

**Keywords:** Employment Guarantee, Wage Rigidity, Welfare, NREGA, India

**JEL Classification:** E62, H53, I38, O13

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# 1 Introduction

The series of stimulus packages rolled out globally following the recent financial crisis have reinvigorated the debate on the effectiveness of fiscal interventions. In response, there is a renewed interest in understanding the size of “the multiplier” – measured as the effect on economic activity due to exogenous fiscal impulses. While most of the related literature has focused on the aggregate effects at the national level, several recent contributions have addressed the local output effects of such policy interventions.

Further, a majority of studies estimate the multiplicative effect of a specific component of government spending – *government purchases* – defined as government investment plus consumption. While many useful lessons have emerged from this literature, government transfers have received surprisingly little attention. This represents a significant disconnect between the research and its policy relevance as most of the recent increase in government spending across the globe can be attributed to government transfers. Oh and Reis (2012) report that transfers accounted for more than 75% of the increase in government spending in the US between 2007 and 2009, comparable to the OECD median of 64%. They further conclude that most of the increase in transfer spending during this period was discretionary.

The heavy reliance on transfer spending as a stimulus instrument may be motivated by the benefits of well-targeted transfers which can have expansionary effects on output while addressing distributional concerns.<sup>1</sup> Also, while new investment projects may involve substantial decision and implementation delays, injecting funds into already existing social projects allow for a timely response to a crisis. As the *Review of the Economy, India 2008* notes: “in the prevailing situation ... speedy implementation of already funded projects at the Central and State levels is important for the fast revival of the economy.” It is therefore not surprising to note that along with the introduction of the fiscal stimulus package in India during early 2009, transfer spending in the form of loan waivers, subsidies, and employment guarantees also peaked at the same time. Hence, to the extent social transfers are used as a stimulus instrument, it becomes important to understand their effect on local economic activity.

In this paper, we estimate the local multipliers corresponding to spending in rural

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<sup>1</sup>Woodford (1990) shows that transfer spending can be expansionary if it alleviates liquidity constraints which result in higher investment and output. Using a heterogeneous agent model, Oh and Reis (2012) show that while transfers across households can increase consumption and labor supply for some households and lower it for others, the former effect can dominate if transfers are well targeted. Using a New Keynesian model with savers and borrowers, Bilbie, Monacelli, and Perotti (2013) show that transfer multipliers are positive under sticky prices and small and close to zero when prices are flexible. Giambattista and Pennings (2016) use a New Keynesian model with constrained and unconstrained agents to show that transfer multipliers can be comparable to purchase multipliers if monetary policy accommodates (or is unable to counter) the higher inflation generated due of transfer spending.

development programs in India.<sup>2</sup> While such programs are implemented for a variety of reasons, a key question concerns the efficacy of spending under these schemes on the local economic activity. We use a novel panel data of expenditure incurred under major rural development programs between 1980 and 2010 to construct a measure of exogenous changes in rural transfer spending at state-level. We use this measure to estimate the transfer spending multipliers for the agricultural output at state-level. Hereafter, we refer the agriculture sector multiplier at the state level just as the *state-sector multiplier*. The state-sector multiplier we estimate differs conceptually from most of the literature that estimates “local multipliers” for output or income at state, province, or county level. Our choice of state-sector multiplier stems from the important advantages it has over area specific local multipliers. The state-sector multiplier, for example, sheds light on the ability of such programs to counter local (agriculture) sector-specific shocks. This issue is even more relevant for emerging economies like India where the agricultural output is highly sensitive to weather shocks.

While we check for sectoral and spatial spillover effects of rural transfers, our focus is on the response of state agricultural output to local variations in transfer spending due to the following observations. First, given that these programs operate exclusively in the rural areas of the states, an ideal choice would be to measure the impact on local economic activity by estimating the spending effects on *state rural income*. However, unavailability of any measure on state rural income restricts such an analysis. Therefore, we focus our attention on the sector that most closely corresponds to the rural economy – the agriculture sector. As Figure 1(a) shows, nearly three-quarters of the rural workforce is employed in the agriculture sector. This underscores the heavy reliance of the rural economy in India on agriculture and allied activities.

Second, most of the programs we study indirectly support the agriculture sector by organizing public works on irrigation and land development. Further, these programs aim to assist the marginal farmers, landless households, and the unemployed workers during the lean agricultural season. Therefore, relative to other sectors, rural transfer spending most comprehensively impacts the agriculture sector. Finally, unlike most developed economies, agricultural output in India forms a substantial portion of the gross domestic product. As Figure 1(b) shows, the national agricultural output accounts for more than 20% of the aggregate national GDP for most of the sample while this ratio is as high as 35% in the early 1980s.<sup>3</sup> Hence, our measure of the state-sector multiplier is likely to capture any first order effects of rural transfer spending on local economic

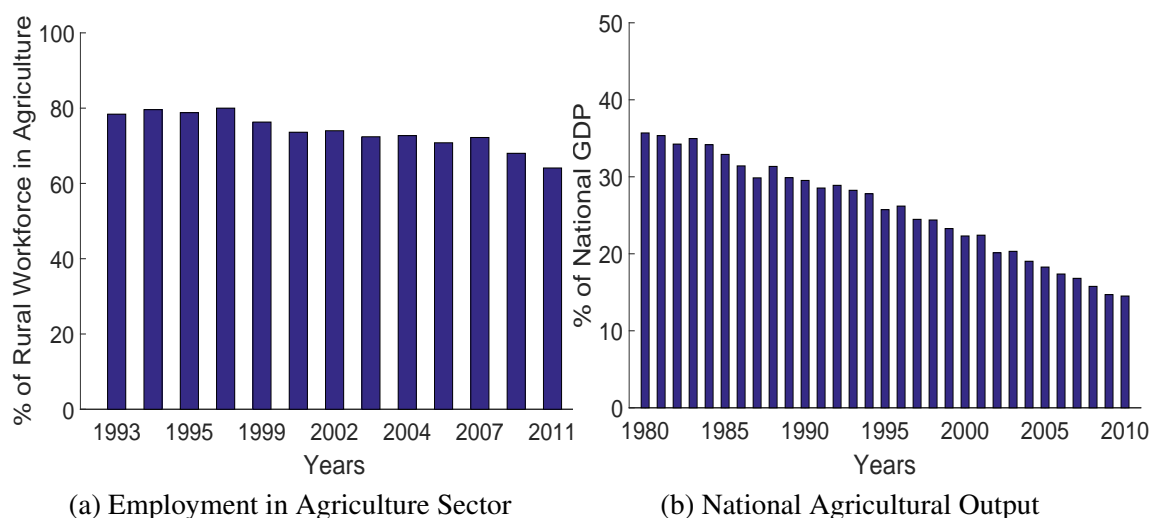
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<sup>2</sup>We interchangeably refer rural development programs as just programs or schemes.

<sup>3</sup>The correlation between growth rates of real agricultural output and total output is  $\approx 0.7$  ( $p$ -value  $< 0.01$ ).

activity.

Figure 1: Agriculture Sector in India: Employment and Output



Source: Employment data is from National Sample Survey (various rounds). Output data is from Central Statistics Office (CSO). Figure 1(a) summarizes the proportion of the employed workers in rural areas that are involved in the agriculture sector. Figure 1(b) shows the national output from agriculture and allied activities as a percentage of aggregate national GDP (at factor cost and constant prices).

Since fiscal interventions can be sensitive to cyclical fluctuations in income, the literature follows three key identification strategies to avoid biased multiplier estimates. The first is the VAR based approach that relies on structural assumptions regarding the dynamics of output and fiscal policy (Blanchard and Perotti, 2002; Mountford and Uhlig, 2009; Ilzetzki, Mendoza, and Végh, 2013). The second identification strategy is to find an instrument that provides an external source of variation in the fiscal policy that is uncorrelated with current output fluctuations. Several recent studies using sub-national data (discussed below) exploit institutional information to employ instrumental variable approach for identification.

The third strategy consists of extracting a subcomponent of spending or taxes that can be argued as independent of current macroeconomic environment. Following Barro (1981), many studies have for example argued that military expenditure in the US during major wars can be regarded as exogenous as these conflicts did not have a direct effect on the US economy and their timing was not influenced by aggregate business cycles. Ramey and Shapiro (1998) and Ramey (2011) use narrative records such as news reports in *Business Week* to identify changes in government purchases due to military build-ups and other events that are independent to the state of the economy.<sup>4</sup> Romer and Romer (2010) use US legislative records to identify tax policy changes that

<sup>4</sup>Ramey (2011) introduces another shock series using forecast errors of professional forecasters.

can be categorized as countercyclical measures or changes that were made for long-run growth or other ideological reasons. They argue the latter subset of tax changes to be uncorrelated to current macroeconomic shocks and hence use them to estimate tax multipliers.

Our identification is based on the third strategy and it most closely follows the Romer and Romer (2010) narrative analysis approach. We use narrative analysis to identify changes in transfer spending at state-level that occur during the implementation of new nation-wide programs which are not motivated by current or prospective fluctuations in local output. Using official government reports and financial statements as narrative evidence, we find that the principal motivation to introduce a new program by the central government is either (i) to address a deep-rooted social or economic issue that has not been sufficiently addressed by any existing scheme or (ii) to replace old program(s) because of the inefficiencies identified in them. Consequently, we use the regional variation in transfer spending that corresponds to the inter-state heterogeneity in implementing new programs to estimate the “state-sector multiplier”. Our identifying assumption is that the central government does not *introduce* a new scheme – like a subsidy program for rural housing in 1995 – because the states that spend a greater amount of program funds during the year of implementation are doing poorly relative to other states.

This assumption is similar in spirit to Nakamura and Steinsson (2014) who exploit the heterogeneity in state-level military spending due to military build-ups and draw-downs, except that we consider the heterogeneity in state-level transfer spending as exogenous only *during* the “introductory period” of a new program. We do not regard the broader variations in state-level transfer spending as exogenous since unlike military spending in the US, transfer spending in India can be highly endogenous to cyclical fluctuations in output. In other words, while local economic conditions may cause disproportionately higher spending in a state under a program that is well established in the system, it is highly unlikely that a new *nation-wide* program is implemented in response to a state-specific shock.

We use our narrative records to identify the size, timing, and motivation of all major program introductions between 1980 and 2010 to construct our state-level “narrative shock series”. We regard this shock series as a measure of change in rural transfer spending that is uncorrelated with local output fluctuations. While relying on the motivation for program introduction helps us rule out reverse causality due to local shocks, an immediate concern is that our estimates may still be downward (or upward) biased if the state-level variation during program introduction is correlated with key district characteristics that may in turn influence local economic activity. To check for this,

we test and find no correlation between our shock series and the state to national rural poverty ratio – which is claimed as a key factor in the allocation of funds from the central government to the states for many programs.

Secondly, our multiplier estimates are quite robust to the exclusion of state fixed effects, indicating that the variation in transfer spending due to the implementation of a new program is largely orthogonal to state characteristics. These findings bolster our identification strategy and are also consistent with recent studies on employment schemes in India that find heterogeneity in program implementation to be likely due to idiosyncratic supply factors like political will and capability to implement rather than the demand for the program (Imbert and Papp, 2015; Bahal, 2016). Finally, the use of sub-national data allows us to estimate local multipliers after controlling for national monetary and fiscal policies, along with aggregate fluctuations using year fixed effects.

We estimate the local multiplier to be around 1.4 on impact which increases to 2.1 after including the effects of past spending. In other words, when relative per capita transfer spending in a state increases by 1% of the state’s agricultural output, the relative per capita agricultural output increases by approximately 1.4% on impact and 2.1% overall. In contrast, using the broader measure of year-on-year changes in total transfer spending at the state-level yields multiplier estimates that are an order of magnitude smaller and insignificantly different from zero, indicating a substantial downward bias in comparison to the estimates obtained from the shock series. This reaffirms our premise that while state-wise heterogeneity in expenditure during the introduction of a new program is largely due to variations in program implementation that are unlikely to be related to local economic conditions, the same cannot be said about the general annual variation in state-level transfer spending. We also find no relevant sectoral or cross-border spillover effects due to transfer spending in adjacent states, suggesting rural economies to be quite “insular” from each other in this regard.

