Estimating Transfer Multiplier using Spending on Rural Development Programmes in India

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Abstract: Rural transfer programmes in India are implemented for a variety of reasons. A key question is whether such transfer programmes are consequential for the economic activity in rural areas. This paper estimates the multiplicative effects of rural transfer spending on agricultural output at state level. Using expenditure data on all major, Centrally Sponsored Rural Development Programmes implemented after 1980, we first construct a measure of state-wise *total rural transfer spending*. Employing narrative records like the *Annual Reports* of the Ministry of Rural Development, we then identify that the principal motivation to restructure old programme(s) into a new programme is typically a response to address past inefficiencies identified in the "delivery mechanism" of previous programme(s). Furthermore, a new "standalone scheme" is introduced to address a deep-rooted social or economic issue that has not been sufficiently addressed by any existing programme. Consequently, the introduction of a new programme is largely independent of the current or prospective output fluctuations. We use our narrative records to further identify the "introductory" and "expansionary" variation during the implementation of these new programmes as a measure of change in rural transfer spending exogenous to variation in local output. The results indicate that the state-sector relative multiplier is around 1.02 on impact. Including the dynamic effects of lagged transfer spending, increases this estimate to around 1.71. The results indicate that local variations in rural transfer spending can have a significant impact on economic activity in rural areas.

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1 Introduction

What is the impact of government spending on output? The question has always been central to the study of macroeconomic policy. The series of stimulus packages rolled out globally following the recent financial crisis reinvigorated the debate both in the research and policy arena on the effectiveness of such fiscal interventions. As a response to this debate, there is a considerable and growing empirical literature which estimates the government spending multiplier using different econometric methodologies[1]. However, there is a significant disconnect between the research and its empirical relevance in the policy arena. While it is true that government expenditures have increased around the world, related literature has largely focused on government purchases (defined as government consumption plus investment). A large proportion of government expenditure: *Transfers* have received very limited attention as a stimulus instrument in the research literature.

As has been reported in a recent study by Oh and Reis (2012), transfer spending accounted for around three quarters of the increase in total government expenditures in the US from 2007 to 2009[2]. Oh and Reis also show that after accounting for automatic stabilizers, around half of the increase in social transfers in the US during 2007-09 may be regarded as discretionary social transfers. India is no exception in this case, and similar to the stimulus packages introduced in many developed countries, transfer spending accounted for a substantial proportion in the stimulus package set-up by the Indian government in early 2009. As is frequently noted in the literature, information, decision, and implementation lags can substantially delay the policy response to a crisis where the lag period can be even longer for developing economies. Against this backdrop, injecting funds into the social projects already in place provides a convenient stimulus channel.

The following observation noted in *Review of the Economy,* Economic Advisory Council, India (2008-09, p. 9) corroborates the above view "*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*". The substantial increase in government spending since 2008, in social transfer programmes like Mahatma Gandhi National Rural Employment Guarantee Act (MG-NREGA) indicates that much of the increase in the rural transfer spending may be a discretionary response to the crisis (discussed in detail in section 4). To the extent social transfers are used as a stimulus instrument, it becomes important to understand their effect on economic activity.

[1] See for e.g., Blanchard and Perotti (2002), Mountford and Uhlig (2009), and Romer and Romer (2010).

[2] In their paper, Oh and Reis (2012) further report that as a median measure, transfers accounted for around 64% of the increase in total government expenditures in 22 OECD and European countries during the same period.

In this paper, we estimate the "state-sector relative multiplier" from the cross-sectional exogenous variation of state-wise rural transfer spending. We study the multiplicative effects of rural transfer spending in India primarily due to the availability of data on all major centrally sponsored Rural Development Programmes (hereafter referred as RDPs) implemented after 1980. Using data on each RDP, we first construct a measure of *total rural transfer spending* by collecting and aggregating state-wise expenditure data on all major RDPs since 1980. We note that new programmes either replace existing programme(s) or are introduced as standalone schemes.

As is explained in the *Annual Plan* documents of the Planning Commission and the *Annual Reports* of the Ministry of Rural Development, the restructuring of old programme(s) into a new programme is typically a response to address past inefficiencies identified in the "delivery mechanism" of the previous programme(s). Furthermore, standalone schemes are introduced to address deep-rooted social or economic issues that have not been sufficiently addressed by any existing programme. Consequently, the introduction of a new programme is largely unrelated to current or prospective fluctuations in local output. Moreover, implementation of a new programme to *all* the rural regions of the country typically takes a couple of years. Hence apart from the "introductory variation" in rural transfer spending in the year a new programme is introduced, there is a significant "expansionary variation" in the subsequent year as well. We use this introductory and expansionary variation as a measure of exogenous changes in rural transfer spending and use our narrative records to identify the size, timing, and the duration of these shocks.

A valid concern in our study is that programmes may be systematically "restructured" at regular intervals, hence making their variations predictable. To check for this, we divide our shock series in period of 10 years and compare them against each other. We find no evidence that the introductions of new programmes are a periodic (and hence predictable) feature. Next, using past output growth rates as a proxy for serially correlated shocks we find: 1) no evidence that our shock series is predictable by past or contemporaneous output fluctuations; and 2) no significant difference in our multiplier estimates with or without lagged output growth as additional controls. Also, as discussed in section 4, there are reasons why anticipation effects may not be a serious concern in our analysis.

Given the unavailability of data on rural income within each state, we instead focus on studying the multiplicative effects of rural transfer spending on the sector that most closely corresponds to the rural economy - the agriculture sector. Our choice of studying the agriculture sector to estimate the "state-sector relative multiplier" of rural transfer spending is based on the following key observations: 1) the high dependence on agriculture in the rural regions; 2) the highly agriculture centric objectives of the rural development programmes;

and 3) the substantial proportion that the agricultural output forms towards the aggregate state output.

The results indicate that the relative effect of local variations in rural transfer spending on state agricultural output corresponds to an impact multiplier of around 1.02. Accounting for the lagged changes in transfer spending shows that the expansionary effect of transfer spending grows after the period of impact and reaches its peak after 3 years. Including the dynamic effects of lagged transfer spending (while also appropriately correcting for the effects working through the lagged growth rates), brings our estimate of the overall multiplier to around 1.71, a value that is significantly different from zero at the standard confidence levels. The results are highly robust and are much larger than those obtained from the more general measure of annual changes in aggregate rural transfer spending. The results suggest significant sector specific impact due to local variations in rural transfer spending.

Under a representative agent model with lump sum taxes and transfers, transfers are output neutral. However in their recent theoretical study, Oh and Reis (2012) explain how well "targeted transfers" can have expansionary effects on output. Transfers can also change marginal incentives by changing relative prices: Subsidizing productive assets to rural families below poverty line is one such example where transfers increase the marginal incentive of the agent to acquire productive capital. An increase in marginal reward might spur economic activity while a decrease in marginal reward by the way of less return to working or saving may further dampen output and employment. Woodford (1990) shows that transfer spending can be expansionary if it alleviates liquidity constraints which results in higher investment and output[3].

Along with the present study, there are a number of recent studies that estimate the government spending multiplier by exploiting cross sectional and regional variations in government spending. Acconcia et al (2013) for example, instrument episodes of large and unanticipated temporary contractions of public spending at the provincial level in Italy (on evidence of mafia infiltration in city councils) to estimate the (contemporaneous) government spending multiplier to be 1.2[4]. Nakamura and Steinsson (2012) exploit the heterogeneity of regional spending to national military build-ups and draw-downs as an instrument to estimate an "open economy relative multiplier" to be around 1.5, while Serrato and Wingender (2011) use as instrument, the census shock due to the measurement error in US census estimates to estimate the government spending multiplier to be around 1.88[5].

[3] Aiyagari and McGrattan (1998) on the other hand, show that higher transfers may reduce the incentive of a household to insure against shocks which thereby reduces precautionary savings and hence reduce capital and output.

[4] Accounting for the effect of lagged spending increases the multiplier estimate to 1.8.

[5] Similarly, Chodorow-Reich et al (2012) study cross sectional variation in Medicaid transfers in US states

Similar to these studies, we use sub-national data and use cross sectional variation to estimate our "state-sector relative multiplier". However, compared to the above studies our analysis differs in the following three aspects. Firstly, while much of the literature focuses on the government purchases multipliers, we study the multiplicative effects of transfer spending. Secondly, our identification strategy of the exogenous variations in transfer spending relies on narrative analysis instead of the use of an instrument. Although narrative analysis has been employed in various multiplier studies, most of them estimate national (tax or spending) multipliers. Romer and Romer (2010) for example, document tax changes using legislative records and use this information to estimate the impact of tax changes on output.

Studies on fiscal policy similar in approach is that of Ramey and Shapiro (1998) and Ramey (2011) who use narrative records such as news reports in *Business Week* to identify changes in government purchases due to military build-ups and other events independent to the state of the economy[6]. In our study, we instead construct a measure of exogenous *state-wise* changes in rural transfer spending using narrative sources. Finally, most of the "local multiplier" studies focus on evaluating the relative multiplicative effects of local variations in government spending on specific regions like states, provinces, districts or counties. We instead define "local" in the sense of the agriculture sector within each state and hence estimate the rural transfer multipliers which are state-sector specific.

The rest of the paper is organized as follows. Section 2 discusses a) the salient features of the Rural Development Programmes in India; and b) the construction of state-wise *total rural development spending.* Data and Narrative Analysis are respectively discussed in Section 3 and 4. Section 5 presents the empirical model. Results and various robustness checks are presented in section 6 and 7 respectively. Possible heterogeneous effects of employment guarantee schemes (among the RDPs) are analyzed in section 8. The aggregate state-level impact of local variations in rural transfer spending is also discussed in section 8. Section 9 concludes.

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and estimate an output multiplier of around 2. 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.

[6] Ramey (2011) introduces another shock series using forecast errors of professional forecasters. Romer and Romer (1989, 2004) are examples of narrative analysis on monetary policy.

2 Rural Transfer Spending in India

This section provides an overview of the RDPs we consider in our study. Before understanding the impact of rural transfer spending on output it is first important to have a measure which appropriately reflects the total annual state-wise rural transfer expenditure. Unfortunately in the case of India, such a state-wise data is not publicly available for the period of our study. As a workaround to this, we focus on all major Rural Development Programmes which have been exhaustively discussed in the *Annual Plan* documents and the *Annual Reports* of the Planning Commission and the Ministry of Rural Development respectively. Apart from the availability of state-wise annual expenditure data, the reason to focus on these RDPs stems from the fact that all of the programmes studied are essentially transfer schemes which are targeted at the poorest and the most backward section of the rural population.

We consider all major RDPs designed and implemented by the Central government in all the states during 1980 to 2010. Such programmes are referred to as Centrally Sponsored Schemes (CSS) where a major proportion of the cost of the programme is borne by the Centre[7]. All the data considered in our study, like the actual expenditure data on all the RDPs and other major state aggregates are of annual frequency and are reported state-wise. Although each programme had specific objectives and hence a corresponding target group, the global objective of all the RDPs (also known as poverty alleviation programmes) was to insure the rural poor against sharp fluctuations in income and employment. Assistance to the rural poor under these programmes was in the form of provision of a) credit and subsidy for the procurement of productive assets to enable self employment; b) cash and/or food as a form of wage payment in employment guarantee schemes (a form of unemployment benefit); and c) cash for the construction of basic dwelling units for the rural poor.

The following are a few salient (but by no means exhaustive) features of the RDPs we study. 1) All the RDPs strictly cover only the rural blocks/regions in a state. 2) The overriding priority has been direct assistance to the rural poor - correspondingly these programmes do not undertake any heavy rural infrastructure projects like building of dams, bridges, and roads. 3) These programmes (especially the employment generation programmes) do not involve creation of any marketable goods or services - a non exhaustive list of works allowed under such programmes are: soil and water conservation projects, afforestation, and land development (among others). 4) Most of the RDPs follow a well structured fund allocation process.

[7]Usually 75% of the cost is financed by the Centre. See Appendix for programme by programme cost ratio between the Centre and the States.

Based on the specific criteria of the programme, funds are first allocated from the Centre to the States. After adding its own share, the states further distribute the funds (according to their own criteria) to its districts. The funds are finally utilized in the rural blocks/regions of each district. The amount of funds a state receives by the centre is always programme specific and varies over the years. However, the proportion of rural poor in a state to the total rural poor in a country is a key determinant of the centre to state fund allocation[8].

The RDPs considered in our study broadly fall under the following three categories: 1) credit-cum-subsidy programmes: like Integrated Rural Development Programme (1980-1998) and Swarnjayanti Gram Swarozgar Yojana (1999-Present) to support income generating projects for the rural poor; 2) employment generation programmes: to provide supplementary wage employment to the unemployed and/or underemployed in the rural regions (these include the majority of the programmes we study).

In the order of year the of implementation, they are: National Rural Employment Programme (1981-89), Rural Landless Employment Guarantee Programme (1983-89), Jawahar Rozgar Yojana (1989-98), Employment Assurance Scheme (1993-2001), Jawahar Gram Samridhi Yojana (1999-2001), Sampoorna Grameen Rozgar Yojana (2002-08), and National Rural Employment Guarantee Act (2006-present); and 3) housing subsidy programme: like Indira Awaas Yojana (1996-present) which provides funds to the rural poor to self-construct (*Annual Report*, Ministry of Rural Development 2006-07, p. 31) basic dwelling units for themselves.

Table 1 lists all the programmes year-wise and highlights the years in which a new programme was introduced. The table conveniently summarizes the evolution of the major RDPs over the years. As is shown in Table 1, in case a new programme replaces old programmes then the new RDP is placed directly below the former programme(s). For example, both NREP and RLEGP were restructured into JRY and ceased to exist after the introduction of JRY in 1989. Similarly IRDP and JRY were respectively enveloped by SGSY and JGSY in 1999. A standalone scheme is entered separately in a new column, for example: NREP in 1980, RLEGP in 1983, IAY in 1995 and NREGA in 2006.

Refer Appendix for a detailed programme by programme description. We construct the state-wise measure of *total rural development spending* as simply the aggregate expenditure that occurred under all the active RDPs (as shown in Table 1) within each state, in a given year.

