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2019

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**Documentos de Trabajo**
**N.º 1922**

BANCO DE **ESPAÑA**
Eurosistema

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(*) We thank Henrique Basso, Jesús Fernández-Villaverde, Enrique Moral-Benito, Omar Rachedi, Roberto Ramos, Tano Santos, Jan Stuhler, Javier Vallés, Ernesto Villanueva, and seminar participants at the Banco de España for helpful comments. The views expressed in this paper are those of the authors and do not necessarily coincide with the views of the Banco de España or the Eurosystem. Alloza: Banco de España, Calle Alcalá 48, 28014 Madrid (Spain), m.alloza@bde.es. Sanz: Banco de España, Calle Alcalá 48, 28014 Madrid (Spain), carlossanz@bde.es.

Documentos de Trabajo. N.º 1922

2019

ISSN: 1579-8666 (on line)

**Abstract**

We estimate the employment effect of a large fiscal stimulus in Spain (*PlanE*), in which the national government transferred funds to municipalities to carry out local investment projects. Using a difference-in-difference approach by exploiting variation in the timing of the execution of projects across municipalities, we find that 100,000 euros of stimulus reduced unemployment by 0.62 jobs per year. We allow for possible spatial effects, i.e. the propagation of the stimulus to neighboring municipalities, and find that these are sizable, representing 8.4% of the "local" effect. We also present evidence on the transmission mechanism, finding that the effect was: (i) initially concentrated in the construction and industrial sectors, but later spilled over to the broader economy, (ii) larger for males than females, (iii) larger when the shock represented a higher share of the budget, and (iv) not larger for municipalities headed by more educated mayors. Our estimate of the multiplier falls in the lower range of previous work.

**Keywords:** fiscal policy, local fiscal multipliers, spillovers.

**JEL classification:** E24, E62, H30, H72.

**Resumen**

En este trabajo, estimamos los efectos en el empleo de un gran estímulo fiscal en España (conocido como «Plan E»), por el cual el Gobierno central transfirió fondos a los municipios para llevar a cabo proyectos de inversión. Explotando la variación temporal en la ejecución de los proyectos en los distintos municipios mediante un análisis de diferencias en diferencias, encontramos que 100.000 euros de estímulo redujeron el desempleo en 0,62 empleos-año. Consideramos la posibilidad de que existan efectos espaciales, esto es, la propagación del estímulo a municipios vecinos, y encontramos que son considerables: suponen un 8,4 % del efecto «local». También presentamos evidencia sobre la transmisión del estímulo, y vemos que el efecto: i) estuvo inicialmente concentrado en la construcción y en la industria, pero se extendió luego al resto de la economía; ii) fue mayor para los hombres que para las mujeres; iii) fue mayor cuando el estímulo fue más grande en proporción al presupuesto municipal, y iv) no fue mayor en municipios con alcaldes de un mayor nivel educativo. Nuestra estimación del multiplicador se encuentra en el rango inferior de lo hallado por estudios anteriores.

**Palabras clave:** política fiscal, multiplicadores fiscales locales, efectos de desbordamiento.

**Códigos JEL:** E24, E62, H30, H72.

# 1 Introduction

In the last decade, there has been a renewed attention to fiscal policy. Given a macroeconomic environment characterized by a constrained monetary policy, understanding how effective government initiatives are in stimulating the economy has become an important topic of discussion among academics and policy makers. This is particularly relevant when analyzing the expansionary fiscal plans that were enacted in recent years to stimulate employment growth during recessions.[1] However, it is challenging to identify the causal effects of such plans.[2]

In this paper, we estimate the employment effects of a large, unanticipated fiscal stimulus in Spain (*Plan Español para el Estímulo de la Economía y el Empleo*, the Spanish Plan for the Stimulus of the Economy and Employment, commonly know as *Plan E*). This stimulus, approved in 2008, channeled almost 13 billion euros (around 1.2% of Spanish GDP) to municipal governments to execute public investment plans.[3]