Together with this study, there are several recent works that exploit sub-national data to estimate local multipliers. Acconcia, Corsetti, and Simonelli (2014) instrument temporary contractions in public spending at province-level in Italy on evidence of mafia involvement to estimate the government spending multiplier between 1.5-1.9. Serrato and Wingender (2010) use as an instrument, the census shock due to the measurement error in US census estimates to estimate the government spending multiplier to be around 1.88.<sup>5</sup> Relative to these studies, our study makes three novel contributions to

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<sup>5</sup>Fishback and Kachanovskaya (2010) study the relative multiplicative effects of government grants in US states during the New Deal using a swing voting measure as an instrument. They report an output multiplier of 1.67 corresponding to grants on public work and relief. Chodorow-Reich et al. (2012) study cross-sectional variation in Medicaid transfers in US states and estimate an output multiplier of around 2. Nakamura and Steinsson (2014) estimate an “open economy relative multiplier” around 1.5.

the literature. First, while most of the works focus on advanced economies, we provide empirical evidence on the size of local multipliers in a monetary and fiscal union for a developing country. Second, unlike most of the literature that focuses on the output and income effects of government purchases, we study government expenditures that can be largely classified as transfers. Finally, we use the narrative analysis approach to construct a measure of *state-wise* innovations in transfer spending to evaluate the state-sector effects of rural transfers.

The rest of the paper is organized as follows. Section 2 constructs the state-level shock series using narrative analysis. Section 3 presents the empirical model. Section 4 and 5 respectively discuss results and extensions. Section 6 concludes.

## **2 Narrative Analysis**

### **2.1 Sources**

The primary sources for the narrative analysis are the *Annual Reports* of the Ministry of Rural Development and *Annual Plan* documents of the Planning Commission of India (various issues). We systematically consult both these official documents to identify the major rural development programs that were operational between 1980 and 2010. The documents contain all relevant information like the date of program implementation, the principal motivation to introduce the program, and whether the new program was implemented in a phase-wise manner. The annual financial statements or progress reports in these documents provide state-wise annual expenditure data for all active programs. Our measure of total rural transfer spending at the state level is simply the aggregate expenditure incurred under all active programs in a financial year.<sup>6</sup> We hence have a panel data of 23 states for 30 years (690 observations).

### **2.2 Rural Development Programs in India**

The first step is to identify the key rural development programs that were introduced between 1980 and 2010. We limit our attention to major nation-wide programs that were implemented by the central government. We do not consider programs that existed as sub-schemes of larger programs. This is to avoid double counting of expenditure under the parent program. Also, expenditure data at the state-level is usually not available for small sub-schemes. Our objective is to study the programs whose introduction had

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<sup>6</sup>The Indian financial year starts from 1<sup>st</sup> April to 31<sup>st</sup> March.



consequential effects on transfer spending.<sup>7</sup>

Table 1 summarizes the ten major rural development programs that we study. The programs considered in our study can broadly be categorized as subsidy or public workfare schemes. As can be seen, workfare schemes form the majority of the programs. The primary objective of these programs is to provide an alternative source of income and employment for the population living at or below the poverty line. Subsidy programs like Integrated Rural Development Program (IRDP) and Swarnjayanti Gram Swarozgar Yojana (SGSY) provide credit and subsidy to individuals (or a group of individuals) in order to promote self-employment and investment in income generating assets. Indira Awaas Yojana (IAY) provides 100% subsidy to below poverty line households for self-construction of basic dwelling units also known as *kutcha houses*. The table also indicates whether a program was implemented in steps. Usually, programs that are implemented in phases take two (or more) years to achieve complete coverage in all the districts of the country. The table also shows if a program substituted any older scheme(s) with similar objectives. For example, Jawahar Rozgar Yojana (JRY) in 1989 was a replacement for the two previous public workfare programs: National Rural Employment Program (NREP) and Rural Landless Employment Guarantee Program (RLEGP).

Since these programs are centrally sponsored schemes, most ( $\approx 80\%$ ) of the program cost is borne by the central government while the state governments contribute only towards some specific components of the programs. In practice, however, almost all of the program expenditure is financed by the central government. The last column of Table 1 shows the average annual expenditure (in 2004 prices) incurred under each program at the national level.

The following are a few salient features of the programs we study. First, all the programs only cover the *rural* blocks or regions in a state. Second, the workfare programs provide manual labor work under public works like soil and water conservation projects, afforestation, irrigation projects, etc. Since the overriding priority is direct assistance to the rural poor, these programs do not undertake any heavy rural infrastructure projects like building of dams, bridges, and roads. Similarly, these programs do not involve the creation of any marketable goods or services. Third, while the criteria of fund allocation from the central government to the states may vary depending on the program, the ratio of the rural poor in a state to the total rural poor in the country is a key determinant in the fund allocation process for many programs. The states then further disburse the funds at the district level based on program specific criteria.

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<sup>7</sup>Expenditure under these programs can largely be classified as transfers as these schemes aim to provide income support to the poorest and the most backward section of the rural population through subsidies or employment at minimum wages through public works.

## 2.3 Identifying Motivation

We use our narrative sources to identify the motivation behind the introduction of the programs we study. We find that a new program either replaces old program(s) or is introduced as a *standalone scheme* to function in addition to the existing schemes. We note that old programs are restructured into a new scheme in order to address their shortcomings and inefficiencies identified over the years. Also, a *restructured scheme* is usually implemented with substantially larger outlay in funds. On the other hand, a standalone scheme is introduced to achieve a social or economic objective that has not been sufficiently addressed by any existing program. Either way, the introduction of a new program is not output or growth motivated. On the contrary, the *Annual Plan* of Ministry of Rural Development (1994-95, p. 245) notes: "...to the extent that the growth process will 'bypass' certain sections of the rural society, it will be necessary to provide supplementary employment and incomes via special programs". We exploit the independence between new program introductions at the national level and local recessionary shocks to construct a series of spending innovations at the state level.

## 2.4 Identifying the Size, Timing, and Duration of the Shocks

We rely on the state-level heterogeneity in program implementation during the introductory period – which we refer as the “introductory variation” – to construct the “narrative shock series”. The introductory period is mostly limited only to the year in which a new program is introduced. Only for Rural Landless Employment Guarantee Program (RLEGP) and National Rural Employment Guarantee Act (NREGA), the introductory period is two years since these programs were implemented in a phase-wise manner. Since state-wise expenditure data for IRDP is not available before 1985, the introduction of IRDP in 1978 does not contribute to our shock series.<sup>8</sup> To calculate the introductory variation for a program, we calculate the year-on-year change in real per capita program expenditure that occurs due to the introduction of the new scheme. If the new program replaces older program(s), then the introductory variation is simply the new program expenditure minus the total expenditure incurred under the previous program(s) in the last year. If the new scheme is instead implemented as a standalone program to further intensify transfer spending in rural areas, we take the whole first-year expenditure as the introductory variation. If the program is implemented in phases, we calculate the sequence of expenditure effects for each year.

We apply these criteria for all program introductions in the period 1980-2010 to

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<sup>8</sup>We still include IRDP in our measure of total rural transfer spending (from 1985) since it was a major development program of its time.

construct the narrative shock series at the state-level that we use to estimate the local multipliers. Since the decision to implement a new program (restructured or standalone) is not systematically related to state-level macroeconomic shocks, we use this evidence to rule out reverse causality from state-year shocks to changes in state-level spending that occur *during* the implementation of a new program. As a robustness check, we test (below) whether our constructed shock series is sensitive to contemporaneous or lagged variations in agricultural output at the state-level. An immediate concern is that the multiplier estimates obtained from this shock series can still be downward biased if the increase in spending during program introductions is systematically large for states that do poorly relative to other states.

For example, our narrative records cite the ratio of rural poor in the state to the total rural poor in the country as a key factor that determines fund allocation to the states under most of the programs. However, we show that in per capita terms, the change in transfer spending due to a new program introduction is not correlated with this backwardness index.

Similarly, we find that our multiplier estimates are essentially the same even after excluding state fixed effects from our empirical model. This suggests that our narrative shock series is largely orthogonal to state-specific characteristics. These findings are in agreement with recent studies that also conclude that variation in program implementation can mostly be attributed to supply driven idiosyncratic factors like the political will and administrative capabilities to implement a new scheme. Imbert and Papp (2014) show that a host of district characteristics like the rate of literacy, poverty rate, agricultural productivity, the level of wages, local elections, along with other worker level controls are unable to account for the stark differences in the level of employment provision across districts under NREGA. Similarly, due to the rationing of demand for work under NREGA, Dutta et al. (2012) find the number of NREGA days generated to be only weakly correlated with poverty.

While we consider changes in the state-level transfer spending due to a new program introduction as independent of local output fluctuations, we make no such assumption regarding the broader measure of changes in state-level transfer spending. This is so since once a program is well integrated into the system, expenditure under the program can be sensitive to cyclical fluctuations in local economic conditions. These broader measures of transfer changes like the year-on-year growth in total transfer spending may contain many observations that are likely to be endogenous. In section 4, we compare the multiplier estimates obtained through the narrative shock series with those obtained from the broader measure of changes in transfer spending.

Below we discuss a detailed analysis of the motivation, timing, and the expenditure

effects of (i) a standalone scheme that was implemented in phases; and (ii) a scheme that qualifies as a restructured scheme. The appendix on the narrative analysis provides a detailed discussion for the rest of the programs considered in our analysis.<sup>9</sup> For a state  $i$  in year  $t$ , we express the exogenous change in real per capita transfer spending as  $\Delta s_{i,t}$  and use  $X_{i,t}$  to denote real per capita expenditure under the program ‘X’ where  $X \in \{\text{RLEGP, IAY}\}$ . For the ease of reference, any expenditure estimates that are cited while discussing the programs are the same as mentioned in the narrative sources (in nominal prices). All expenditure estimates used in the empirical analysis, however, are converted to 2004 prices. We use the state GDP deflator series to allow for differential price trends among states.

### **Rural Landless Employment Guarantee Program (RLEGP)**

Implemented: 1983; Until: 1988

Program expenditure at the national level:

1983: ₹0.27 billion (2004 prices)

1984: ₹14.99 billion (2004 prices)

Exogenous change in state-level transfers:

$\Delta s_{i,1983}$ :  $RLEGP_{i,1983}$

$\Delta s_{i,1984}$ :  $RLEGP_{i,1984} - RLEGP_{i,1983}$

RLEGP was launched in different states/union territories on 15<sup>th</sup> October 1983. Although several anti-poverty programs were in place for the poor at the time, RLEGP was specifically launched to address the problem of unemployment for the landless laborers during the lean agricultural season. As the *Annual Report 1983-84* of the Ministry of Rural Development states “the hardcore of rural poverty, particularly pertaining to employment opportunities for the landless during the lean agricultural periods . . . has to be tackled in a more direct and specific manner” (p. 41). The program’s objectives were: (i) to provide up to 100 days of employment per year to at least one member of every landless labor household and (ii) to create durable assets to strengthen rural infrastructure. The program was fully funded by the central government.

Construction of rural link road, land development, and soil and water conservation

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<sup>9</sup>Employment Assurance Scheme (1993) is the only program whose introduction is not treated as exogenous. This is so since EAS was implemented as a demand-based scheme with no fixed allocations to state. See Appendix 1.B for details.

projects (among others) were allowed under RLEGP with the restriction that “the wage component in a project should . . . not be less than 50% of the total cost of the project” (p. 42). Since the program was introduced in the second half of the financial year 1983-84, it was implemented in only 13 (out of the 25) Indian states and was later extended to cover the rest of the states by 1984-85. During the first year, around ₹1 billion were allocated for RLEGP out of which only ₹62 million were spent, possibly indicating capacity and supply side constraints. However, expansion of the program in the following year along with delivery mechanisms set in place, resulted in program expenditure of ₹3.7 billion (out of the ₹4 billion that were made available). Since the motivation to introduce the program was unrelated to current or prospective output fluctuations, the resulting changes in transfer spending during the first two years of program implementation and expansion are regarded as exogenous variation in transfer spending. As RLEGP did not substitute any older program, it is treated as a standalone scheme where the program expenditure at the state-level during the year of implementation is regarded as the exogenous change in transfer spending. Moreover, since there is evidence that the program expanded substantially during 1984-85, the *increase* in program expenditure in 1984 is also considered as exogenous introductory variation.