[8]Employment Assurance Scheme (EAS) during 1993-94 to 1998-99 was a demand based scheme and did not follow centre to state fund allocation. The most recent Mahatma Gandhi National Rural Employment Guarantee Act (MG-NREGA) is also a demand based programme with no specific centre to state fund allocation.

3 Data

Rural Development Programmes: Expenditure data corresponding to RDPs (shown in Table 1) is electronically available only for the more recent years. Financial statements corresponding to RDPs for most of the years are publicly available only in the hard copies of the annual reports of the Ministry of Rural Development[9]. Overall the data has been collected and validated from the following sources[10]: 1) *Annual Reports* (1980-81 to 2010-11), Ministry of Rural Development; 2) *Annual Plan* documents (1980-81 to 2010-11), Planning Commission; 3) *Rural Development Statistics* 2011-12, National Institute of Rural Development; 4) indiastat.com; 5) iay.nic.in; 6) sgsy.nic.in; and 7) nrega.nic.in.

The nominal expenditure data is collected state-wise with annual frequency. The data on a particular programme is for the years during which it is active. We have focused only on actual expenditure estimates and not on total allocations or provisional expenditure estimates. As has been noted during the process of data collection, the difference between the actual and provisional expenditure estimates can be large. Great care has been taken to ensure that only actual estimates of the state-wise expenditure on RDPs are included. We hence have a panel data of 23 states for 30 years (690 observations)[11].

State Aggregates: Both nominal and real data for the Gross State Domestic Product (GSDP) and the agricultural component of the GSDP has been collected from the National Accounts Statistics of the Ministry of Statistics and Programme Implementation (mospi.nic.in)[12]. Data on real national GDP and real national agricultural output is from *Handbook of Statistics on Indian Economy* 2011-12, Reserve Bank of India.

Employment/Unemployment and Rural Poverty: Data on employment and unemployment is from National Sample Survey Reports (various rounds) on the *Key Indicators of Employment and Unemployment in India*[13]. Rural poverty statistics has been collected from *Handbook of Statistics on Indian Economy* 2011-12, Reserve Bank of India and data.gov.in.

[9] The Department of Rural Development came into existence as a part of Ministry of Food and Agriculture in 1974. Since then, the department's name and its status as an independent ministry changed a number of times before it was finally upgraded to its present status as the Ministry of Rural Development in 1999.

[10] Although data on RDPs is compiled by the Ministry of Rural Development, not all the data is available in the Annual Reports of Ministry of Rural Development. Hence we use other official sources of data as well.

[11] 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 up to the year 1999 and 1993 respectively.

[12] Downloaded 28-03-2013.

[13] Earlier published as *Employment and Unemployment Situation in India.*

4 Narrative Analysis

We exploit two unique characteristics of the RDPs to construct our narrative shock series: 1) motivation for the introduction of a new programme and 2) expansion of the new programme to cover all rural blocks/regions of the country.

Motivation: As has been evident from the *Annual Plan* documents of the Planning Commission and *Annual reports* of the Ministry of Rural Development, a new programme either replaces existing programme(s) or is introduced as a standalone scheme to function additional to the existing programmes. While the motivation to completely restructure old programmes is to address their shortcomings and inefficiencies identified over the years, a standalone scheme is introduced to achieve a social or economic objective in the rural areas which is not sufficiently targeted by any of the existing programmes. In other words, the motivation behind the introduction of a new programme (restructured or standalone) is not output or growth motivated[14]. As is also noted by the *Annual Plan*, Planning Commission (1994-95, p. 245): "*... 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 programmes*".

Expansion: Irrespective of whether a programme replaces earlier programme(s) or is introduced as a standalone scheme, there is an 'expansion lag' associated with a new programme. Expansion lag refers to the time required to extend a new programme to all the rural blocks/regions of the country. Pilot implementation of the new programme in select districts/states in the introductory year(s), delayed retraction of erstwhile programme(s), or implementation delays can result in a significant expansion lag. Limited coverage of the programme in the introductory year can be even more pronounced if the new programme is introduced in the latter half of the financial year. For the RDPs considered in our study, it usually takes two years (sometimes more) for a new programme to fully cover all the rural regions of the country (see Appendix for details).

Given that the introduction of a new RDP is not a response to current or past output fluctuations, any excess introductory expenditure occurred due to the implementation of a new programme in the year of implementation is considered as exogenous transfer spending and we term it as *introductory exogenous spending*. Similarly, expenditure corresponding to the extension of a new programme in the subsequent year to the previously uncovered blocks/districts should in principle be independent of growth or output concerns. This expansionary variation which we term as *exogenous expansionary variation* need not necessarily

[14] As a robustness check, we test whether our constructed shock series is sensitive to contemporaneous or lagged income variations later in this section.

imply excess expenditure. A revision in the fund allocation criteria to blocks/regions or delay in the initiation of projects under the new scheme might also result in a contraction of RDP expenditure.

Introductory exogenous spending: Construction of the introductory exogenous spending differs according to whether the new programme replaces old programme(s) or whether it is a standalone programme. In the case of a standalone programme, total actual expenditure for each state is the introductory exogenous spending for the year of programme introduction. If a new programme replaces earlier existing programme(s), then the difference between the new programme expenditure (in the year of introduction) from the old (merged) programme(s) expenditure (in the previous year) is taken as the introductory exogenous spending. The intuition for this is as follows: restructuring of previous programme(s) into a new programme is usually followed by a significant increase in outlay (and hence expenditure) relative to the previous (merged) programme(s).

Hence although the excess expenditure following the introduction of a new programme is not motivated by factors influencing output (such as the level of poverty/unemployment, drought, flood, earthquake, or cyclone), the same cannot be said about the 'base expenditure' attributed to the amount of funds utilized in the previous (merged) programme(s) during the previous year. Consequently, in case the new programme replaces old programme(s), we only take the "excess" expenditure as the introductory exogenous variation. On the other hand, in case the new programme is a standalone scheme with no analogous previous programme(s), then all of the expenditure under the new programme in the year of introduction serves as the introductory exogenous variation.

Expansionary exogenous spending: Irrespective of whether a programme is a standalone scheme or replaces old programmes, the expansionary variation for all new programmes is constructed as the difference in actual expenditure under the new programme between the years $t+1$ and t, where the year t represents the year in which the new programme was introduced. Refer appendix for a detailed programme by programme construction of the shock series.

Sector specific multiplier: Our identification of exogenous transfer spending is based on the variation in transfer spending among the *rural regions* of different states during the year of introduction or expansion. Correspondingly, to analyze any multiplicative effects of rural transfer spending, one would ideally require an estimate of the total *rural income* within each state. However, unavailability of data on state-wise rural income restricts such an analysis. Alternatively, given that the funds from such RDPs were utilized only in the rural regions of the state, we focus our attention to the sector that most closely corresponds to the rural economy - the agriculture sector.

Data on such industry-wise state domestic product is available for the period of our study[15]. The motivation to specifically consider the agriculture sector to evaluate any multiplicative effects of rural transfer spending stems from the following key observations. 1) Rural economy is predominantly agricultural. Figure 1 below summarizes the proportion of the usually working population in the rural regions involved in the agriculture sector[16]. As the figure shows, nearly three quarters of the rural workforce is employed in the agriculture sector. The proportion of rural workforce employed in the agriculture sector is even higher for the period before liberalization (1991).

Hence although the absence of data on rural income restricts any comparison on the contribution of the agriculture sector to the total rural income, employment records show that the dependence on the agriculture sector is extremely high in the rural regions. 2) The primary or the secondary objective of most of the RDPs, apart from insuring the rural poor against sharp fluctuations in income and employment, is to increase agricultural productivity. Although the objective is not to produce direct marketable goods, most of the works allowed under such RDPs like soil and water conservation, flood protection, land development, and afforestation (among others) aim to strengthen the rural infrastructure and thereby increase agricultural productivity. Furthermore, most of the employment guarantee programmes aim to the support the unemployed or underemployed in the agriculture sector.

These programmes specifically aim to assist the marginal farmers, landless households, and the unemployed (or underemployed) during the lean agricultural season. Therefore, relative to all the sectors, rural transfer spending most comprehensively impacts the agriculture sector. 3) Unlike most of the developed economies where the agricultural output is just 1-2% of the GDP, the agricultural output in India forms a substantial portion of the gross domestic product. As Figure 2 shows, national agriculture output accounts for as high as 35% of the national GDP in the early 1980s and although the share of agricultural output has been declining over the years[17], the share of national agricultural output is above 20% (of the national GDP) for most of the period[18].

[15] Agricultural component of the state domestic product includes: agriculture, forestry and fishing which are together known as Agriculture and Allied Activities.

[16] Source: National Sample Survey Reports (various rounds) on the *Employment and Unemployment Situation in the Country*. *Usually working* refers to the pool of *employed* persons. Figure 1 reports the data corresponding to the principal status (ps) as well as subsidiary status (ss) workers. Principal activity refers to the main economic (non-economic) activity pursued by person in a year. Subsidiary activity refers to any economic activity pursued by a person for a relatively shorter time. The NSS rounds (in the order shown in the figure) correspond to the following years respectively: 1993-94, 1994-95, 1995-96, Jan-Dec 1997, 1999-2000, 2001-02, July-December 2002, Jan-June 2004, 2004-05, 2005-06, 2007-08, 2009-10, and 2011-12. For detailed definitions, see "Concepts and Definitions" section of National Sample Survey Report (2013).

[17] Essentially due to the impressive growth in the services sector.

[18] Source: RBI, *Handbook of Statistics on Indian Economy* 2011-12. The correlation between real growth rates of agricultural GDP and total GDP is around 0.71 with a p-value of 0.00.

Hence we are not overly restrictive in understanding the first order impact of rural transfer spending. Consequently, our primary analysis deals with the multiplicative effects of rural transfer spending on the agriculture sector. We discuss spillover/aggregate state effects in section 8 (Extensions).

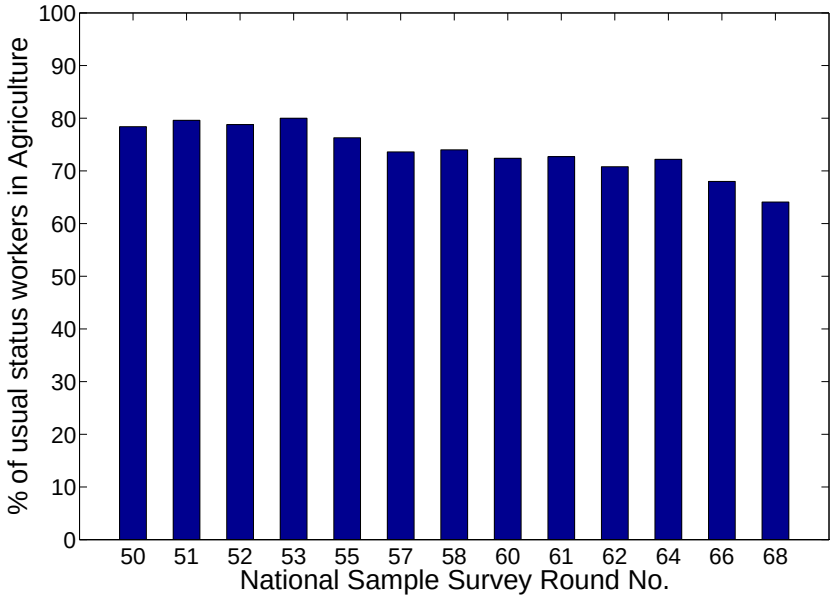

Figure 1: Proportion of *Employed* Rural Workers in Agriculture Sector

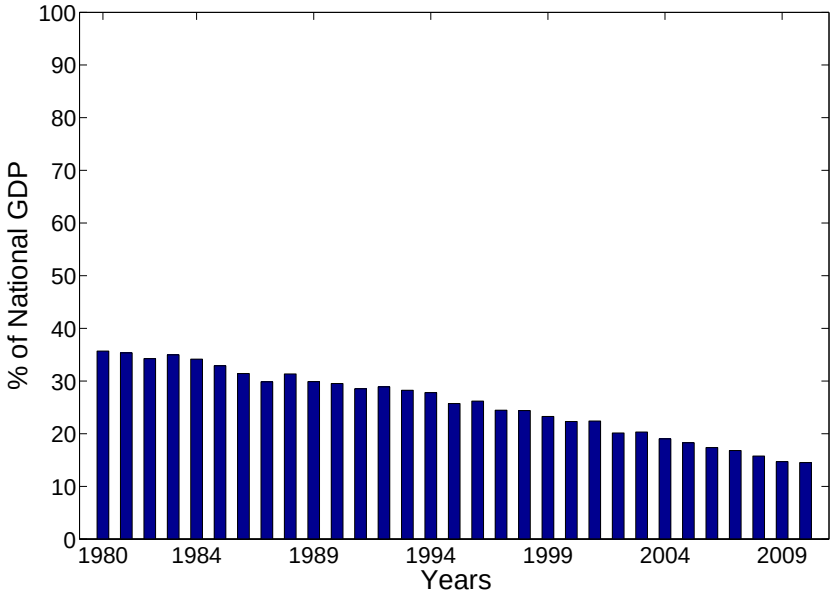

Figure 2: National Agricultural Output as a % of National GDP

All changes and Exogenous changes in Rural Transfer Spending

Given that the expenditure data on RDPs is in nominal prices, we normalize changes in transfer spending by nominal lagged agricultural output. Corresponding to our measure of state-wise total rural transfer spending (see section 2), Figure 3 shows annual changes in this aggregate measure as a percent of previous year's state agricultural output for the period 1986-2010[19]. As the figure shows, the years 2008 and 2009 witnessed large increases in transfer spending. Although a part of this increase can be attributed to the scale up of MG-NREGA, nonetheless the increase in transfer spending overlapped the crisis period and is clearly well above the usual annual variations in aggregate transfer spending. Hence the large injections of transfer funds during 2008-09, may be a discretionary policy response.