In our empirical analysis, we use municipality and monthly-level data on unemployment and the stimulus. While all municipalities received the same amount of resources (in *per capita* terms), there was variation in the timing of the execution of the projects. We exploit this variation in our estimation, performing a difference-in-difference analysis to establish a causal relationship. The key assumption is that "early" and "late" starters were on parallel trends around the time of the stimulus. While we cannot directly test whether this was the case, we perform a test of parallel trends through an event study, i.e. we do placebo tests to see whether "early" and "late" starters were on parallel trends in the months before the stimulus. Reassuringly, the results of these tests indicate that this was indeed the case.

We then extend our benchmark specification to analyze potential spatial effects, i.e. how a fiscal stimulus in municipality $i$ affected other municipalities close to $i$.[4] Restricting the estimation of the fiscal stimulus to a particular municipality ignores two important aspects. First, it omits a direct effect, particularly relevant for the case of small and medium-sized municipalities, due to the fact that some of the works may be contracted to firms in neighboring (potentially larger) municipalities. Second, it ignores general equilibrium effects, since enhanced local economic activity could spillover to close areas.

Our estimate of the jobs multiplier (the number of jobs created per million euros of public spending) is 5.7 jobs at the peak (significant at conventional levels). Regarding the

[1] For example, in the case of the United States, the 2009 American Recovery and Reinvestment Act (ARRA) states employment protection and expansion as its primary objectives.

[2] When using variation from aggregate time series data, fiscal policies originated by a response to changing macroeconomic conditions cannot be used to established a causal relationship. See for example Nakamura and Steinsson (2014) and Suárez Serrato and Wingender (2016) as examples of using disaggregated data to circumvent this empirical challenge.

[3] To the best of our knowledge, only two other research papers have used *Plan E* data. Montolio (2018) studies the effects of the plan on crime using data from the region of Catalonia. Carozzi and Repetto (2017) study how local governments allocated investment projects across neighborhoods.

[4] For this, we follow the empirical design of Huber (2018), which is based on adding to the baseline equation the shock in municipalities that fall within a given radius of $i$.

cumulative multiplier (the sum of the point multipliers over the considered horizon), we find that 100,000 euros reduced unemployment by 0.62 job-years. Our results also point to the presence of substantial spatial effects, representing 8.4% of the local effect (and about 20% during the first 6 months).

While jobs multipliers are an object of interest in their own right (see, e.g., Wilson (2012)), they offer an incomplete view of the overall effects of the stimulus. To (partially) address this issue, we follow Chodorow-Reich (2019) and translate the employment multiplier to an output multiplier. Our estimates are consistent with an output multiplier of 0.3–0.5. This figure represents a strict lower bound, since our approximation ignores that new public capital could enhance economic activity in the medium and long run.

Finally, we provide evidence on the transmission channel of the stimulus. We find that: (i) most of the effect was initially in the construction and industrial sectors, but later spilled over to the broader economy, (ii) the stimulus reduced male more than female unemployment, suggesting non-neutral gender implications of the fiscal action, (iii) while all municipalities received the same (*per capita*) amount of resources, the stimulus was more effective when the received amount represented a higher percentage of the budget, and (iv) there is no evidence that the stimulus was more effective in reducing unemployment in municipalities with more educated mayors.

Our work contributes to the growing literature that employs sub-national units of analysis to estimate jobs multipliers.[5] Several papers have studied the effects of the American Recovery and Reinvestment Act (ARRA), with a wide range of estimates. For example, while Chodorow-Reich, Feiveson, Liscow, and Woolston (2012) find that 100K dollars increased employment by 3.8 job-years, Conley and Dupor (2013) find that 100K dollars increased employment by 0.5 job-years if fungibility between ARRA and lost tax revenue is imposed (0.76 if fungibility not imposed). The estimate implied by Wilson (2012), 1.75 job-years, falls in between the other two.[6] Non-ARRA papers have found (on average) larger multipliers. For example, Suárez Serrato and Wingender (2016) find that 100K spending increases employment by 3.25 job-years while Shoag (2015) estimates this multiplier at 2.89 job-years.