### **Indira Awaas Yojana (IAY)**

Implemented: 1995; Until: Present

Program expenditure at the national level:

1995: ₹17.60 billion (2004 prices)

Exogenous change in state-level transfers:

$$\Delta s_{i,1995}: IAY_{i,1995} - IAY_{i,1994}$$

The *Annual Report* 1995-96 of the Ministry of Rural Development notes:

“The Indira Awaas Yojana (IAY) was launched in 1985-86 as a sub scheme of Rural Landless Employment Guarantee Program to provide houses free of cost to the members of Scheduled Castes/Scheduled Tribes, and freed bonded laborers in rural areas. From 1989-90, the scheme has continued as a sub-scheme of Jawahar Rozgar Yojana. In the beginning, 6 per cent of the total allocation of the JRY was earmarked for the scheme. From 1993-94 . . . the JRY allocation for the implementation of IAY was raised from 6 per

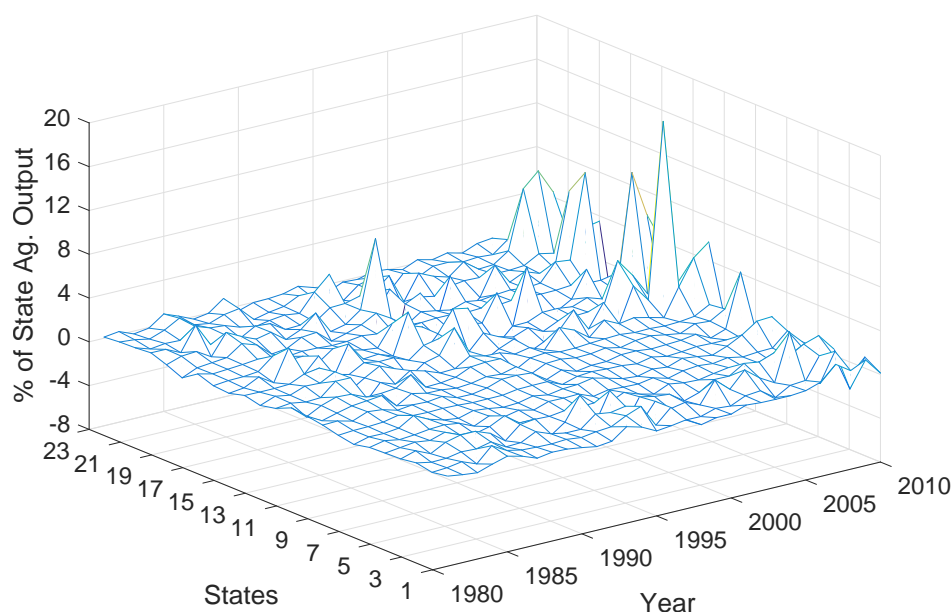
cent to 10 per cent . . . From 1 January 1996 the Indira Awaas Yojana has been taken out of JRY and made an independent scheme by itself” (p. 20).

In order to effectively target the mutually exclusive objectives of rural employment and rural housing, IAY was implemented as an independent scheme in the financial year 1995-96. Around 75-80% of the program cost is financed by the central government. State poverty ratio and the shortage in rural housing are the prime determinants in the allocation of funds from the central government to the states. As a result of being implemented as an independent scheme, expenditure under IAY increased to around ₹12 billion in 1995-96, substantially higher than  $\approx$  ₹5 billion that was spent under IAY as a sub-scheme of JRY in 1994-95 (*Annual Report 1998-99*, Ministry of Rural Development). Hence, the exogenous implementation variation for the year 1995-96 is calculated as the difference between IAY expenditure in 1995-96 and 1994-95. The challenge in calculating the state-wise exogenous variation due to IAY implementation is that state-wise data for IAY is only reported from 1995-96. Expenditure under IAY is not available at the state level when it was implemented as a sub-scheme of JRY. To overcome this constraint, we approximate the state-wise expenditure under IAY in 1994-95 using: (i) state-wise expenditure data under JRY; and (ii) the allocation criteria which required 10% of JRY funds to be earmarked for IAY. Using this approximation, we calculate the difference in IAY expenditure between 1995-96 and 1994-95:  $IAY_{1995} - IAY_{1994}$  as the exogenous introductory variation in transfer spending. Since there is no evidence to suggest that Indira Awaas Yojana underwent any expansion in terms of coverage after 1995-96, exogenous variation is limited to the year IAY was introduced as an independent scheme.

## 2.5 Narrative Shock Series

We now discuss the result of our narrative analysis which gives us a measure of state-wise transfer changes that are largely exogenous to local output fluctuations. We also compare this shock series with the broader measure of changes in total rural transfer spending. Finally, we discuss the properties of our narrative shock series before using it to estimate the state-sector multipliers. As explained above, our narrative shock series can be expressed as the *change* in real per capita transfer spending for state  $i$  in year  $t$  –  $\Delta s_{i,t}$  – due to program introductions. We normalize these exogenous changes in transfer spending with lagged real per capita agricultural output at the state level. We define the broader measure of transfer changes as the year-on-year change in real per capita total rural transfer spending, also normalized with lagged real per capita agricultural output at the state level. Below we refer the exogenous changes in transfer spending

Figure 2: Aggregate Annual Variations in Rural Transfer Spending



as a percent of lagged agricultural output simply as exogenous changes. Similarly, the broader measure of year-on-year changes in total rural transfer spending divided by lagged agricultural output is often referred as *all changes*.<sup>10</sup>

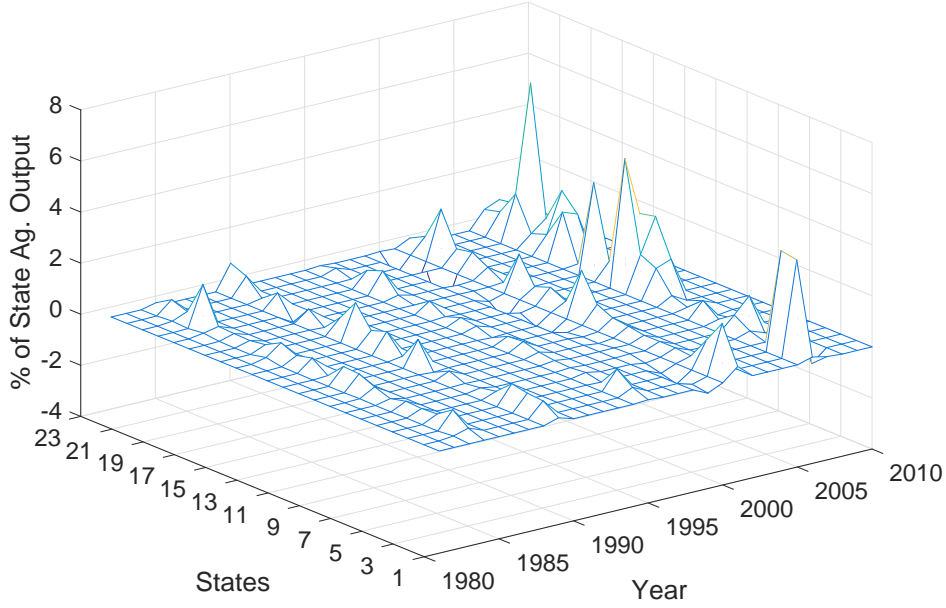
Figure 2 shows the broader measure of year-on-year changes in total rural transfer spending as a percent of previous year's state agricultural output for the period 1981-2010. As the figure shows, there are large increases in transfer spending in nearly all the states during 2008 and 2009. While a large part of this increase can be attributed to the scale up of NREGA, the period between 2008 and 2009 also witnessed a peak in loan waivers, subsidies, and a fiscal stimulus package that followed the financial crisis. Hence, part of the increase in transfer spending during 2008-09 may be a discretionary counter-cyclical policy response.<sup>11</sup>

Figure 3 shows our narrative shock series: the state-wise exogenous changes in transfer spending as a percent of state agricultural output for the period 1981-2010. As can be seen from Figure 3, the shock series mostly takes the value zero except for the exogenous impulses that correspond to the introductory variation due to program introductions. The mean of the shock series (across states and time) is 0.12 percent of state agricultural GDP with a standard deviation of 0.51 percent. A substantial number of exogenous changes are above ½ percent of state agricultural GDP with a maximum value of 6 percent.

<sup>10</sup>Total rural transfer spending is defined at the beginning of section 2.

<sup>11</sup>This is why we limit the introductory period of NREGA implementation to 2006 and 2007, even though the complete implementation of the program took three years. See Appendix 1.B for details.

Figure 3: Exogenous Changes in Rural Transfer Spending



## 2.6 Properties of Exogenous Transfer Changes

Next, we discuss some properties of the narrative shock series. First, we test for serial correlation in the shock series by conducting the Wooldridge (2002) test for linear panel data models. The test checks for serial correlation among the residuals  $\varepsilon_{i,t}$  obtained from the first difference estimation of the following model

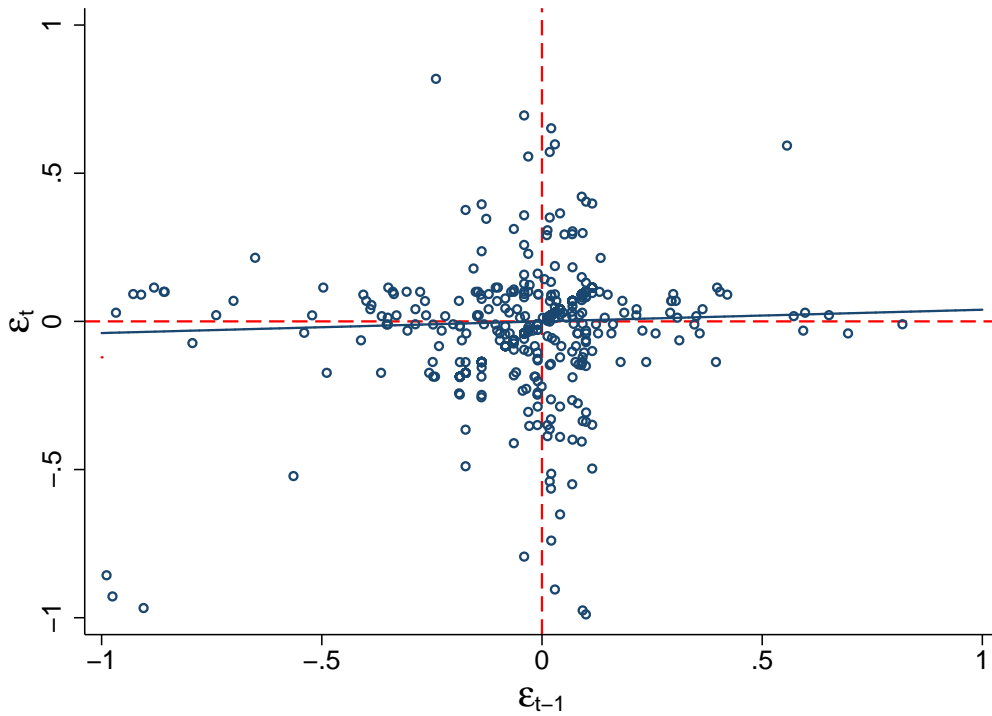
$$S_{i,t} = \gamma_t + \alpha_i + \varepsilon_{i,t} \quad i \in \{1, 2, \dots, 23\}, t \in \{1981, \dots, 2010\} \quad (1)$$

where  $S_{i,t} = \Delta s_{i,t} / y_{i,t-1}$  is the exogenous change in real per capita transfer spending in state  $i$  in year  $t$  divided by the lagged real per capita agricultural output in the same state. We find no evidence of serial correlation in the spending innovations identified through narrative analysis. The null of no serial correlation cannot be rejected at a  $p$ -value of 0.29. Figure 4 shows the scatter plot of  $\varepsilon_{i,t}$  – as obtained from a regression of Equation 1 – on its lag. As can be seen, the shock series is largely orthogonal to its lag and the slope of correlation is very flat and not statistically different from zero. This is evidence against the hypothesis that expenditure variations at state-level that occur due to program introductions are serially correlated and hence predictable using previous shocks.

Second, we check whether the exogenous changes in transfer spending are systematically correlated with the ratio of rural poor in the state to the total rural poor in the



Figure 4: Serial Correlation of Narrative Shocks



country. As discussed earlier, the state poverty ratio is a key determinant in the allocation of funds from the central government to the states for many programs. However, we find a statistically insignificant correlation of 0.07 between our shock series and the rural poverty ratio. This supports our premise that the heterogeneity in transfer spending during program introductions is largely due to idiosyncratic supply factors and not due to state characteristics that are likely to be correlated with local output. This is further confirmed by our results which are largely invariant to the exclusion of state fixed effects (discussed in section 5).

A natural concern in our study is that although the stated political motivation of the introduction of a new program seems independent of output fluctuations or factors influencing output, it might not truly be so. Since these programs are largely agriculture-centric, a few consecutive bad harvest years may influence the introduction of a new program as an additional safety net for the rural poor and unemployed. If this is the case, then changes in transfer spending are not truly exogenous and are predictable using such serially correlated shocks to the local output. We take lagged changes in agricultural output as a proxy to such serially correlated shocks that can impact the rural economy and use them to study if they can predict our measure of exogenous

transfer spending.