Following the construction of our measure of exogenous transfer spending (see Appendix for details), Figure 4 shows the state-wise exogenous changes in transfer spending as a percent of state agricultural output for the period 1981-2010[20]. As Figure 4 shows, most of the exogenous changes are increases in transfer spending and except for the periods of introductory or expansionary spending, our measure of the shock series takes the value zero. The mean of the shock series (across states and time) is 0.20 with a standard deviation of 0.59. A significant number of shock values are above 0.5 with a maximum value of 6.39[21].

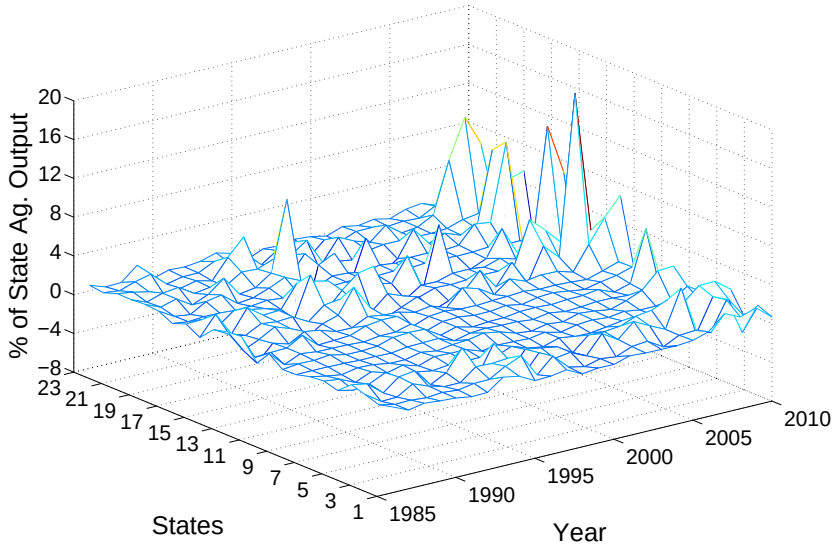

Figure 3: Aggregate Annual Variations in Rural Transfer Spending

[19] Unavailability of state-wise annual expenditure data on IRDP (before 1985) implies that our *total rural development spending* series starts from the year 1985.

[20] Since IRDP does not contribute in the construction of our exogenous measure of transfer spending and due to the availability of data for other relevant programmes before 1985, our shock series is constructed for the period 1981-2010.

[21] As shown in section 7, the results are robust to high leverage or influential points.

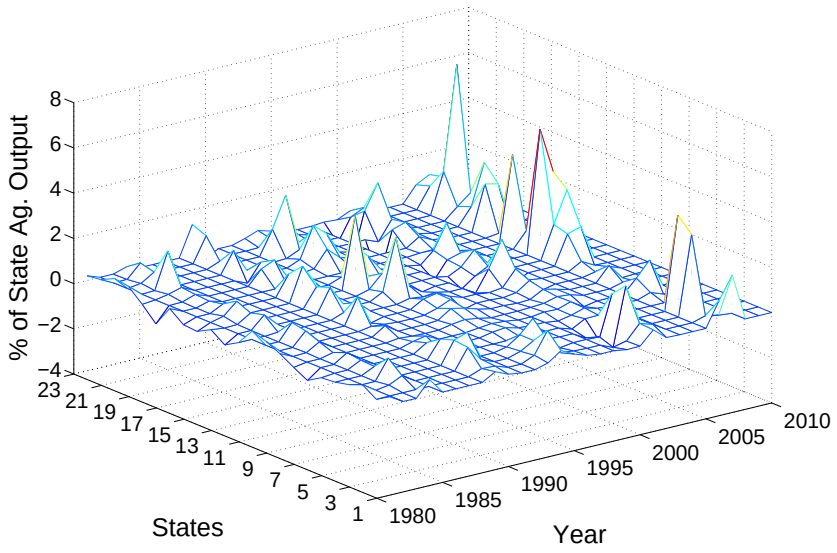

Figure 4: Exogenous Changes in Rural Transfer Spending

Next we test for serial correlation among the shock series shown in Figure 5. We test whether there is any systematic correlation between the introductions of new programmes over a period of 10 years. Figure 5 (a) and (b) graph the scatter plots (along with the fitted line and 95% confidence interval) of: 1) shocks in 1991-2000 versus those in 1981-1990 and 2) shocks in 2001-2010 versus those in 1991-2000 respectively. As can be seen from the figure, the shocks, when compared over a 10 year period are largely orthogonal to each other. Furthermore, the fitted line is very flat and is insignificantly different from zero indicating that past shock values do not have any power in predicting future shocks. Similarly, the correlation of the shocks corresponding to the two cases shown in the Figure 5 is around 0.02 and -0.09 respectively (both the values are insignificantly different from zero). This is evidence against the hypothesis that introductions of new programmes are a periodic feature and are hence predictable.

A natural concern in our study is that although the stated political motivation of the introduction of a new programme seems independent of output fluctuations or factors influencing output, it might not truly be so. Since these programmes are largely agriculture centric, a few consecutive bad harvest years for example might influence the introduction of a new programme as an additional safety net for the rural poor and unemployed. If this is the case, then the transfer spending changes are not truly exogenous and are predictable using such serially correlated shocks to the rural 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.

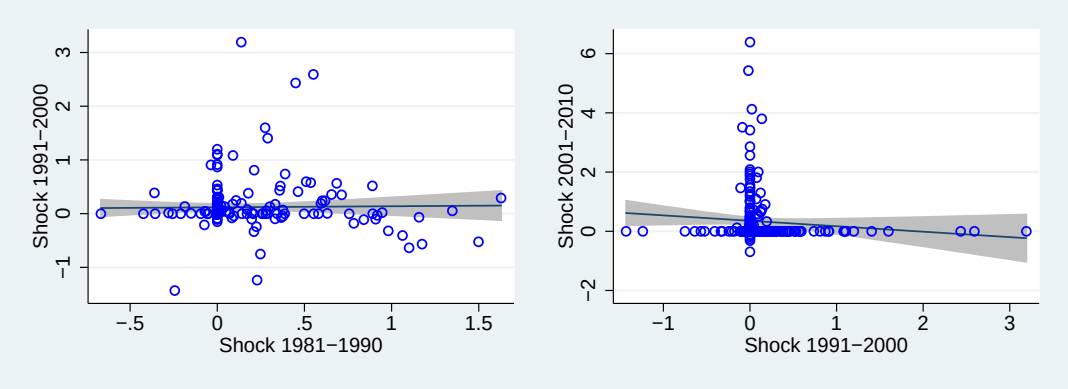

Figure 5: Serial Correlation of Rural Transfer Shock

As a test we regress our shock series on contemporaneous and past changes in real agricultural output (up to lag 3)[22]. All the coefficients of lagged changes in real agricultural output are zero up to two decimal places and are highly insignificantly different from zero ($p = 0.72$). Similarly, as is discussed below in section 6, accounting for lagged changes in real agricultural output as additional controls while calculating the effect of changes in transfer spending does not alter the results in any significant way. This is reassuring and extends support to our identification strategy.

Anticipation effects

Systematic anticipation effects associated with the implementation of a new programme 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, corresponding to the discussion in section 3, the significant difference between the provisional and actual expenditure series underlines that the contemporaneous information set of the implementing authority - the public sector, is itself incomplete[23]. Consequently, it is highly implausible that the information set of the private sector ex-ante the local variations in rural transfer spending was better than that of the implementing authority ex-post - a point also stressed by Ilzetzki et al (2013) for developing economies[24].

Secondly, the target group - the rural poor, faces severe informational constraints regarding such RDPs and about the complete list of benefits that can be availed under such programmes. As a response to alleviate such informational constraints, the government or-

[22] Along with the state-year dummies. Results are invariant to the number of lags chosen.

[23] For example, corresponding to the Indian financial year 2009-10, only provisional estimates of RDP expenditure are released during the year 2010. Actual expenditure estimates are published in the following year.

[24] In fact, in the case of India, there are no private forecasts on rural (or state-wide) transfers.

ganizes various outreach activities *after* a new programme has been implemented. Hence apart from the 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 programme[25].

5 Empirical Model

Since we want to estimate the short run multiplicative effects of transfer spending on output, our baseline model is given by equation (1). For the state i at year t, $Y_{i,t} = y_{i,t} - y_{i,t-1}$ is the difference in the log real agricultural output. $Tr^A_{i,t} = \Delta tr_{i,t}/y_{i,t-1}$ is our exogenous measure of nominal change in transfer spending in year t divided by the nominal agricultural state output in the year $t-1$[26]. The superscript A denotes that the normalization of our nominal measure of exogenous changes in transfer spending is by the agricultural output and not by the aggregate state output. α_i and γ_t respectively denote the state and year fixed effect.

$$Y_{i,t} = \alpha_i + \gamma_t + \sum_{j=0}^{m} \beta_j Tr^A_{i,t-j} + \varepsilon_{i,t} \tag{1}$$

Through the state fixed effect we account for possible endogeneity issues emanating from any state specific characteristics that may be correlated with the spending allocation criteria of the RDPs. For example, as has been noted in section 2, a key determinant of the Centre to the State RDP fund allocation is the ratio of the state poverty to the national poverty. If this state feature is not properly accounted for, the multiplier estimates can be significantly downward biased. It is worthwhile to note that this relative measure of state poverty can be treated as a state fixed effect. That is, for a given state, there have been only minor fluctuations in this relative poverty estimate over the past three decades[27]. Figure 6 plots the state to national rural poverty ratio for all the states.

[25] Kaplan and Violante (2013) in their two asset model show that even wealthy households optimally choose to hold zero or little liquid wealth. Kaplan and Violante report that such "wealthy hand-to-mouth" households 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.

[26] We do not convert the nominal RDP expenditure series into constant price series since there is no uniform GDP deflator for the whole period of our study. There are four different base years spanning 1980-2010: 1980-81, 1993-94, 1999-2000, and 2004-05. Rebasing RDP expenditure data can induce distortions since change in base year not only includes revision of base prices but also includes changes in the methodology and coverage of the national accounts over the previous years (for details see *Sources and Methods,* National Account Statistics 2012).

[27] This is not to say that aggregate poverty has not reduced over the years. It is the relative measure of the state poverty to the national aggregate that has been nearly constant over the period we study.

The "Year" in Figure 6 represents the 'Below Poverty Line' (BPL) census estimation year[28]. As is evident from figure, the relative measure of state poverty to national poverty is highly state specific. Hence, the leading state indicator which determines the level of total outlay and therefore the actual expenditure can be safely regarded as a time invariant state fixed effect. Accounting for the state dummies in our empirical model should take care of any endogeneity concerns arising from the above channel.

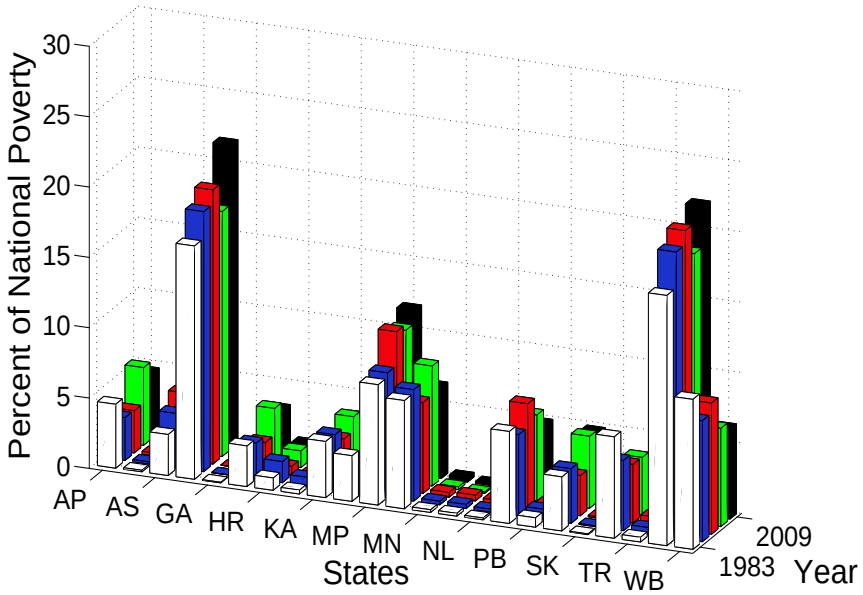

Figure 6: State Rural Poverty as a Ratio of the National Rural Poverty

The year fixed effect controls for aggregate variations common to all the states in a given year. This may include fiscal, monetary, trade, and agricultural policy at the national level[29]. The year fixed effect also controls for changes at the national level during a year that may impact agricultural output in all the states. New technology adoption at country level or the aggregate monsoon performance for example, is controlled by the year fixed effect. Failure to account for such aggregate variations may spuriously result in higher (or lower) multiplier estimates. β_0 estimates the impact multiplier while coefficients of the lags of the transfer shock variable help estimate the cumulative effect of transfer spending on state agricultural output compared to the counterfactual (in logs).

[28] The following BPL census years are shown in Figure 6: 1983 1993 1999 2004 and 2009.

[29] See for example, Christiano et al. (2011), Corsetti et al. (2012), and Ilzetzki et al (2013) (among others) for a discussion on the sensitivity of the multiplier estimates to monetary-fiscal policy mix and to the degree of openness of the economy.

A key feature of the transfer spending in our case is that they are funded by the central government. All the RDPs we study are Centrally Sponsored Schemes (CSS), consequently a large proportion of funds comes from the central government (as discussed in the section 2) while the state governments only have to pool in a small proportion of the aggregate expenditure. On the taxation side, Article 246 of *The Constitution of India* empowers the parliament to make laws on income tax, corporation tax, customs, and excise[30]. Hence throughout the period of our study, the spending on RDPs sponsored by the central government was not matched by variations in the tax burden of the local rural residents. Therefore, specific to our study, omission of tax changes from our set of controls does not cause any problems in our multiplier estimates[31]. While as stated earlier, the year fixed effect controls for the tax policy changes at the national level.

To avoid misleading inference in panel estimation one has to control for any serial or spatial correlation among groups or observations (see for e.g., Bertrand et al. 2004, Angrist and Pischke 2009). We include lagged agricultural output growth to equation (1) to account for serial correlation. Hence in addition to equation (1) we estimate equation (2). We discuss spatial correlation in the section 7 (Robustness). Allowing for spatial correlation among different states in a region does not change the standard errors or our inferences in any significant way.

$$Y_{i,t} = \alpha_i + \gamma_t + \sum_{j=0}^{m} \beta_j Tr^A_{i,t-j} + \sum_{j=1}^{n} \xi_j Y_{i,t-j} + \varepsilon_{i,t} \tag{2}$$

Including lagged agricultural output growth 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 also accounts for hidden motivation of seemingly exogenous policy changes. Specific to our case, if seemingly exogenous introduction of new programmes 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.

[30] List II and III of Article 246 enumerates the areas in which either only state or both the state and parliament can make laws. This however, accounts for a very small proportion of the revenue component.

[31] Acconcia et al (2013) use similar characteristics of the fiscal federalism in Italy in their study to estimate government purchases multiplier.

6 Results

We estimate equation (1) with three lags of the transfer shock series, while equation (2) is estimated with three lags of the dependent variable as additional controls. Corresponding to equation (1), Figure 7 shows the implied effect of an increase in transfer spending of 1% (of the state agricultural GDP) on real state agricultural GDP relative to the counterfactual (in logs) with one (robust) standard error bands. The impact after two years for example, is simply the sum of the coefficients of the contemporaneous and the first two lags of the transfer shock variable. According to the figure, transfer increases have a sustained positive impact on the agricultural output.

The impact multiplier is estimated to be around 0.89 which is significantly different from zero only at one standard error confidence interval (p-value of 0.21). The maximum impact of an increase in output is around 2.22 percent which is barely significant at 10% level. Corresponding to equation (2), Figure 8 summarizes the impact of an increase in transfer spending after accounting for the lags of the dependent variable as additional controls. Hence apart from the direct effect of the transfer spending, the indirect effects working through the lagged (agricultural) output growth are also accounted for while calculating the cumulative effect. As a comparison, Figure 8 also plots the implied effect on agricultural output, as obtained from equation (1). The figure shows the cumulative effect of an increase in transfer spending of 1% of agricultural output for 5 years (relative to normal)[32].

The point estimates are remarkably close for both the specifications while the standard errors corresponding to the case with additional controls are consistently smaller than the former specification. The impact multiplier is estimated to be around 1.02 ($t = 1.49$). The positive effect on output consistently grows to a maximum of 2.07 percent after 3 years which is significantly different from zero ($t = 2.28$) at the standard confidence level. The effect decreases a bit in the last two years but remains positive and significant ($t > 2$). Accounting for the dynamic effects of lagged spending and appropriately correcting for the lagged output growth, the overall multiplier[33] is estimated to be around 1.71 which is statistically different from zero at 5% level. Given that our results continue to be stable after accounting for lagged dependent variable as additional controls in equation (2) is reassuring and lends support to our identification of exogenous rural transfer spending. Estimates corresponding to Figure 8 are summarized Table 2[34].

[32] With lags of the dependent variable as additional controls, we can estimate the cumulative effect beyond 3 years. Just to show that the maximum effect is reached after 3 years, we simulate the cumulative effect up to 5 years.

[33] Calculated as $\left(\sum_{i=0}^{m}\beta_{t-i}\right) / \left(1 - \sum_{i=0}^{n}\xi_{t-i}\right)$.

[34] Estimates reported in the column 1 and 2 in Table 2 are cumulative estimates. These are computed

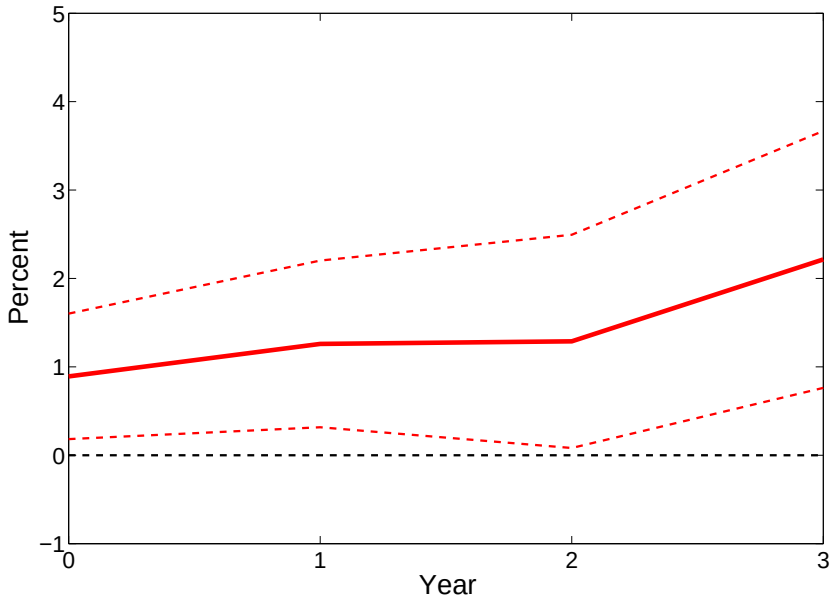

Figure 7: Estimated Impact of a Transfer Increase of 1% of State Agricultural Output on State Agricultural Output using Exogenous Changes (Without Additional Controls)

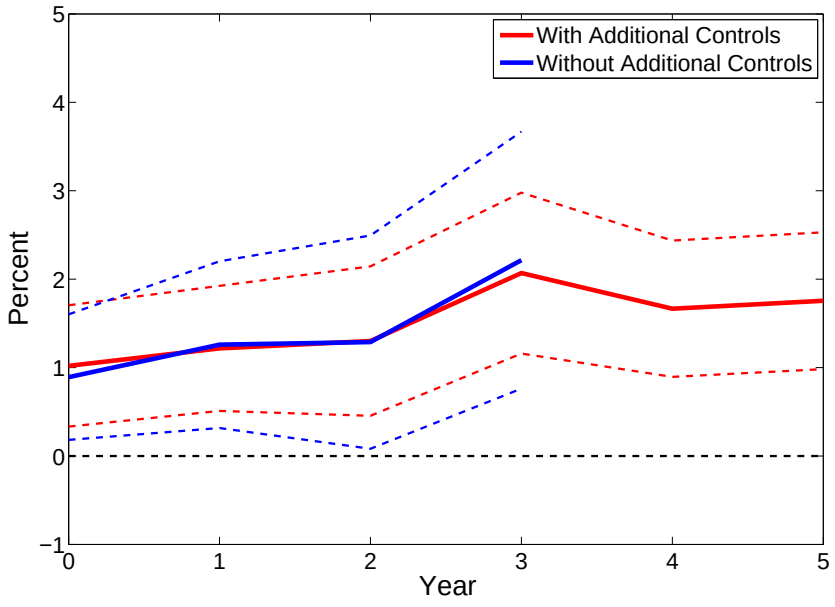

Figure 8: Estimated Impact of a Transfer Increase of 1% of State Agricultural Output on State Agricultural Output using Exogenous Changes

from the first stage estimates of equation (1) and (2) respectively. First stage results are not reported and are available on request. This is true for all the specifications we discuss below.

Comparison with 'All Changes' in transfer spending

The main motivation of this paper is to construct a measure of exogenous changes in rural transfer spending to identify the true output effects of rural transfer spending. Many changes in the transfer spending are correlated with the current state of the economy. Natural disasters like droughts, floods, and cyclones which can cause significant social and economic distress (especially in the rural regions), can also result in higher transfer spending. This increase in transfer spending can be due to a higher demand for such transfer/welfare schemes in times of crisis and can also be a result of higher discretionary spending in the form of disaster aid.

Inability to successfully extract such variations in transfer spending can spuriously downward bias the effect that changes in transfer spending has on output. It is hence useful to see how our baseline results of exogenous transfer spending compare with a broader measure of changes in transfer spending. Our choice of this broader measure is the state-wise, annual variation in *total rural development spending* (as constructed in section 2). We refer to this broader measure as *All Changes*. Figure 9 shows the effect of a 1% increase in transfer spending on output with *All Changes* as the regressor in equations (1) and (2) instead of the shock series. Both the specifications, with and without the lags of the dependent variable (as additional controls) are compared. In both the cases, the effect is small and negative at impact and becomes positive by the third year. The cumulative estimates corresponding to the Figure 9 are summarized in Table 3.

For all the years, the cumulative effect on output in both the specifications never exceeds 0.32 percent and is highly insignificantly different from zero. Figure 10 compares the effect on agricultural output calculated from *All Changes* and *Exogenous Changes* of a 1% (of state agricultural output) increase in transfer spending under the case of additional lagged dependent variables as regressors - our preferred specification. As the figure shows, the effect on output estimated through the exogenous changes is consistently and significantly higher than the effect estimated from the broader measure of transfer changes. The difference in the cumulative effects is significant to the extent that the lower bound of the exogenous effect is almost always higher than the corresponding upper bound effect of *All Changes.* Hence the results show that the effect on output when calculated from the broader measure of transfer spending is significantly downward biased toward zero.

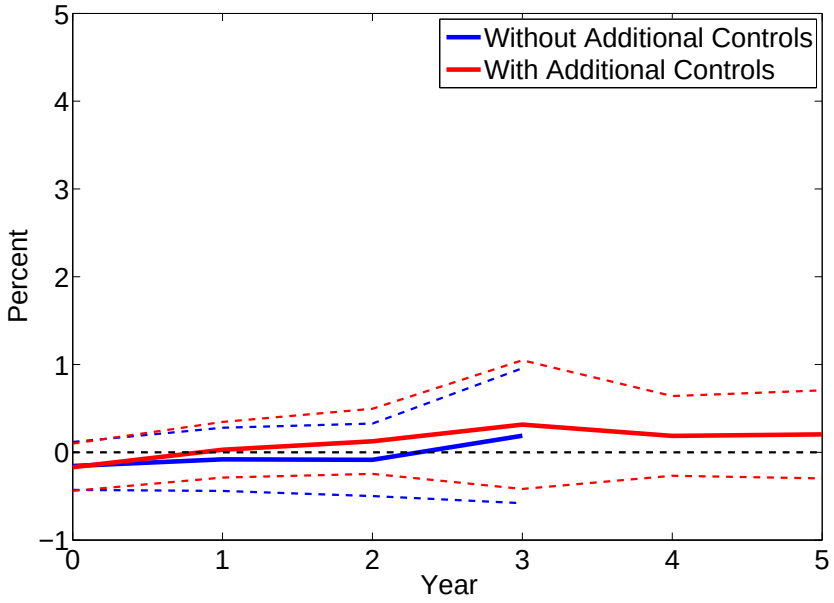

Figure 9: Estimated Impact of a Transfer Increase of 1% of State Agricultural Output on State Agricultural Output using *All Changes*

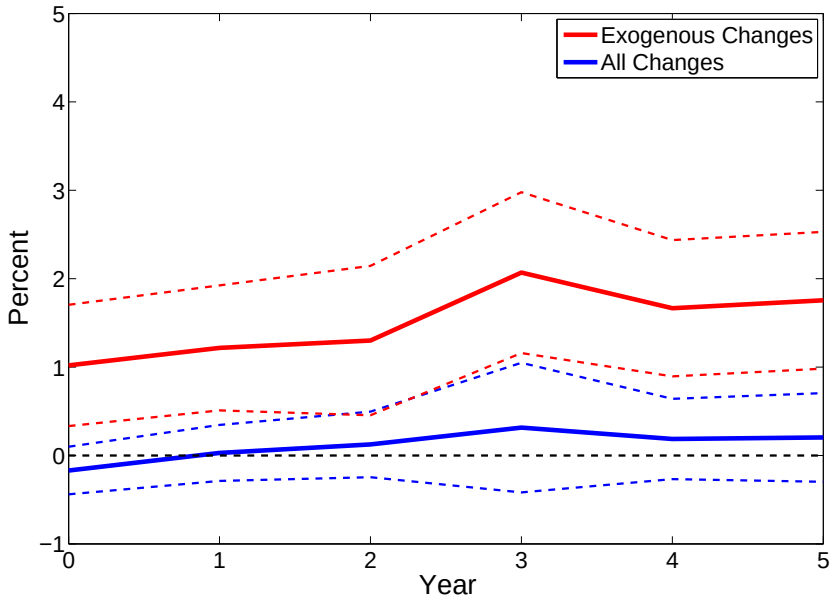

Figure 10: Estimated Impact of a Transfer Increase of 1% of State Agricultural Output on State Agricultural Output

7 Robustness

Sensitivity to Outliers and Influential Points

Figure 11 plots log deviations in real agricultural GDP with our transfer shock series. As is evident from the graph, there are some very large fluctuations in the agricultural output in some states. However, these extreme outliers are not data errors but correspond to extreme weather (or other related) events[35]. As an example, the extreme negative and positive growth rates marked by the red pointers are observations corresponding to the state of Gujarat in the financial years 1987-88 and 1988-89 respectively. The predominantly weather dependent agricultural system in India coupled with a massive drought in Gujarat in the financial year 1987-88 can help explain the severe decline in the agricultural output. As is quoted in a study report of the Planning Commission (see Hirway and Roy 2007, p. 14) the drought of 1987-88 in Gujarat "affected more than 87% area of the state" and "... was the worst ever drought during 1973-74 to 2004-05". An extreme drought followed by a better than average monsoon the following year can hence explain the two outliers. Similarly the graph also shows the presence of some high leverage points. It is important to check the sensitivity of our baseline results to such outliers and influential observations.

Figure 11: Change in Log State Agricultural Output versus Rural Transfer Shock

[35] As confirmed by the National Accounts unit of the Central Statistics Office (CSO), New Delhi.

We employ four robustness tests to check the sensitivity of our results to outliers and influential observations. First we estimate equation (2) 23 times, dropping one state at a time. Figure 12 compares the baseline result which includes all states (together with one standard error bands), with only the point estimates of the cases which respectively correspond to the maximum and minimum effect achieved after three years (among the 23 possible cases). As the figure shows, dropping one state at a time does not change the results in any significant way. In both the (maximum and minimum) cases, the effect on output due to an increase in transfer spending closely traces the path of the baseline result.