We regress the real per capita exogenous changes in rural transfer spending at state-level on contemporaneous and three lags of year-on-year growth in real per capita state agricultural GDP, along with the state and year fixed effects. All the coefficients of lagged changes in real agricultural output are zero up to two decimal places and are insignificantly different from zero ( $p = 0.53$ ). Similarly, as is discussed in the results section, accounting for lagged changes in real agricultural output as additional controls in our empirical model do not alter the results in any significant way.

## 2.7 Anticipation Effects

Systematic anticipation effects associated with the implementation of a new program may spuriously raise (or lower) the multiplier estimates. Ramey (2011) for example shows that both the narrative shocks and the professional forecasts (in US) Granger-cause the VAR errors. Anticipation effects, however, may not be a concern in our study for the following reasons. First, there is often a significant revision in the expenditure estimates by the government where these revisions often take many months to complete. This suggests that the information set of the implementing authority is itself incomplete. Consequently, it is extremely unlikely that the information set of the private sector *prior* to the spending shocks is better than that of the government *after* the shocks - a point also stressed by Ilzetki, Mendoza, and Végh (2013) for developing economies.

Secondly, the target group - the rural poor, faces severe informational constraints regarding such programs and about the complete list of benefits that can be availed under such programs. As a response to alleviate such informational constraints, the government organizes various outreach activities *after* a new program has been implemented. Hence apart from being severely liquidity constrained, the target group is also informationally constrained. Therefore, there is little reason to expect any systematic anticipatory response by the transfer beneficiaries before the implementation of a new program.<sup>12</sup>

## 3 Empirical Model

We estimate the state-sector multiplier using data on agricultural output and transfer spending shock series for 23 states of India over the thirty-year span between 1980 and

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<sup>12</sup>Kaplan and Violante (2014) show that even wealthy households optimally choose to hold little liquid wealth and hence show large propensities to consume out of transitory income while the response to the news of future income is small. In comparison, the target group in our study is extremely poor, which further alleviates concerns for any systematic anticipation effects.

2010. Therefore, we have 690 annual observations. Let  $y_{i,t}$  denote the real per capita agricultural output for state  $i$  in year  $t$ , and  $Y_{i,t} = (y_{i,t} - y_{i,t-1})/y_{i,t-1}$  its rate of growth.  $S_{i,t}$ , as defined in Equation 1, is the year-on-year exogenous change in real per capita transfer spending divided by the lagged real per capita agricultural output. Consistent with the recent literature (see for e.g., Barro and Redlick, 2011; Acconcia, Corsetti, and Simonelli, 2014) we estimate spending multiplier by relating agriculture output growth ( $Y_{i,t}$ ) to the year-on-year exogenous change in transfer spending in the same state ( $S_{i,t}$ ). Our empirical model is

$$Y_{i,t} = \sum_{j=0}^3 \beta_j S_{i,t-j} + \sum_{j=1}^3 \xi_j Y_{i,t-j} + \alpha_i + \gamma_t + r_{i,t} + \varepsilon_{i,t} \quad (2)$$

where  $\beta_0$  estimates the impact multiplier while the coefficients on the three lags of the shock variable  $S_{i,t}$  provide estimates of the dynamic multipliers. State and year fixed effects are denoted by  $\alpha_i$  and  $\gamma_t$  respectively;  $r_{i,t}$  is the number of rural poor in the state as a percent of total rural poor in the country. Through the state fixed effects we account for possible endogeneity issues emanating from any state-specific trends that may influence fund allocation from the central government. For example, if per capita expenditure is higher for states with low average agricultural growth, then the omission of state-specific trends may downward bias our estimates.

The year fixed effects control for aggregate annual variations that are common to all the states. This may include fiscal, monetary, trade, and agricultural policy at the national level.<sup>13</sup> The year fixed effects also control for changes at the national level during a year that may impact agricultural output in all the states. New technology adoption at the country level or the aggregate monsoon performance, for example, is controlled by the year fixed effects. Failure to account for such aggregate variations may spuriously result in higher (or lower) multiplier estimates. A key advantage specific to our data is that while most of the spending under these programs comes from the central government, on the taxation side, the local state governments have very limited power to change tax rates. According to the Article 246 of The Constitution of India, the parliament has the exclusive power to make laws on income tax, corporation tax, customs, and excise.<sup>14</sup> Therefore, the state-level variation in transfer spending due to a new program is not matched by changes in tax burden for the local residents. Hence specific to our study, omission of tax changes from our set of controls is unlikely to be

<sup>13</sup>See for example, Christiano, Eichenbaum, and Rebelo (2011), Corsetti, Meier, and Müller (2012), and Ilzetzki, Mendoza, and Végh (2013) among others for a discussion on the sensitivity of the multiplier estimates to fiscal-monetary policy mix and to the degree of openness of the economy.

<sup>14</sup>List II and III of Article 246 enumerates the areas in which only the state or both the state and the parliament can make laws. However, this accounts for a very small proportion of the revenue component.

a cause of concern since the year fixed effects control for the tax changes made at the national level.<sup>15</sup>

To avoid misleading inference in panel estimation one has to control for any serial or spatial correlation among observations (see for e.g., Bertrand, Duflo, and Mullainathan, 2004; Angrist and Pischke, 2008). Regarding spatial correlation, we posit that states in the same region may be correlated due to unobserved cluster effects like common agricultural practices, natural resources, and weather conditions. Therefore, we use standard errors that are robust to contemporaneous spatial correlation allowing for 240 clusters (i.e., 30 annual observations for 8 regions), and robust to heteroskedasticity as well.<sup>16</sup> Regarding serial correlation, we use three lags of the dependent variable  $Y_{i,t}$ .

Including lags of agricultural output growth also helps us to control for a multitude of other influences which affect  $Y_{i,t}$  and are likely to be serially correlated. Furthermore, following Romer and Romer (2010), including lagged output growth accounts for hidden motivation of seemingly exogenous policy changes. Specific to our case, if seemingly exogenous program introductions are more common when agricultural growth is below normal, then stimulatory effects of transfer spending would in part be due to the agricultural growth returning back to normal. Accounting for lagged values of agricultural growth helps us to control for this possibility. It is worth noting that the inclusion of lagged dependent variable in Equation 2 does not result in any consequential dynamic panel bias (Nickell, 1981). We confirm this by comparing multiplier estimates obtained from the Least Squares Dummy Variable (LSDV) estimation of Equation 2 with the *bias-corrected* estimator (LSDVC) (see Bruno, 2005 for details and related literature). Both estimators yield the same parameter estimates up to two decimal places.<sup>17</sup> This is expected since the time dimension of our panel data is large. For computational convenience, all our regressions use LSDV estimator.

## 4 Results

Corresponding to the estimates obtained in Equation 2, Table 2 summarizes the implied effect of spending increase of one percent of state agricultural output on the path of state agricultural GDP relative to normal. The table reports the *impact* and *cumulative*

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<sup>15</sup>Acconcia, Corsetti, and Simonelli (2014) use similar characteristics of fiscal federalism in Italy in their study to estimate government purchase multiplier.

<sup>16</sup>We group 23 states into 8 regions. For example, we group all north-eastern states like Arunachal Pradesh, Assam, Manipur, Meghalaya, Nagaland, Sikkim, and Tripura in one region; the southern region consists of Andhra Pradesh, Karnataka, Kerala, and Tamil Nadu and so on.

<sup>17</sup>For a macro panel like the one used in this study, Judson and Owen (1999) strongly advocate LSDVC for bias correction. LSDVC estimation results are available on request.

multipliers for up to 5 years denoted by  $M(t), M(t+1), \dots, M(t+5)$ . Columns 1-3 report multipliers obtained using our narrative shock series. In Column 1, we compute cumulative multipliers based on the impact and dynamic estimates obtained in Equation 2, without controlling for lagged output growth and state-country poverty ratios. Because of the simple specification used in column 1, the cumulative effect after 2 years, for example, is simply the sum of the coefficients on the contemporaneous and the first two lags of the spending variable. The impact multiplier for this specification is estimated to be around 1.3 which is statistically different from zero at 5 percent significance level. The cumulative effect of spending after three years grows to around 2.7, significant only at 10 percent level.<sup>18</sup> However, there is clear evidence of negative serial correlation among the regression residuals under this specification.<sup>19</sup> While parameter estimates are still unbiased in the presence of serial correlation, negative serial correlation *may* actually cause standard errors to be overestimated.

We next present results that control for lagged dependent variable as additional regressors in column 2 of Table 2. The estimates in column 2 again show the implied effect of transfer spending increase of 1 percent of state agricultural output on state agricultural GDP. However, the cumulative effect now not only includes the direct effect of transfer spending but also accounts for the feedback effect from the lagged dependent variable. That is, the dynamic effect on state agricultural GDP now also accounts for implied change in the path of lagged output growth. Since accounting for lagged dependent variable allows for the possibility of calculating effects of transfer spending beyond 3 years, we simulate the effect on state agricultural output growth till 5 years.

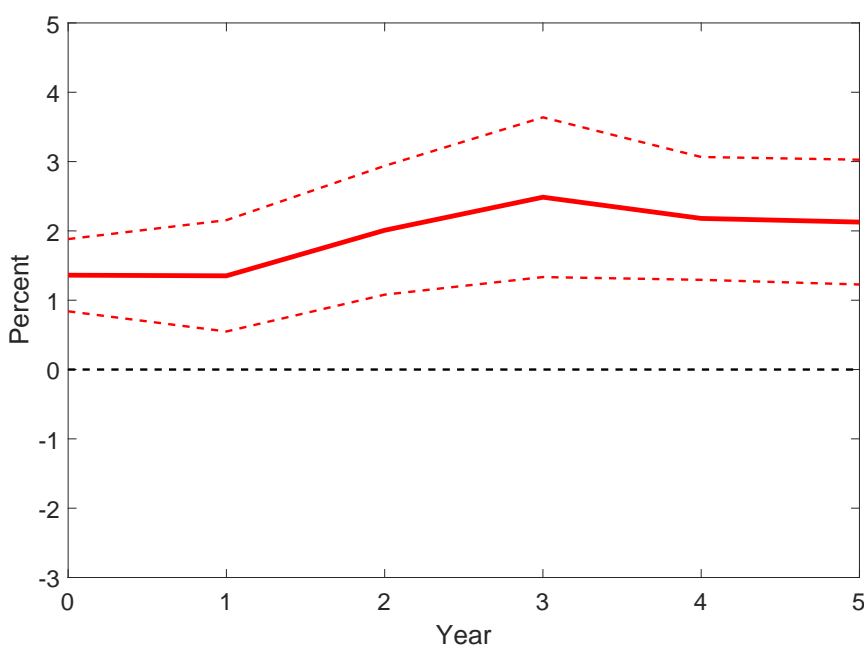
Overall, the multiplier estimates reported in column 1 and 2 are very comparable which is evidence against the possibility that the output response to spending innovations simply reflects the normal dynamics of the agricultural economy. However, the standard errors in column 2 are substantially smaller than those reported in column 1. Hence, negative serial correlation does seem to overstate the standard errors in column 1. We find no evidence of serial correlation after controlling for lagged dependent variables. The impact multiplier in column 2 is 1.4 which is highly significant ( $t > 2.6$ ), the effect after three years is 2.6 which reduces to 2.3 after 5 years (both estimates are significant at 5 percent significance level).

Finally, column 3 estimates the impact and cumulative effect of transfer spending after adding the state-country poverty ratios as additional controls in Equation 2. As

<sup>18</sup>Standard errors for the cumulative multipliers are estimated using the delta method (Oehlert, 1992).

<sup>19</sup>A regression of  $\varepsilon_{i,t}$  on its lag gives a coefficient of  $-0.4$  which is highly significant ( $t > 10$ ). Assuming that error estimates follow a first-order autoregressive process:  $\varepsilon_{i,t} = \rho\varepsilon_{i,t-1} + v_{i,t}$ , we get the same correlation estimate of  $\rho$  (of  $-0.4$ ) using  $\rho = 1 - d_p/2$  where  $d_p$  is the generalized Durbin-Watson statistic calculated for panel data as in Bhargava, Franzini, and Narendranathan (1982).