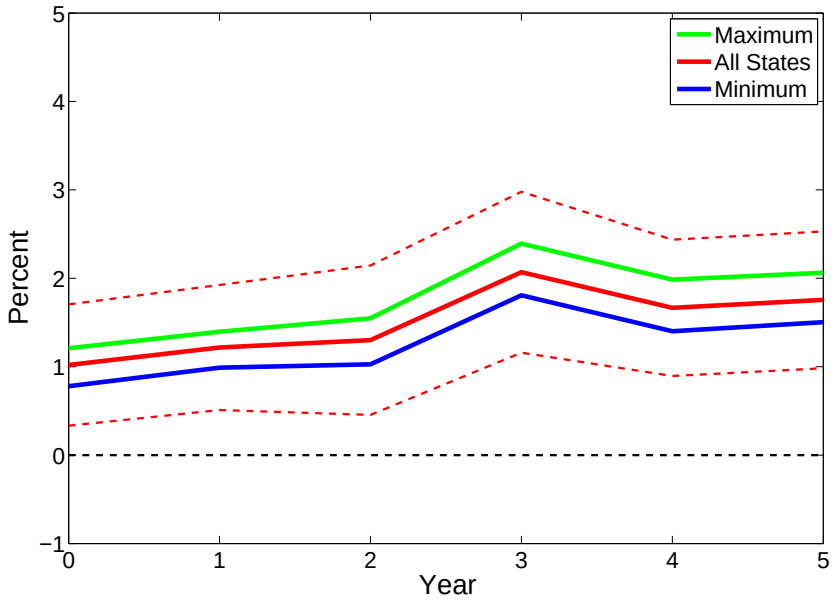

Figure 12: Baseline Result with 'All States' Compared with the Maximum and Minimum Estimates of the 23 Regressions

Second, we estimate robust regression of equation (2) following Li (1985). The procedure initially screens out gross outliers with Cook's distance > 1 and iterates until convergence based on Huber weights (see Huber 1964) followed by biweights (see Beaton and Tukey 1974, 151–152). Figure 13 compares the baseline result (together with one standard error bands) with the calculated cumulative effect on output obtained from the robust regression of equation (2) (without the confidence bands)[36]. The point estimates obtained from the robust regression, although close to the OLS results, are always comparatively higher. The impact multiplier is around 1.27 and is statistically significant at 5% level.

[36]We do not report standard errors of the robust regression (which are calculated using the pseudo values approach described in Street, Carroll, and Ruppert 1988) since the objective is to check the robustness of the point estimates and not of the standard errors.

Accounting for the lags in transfer spending and past output growth, the overall multiplier is around 2.31 and is highly significant ($t = 2.80$). The second column in Table 4 compares the cumulative robust regression estimates of equation (2) with the baseline results in the first column.

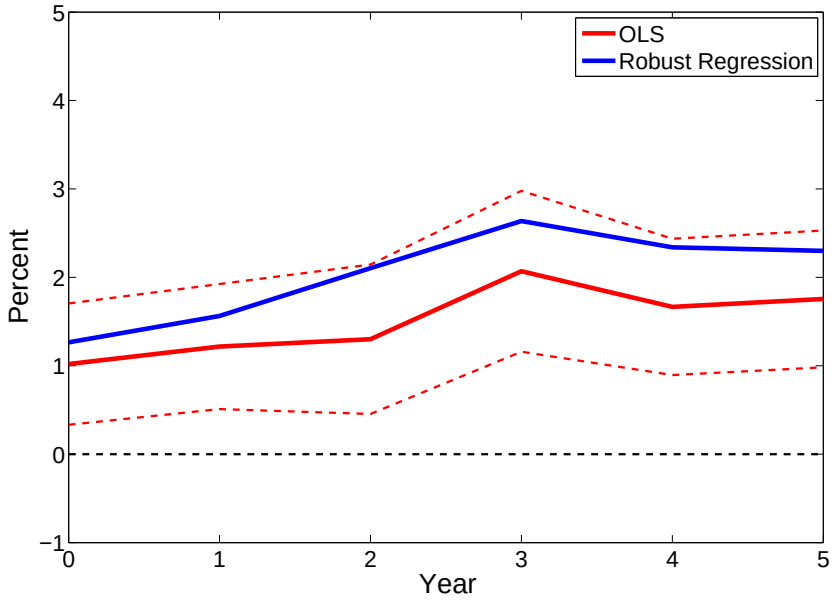

Figure 13: OLS and Robust Regression Estimates

Third 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). Figure 14 shows the baseline results along with cumulative effect of transfer spending derived from the regression estimates of equation (2) after excluding the identified influential points based on the DFITS statistics. The impact multiplier using the DFITS statistics is 1.16 ($t = 1.88$), very similar to the baseline result of 1.02. The effects of transfer spending from then on correspond very closely to the robust regression case - although close to the baseline results, they are consistently higher and highly significant. The overall multiplier (after accounting for the indirect effects of lagged growth) is 2.29 and is highly significant ($t = 3.29$).

Finally, we use the covariance ratio (COVRATIO) measure to identify influential observations based on the influence exerted on the variance-covariance matrix of the estimates (see Belsley, Kuh, and Welsch 1980). After excluding the influential observations based on COVRATIO measure, the estimated effect on output tracks the baseline result very closely (Figure 15). The standard errors using the COVRATIO are slightly larger than the standard errors calculated using all observations.

Columns 3 and 4 of Table 4 show the cumulative regression estimates of equation (2) after respectively excluding influential observations using DFITS and COVRATIO statistic (as shown in Figures 14 and 15)

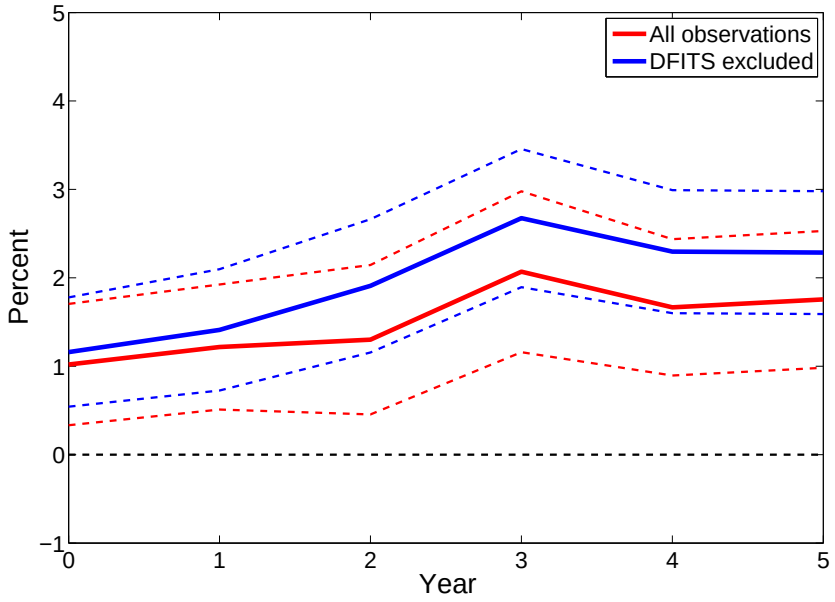

Figure 14: Rural Transfer Spending Impact Based on DFITS Excluded Observations

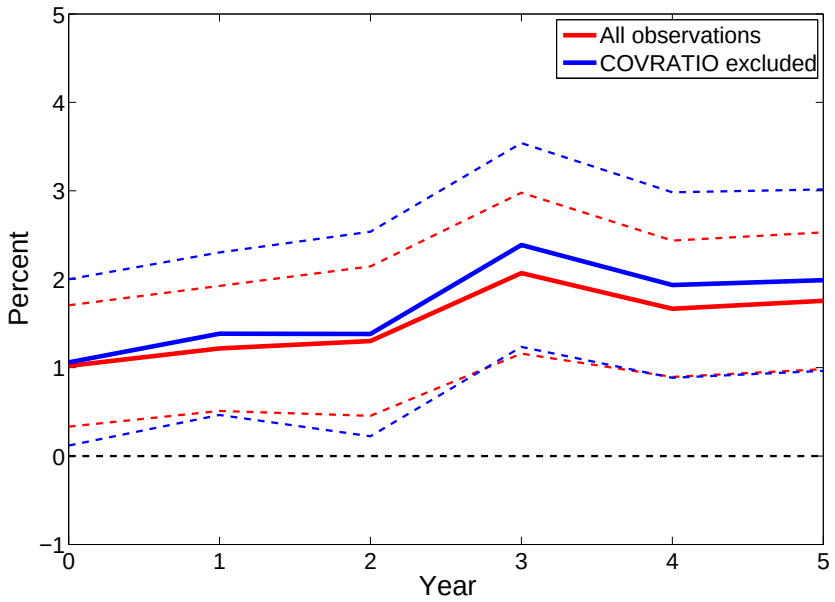

Figure 15: Rural Transfer Spending Impact Based on COVRATIO Excluded Observations

Dynamic Panel Bias

It is well known that estimating a Least Squares Dummy Variable (LSDV) model with lagged dependent variable results in biased estimates of the coefficients in equation (2). Nickell (1981) derives an analytical expression of the bias in ξ_1 in equation (2)[37] to be of the order $1/T$ for *reasonably large* T. Given that T in our case is 27 (for the estimated sample); one should expect a small bias. However, as Judson and Owen (1999) note - the dynamic panel bias in macro panels *can* be significant even for T as large as 30. Following Bruno (2005a, 2005b) who extends the LSDV bias approximations by Bun and Kiviet (2003) to unbalanced panels, we estimate a *corrected version* of the Least Squares Dummy Variable (LSDVC) for equation (2)[38].

The last column in Table 4 reports the LSDVC estimates as shown in Figure 16. Figure 16 compares the cumulative effect of transfer spending under LSDV (baseline estimation) and LSDVC. As the figure shows, the resulting cumulative effect from the two estimators give identical results with the bootstrapped standard errors of LSDVC slightly larger than the robust standard errors of LSDV[39].

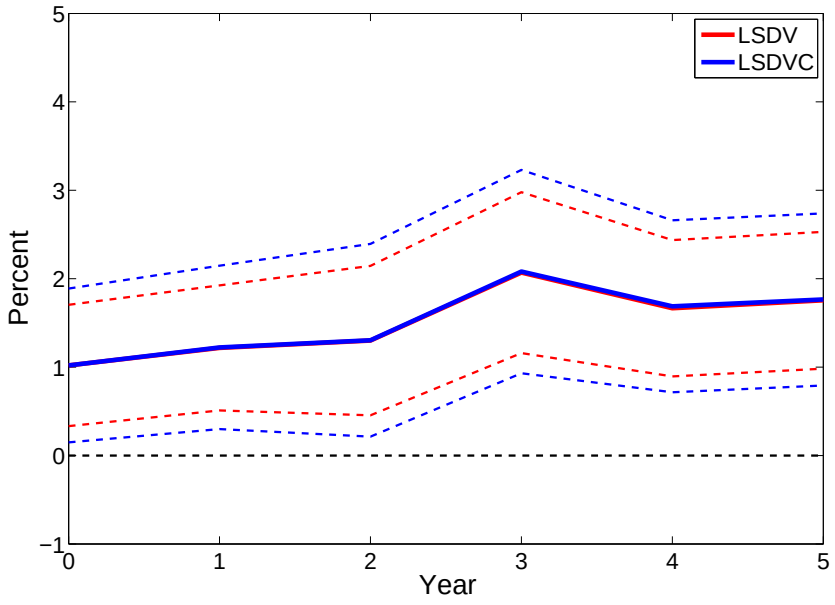

Figure 16: Baseline (LSDV) and Corrected LSDV

[37] After ignoring the time fixed effects and other exogenous variables $x_j's$.

[38] For a macro panel of our size with relatively small N and large T, Judson and Owen (1999) strongly advocates LSDVC over traditional estimators like Anderson Hsiao and GMM estimators (using Monte Carlo evidence).

[39] We take Anderson Hsiao as the initial estimator (see Anderson and Hsiao 1982), in LSDVC. The bias is corrected up to the order $1/(NT)^2$ and bootstrapped standard errors are based on 1000 repetitions.

Spatial Correlation

Since our dependent variable is growth rate in agricultural GDP, it is possible that factors determining agricultural output in a state are region-wise correlated. This may be due to intra-regional trade, technology transfer and sharing of common natural resources like rivers and dams among other factors. Importantly, there can be heterogeneity in the distribution of rainfall among different regions of the country (which is not controlled by the state-year fixed effect). Given that rainfed agriculture is an important component of the agriculture sector in India, this may further intensify any such spatial correlation. It is hence important to check the implications of relaxing the independence (of observations) assumption. We therefore group the 23 states we study in 6 regions: North, North-East, East, West, Central, and South[40] and base our inference on standard errors which are robust to such contemporaneous spatial correlation by allowing for 180 clusters (6 regions $\times$ 30 years = 180 clusters)[41]. Figure 17 compares clustered standard errors with robust standard errors (our default choice) for the baseline results. The standard error bands in both the cases are roughly identical. Relaxing the assumption of independence of observations among clusters makes little difference in the interpretation of our results.

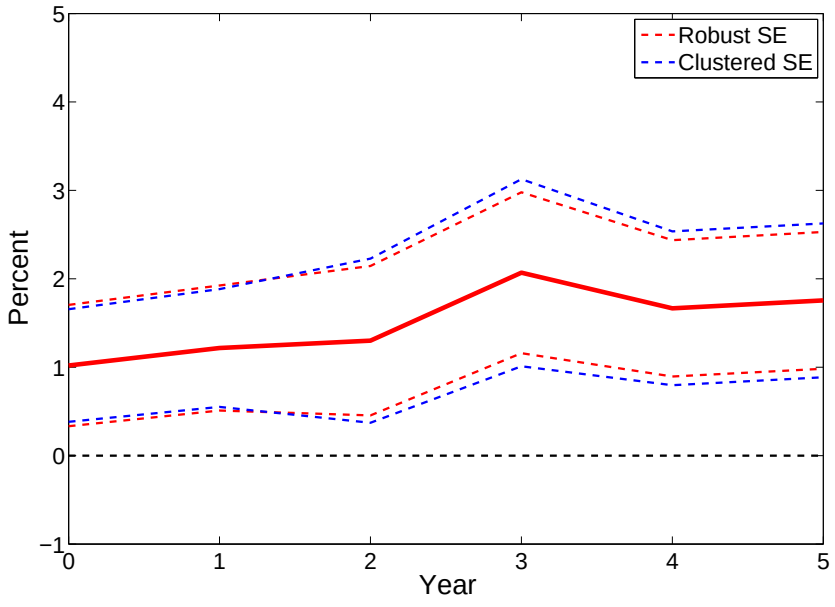

Figure 17: Clustered versus Robust Standard Errors

[40] *Northern region*: Haryana, Himachal Pradesh, Punjab, Rajasthan, and Uttar Pradesh; *North-East region*: Arunachal Pradesh, Assam, Manipur, Meghalaya, Nagaland, Sikkim, and Tripura; *Eastern region*: Bihar, Odisha, and West Bengal; *Western region*: Goa, Gujarat, Maharashtra; *Central region*: Madhya Pradesh; and *Southern region*: Andhra Pradesh, Karnataka, Kerala, and Tamil Nadu.

[41] 162 clusters corresponding to the estimated sample ($T = 27$).

8 Extensions

Heterogeneity

Up till now we have analyzed the multiplicative output effects of rural transfer spending. Our measure of total rural transfer spending involved all major RDPs since 1980. However, as a policy perspective it is important to check for any heterogeneity in the output response to different kinds of rural development programmes. Accordingly, in this section we analyze the output response of *Employment Generation Spending* (hereafter referred to as EGS). As has been highlighted in section 2, with the exception of IRDP (later restructured to SGSY) and IAY which were respectively credit-cum-subsidy and housing schemes, the rest of the RDPs were employment generation programmes. EGS hence refers to the expenditure occurred under all employment generation programmes.

Total Employment Generation Spending is simply the aggregate expenditure that occurred under all the active employment generation programmes within each state, in a given year. The construction of EGS shock series exactly follows the narrative analysis in section 4 except that the measure of exogenous changes in EGS excludes the exogenous expenditure attributed to IAY in 1995-96 and 1996-97 and SGSY in 1999-2000 and 2000-01[42]. Figures 18 and 19 show *All Changes* in EGS and *Exogenous Changes* in EGS (both normalized by previous year's nominal state agricultural output).

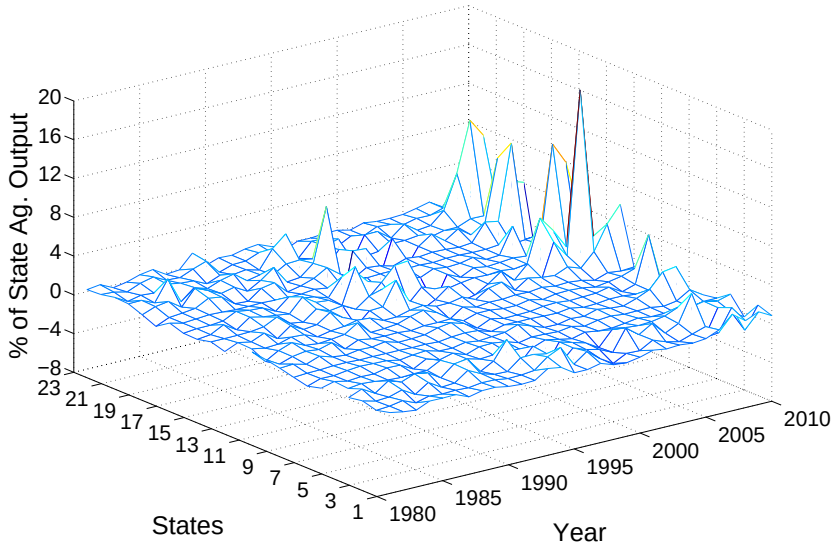

Figure 18: Aggregate Annual Variations in Transfer Spending with Only Employment Generation Programmes

[42] As discussed in Appendix, IRDP does not contribute in the construction of exogenous shock series since it was already introduced in 1978.

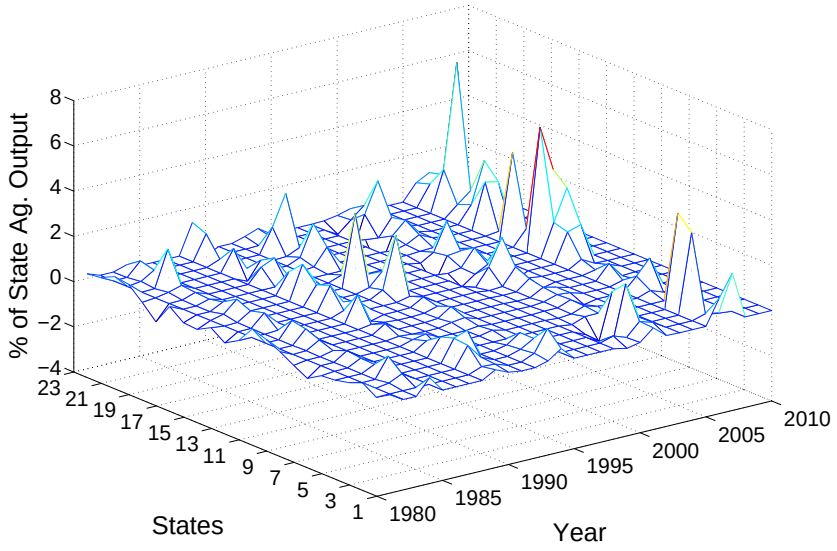

Figure 19: Exogenous Transfer Spending with Only Employment Generation Programmes

Analogous to equation (2), $Es^A_{i,t}$ in equation (3) represents the EGS shock for state i in year t where the superscript A denotes normalization by nominal lagged agricultural output. Corresponding to equation (3), Figure 20 shows the cumulative effect of a 1% (of agricultural GDP) increase in employment spending on agricultural output relative to normal (in logs). For comparison, the figure also shows the response of agricultural output following increases in spending due to two other measures - *All changes* in employment spending[43] and the transfer shock (refer Figure 8).

$$Y_{i,t} = \alpha_i + \gamma_t + \sum_{j=0}^{m} \beta_j Es^A_{i,t-j} + \sum_{j=1}^{n} \xi_j Y_{i,t-j} + \varepsilon_{i,t} \tag{3}$$

Similar to *All Changes* in transfer spending, *All Changes* in employment spending is simply the state-wise, annual variation in *Total Employment Generation Spending* (as defined above) and represents the broader measure of variations in employment spending. Table 5 shows the implied cumulative effect due to EGS shock. As a comparison, Table 5 also reports the total implied effect calculated using *All Changes* in total employment spending.

The expansionary effect on agricultural output due to the employment spending shock is almost identical to our baseline exogenous measure - the transfer spending shock. Interestingly, the impact calculated from *All Changes* in employment spending is zero for a couple of years and continues to be statistically insignificant from zero for all the periods.

[43]Simulated from the regression estimates of equation (3) with *All changes* in EGS instead of the EGS shock: Es^A.

The intuition is straightforward; many changes in employment generation spending are due to factors which are correlated with the state of the economy. Droughts, floods, or periods of high rural unemployment may result in higher expenditure in EGS due to a) the automatic component associated with demand based programmes and/or b) the higher discretionary spending as emergency relief. Hence, analogous to the case of the broader measure of changes in rural transfer spending (in section 6), the effect on output when calculated from the broader measure of changes in employment spending is also significantly downward biased toward zero.

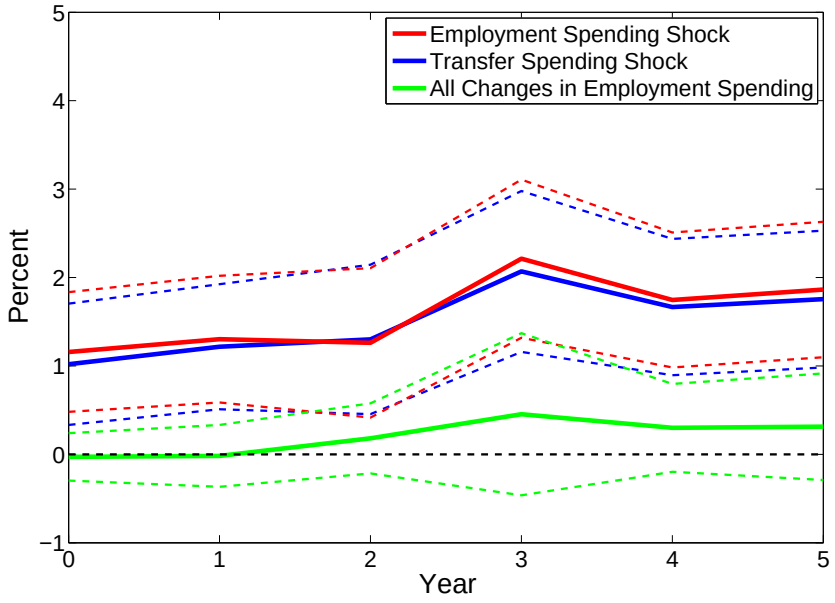

Figure 20: Estimated Impact of a Transfer Increase of 1% of State Agricultural Output on State Agricultural Output using Various Measures

The impact multiplier associated with employment spending shock is about 1.16 and is statistically different from zero at 10% level. The effect on output due to EGS shock reaches a maximum of around 2.21 percent ($t = 2.48$) after 3 years before declining to around 1.7-1.8 percent from the fourth year onwards. The overall multiplier (after correcting for the indirect effects of lagged output growth) is estimated to be around 1.80 ($t = 2.36$). Hence, as the results show, the multiplicative effects of rural transfer spending on the agriculture sector are very similar irrespective of whether the higher spending occurs in employment generation programmes or in any other rural development programme.

Aggregation

We now discuss the state-level effects of variations in rural transfer spending. Such an aggregation may shed some light on the possible spillover effects of rural transfer spending.

If higher rural transfer spending results in a higher demand of products and services from sectors other than the agriculture sector, then this will result in positive spillover effects. On the contrary, if higher rural transfer spending results in substitution of labor leading to a shortage of labor supply and higher wages in other sectors, then this will result in negative spillovers. To estimate the state level impact of rural transfer spending, we estimate equation (4) which is the same as equation (2) except that the dependent variable: $Y_{i,t}^S$ is now the difference in log state GDP while the transfer shock series Tr is normalized by the previous year's nominal state GDP.

$$Y_{i,t}^S = \alpha_i + \gamma_t + \sum_{j=0}^{m} \beta_j Tr_{i,t-j} + \sum_{j=1}^{n} \xi_j Y_{i,t-j}^S + \varepsilon_{i,t} \tag{4}$$

As expected, normalizing the rural transfer shocks by the state GDP results in significantly smaller shocks. The mean of the new transfer shock series normalized by the state output is just around .05 with a standard deviation of .16. Most of the observations in the state GDP normalized shock series are 3-4 times smaller than the respective shocks normalized by the state agricultural output. As a result of 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 21 shows the cumulative effect of a 1% (of state GDP) increase in rural transfer spending on state GDP relative to normal (in logs). The figure compares the impact of transfer spending shock and the broader measure of *All Changes* in total transfer spending (both normalized by the lagged nominal state GDP). Table 6 reports the estimates corresponding to the Figure 21.

The effect on state output estimated by *All Changes* in transfer spending is insignificantly different from zero for a couple of years but becomes significantly below zero after 2 years at -1.46 percent ($t = -2.31$). The point estimate corresponding to the transfer spending shock is just below zero and insignificant at impact and becomes positive after the first year reaching a maximum of 1.38 percent after 3 years. As expected, the standard errors in the transfer shock case are almost 200-250% larger than the case when the transfer shock series is normalized by the lagged agricultural state output. Consequently, the cumulative effect on state output estimated from the shock series is never significantly different from zero even after the positive point estimates after the first year. Furthermore, as Figure 22 shows, the one standard error confidence interval corresponding to the state GDP normalized transfer shock almost always envelops the corresponding interval of state agricultural output normalized transfer shocks.