Figure 5: Estimated Impact of a Transfer Increase of 1% of State Agricultural Output on State Agricultural Output using Exogenous Changes

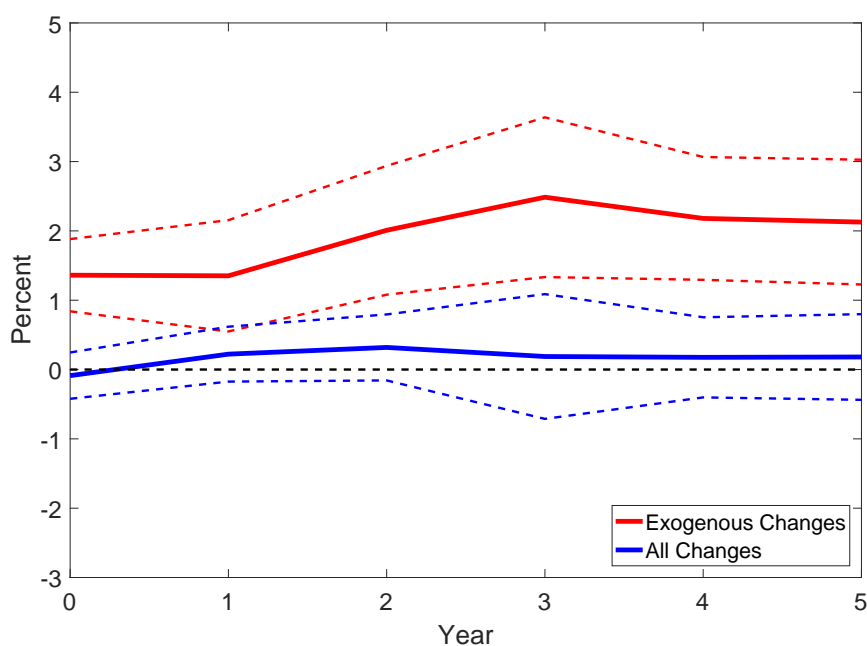


can be seen, this changes the results only marginally. The impact multiplier is still 1.4 and highly significant ( $t > 2.6$ ) while the overall effect of the spending increase after 3 years is 2.5 which reduces to 2.1 after 5 years (both estimates are significant at 5 percent level). We treat the specification in column 3, which includes all the controls specified in Equation 2 as our *preferred specification*. Corresponding to the estimates obtained in column 3, Figure 5 summarizes the implied effect of an increase in transfer spending of 1 percent of the state agricultural output on the path of state agricultural output relative to normal together with one standard error bands.

#### 4.1 Comparison with Broader Measures

A key objective of this paper is to highlight the consequences of ignoring changes in transfer spending that are correlated with local output fluctuations due to shocks like droughts, floods, or other natural disasters. As a result, using a broader measure like *all changes* in transfer spending can spuriously downward bias the multiplier estimates. It is hence important to compare the results obtained from the narrative shock series with those obtained from the broader measure of all changes in transfer spending. To do this comparison, we substitute the exogenous changes in transfer spending ( $S_{i,t}$ ) in Equation 2 with *all changes*: the year-on-year growth in total rural transfer spending (as shown in Figure 2).

Figure 6: Estimated Impact of a Transfer Increase of 1% of State Agricultural Output on State Agricultural Output



The last column of Table 2 shows the impact and cumulative multipliers obtained from the broader measure of changes in transfer spending, controlling for all other regressors as in our preferred specification. As can be seen, all of the estimates in column 4 are nearly an order of magnitude smaller than the respective estimates in column 3. Figure 6 compares the impact and cumulative multipliers obtained using our narrative shock series with those obtained from the broader measure of changes in transfer spending. As the figure shows, all estimates obtained using the broader measure are insignificant from zero. The difference is significant to the extent that the whole confidence interval that corresponds to the narrative shock series is almost always higher than the confidence bounds obtained from the broader measure of changes in transfer spending. This comparison hence confirms that failure to address the endogeneity problem can severely downward bias the multiplier estimates towards zero.

## 4.2 Robustness

While outliers and high leverage observations can be perfectly valid observations, it is important to check the sensitivity of our results to such observations. For example, the smallest ( $-44\%$ ) and the largest ( $139\%$ ) growth rates of the state agricultural output respectively correspond to the financial years 1987-88 and 1988-89 for the state of Gujarat. However, these extreme outliers are not data errors and instead correspond

to the drought of 1987-88 in Gujarat that “affected more than 87% area of the state” and “was the worst ever drought during 1973-74 to 2004-05” (p. 14, Roy and Hirway, 2007). An extreme drought followed by an average monsoon in the following year can hence explain the two outliers.

We first estimate a robust regression of Equation 2 to check the robustness of our results to outliers and high leverage points. The procedure initially screens out gross outliers with Cook’s distance  $D > 1$  followed by an iteration process to weigh each observation based on Huber and biweights. Convergence is achieved when the maximum change in weights from one iteration to the next is below tolerance (see Li, 1985 for details). Table 3 reports the robust regression estimates of Equation 2 in column 2. For comparison, column 1 of Table 3 reports the baseline results from our preferred specification in Table 2. As can be seen, robust regression yields very comparable estimates to the baseline results. The impact multiplier is estimated to be slightly lower at 1.3 which highly significant ( $t > 2.9$ ). Similar to the baseline, the maximum cumulative effect achieved after 3 years is 2.4 and it decreases to 2.1 after 5 years ( $t > 2.9$  for both estimates).

Next, we test for the robustness of our results to the exclusion of influential observations. We employ the DFITS statistic which measures the influence an observation has on the overall fit of the model to identify influential observations (see Welsch and Kuh, 1977). Column 3 of Table 3 shows results of estimating Equation 2 after excluding the identified influential points based on the DFITS statistics. The impact multiplier using the DFITS statistics is 1.3 ( $t > 2.7$ ), again very similar to the baseline result. The cumulative effects of transfer spending from then on are consistently higher and more significant than the corresponding baseline estimates. The overall multiplier after 5 years, for example, is estimated at 2.5 with  $t > 3$ . Hence while our results are robust to outliers, high leverage points, and other influential observations, we have no compelling reason to omit these observations from our analysis.

## 5 Extensions

In this section, we further analyze the properties of our model by first looking at the cross-border and sectoral spillover effects of rural transfer spending. Next, we test the implications of restricting our model to spending under employment guarantee schemes. Finally, we test the heterogeneity of our estimates across macro areas and also check the influence of state and year dummies.



## 5.1 Spillover Effects

We first test whether there are any cross-border effects of spending under these programs. For example, an increase in transfer spending in a state may spur economic activity in adjacent states due to demand *leakages*. On the other hand, if higher spending under such programs results in the relocation of factors of production from the neighboring states, then an increase in agricultural growth rate in one state may negatively affect the agriculture sector in an adjacent state. If either type of spillover is empirically relevant, then our baseline results will confound the local output effects of transfer spending with demand leakage or relocation effect. Following Acconcia, Corsetti, and Simonelli (2014), we test the cross-border effects by controlling for regional spending in our model. Therefore, we estimate the following model

$$Y_{i,t} = \sum_{j=0}^3 \beta_j S_{i,t-j} + \sum_{j=1}^3 \xi_j Y_{i,t-j} + \sum_{j=0}^1 \zeta_j SR_{i,t-j} + \alpha_i + \gamma_t + r_{i,t} + \varepsilon_{i,t} \quad (3)$$

which is the same as Equation 2 but with  $SR_{i,t}$  and its lags as additional controls. We define  $SR_{i,t} = \Delta sr_{i,t} / sy_{i,t-1}$  where  $\Delta sr_{i,t}$  is the aggregate year-on-year exogenous change in real per capita transfer spending in the states that belong to the same region as the state  $i$ , excluding the state  $i$  itself;  $sy_{i,t-1}$  is the lagged real per capita agricultural output of the region defined accordingly. Although estimation of Equation 3 yields coefficients of  $SR_{i,t}$  and its lag to be below zero, both coefficients are insignificant from zero ( $t < 1$ ). Column 2 of Table 4 shows the impact and cumulative multipliers computed from the estimates of Equation 3. The results are slightly higher but always comparable to the baseline results.

Column 3 reports the computed multipliers after adding the interaction term  $S_{i,t} \times SR_{i,t}$  to Equation 3, where both the variables are expressed in deviations from their respective mean value. By controlling for this interaction term, we check whether the effect of local spending reflects complementarity due to demand leakages or substitutability due to the relocation of factors of production. However, the coefficient on the interaction term is estimated to be only 0.1 which is statistically indistinguishable from zero ( $p$ -value = 0.8). The computed multipliers are again comparable but slightly higher than the baseline results. Finally, column 4 shows the results computed from the estimates of Equation 2 but after aggregating small states with adjacent states, thereby reducing the total number of states in our sample from 23 to 16. This increases the impact multiplier to around 1.7 but the overall impact after five years is 2.3, comparable to the corresponding baseline result of 2.1. Overall, the evidence from these tests suggests weak spillover effects.

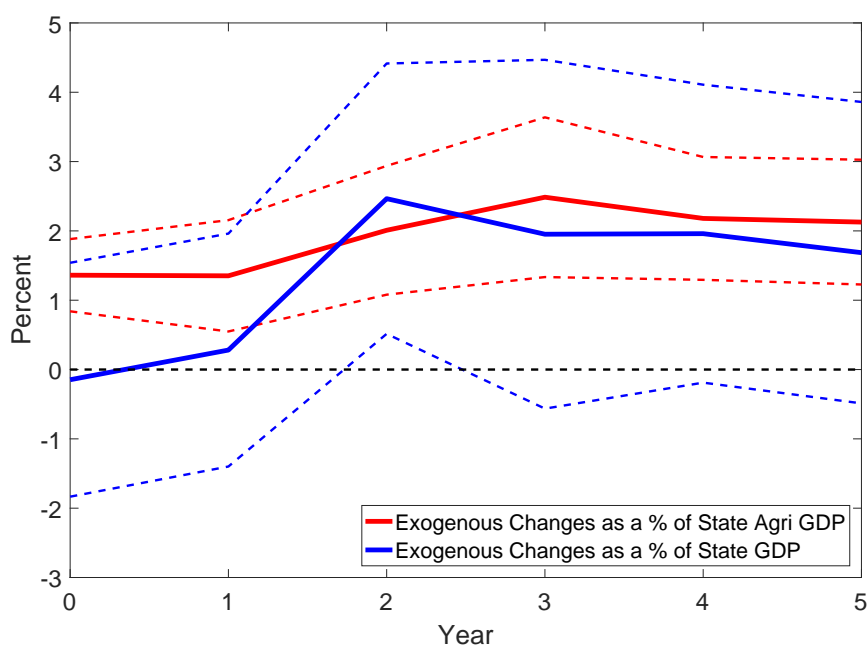
Next, we discuss the sectoral spillover effects of local spending within a state. Demand leakages or relocation of factors of production to other sectors *within* a state are equally relevant issues that need to be examined in order to understand the local economic effects of spending under rural programs. If higher rural transfer spending results in a higher demand for products and services from sectors other than the agriculture sector, then this will result in positive spillover effects. On the other hand, if higher growth in agriculture sector crowds out factors of production from other sectors into the agricultural sector, then this will result in negative spillovers. To estimate such spillover effects, we estimate Equation 4 which is the same as Equation 2 except that the dependent variable:  $Y_{i,t}^s$  is now the growth rate of real per capita *state output* while the transfer shock series  $S^s$  is the year-on-year exogenous change in real per capita transfer spending due to program introductions normalized by the lagged real per capita state GDP.

$$Y_{i,t}^s = \sum_{j=0}^3 \beta_j S_{i,t-j}^s + \sum_{j=1}^3 \xi_j Y_{i,t-j}^s + \alpha_i + \gamma_t + r_{i,t} + \varepsilon_{i,t} \quad (4)$$

A tradeoff of normalizing exogenous spending changes by the state GDP is that the spending innovations are significantly smaller relative to aggregate state output than they are to state agricultural output. The mean of the transfer shock series normalized by the state output is just 0.03 percent of state GDP with a standard deviation of 0.13 percent of state GDP. Most of the spending shocks normalized by the state GDP are on average 3-4 times smaller than the respective shocks normalized by the state agricultural output. As a result of this aggregation, we expect a loss of power in determining the true sign and magnitude of the effect that transfer spending will have on state output. Figure 7 shows the cumulative effect of a 1% (of state GDP) increase in rural transfer spending on state GDP relative to normal. The point estimate corresponding to the transfer spending shock is close to zero on impact while the cumulative effect grows to 1.7 after 5 years.