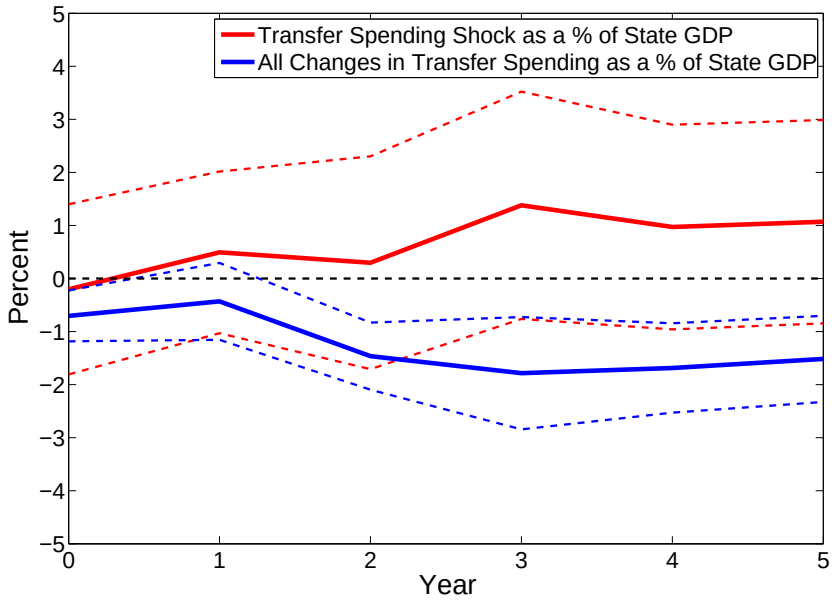

Figure 21: Estimated Impact of a Transfer Increase of 1% of State GDP on State GDP using Shock and *All Changes*

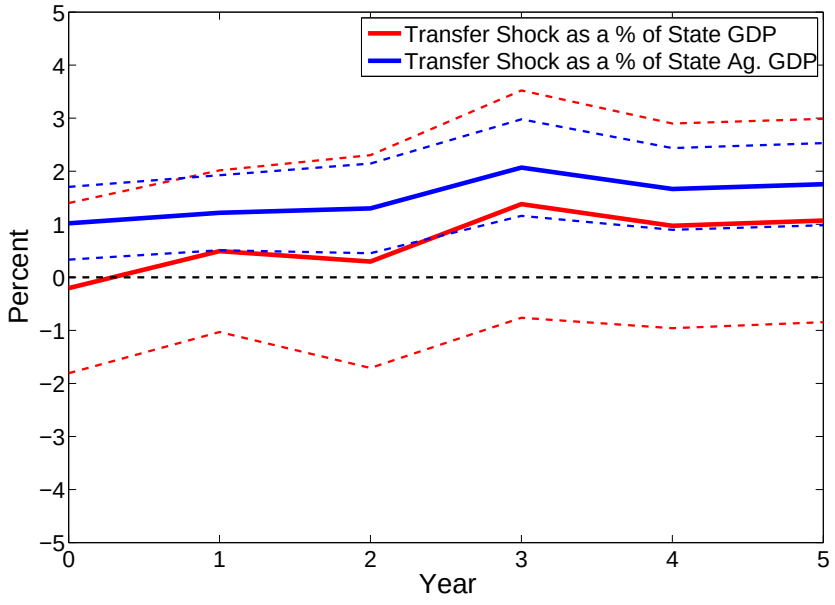

Figure 22: Comparing Transfer Spending Shock Normalized by State GDP and State Agricultural GDP

Another point to take into consideration while aggregating to state-level is to judge the relevance of such an aggregation given our natural experiment. The identification of the exogenous transfer spending is based on the variation in the *rural regions and blocks* within each state during the introduction or expansion of a new programme. The reliability of aggregate state effects estimated from the rural transfer spending shocks depends on how representative are the rural transfer shocks as aggregate statewide changes in transfer spending. Although we do not study statewide transfer programmes for such a comparison (primarily due to the challenge in obtaining the relevant data for the period of our study), it's a promising route for future analysis. Hence, although the effect of rural transfer spending on aggregate state output seems to be smaller than that on state agricultural output, the spillover effects are ambiguous at best.

9 Conclusions

Rural development programmes in India are implemented for a variety of reasons. A key question is whether such transfer programmes are consequential for the local economic activity. This paper estimates the multiplicative effects of rural transfer spending on agricultural output at the state level. Using narrative records of the Government of India, we construct the introductory and expansionary variation which occurs during the implementation of a new RDP. We use this variation as a measure of change in rural transfer spending exogenous to fluctuations in local output. Our state-sector relative multiplier is estimated to be 1.02 while accounting for the dynamic effects of lagged spending, raises this estimate to 1.71. The results are highly robust to various forms of misspecifications. The multiplier estimates calculated from the exogenous expenditure variation in employment generation programmes (among the RDPs we study) are nearly identical to the baseline results.

The state-sector relative multiplier corresponding to employment spending is 1.16 on impact and increases to 1.80 after adding the dynamic effects of lagged spending. Our results of greater than one state-sector relative multiplier do not reflect aggregate tax or monetary policy interactions. Although tax and monetary policy are key determinants while estimating aggregate national multipliers, in our analysis they are controlled for by the year fixed effect[44]. Aggregation at the state level, to analyze any spillover effects due to rural transfer shocks on other sectors of the state gives inconclusive evidence. This partly corresponds to the decline in the magnitude of the shocks when normalized by aggregate state output which results in large standard errors. Another point to consider is that although our identification

[44]See for e.g., Nakamura and Steinsson (2012) and Shoag (2011) who discuss the theoretical mapping in an open economy framework to compare local fiscal multipliers to national multipliers.

strategy specifically identifies exogenous variations in rural transfer spending, it does not necessarily represent exogenous state-wide transfers (which would include both rural and urban transfers). The results indicate that local variations in rural transfer spending may have significant impact on economic activity in rural areas.

We have been largely silent on the transmission mechanism of the sector specific multiplicative effects of rural transfer spending. Although impact multipliers greater than 1 suggest substantial demand stimulus due to social transfers, the growing and persistent positive effect on output may also indicate supply side responses. Finally, our measure of state-wise *total rural transfer spending* is an important contribution of our study that can be employed in future studies which analyze transfer spending in India. A promising extension to the study can be to analyze labor market responses specifically due to the employment guarantee programmes. Such an analysis can facilitate a better understanding of the output and labor market effects due to such transfer programmes.

Table 1: Major Centrally Sponsored Rural Development Programmes Since 1980

1980	IRDP	NREP					
1981	IRDP	NREP					
1982	IRDP	NREP					
1983	IRDP	NREP	RLEGP				
1984	IRDP	NREP	RLEGP				
1985	IRDP	NREP	RLEGP				
1986	IRDP	NREP	RLEGP				
1987	IRDP	NREP	RLEGP				
1988	IRDP	NREP	RLEGP				
1989	IRDP		JRY				
1990	IRDP		JRY				
1991	IRDP		JRY				
1992	IRDP		JRY				
1993	IRDP		JRY		EAS		
1994	IRDP		JRY		EAS		
1995	IRDP		JRY		EAS		IAY
1996	IRDP		JRY		EAS		IAY
1997	IRDP		JRY		EAS		IAY
1998	IRDP		JRY		EAS		IAY
1999	SGSY		JGSY		EAS		IAY
2000	SGSY		JGSY		EAS		IAY
2001	SGSY			SGRY			IAY
2002	SGSY			SGRY			IAY
2003	SGSY			SGRY			IAY
2004	SGSY			SGRY			IAY
2005	SGSY			SGRY			IAY
2006	SGSY			SGRY		NREGA	IAY
2007	SGSY			SGRY		NREGA	IAY
2008	SGSY				NREGA		IAY
2009	SGSY				NREGA		IAY
2010	SGSY				NREGA		IAY

IRDP: Integrated Rural Development Prog.
NREP: National Rural Employment Prog.
RLEGP: Rural Landless Employment Guarantee Prog.
JRY: Jawahar Rozgar Yojana
EAS: Employment Assurance Scheme
IAY: Indira Awaas Yojana
JGSY: Jawahar Gram Samridhi Yojana
SGSY: Swarnjayanti Gram Swarozgar Yojana
SGRY: Sampoorna Grameen Rozgar Yojana
NREGA: National Rural Employment Guarantee Act

Source: *Annual Report*, Ministry of Rural Development (various years). The years in which a new programme was introduced are underlined.

Table 2: Cumulative Estimates Obtained from Estimates of Equation 1 and 2

Year	Equation 1	Equation 2
0	0.892	1.019
	(0.710)	(0.686)
1	1.260	1.217*
	(0.943)	(0.707)
2	1.288	1.300
	(1.206)	(0.845)
3	2.215	2.069**
	(1.453)	(0.910)
4		1.666**
		(0.771)
5		1.756**
		(0.774)
Observations	621	621

*p<0.10,**p<0.05,***p<0.01. Standard errors reported in parentheses are computed using the Delta method. State-year fixed effects are included in all the regressions.

*Unless otherwise mentioned, the first stage estimates (not reported) use Robust Standard errors in all the regressions.

Table 3: Cumulative Estimates with *All Changes* in Total Rural Transfer Spending

Year	Equation 1	Equation 2
0	-0.156	-0.170
	(0.273)	(0.269)
1	-0.081	0.029
	(0.360)	(0.317)
2	-0.086	0.125
	(0.413)	(0.371)
3	0.189	0.315
	(0.769)	(0.733)
4		0.187
		(0.455)
5		0.204
		(0.502)
Observations	506	506

*p<0.10,**p<0.05,***p<0.01. Standard errors reported in parentheses are computed using the Delta method. State-year fixed effects are included in all the regressions.

Table 4: Results from Various Robustness Specifications

Year	Baseline	RReg	DFITS	COVRATIO	LSDVC
0	1.019	1.265**	1.160*	1.059	1.019
	(0.686)	(0.643)	(0.618)	(0.940)	(0.870)
1	1.217*	1.563**	1.412**	1.384	1.223
	(0.707)	(0.753)	(0.686)	(0.919)	(0.924)
2	1.300	2.104**	1.909**	1.380	1.304
	(0.845)	(0.894)	(0.754)	(1.158)	(1.090)
3	2.069**	2.637***	2.675***	2.387**	2.081*
	(0.910)	(0.978)	(0.781)	(1.153)	(1.150)
4	1.666**	2.340***	2.296***	1.934*	1.688*
	(0.771)	(0.835)	(0.696)	(1.049)	(0.973)
5	1.756**	2.300***	2.285***	1.989*	1.765*
	(0.774)	(0.823)	(0.696)	(1.026)	(0.973)
Observations	621	621	585	586	621

*p<0.10,**p<0.05,***p<0.01. RReg: Robust regression; DFITS: Observations excluded using DFITS statistic; COVRATIO: Observations excluded using COVRATIO statistic; LSDVC: LSDV corrected. Standard errors reported in parentheses are computed using the Delta method. The first stage estimates for RReg use standard errors calculated using pseudo values approach (see Street, Carroll, and Ruppert 1988). While the first stage estimates of LSDVC report Bootstrapped standard errors with 1000 repititions. State-year fixed effects are accounted for in all the regressions.

Table 5: Cumulative Estimates of Equation 3 with: 1) EGS Shock and 2) *All Changes* in Total EGS

Year	EGS Shock	EGS *All Changes*
0	1.158*	-0.028
	(0.677)	(0.268)
1	1.303*	-0.017
	(0.716)	(0.350)
2	1.261	0.181
	(0.845)	(0.396)
3	2.213**	0.454
	(0.894)	(0.917)
4	1.746**	0.300
	(0. 763)	(0.497)
5	1.864**	0.313
	(0.766)	(0.603)
Observations	621	619

*p<0.10,**p<0.05,***p<0.01. EGS: Employment Generation Spending. Standard errors reported in parentheses are computed using the Delta method. State-year fixed effects are included in all the regressions.

Table 6: Cumulative Estimates with State GDP Normalized Regressors

Year	Shock	*All Changes*
0	-0.203	-0.704
	(1.604)	(0.480)
1	0.493	-0.431
	(1.523)	(0.725)
2	0.297	-1.463**
	(2.006)	(0.633)
3	1.381	-1.785*
	(2.143)	(1.059)
4	0.971	-1.686**
	(1.929)	(0.843)
5	1.071	-1.515*
	(1.917)	(0.812)
Observations	621	619

*p<0.10,**p<0.05,***p<0.01. Both Shock and *All Changes* are normalized by State GDP. Standard errors reported in parentheses are computed using the Delta method. State-year fixed effects are included in all the regressions.

Appendix

Below we discuss a thorough programme by programme construction of our rural transfer spending shock series. X_t denotes the actual expenditure occurred in the programme 'X' during the financial year t to $t+1$[45].The Financial Year (FY) in India starts from 1^{st} April to 31^{st} March of the next calendar year. We refer *Annual Report* of Ministry of Rural Development as AR MORD.

Integrated Rural Development Programme (IRDP) 1978-79 to 1998-99

1980-81: Nil (Exogenous introductory spending)

1981-82: Nil (Exogenous expansionary spending)

IRDP was arguably the first major rural development programme which was launched in 1978-79 (AR MoRD 1980-81). The objective of the programme was to alleviate rural poverty by "providing income generating assets and access to credit and other inputs" (AR MoRD 1980-81, p. 3) to "all persons who live below the poverty line" (p. 4). The programme 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^{nd} October 1980. The cost related to the programme was equally shared between the Centre and the States (p. 5). Since our analysis start from the FY 1980-81, and since introduction and expansion of IRDP to cover the entire country already happened by FY 1980-81, expenditure occurred under IRDP does not enter our shock series[46].

National Rural Employment Programme (NREP) 1980-81 to 1988-89

1980-81: NREP_{1980} (Exogenous introductory spending)

1981-82: $\text{NREP}_{1981} - \text{NREP}_{1980}$ (Exogenous expansionary spending)

To tackle the serious problem of rural unemployment and under-employment the food for work programme was launched on the 1^{st} of April 1977 (AR MoRD 1980-81, p. 17). The report also quotes that although the programme was "successful in achieving its basic objectives ... a number of shortcomings and drawbacks were noticed in the implementation of the food for work programme" (p. 18). With the motivation to address the shortcomings in the food for work programme, NREP "replaced the Food for Work Programme in October, 1980" (AR MoRD 1982-83, p. 30) and was financed by the Central government up till 31^{st} March 1981 while the cost was equally shared between the Centre and the States from the FY 1982-83 (p. 32). The programme envisaged generation of gainful employment for both unemployed and underemployed workers in the rural areas.

Allocation of funds from the Centre to States was based on a weighted average of the

[45] We ignore the 'State' subscript i in $X_{i,t}$ since expenditure discussed under any programme is always discussed state-wise and not as a national aggregate.

[46] Also, state-wise annual expenditure under IRDP is reported only from the FY 1985-86.

population of agricultural laborers, marginal farmers, and the incidence of poverty in each state (AR 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 the FY 1980-81 serves as exogenous introductory spending. Similarly any *additional* expenditure under NREP during its advancement as a major rural employment programme in the country in FY 1981-82 is also treated as exogenous variation. However, since NREP was already set in motion in 1980, we only take the difference in expenditure $\text{NREP}_{1981} - \text{NREP}_{1980}$ as the exogenous expansionary variation.

Rural Landless Employment Guarantee Programme (RLEGP) 1983-84 to 1988-89

1983-84: RLEGP_{1983} (Exogenous introductory spending)

1984-85: $\text{RLEGP}_{1984} - \text{RLEGP}_{1983}$ (Exogenous expansionary spending)

RLEGP was launched in different States/Union Territories on 15^{th} October 1983 (AR MoRD 1983-84, p. 41). The report further states that although there were several schemes like 'Training of Rural Youth for Self-Employment' (TRYSEM), 'Development of Women and Children in Rural Areas' (DWCRA), and 'Drought Prone Areas Programme' (DPAP) which were operational during the time and were geared towards the provision of rural employment/poverty alleviation, RLEGP was introduced to specifically target the core of rural poverty: landless farmers and laborers[47]. RLEGP was introduced with an objective to (*i*) provide 100 days of employment in a year to at least one member of every landless labor household and (*ii*) create durable assets to strengthen rural infrastructure (p. 41-42). The programme was fully funded by the central government.