As expected, the standard errors corresponding to the multiplier estimates are approximately 2-3 times larger than those obtained in our baseline results. Consequently, the estimated effect of rural transfer spending on state output is never significantly different from zero. In fact, as the figure shows, the confidence bands of the state output multipliers almost always envelope the confidence interval of the state-sector multipliers. Table 5 compares the state output multipliers obtained using the narrative shocks series versus when the broader measure of changes in transfer spending is normalized by the state GDP. Like in section 4, there appears to be a substantial downward bias in

Figure 7: Comparing Transfer Spending Shock Normalized by State GDP and State Agricultural GDP



the computed multipliers, none of which are statistically different from zero.

Overall, while there appears to be no significant effect of spending innovations on the aggregate state output, the confidence intervals in this test are large enough to accommodate our baseline results. Hence, we do not find any concrete evidence of positive or negative sectoral spillover effects within a state. Apart from the issues of large standard errors and low power of tests, such an aggregation is relevant only if innovations in rural transfers are representative of innovations in statewide transfers.

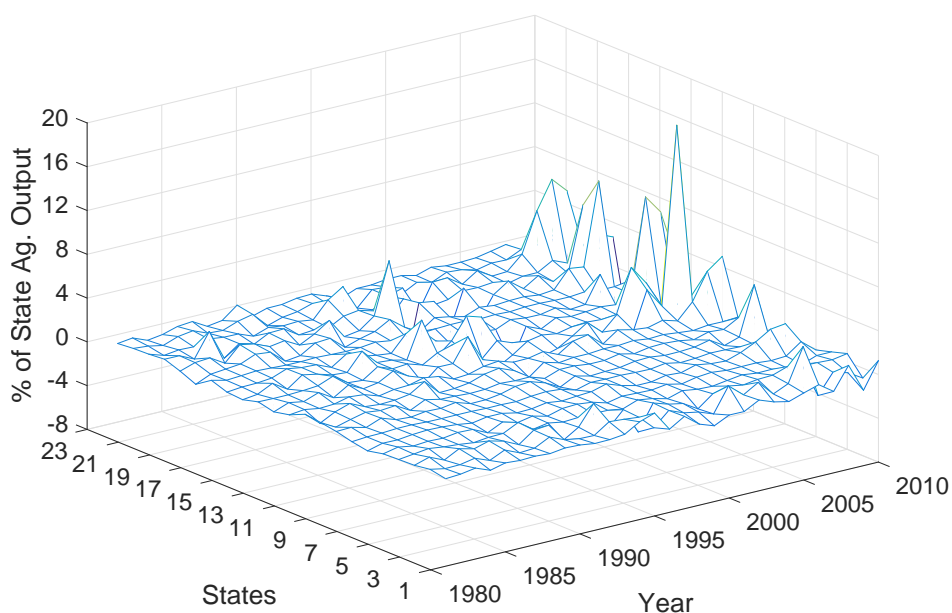
## 5.2 Effect of Spending under Employment Schemes

Up till now, we have analyzed the multiplicative output effects of spending under all major rural development programs in India between 1980 and 2010. However, from a policy perspective, it is important to check for heterogeneity in the output response to different kinds of development programs. Accordingly, in this section, we calculate the state-sector multipliers due to programs that can be categorized as employment schemes. As is noted in section 2, employment schemes form the majority of the programs that we study. We refer to spending on these programs as *employment spending*.

Figure 8 and Figure 9 respectively show: i) the broader measure of year-on-year changes in total employment spending, and ii) the exogenous changes in employment spending, both taken as a percent of lagged state agricultural output. Both measures

are constructed exactly like in section 2, but exclusively for employment schemes. For example, the narrative shock series in Figure 9 is exactly the same as the one shown in Figure 3 except for the following changes. First, the year 1995 does not have any spending innovations due to the introduction of a housing subsidy scheme IAY.

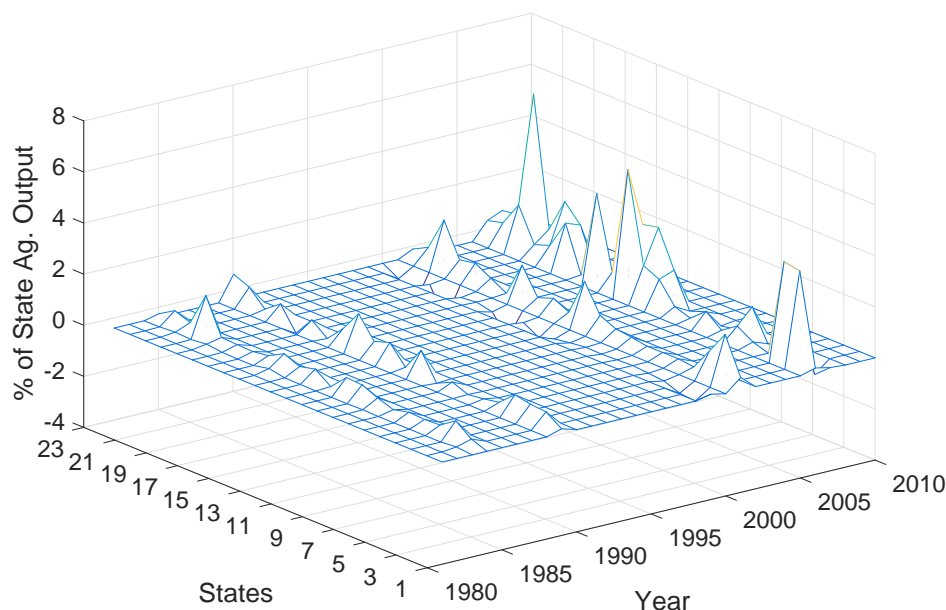
Figure 8: Aggregate Annual Variations in Transfer Spending with Only Employment Generation Programs



Second, the exogenous variation in 1999 is only due to the introduction of JGSY (employment scheme) while we ignore the changes in spending that occurred due to the introduction of a credit-cum-subsidy scheme SGSY that was also implemented in the same year (to replace IRDP). Column 1 of Table 6 shows the impact and cumulative multipliers computed under our preferred specification with the exogenous changes in employment spending as shown in Figure 9.

The results are highly comparable to our baseline results. The impact multiplier is estimated to be around 1.5 ( $t > 2.96$ ) which grows to around 2.2 (significant at standard confidence levels) after five years. Column 2 of Table 6 shows the results from the broader measure of changes in employment spending. Not surprisingly and consistent with our results in section 4, multiplier estimates obtained from this broader measure of changes in employment spending are substantially downward biased towards zero (and insignificant).

Figure 9: Exogenous Transfer Spending with Only Employment Generation Programs



### 5.3 Influence of Regions, Time, and State-Specific Dummies

In Table 7, we test the implications of excluding cluster of states based on geographic locations and dropping the year and state fixed effects. We respectively drop the states that belong to the northern, southern, and central India in the columns named “Drop North”, “Drop South”, and “Drop Center” as a way to test the heterogeneity in our results across macro areas. Next, we drop the year fixed effects to understand how our baseline results change when we allow the multiplier estimates to be influenced by national fiscal, monetary, and agricultural policies along with aggregate cyclical fluctuations. Similarly, we drop the state fixed effects to see the extent to which cross-sectional effects impinge on our results. While dropping states from the northern and central regions yield slightly higher impact multipliers of 1.5 and 1.7 respectively, excluding the southern states from the sample yield an impact multiplier that is a bit lower at 1. However, the aggregate effect after 5 years in all three cases is very close to the baseline estimate of 2.1 (all significant at 5 percent confidence level). In column 4, excluding the year fixed effects yield a slightly higher impact multiplier of 1.7 which reaches a maximum of 1.8 after three years before declining to around 1.6 after five years. Dropping the state fixed effects in the last column yield an impact multiplier of around 1.2 with cumulative multipliers of around 2 and 1.8 after 3 and 5 years respectively (all significant at 5 percent confidence level). Given that our results are robust to the exclusion of state fixed effects is encouraging and lends support to our identification strategy that assumes the narrative shock series to be largely orthogonal to state charac-

teristics. In conclusion, none of the tests produce results that are significantly different from our baseline estimates.

## 6 Conclusions

This paper exploits the state-level variation in transfer spending associated with the implementation of new welfare schemes in India to estimate the effect of government transfers on state agricultural output. Using official records as narrative evidence, we identify local variations in transfer spending that are largely independent of current or prospective fluctuations in local output to estimate the “state-sector multiplier”. We estimate the multiplier to be 1.4 on impact which increases to 2.1 after five years. Comparison with a broader measure of changes in transfer spending yields multiplier estimates that are an order of magnitude smaller than those obtained from our narrative shock series. Thus, failure to disentangle the cyclical fluctuations in transfer spending can substantially downward bias the output effects of government transfers.

The study has important policy implications. First, the multiplier estimates suggest that welfare schemes can be an effective fiscal tool to address local area or sector specific downturns, where the local adjustment in spending can be achieved mainly through redistribution of resources. Given that we do not find any relevant regional or sectoral spillover effects, the local gains from temporary variations in transfer spending seem to be quite insular. Second, cuts in transfer spending at the aggregate level (in times of crisis) can lead to large variations in the extent of retrenchment at the local level. Our estimates suggest that differences in the intensity of contraction at the local level may result in significant variation in the local economic activity.

Our results are largely comparable to the recent and growing literature that uses sub-national data to report greater than one spending multipliers (Acconcia, Corsetti, and Simonelli, 2014, Chodorow-Reich et al., 2012; Nakamura and Steinsson, 2014; Fishback and Kachanovskaya, 2010; Serrato and Wingender, 2010; Shoag, 2010). In a recent paper, Giambattista and Pennings (2016) report that conditional on monetary accommodation of the inflation generated by transfer spending, transfer multipliers can be as large as purchase multipliers. In that respect, our study provides indirect evidence that transfer and purchase multipliers can indeed be very comparable in studies using sub-national data where aggregate tax and monetary policy interactions are controlled by the year fixed effects.

It is worth noting that while our estimates are overwhelmingly significant, the confidence intervals are nonetheless substantial. Hence, when we ask narrower questions like whether the multipliers are significantly greater than unity, the confidence bands

are generally quite wide. We have also been largely silent on whether the output expansion is due to the supply or demand side factors. While the sizeable impact multiplier suggests a demand stimulus, transfer spending can also boost investment (and hence output) by alleviating liquidity constraints. This can be especially relevant for small and marginal farmers. The growing and persistent positive effect on output may also reflect increases in agricultural productivity. A promising area for future work can be to analyze the general equilibrium labor market effects of employment guarantee schemes that may facilitate a better understanding of how such anti-poverty programs affect the local economic activity.

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## Tables

Table 1: Major Rural Development Programs Since 1980

Program	Years Active	Type of Program	Replaced	Phase-wise Roll-out	Average Expenditure
IRDP	1978-1998	Credit & Subsidy	None	Yes	₹17.64 Billion
NREP	1980-1988	Public Workfare	None	No	₹19.07 Billion
RLEGP	1983-1988	Public Workfare	None	Yes	₹15.71 Billion
JRY	1989-1998	Public Workfare	NREP & RLEGP	No	₹54.05 Billion
EAS	1993-2000	Public Workfare	None	Yes	₹27.37 Billion
IAY	1995-Present	Housing Subsidy	None <sup>†</sup>	No	₹37.82 Billion
SGSY	1999-Present	Credit & Subsidy	IRDP	No	₹13.99 Billion
JGSY	1999-2000	Public Workfare	JRY	No	₹35.83 Billion
SGRY	2001-2007	Public Workfare	EAS & JRY	No	₹48.91 Billion
NREGA	2006-Present	Public Workfare	SGRY <sup>‡</sup>	Yes	₹190.21 Billion

Notes: The table shows all major rural development programs in India that were operational between 1980 and 2010. IRDP: Integrated Rural Development Program; NREP: National Rural Employment Program; RLEGP: Rural Landless Employment Guarantee Program; JRY: Jawahar Rozgar Yojana; EAS: Employment Assurance Scheme; IAY: Indira Awaas Yojana; SGSY: Swarnjayanti Gram Swarozgar Yojana; JGSY: Jawahar Gram Samridhi Yojana; SGRY: Sampoorna Grameen Rozgar Yojana; NREGA: National Rural Employment Guarantee Act. Average expenditure for a program is the aggregate national expenditure incurred under the program from program inception to the last year of the program divided by the number of years the program was active. The expenditure is expressed in 2004 prices using state GDP deflator. Data for IRDP is available from 1985 (average is calculated for 14 years). <sup>†</sup> Indira Awaas Yojana existed as a sub-scheme of Jawahar Rozgar Yojana (JRY) until 1994. <sup>‡</sup> SGRY was operational during the first two years of NREGA.