Construction of rural link road, land development, and soil and water conservation projects (among others) were allowed under RLEGP with the restriction that "the wage component in a project should ... not be less that 50% of the total cost of the project" (p. 42). Since the programme was introduced in the second half of the financial year 1983-84, the total funds allocated to RLEGP during 1983-84 were limited to around 100 crore rupees. While a provision of 400 crores was made for the following year[48]. Correspondingly RLEGP was introduced in only 13 (out of the 25) Indian states in 1983-84 and was extended to cover 24 states by 1984-85. Hence similar to the case of NREP, exogenous introductory variation in 1983-84 is taken as the actual expenditure incurred under RLEGP during 1983-84.

[47] We do not study programmes like TRYSEM, DWCRA, and DPAP separately since these programmes were much smaller in scope and total outlay of funds. Furthermore, some of these programmes served as sub schemes of larger programmes like IRDP already included in our study.

[48] Although actual expenditure under RLEGP during 1983-84 was only 6.21 crore rupees while nearly 372 crore rupees were spent the following year.

For the year 1984-85, $\text{RLEGP}_{1984}-\text{RLEGP}_{1983}$ is considered as the exogenous expansionary variation.

Jawahar Rozgar Yojana (JRY) 1989-90 to 1998-99

1989-90: JRY_{1989} − (NREP_{1988}+ RLEGP_{1988}) (Exogenous introductory spending)

1990-91: $\text{JRY}_{1990}-\text{JRY}_{1989}$ (Exogenous expansionary spending)

JRY was launched on 1^{st} April 1989 (AR MoRD 1990-91, p. 37). As outlined in the *Annual Plan* 1990-91 Planning Commission, for the two programmes NREP and RLEGP, there were weaknesses and inefficiencies identified in a) the distribution of funds from the States to its districts and regions; b) the creation of productive assets; and c) the process of project approval. To intensify the process of employment generation and address the above concerns, JRY was introduced as a comprehensive employment generation programme which enveloped NREP and RLEGP. In line with previous rural employment generation programmes, the primary objective of JRY was creation of additional employment for the unemployed and underemployed in the rural areas with the secondary objective as the development of rural infrastructure (AR MoRD 1990-91, p. 37). The report further details that the cost ratio between the Centre and the States was 80:20. Under the programme, Central assistance to the States was determined on the "proportion of rural poor in a State/UT to the total rural poor in the country" (p. 37). Projects allowed under JRY were similar to the ones planned under the previous programmes. Since JRY was a result of the restructuring of two previous programmes NREP and RLEGP, we do not treat it as a 'new standalone' programme. However given the significant increase in outlay earmarked for rural employment generation (under JRY), we consider JRY_{1989} − (NREP_{1988}+ RLEGP_{1988}) as exogenous introductory spending for the year 1989-90. While $\text{JRY}_{1990}-\text{JRY}_{1989}$ is the exogenous expansionary variation for the year 1990-91.

Employment Assurance Scheme (EAS) 1993-94 to 2001-02

1993-94: EAS_{1993} (Exogenous introductory spending)

1994-95: $\text{EAS}_{1994}-\text{EAS}_{1993}$ (Exogenous expansionary spending)

EAS was implemented from 2^{nd} October 1993 in 1778 rural blocks of the country (AR MoRD 1998-99, p. 17). 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" (AR MoRD 1993-94, p. 19). EAS was a demand driven scheme; hence there was no fixed allocation of funds for any district or block. The cost was shared between the Centre and States in the ratio 80:20. EAS guaranteed 100 days of employment to a maximum of two adults per family (AR MoRD 1998-99, p. 37)[49]. The report further quotes that in

[49]With effect from 1^{st} April 1999 the cost sharing ratio between the Centre and the States was revised to

the financial year 1994-95, EAS was extended to another 665 rural blocks of the country (p. 17)[50]. Since EAS was a new programme, the expenditure occurred under EAS during 1993-94 is taken as the exogenous introductory spending for the same year. For the year 1994-95, $EAS_{94} - EAS_{93}$ is our measure for exogenous expansionary variation[51].

Indira Awaas Yojana (IAY) 1995-96 to present

1995-96: IAY_{1995} (Exogenous introductory spending)

1996-97: $IAY_{1996} - IAY_{1995}$ (Exogenous expansionary spending)

IAY was first implemented as a sub scheme of RLEGP in 1985-86 with an objective to provide free of cost housing to the members of backward castes like Scheduled Castes/Scheduled Tribes (SC/ST) in rural areas, it then continued as a sub scheme of JRY from 1989-90[52] (AR MoRD 1995-96, p. 20). The report further states that in order to effectively target the mutually exclusive objectives of rural employment and rural housing, IAY was implemented as an independent, centrally sponsored scheme from 1^{st} January 1996 (p. 20). As stated in the *Annual Plan* 2001-02 Planning Commission, the funding pattern between the Centre and the States was in the ratio of 75:25[53]. State poverty ratio and rural housing shortage are the prime determinants in the allocation of funds from the Centre to the States. The total expenditure occurred under IAY after it became independent in 1995-96 was at around 1,200 crore rupees, which was more than double the expenditure of 500 crore rupees occurred under IAY during 1994-95 as a sub-scheme of JRY (AR MoRD 1998-99, p. 43). Hence for the year 1995-96, we take expenditure occurred under IAY as the exogenous introductory spending. Given that IAY was made independent in the last quarter of the financial year 1995-96, we also consider $IAY_{1996} - IAY_{1995}$ as the exogenous expansionary variation for the financial year 1996-97.

Swarnjayanti Gram Swarozgar Yojana (SGSY) 1999-2000 to present

Jawahar Gram Samridhi Yojana (JGSY) 1999-2000 to 2001-02

1999 - 2000: $(SGSY_{1999} - IRDP_{1998}) + (JGSY_{1999} - JRY_{1998})$ (Exogenous introductory spending)

75:25. Moreover, the fund allocation from the Centre to the States/Districts became more structured (AR MoRD 1999-2000).

[50] Out of the 665 blocks, 409 blocks had a significant proportion of tribal population. 256 more blocks were included in March 1995.

[51] Although the expansion of EAS to cover all 5448 rural blocks of the country continued till 1997-98 we do not consider any part of EAS spending as exogenous variation after 1994-95. This is so since some of the expansion was a result of the integration of EAS with a similar existing programme, while the expenditure occurred due to the extension to the remaining districts is difficult to identify (given that we only have aggregate programme expenditure data).

[52] 6% of the total JRY allocation was reserved for IAY. From 1993-94 IAY was extended to cover non SC/ST rural poor and the JRY funds earmarked for IAY were increased from 6% to 10%.

[53] Earlier the Centre to the States ratio was quoted to be 80:20 (AR MoRD 1998-99, p. 41).

2000 - 2001: $(\mathbf{SGSY}_{2000} - \mathbf{SGSY}_{1999}) + (\mathbf{JGSY}_{2000} - \mathbf{JGSY}_{1999})$ **(Exogenous expansionary spending)**

SGSY is a credit-cum-subsidy programme which was implemented from 1^{st} April 1999 (AR MoRD 1999-2000, p. 40). Apart from IRDP, which was a major rural development programme, there were many other small programmes which were implemented either independently or as a sub-scheme of IRDP with similar objectives of self-help/self-employment like TRYSEM, DWCRA, and SITRA (among others). Given the multiplicity of such programmes over the years, achieving individual programme targets was replacing the larger objective of income generation for the rural poor. With the introduction of SGSY, IRDP and its allied programmes 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 AR MoRD (1999-2000, p. 42), in case of individuals, the subsidy was uniform at 30% of the project cost with the subsidy ceiling of ten thousand rupees (nearly 230 US dollars). For group projects, the subsidy was at 50% of the project cost subject to a maximum subsidy grant of a hundred and twenty five thousand rupees (nearly 2,900 US dollars)[54].

JGSY was implemented along with SGSY on 1^{st} April 1999. It was a result of restructuring the erstwhile JRY programme. Unlike the previous programmes like JRY and EAS where the primary objective was generation of wage employment, the overriding priority of JGSY was creation of demand driven community village infrastructure with the secondary objective being the generation of wage employment for the unemployed (AR MoRD 1999-2000). 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 (AR MoRD 1999-2000, p. 29-30). For both SGSY and JGSY, the programme 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 programmes: IRDP and JRY were restructured into SGSY and JGSY respectively. Our exogenous measure of transfer spending for the year 1999-2000 accordingly measures any excess spending of the new restructured programmes over their old respective counterparts. Hence for 1999-2000, $(\text{SGSY}_{1999} - \text{IRDP}_{1998}) + (\text{JGSY}_{1999} - \text{JRY}_{1998})$ is our

[54] Calculated according to the annual average exchange rate of the Indian National Rupee (INR) and the US dollar.

measure of exogenous introductory spending while ($SGSY_{2000} - SGSY_{1999}$) + ($JGSY_{2000} - JGSY_{1999}$) measures any expansionary exogenous variation in the following year 2000-01.

Sampoorna Grameen Rozgar Yojana (SGRY) 2002-03 to 2007-08

2001-02: $(\mathbf{EAS}_{2001} - \mathbf{EAS}^{*}_{2001}) + (\mathbf{JGSY}_{2001} - \mathbf{JGSY}^{*}_{2001})$ **(Exogenous introductory spending)**

2002-03: $\mathbf{SGRY}_{2002} - (\mathbf{EAS}_{2001} + \mathbf{JGSY}_{2001})$ **(Exogenous expansionary spending)**

SGRY was launched on 25^{th} September 2001 (AR MoRD 2002-03, p. 9). After the restructuring of JRY into JGSY in 1999, EAS became the major wage-employment generation programme while JGSY was more focused on the creation of rural infrastructure. To achieve comprehensive rural development, an ambitious programme: SGRY was launched to take care of food security, employment generation, and rural infrastructure development. SGRY was operated under two streams. EAS was restructured into SGRY-I, while JGSY was restructured in SGRY-II. 50% of total SGRY funds were earmarked for each of the two streams (AR MoRD 2003-04, p.7)[55]. 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^{st} April 2002 (AR MoRD 2002-03, p. 11).

The special component was a demand driven sub scheme where the Central assistance would be 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 Centre and the States divided the programme cost in the ratio 75:25. Before SGRY, any 'restructured programme' was always introduced at the start of a financial year (1^{st} April). However, SGRY was introduced during the end of the 2^{nd} quarter of the financial year 2001-02. Construction of the exogenous variation becomes a challenge in this case since financial performance of a programme is always reported year-wise.

Hence for example actual expenditure estimates of EAS/SGRY-I for 2001-02 contains funds utilized both before and after EAS was restructured into SGRY-I. As a workaround to this, we use the provisional expenditure estimates of EAS and JGSY during 2001-02 to estimate the funds utilized before these programmes were restructured into SGRY. Annexures in the *Annual Reports* of the Ministry of Rural Development usually provide actual estimates of the funds utilized in a programme during the previous year(s). However, for the current year, only provisional estimates are provided. Furthermore, the 'reporting month' (the month up to which the programme expenditures have been reported) is also mentioned in the

[55] SGRY was implemented as one from 2004-05 (AR MoRD 2005-06, p. 2).

financial report. In the *Annual Report* 2001-02 MoRD, the provisional estimates for both EAS and JGSY 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 occurred prior to the restructuring of these programmes. Hence ($EAS_{2001}-$ EAS^{*}_{2001})+ ($JGSY_{2001}-$ $JGSY^{*}_{2001}$)[56] represents the exogenous introductory spending for the financial year 2001-02, while $SGRY_{2002}-$ ($EAS_{2001}+$ $JGSY_{2001}$) is the exogenous expansionary variation for the financial year 2002-03[57].

National Rural Employment Guarantee Act (NREGA) 2006-07 to present

2006-07: $NREGA_{2006}$ (Exogenous introductory spending)

2007-08: $NREGA_{2007}-$ $NREGA_{2006}$ (Exogenous expansionary spending)

NREGA is considered as the world's largest welfare program, the Act was enacted in September 2005. NREGA was implemented in phases. As is informed in AR MoRD 2008-09: in the pilot phase (first phase) NREGA was implemented in 200 districts (of 27 States) on 2^{nd} February 2006, while 130 additional districts were covered in 2007-08 (p.1). The program was implemented throughout the country from 1^{st} April 2008 and the erstwhile SGRY was completely subsumed under NREGA[58]. Key features of the *National Rural Employment Guarantee Act* (2005) are: 1) NREGA guarantees 100 days of wage employment to adult members of any rural household willing to do unskilled manual work. 2) The work is guaranteed within 15 days of the application for work. In case the applicant is not provided with employment within this period, the applicant is eligible to get unemployment allowance (*The National Rural Employment Guarantee Act*, 2005). 3) NREGA is a demand driven scheme. The Centre pays for the wage cost while the unemployment allowance is funded by the States. Material costs are shared in the ratio 75:25 between the Centre and the States. $NREGA_{2006}$ represents the exogenous introductory spending for the financial year 2006-07 while $NREGA_{2007}-$ $NREGA_{2006}$ is our measure of exogenous expansionary variation in the financial year 2007-08.

[56] * denotes provisional expenditure estimates.

[57] We also consider an alternative shock series where we consider SGRY to start from 2002. The corresponding shock years are 2002-03 and 2003-04 with $SGRY_{2002}$ - (EAS_{2001} + $JGSY_{2001}$) and $SGRY_{2003}-$ $SGRY_{2002}$ as the respective shock values. The results are very similar in either case.

[58] In 2009-10, the term 'Mahatma Gandhi' was added as a prefix to NREGA. NREGA was hence renamed as MG-NREGA (AR MoRD 2009-10).

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