Table 2: Cumulative Multipliers

	Exogenous Changes			All Changes
	(1)	(2)	(3)	(4)
$M(t)$	1.323** [0.631]	1.402*** [0.527]	1.361*** [0.521]	-0.088 [0.334]
$M(t+1)$	1.479 [0.929]	1.417* [0.783]	1.352* [0.803]	0.222 [0.396]
$M(t+2)$	2.169* [1.284]	2.106** [0.913]	2.009** [0.929]	0.319 [0.476]
$M(t+3)$	2.669* [1.612]	2.612** [1.111]	2.486** [1.152]	0.188 [0.900]
$M(t+4)$		2.284*** [0.849]	2.180** [0.886]	0.176 [0.578]
$M(t+5)$		2.231*** [0.857]	2.127** [0.900]	0.181 [0.618]
Poverty Ratios	No	No	Yes	Yes
Lags of Y	No	Yes	Yes	Yes
Observations	621	621	621	621

Notes: Data is annual from 1980 to 2010 at the state-level. The table reports the implied effect of transfer spending increase of 1% real per capita agricultural GDP on the path of real per capita agricultural GDP relative to normal using estimates from Equation 2. The dependent variable in all the regressions is  $Y_{i,t}$ : the year-on-year percentage change in real per capita agricultural output. Columns 1 to 3 report multipliers using the *exogenous changes* in real per capita transfer spending taken as a percentage of previous year's real per capita agricultural GDP. The fourth column reports multipliers using the broader measure of *all changes* in real per capita total transfer spending taken as a percentage of previous year's real per capita agricultural output. All regressions include state and year fixed effects. The standard errors clustered at the region  $\times$  year level and robust to heteroskedasticity are reported in square brackets (the standard errors for cumulative multipliers  $M(t+1), \dots, M(t+5)$  are calculated from the variance-covariance matrix of the corresponding structural specification using the delta method).

\* p<0.10, \*\* p<0.05, \*\*\* p<0.01

Table 3: Robustness

	(1) Baseline	(2) Robust	(3) DFITS
$M(t)$	1.361*** [0.521]	1.265*** [0.425]	1.316*** [0.481]
$M(t + 1)$	1.352* [0.803]	1.318* [0.676]	1.467** [0.729]
$M(t + 2)$	2.009** [0.929]	2.155*** [0.752]	2.412*** [0.826]
$M(t + 3)$	2.486** [1.152]	2.380*** [0.855]	2.853*** [0.988]
$M(t + 4)$	2.180** [0.886]	2.236*** [0.744]	2.569*** [0.813]
$M(t + 5)$	2.127** [0.900]	2.135*** [0.731]	2.455*** [0.807]
Poverty Ratios	Yes	Yes	Yes
Lags of Y	Yes	Yes	Yes
Observations	621	609	591

Notes: The dependent variable in all the regressions is the year-on-year percentage change in real per capita agricultural output. All regressions include state and year fixed effects. All regressions include state and year fixed effects. The standard errors clustered at the region  $\times$  year level and robust to heteroskedasticity are reported in square brackets.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 4: Cross Border Effects

	(1) Baseline	(2) Spill-over1	(3) Spill-over2	(4) Aggregate
$M(t)$	1.361*** [0.521]	1.605*** [0.538]	1.489* [0.819]	1.647** [0.720]
$M(t + 1)$	1.352* [0.803]	1.803** [0.730]	1.748** [0.768]	1.539 [0.996]
$M(t + 2)$	2.009** [0.929]	2.393** [0.940]	2.325** [0.984]	2.213** [1.010]
$M(t + 3)$	2.486** [1.152]	2.810** [1.150]	2.763** [1.165]	2.670** [1.115]
$M(t + 4)$	2.180** [0.886]	2.501*** [0.902]	2.435** [0.949]	2.345*** [0.862]
$M(t + 5)$	2.127** [0.900]	2.470*** [0.905]	2.412** [0.938]	2.272** [0.908]
$SR_{i,t-j} _{j=0}^1$	No	Yes	Yes	No
$SR_{i,t} \times S_{i,t}$	No	No	Yes	No
Lags of $Y$	Yes	Yes	Yes	Yes
Poverty Ratios	Yes	Yes	Yes	Yes
Observations	621	621	621	432

Notes: The dependent variable in all the regressions is the year-on-year percentage change in real per capita agricultural output. Column 1 shows the baseline results. Multipliers reported in column 2 and 3 are computed from estimates of Equation 3. Column 3 additionally controls for the interaction of  $SR_{i,t}$  with  $S_{i,t}$ , where both the variables are measured in deviation from the mean. Here  $SR_{i,t}$  denotes the transfer spending shock in the states that are in the same region as state  $i$ , excluding state  $i$  itself. Column 4 multipliers are computed from estimates of Equation 2 but where small states are aggregated into big states. All regressions include state and year fixed effects. The standard errors clustered at the region  $\times$  year level and robust to heteroskedasticity are reported in square brackets.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 5: Cumulative Multipliers for Aggregate State Output

	(1) Shock as % of State GDP	(2) All Changes as % of State GDP
$M(t)$	-0.147 [1.687]	-0.230 [0.513]
$M(t + 1)$	0.281 [1.680]	0.556 [0.666]
$M(t + 2)$	2.465 [1.950]	-0.541 [0.746]
$M(t + 3)$	1.952 [2.516]	-1.506 [1.394]
$M(t + 4)$	1.960 [2.149]	-1.335 [1.116]
$M(t + 5)$	1.686 [2.174]	-1.202 [1.109]
Poverty Ratios	Yes	Yes
Lags of $Y_{state}$	Yes	Yes
Observations	621	621

Notes: The dependent variable  $Y_{state}$  in all the regressions is the year-on-year percentage change in real per capita *state output*. Multipliers reported in column 1 are computed using the estimates of Equation 4. Column 2 reports the computed multipliers using estimates obtained by replacing *exogenous changes* in transfer spending with *all changes* in transfer spending in Equation 4. All regressions include state and year fixed effects. The standard errors clustered at the region  $\times$  year level and robust to heteroskedasticity are reported in square brackets.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 6: Cumulative Multipliers from Spending under EGS

	(1) EGS Shock	(2) All Changes
$M(t)$	1.474*** [0.497]	0.014 [0.321]
$M(t + 1)$	1.348* [0.808]	0.115 [0.424]
$M(t + 2)$	1.951** [0.939]	0.267 [0.496]
$M(t + 3)$	2.604** [1.133]	0.424 [1.116]
$M(t + 4)$	2.250** [0.881]	0.314 [0.669]
$M(t + 5)$	2.205** [0.891]	0.314 [0.742]
Poverty Ratios	Yes	Yes
Lags of $Y$	Yes	Yes
Observations	621	621

Notes: The dependent variable in all the regressions is the year-on-year percentage change in per capita real agricultural output. Multipliers reported in column 1 are computed using the estimates of Equation 2 but with exogenous changes in *employment spending*. Column 2 reports the multipliers that are computed using estimates obtained by replacing *exogenous changes* with *all changes* in employment spending in Equation 2. All regressions include state and year fixed effects. The standard errors clustered at the region  $\times$  year level and robust to heteroskedasticity are reported in square brackets.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



Table 7: Multipliers after Dropping Major Regions

	(1) Drop North	(2) Drop South	(3) Drop Center	(4) Drop $\gamma_t$	(5) Drop $\alpha_i$
$M(t)$	1.534*** [0.536]	1.035* [0.607]	1.716*** [0.576]	1.688*** [0.467]	1.152** [0.534]
$M(t+1)$	1.575* [0.892]	1.289 [0.848]	2.008*** [0.739]	0.865 [0.655]	1.102 [0.734]
$M(t+2)$	2.274** [1.048]	1.849* [1.027]	2.670*** [0.906]	1.241* [0.662]	1.655** [0.844]
$M(t+3)$	2.386* [1.268]	2.524** [1.237]	2.196* [1.304]	1.775** [0.818]	2.044** [0.876]
$M(t+4)$	2.234** [0.988]	2.077** [0.960]	2.277** [1.102]	1.657*** [0.641]	1.843** [0.731]
$M(t+5)$	2.153** [1.002]	2.083** [0.970]	2.220** [1.111]	1.553** [0.674]	1.811** [0.731]
Poverty Ratios	Yes	Yes	Yes	Yes	Yes
Lags of $Y$	Yes	Yes	Yes	Yes	Yes
Observations	513	513	459	621	621

Notes: The dependent variable in all the regressions is the year-on-year percentage change in real per capita agricultural output. Northern region: Haryana, Himachal Pradesh, Punjab, Uttarakhand, and Uttar Pradesh; Southern region: Andhra Pradesh, Karnataka, Kerala, Tamil Nadu; Central: Chhattisgarh, Goa, Gujarat, Maharashtra, Madhya Pradesh, Orissa, Rajasthan. Results are stable (not reported) if we drop the small states in the north-east as well like Arunachal Pradesh, Meghalaya, Manipur, Nagaland, and Sikkim. Regressions in column 1-3 include state and year fixed effects. Column 4 drops the year effects while column 5 drops state fixed effects. The standard errors clustered at the region  $\times$  year level and robust to heteroskedasticity are reported in square brackets.

\* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$

# Appendix

## A Data Appendix

**Program Expenditure:** Expenditure data for the development programs has been collected from (i) *Annual Reports* of the Ministry of Rural Development (hereafter referred as MORD) from 1980-81 to 2010-11; (ii) *Annual Plan* documents of the Planning Commission from 1980-81 to 2010-11; (iii) Rural Development Statistics from the National Institute of Rural Development; (iv) [indiastat.com](http://indiastat.com); and (v) [nrega.nic.in](http://nrega.nic.in). Our expenditure figures correspond to the latest estimates as there can be a substantial difference between the provisional and revised expenditure estimates.

The expenditure data is collected state-wise with annual frequency from 1980 to 2010. The data on a particular program is for the years during which it is active. In the year 2000, three new states: Jharkhand, Chhattisgarh, and Uttarakhand were carved from Bihar, Madhya Pradesh, and Uttar Pradesh respectively. For the sake of homogeneity and a balanced panel, we aggregate all the data of these new states with their respective former states. Furthermore, Mizoram and Jammu & Kashmir are not included in the study due to the unavailability of real agricultural output data before 1993. We hence have a panel data of 23 states and 30 years (690 observations).

**State Aggregates:** Data for the Gross State Domestic Product (GSDP) and the state agricultural output has been collected (as on 28-03-2013) from the National Accounts Statistics, Central Statistics Office. We use the real and nominal state GDP series to construct state-specific deflators.

**Poverty:** Poverty estimates are from the Planning Commission *Report of the Expert Group to Review the Methodology for Measurement of Poverty*. The report gives state-wise information on the number of persons below the poverty for the years: 1977, 1983, 1987, 1993, 1999, 2004, 2009, and 2011. We interpolate (assuming constant growth between any two data points) to get annual poverty estimates between 1980 and 2010.

**Data for Figure 1:** Employment data is from National Sample Survey reports on the *Employment and Unemployment Situation in the Country* for various rounds between 1993 and 2011. National output and national agricultural output data is from Central Statistics Office (CSO).

**Population:** 1971, 1981, 1991, 2001, and 2011 Census of India.

## B Narrative Analysis

Below we discuss a thorough program by program construction of our narrative shock series.  $X_{i,t}$  denotes the expenditure incurred under the program 'X' for the state  $i$  in the

year  $t$  where  $X \in \{\text{NREP, JRY, SGSY, JGSY, SGRY, NREGA}\}$

### **Integrated Rural Development Program (IRDP)**

Implemented: 1978; Until: 1998

*Introductory variation does not contribute to the shock series*

*Reason: State-wise data not available before 1985*

IRDP was arguably the first major rural development program that was launched in 1978. The program cost was equally shared between the central government and the states. The objective of the program was to alleviate rural poverty by “providing income generating assets and access to credit and other inputs” (*Annual Report MoRD 1980*, p. 3) to “all persons who live below the poverty line” (p. 4). The program was initially introduced in 2,300 blocks in the country but was “extended to all the development blocks in the country” (p. 4) by 2<sup>nd</sup> October 1980. The principal motivation to introduce the program was therefore not output or growth related. Hence the increase in transfer spending during program introduction can be regarded as exogenous variation. However, since state-wise data for IRDP is not available before 1985, IRDP does not contribute to the construction of the narrative shock series.

### **National Rural Employment Program (NREP)**

Implemented: 1980; Until: 1988

Program expenditure at the national level:

1980: ₹11.97 billion (2004 prices)

Exogenous change in state-level transfers:

$\Delta s_{i,1980}$ :  $NREP_{i,1980}$

To tackle the serious problem of rural unemployment and under-employment, the food for work program was launched on the 1<sup>st</sup> of April 1977 (*Annual Report MoRD 1980-81*, p. 17). The report also quotes that although the program was “successful in achieving its basic objectives ... a number of shortcomings and drawbacks were noticed in the implementation of the food for work program” (p. 18). With the motivation to address the shortcomings in the food for work program, NREP “replaced the Food for Work

Program in October, 1980” (*Annual Report MoRD 1982-83*, p. 30) and was financed by the central government up till 31<sup>st</sup> March 1981 while the cost was equally shared between the central and the state government from 1982-83 (p. 32). The program envisaged generation of gainful employment for both unemployed and underemployed workers in the rural areas.

Allocation of funds from the central government was based on the incidence of poverty and the population of agricultural laborers in a state (*Annual Report MoRD 1982-83*). Payment of wages was partly in the form of food grains and partly in cash. Projects like afforestation, drinking water wells, and community irrigation wells were undertaken under NREP. Expenditure occurred under NREP in all the states during 1980-81 serves as exogenous introductory spending.

### **Jawahar Rozgar Yojana (JRY)**

Implemented: 1989; Until: 1998

Program expenditure at the national level:

1989: ₹66.34 billion (2004 prices)

Exogenous change in state-level transfers:

$$\Delta S_{i,1989}: JRY_{i,1989} - NREP_{i,1988} - RLEGP_{i,1988}$$

The *Annual Plan 1990-91* of the Planning Commission notes:

“Experience of implementation of NREPIRLEGP over the years showed that whereas the Government of India’s objective in starting these programs was to tackle the problems of unemployment of unskilled rural laborers, the distribution of funds by the States in different regions and districts did not follow this logic. The funds did not invariably go to the areas of the concentration of unemployed landless and rural labour ... The type of assets being created were not economically productive. The system of approval of the projects was such that it left much to be desired” (p. 65).

To intensify the process of employment generation and address the above concerns, the two older employment schemes NREP and RLEGP were merged into a substantially bigger program: Jawahar Rozgar Yojana on 1<sup>st</sup> April 1989. In line with previous employment programs, the primary objective of JRY was the provision of employment for

the rural poor with the secondary objective as the development of productive infrastructure. The central government financed 80% of the program cost. Under the program, assistance from the central government to the states was determined by the “proportion of rural poor in a State/UT to the total rural poor in the country” (p. 37) *Annual Report 1990-91 MoRD*. Projects allowed under JRY were similar to the ones planned under the previous programs. Since JRY was a result of the restructuring of two previous programs NREP and RLEGP, we do not treat it as a standalone program. However given the significant increase in outlay earmarked for employment generation under JRY, we consider  $JRY_{1989} - (NREP_{1988} + RLEGP_{1988})$  as exogenous introductory spending for the year 1989-90. Since there is no evidence to suggest that JRY was implemented in phases, only the change in employment expenditure due to the introduction of JRY in 1989-90 is considered to be exogenous variation in transfer spending.

### **Employment Assurance Scheme (EAS)**

Implemented: 1993; Until: 2000

*Introductory variation does not contribute to the shock series*

*Reason: Introductory variation is arguably endogenous*

EAS was implemented from 2<sup>nd</sup> October 1993 in 1778 rural blocks of the country. The program guaranteed 100 days of employment to a maximum of two adults per family. The primary objective of EAS was to provide unskilled manual work during the lean agricultural season to anyone who is “desirous of work, but cannot find it” (*Annual Report 1993-94 MoRD*, p. 19). During its second year, the program was extended to another 665 rural blocks of the country (p. 17). Expansion of EAS to cover all the rural blocks of the country continued till 1997-98. The program cost was shared between the central and the state government in the ratio 80:20 (later revised to 75:25). However, we do not treat the introductory variation due to the implementation of EAS as exogenous. This is because EAS was a demand driven scheme with no fixed allocation of funds for any district or block. Hence according to the narrative evidence, any variation in program expenditure is likely to be due to variation in the demand for the program which is likely to be correlated with local economic conditions. Therefore, implementation of EAS does not contribute towards the construction of our narrative shock series.

### **Swarnjayanti Gram Swarozgar Yojana (SGSY) & Jawahar Gram Samridhi Yojana (JGSY)**

SGSY Implemented: 1999; Until: Present

JGSY Implemented: 1999; Until: 2001

Program expenditure at the national level:

1999: ₹11.39 billion in SGSY (2004 prices)

1999: ₹24.19 billion in JGSY (2004 prices)

Exogenous change in state-level transfers:

$$\Delta s_{i,1999}: (SGSY_{i,1999} - IRDP_{i,1998}) + (JGSY_{i,1999} - JRY_{i,1998})$$

Integrated Rural Development Program (that was implemented in 1978) also enveloped several sub-schemes like Training of Rural Youth for Self Employment (TRYSEM), Development of Women and Children in Rural Areas (DWCRA) among others. While all these schemes were supposed to operate complimentary to each other, over the years individual program targets replaced the larger objective of poverty alleviation (p.40 *Annual Report 1999-2000 MoRD*). To address the deficiencies identified in the older schemes, a comprehensive credit-cum-subsidy scheme: Swarnjayanti Gram Swarozgar Yojana (SGSY) was launched on April 1, 1999, while IRDP and its allied programs ceased to be in operation.

The focus of SGSY was to create self-employment among the rural poor. To achieve this, a multi-pronged approach of micro-enterprise development, capacity building of the poor (self-help groups), and credit technology was followed. The subsidy component extended to individuals/groups under SGSY was only meant as an enabling element, while a greater reliance was on the availability of credit. As mentioned in *Annual Report 1999-2000 MoRD*, individuals under the program were eligible to receive a subsidy of 30% of the project cost with the subsidy ceiling of ₹10,000. For group projects, the subsidy was at 50% of the project cost subject to a maximum subsidy grant of a ₹1,25,000.

In the same year, the older employment program: Jawahar Rozgar Yojana (JRY) was restructured into Jawahar Gram Samridhi Yojana (JGSY) on 1<sup>st</sup> April 1999. Unlike the previous programs like JRY and EAS where the primary objective was generation of wage employment, the overriding priority of JGSY was the creation of demand driven community village infrastructure with the secondary objective being the generation of wage employment for the unemployed. The rule of 60:40 wage-material cost ratio outlined under the previous JRY for the creation of rural infrastructure was relaxed under JGSY so as to enable the build-up of demand-driven rural infrastructure. Development of infrastructure support for SGSY was given a priority under JGSY. However, heavy

infrastructure investment related projects like building of bridges, secondary schools, colleges, and roads were not permitted under JGSY (*Annual Report 1999-2000 MoRD*).

For both SGSY and JGSY, the program cost was divided between the Centre and the States in the ratio 75:25. Hence in the financial year 1999-2000, two major rural development programs: IRDP and JRY were restructured into SGSY and JGSY respectively. Our exogenous measure of transfer spending for the year 1999-2000 accordingly measures change in program expenditure because of the implementation of the new restructured programs. Thus, for 1999-2000,  $(SGSY_{1999} - IRDP_{1998}) + (JGSY_{1999} - JRY_{1998})$  is our measure of exogenous introductory spending. Since there is no evidence that either SGSY or JGSY was implemented in phases, the introductory variation is limited to the year of introduction of the two programs. Since IRDP along with many other small duplicate schemes were discontinued and replaced with SGSY, this generally resulted in an exogenous contraction of transfer spending during 1999-2000.

### **Sampoorna Grameen Rozgar Yojana (SGRY)**

Implemented: 2001; Until: 2007

Program expenditure at the national level:

2001: ₹47.02 billion (2004 prices)

Exogenous change in state-level transfers:

$$\Delta s_{i,2001}: (SGRY-I_{i,2001} - SGRY-I_{i,2001}^{prov}) + (SGRY-II_{i,2001} - SGRY-II_{i,2001}^{prov})$$

SGRY was launched on 25<sup>th</sup> September 2001. After the restructuring of JRY into JGSY in 1999, EAS became the major wage-employment generation program while JGSY was more focused on the creation of rural infrastructure. To achieve comprehensive rural development, an ambitious program: SGRY was launched to take care of food security, employment generation, and rural infrastructure development. SGRY initially operated under two streams where EAS and JGSY were respectively restructured into SGRY-I and SGRY-II. Total SGRY funds were equally divided between the two streams. SGRY was implemented as one from 2004-05 (*Annual Report 2005-06 MoRD*, p. 2). Rural infrastructure development under SGRY involved projects like drought proofing (for e.g., soil and moisture conservation), afforestation, and promotion of traditional water resources, among others. A ‘Special Component’ of SGRY geared towards augmenting food security started from 1<sup>st</sup> April 2002 (*Annual Report 2002-03 MoRD*, p. 11).

The special component was a demand driven sub-scheme where the central assistance was extended (only in terms of food grains) to the states in times of natural disasters like drought, earthquake, cyclone, flood, etc. Naturally, expenditure pertaining to the special component is not included in our shock series which may otherwise downward bias our multiplier estimates. The central government financed 75% of the program cost. However, unlike most new programs, SGRY was not introduced at the start of the financial year and was instead implemented during the end of the second quarter of the financial year 2001-02. Construction of the exogenous variation becomes a challenge in this case since the state-wise annual expenditure estimates for SGRY-I and SGRY-II during 2001-02 also contain funds utilized under EAS and JGSY until October 2001.

As a workaround to this, we use the *provisional* expenditure estimates of SGRY-I and SGRY-II during 2001-02 to estimate the funds utilized under EAS and JGSY between April-October 2001. Annexures in the *Annual Reports* of MORD provide actual program expenditure estimates for the previous years, while only provisional estimates are provided for the current fiscal year. Furthermore, the ‘reporting month’ (the month up to which the program expenditures have been reported) is also mentioned in the financial report. In the *Annual Report* 2001-02 MORD, the provisional estimates for both SGRY-I and SGRY-II correspond to the reporting month of October or November 2001 (for most of the states). This consequently provides us with an estimate of the expenditure that occurred before the introduction of SGRY. Hence  $(SGRY-I_{i,2001} - SGRY-I_{i,2001}^{prov}) + (SGRY-II_{i,2001} - SGRY-II_{i,2001}^{prov})$  represents the exogenous *change* in transfer spending at state-level due to the introduction of SGRY in 2001. The introductory variation is limited to 2001 since there is no evidence that the program was expanded after 2001-02.

### **National Rural Employment Guarantee Act (NREGA)**

Implemented: 2006; Until: Present

Program expenditure at the national level:

2006: ₹78.69 billion (2004 prices)

2007: ₹132.30 billion (2004 prices)

Exogenous change in state-level transfers:

$\Delta s_{i,2006}$ :  $NREGA_{i,2006}$

$\Delta s_{i,2007}$ :  $NREGA_{i,2007} - NREGA_{i,2006}$



NREGA (later renamed as Mahatma Gandhi – NREGA) is often regarded as the largest workfare program in the world. The program guarantees 100 days of manual labor work to every rural household by organizing public works that are aimed at generating and maintaining village infrastructure. NREGA was implemented in phases where 200 districts were covered under the program's first phase in 2006-07. In the second phase, 130 additional districts were covered in 2007-08. The program covered all the districts of the country by 2008-09. The program is almost completely funded by the central government.

Since NREGA did not replace SGRY until 2008, NREGA's first-year expenditure is regarded as exogenous introductory variation while the *increase* in NREGA expenditure from 2006 to 2007 is considered as exogenous variation for the year 2007. Although there is clear evidence that NREGA expanded further in the third year, we do not construct exogenous variation after the second year of NREGA. This is because by the third year, it becomes progressively difficult to isolate exogenous changes in program expenditure that occur specifically due to program expansion. The increase in government expenditures across the board due to the stimulus package set-up by the Indian government in early 2009, suggests that part of the counter-cyclical increase in social transfers could be arguably endogenous. The exogenous introductory variation under NREGA is therefore limited to 2006-07 and 2007-08.

Another valid concern is that given the size of NREGA, overall expenditure under other programs may have declined during 2006-07 and 2007-08. However, we find no significant negative correlation between NREGA expenditure and the year-on-year change in aggregate expenditure under SGSY, SGRY, and IAY (at state-level) during 2006 and 2007. It is reassuring to note that the results do not change even if we replace the NREGA shocks with the year-on-year variations in aggregate expenditure: SGSY + SGRY + NREGA + IAY in 2006 and 2007. The latter choice of shocks is, however, susceptible to endogeneity since expenditure variations in the other programs may reflect local output fluctuations and hence is not our preferred choice of expenditure shocks due to NREGA implementation.