We contribute to this literature in various ways. First, we exploit an arguably exogenous variation in the timing of the implementation of the stimulus, allowing us to provide credible estimate of causal effects. In this regard, we provide evidence supporting the identification assumption through an event study, i.e. placebo tests on lagged outcomes. Second, we employ a very fine unit of observation at both the cross-sectional level (municipality) and the time level (monthly). Hence, we have a large number of observations (3,255 municipalities over 108 months) and obtain precise estimates.[7] To the best of our knowledge, we are the

[5]For a review, see Chodorow-Reich (2019). More broadly, our paper is related to the literature that uses a sub-national level of analysis to estimate local *income* multipliers—see, for example, Nakamura and Steinsson (2014).

[6]This number comes from a calculation by Chodorow-Reich (2019), as Wilson (2012) does not provide an estimate of the cumulative multiplier.

[7]For example, Wilson (2012) obtains a 95% confidence interval of [1.1,15] for the one-year point multiplier, while ours is 35% smaller ([0.1,9]).

first to use municipality and monthly data to estimate fiscal multipliers. Third, we extend the benchmark framework of local fiscal multipliers and estimate spatial effects, i.e. the impact of a stimulus in a given municipality on neighboring municipalities. Fourth, we provide evidence on the underlying transmission channel by exploiting detailed data on the sectoral composition of unemployment, budget size, and the education level of mayors.

Our estimates of the multiplier fall in the lower range of previous estimates. Our paper also suggests that it is important to take spatial effects into account when estimating local fiscal multipliers.[8] Still, our estimate of the multiplier remains in the lower range of previous work even after including spatial effects. Our jobs multiplier (and implied output multipliers) are compatible in size with aggregate estimations of nation-wide effects. This evidence suggests that there might be instances where the local multipliers are not always larger than nation-wide multipliers. Following Farhi and Werning (2016), we argue that this might be the case for countries belonging to a currency union, like Spain.

The rest of the paper is organized as follows. Section 2 provides some background on the *Plan E* stimulus. Section 3 describes the data and the empirical strategy. Section 4 presents the main results and robustness checks. Section 5 extends the analysis to take spatial effects into account. Section 6 discusses the magnitude of estimates in light of previous literature. Section 7 looks into the transmission mechanism. Section 8 concludes.

# 2 Background: The *Plan E* Stimulus

Spain was heavily affected by the Great Recession that started in 2007. GDP growth slowed down to 1.1% in 2008 (from 4.2 and 3.8% in 2006 and 2007, respectively). The most dramatic change at the beginning of the crisis, however, was the deterioration of the labor market. Unemployment rate soared from 8.2% in 2007 to 11.2% in 2008.[9] Unemployment rate kept on growing during the following years, reaching a peak at 26% in 2013.

In this context, the government of primer minister José Luis Rodríguez Zapatero approved, on November 28, 2008, a fiscal stimulus with the goal of "creating jobs". Through this stimulus, officially named the *Fondo Estatal de Inversión Local* and popularly known as *Plan E*,[10] the national government would transfer 8 billion euros to municipalities proportionally to their population size: each municipality would receive up to 177 euros per capita, which had to be spent on local public investment projects, of up to 5 million euros per project. Importantly, the stimulus was approved when the budgets for the next year

[8] Admittedly, the size of spatial effects may be contingent on the size of the unit of analysis, i.e. it may be more relevant at the municipality level than at a larger level.

[9] It has been argued that this was due to the rigidity of the labor market, which made firms to adjust through employment rather than wages, and to the large gap between the firing costs of permanent and temporary contracts (Bentolila, Cahuc, Dolado, and Le Barbanchon (2012)).

[10] When initially conceived, the fiscal stimulus was given the official name of *Fondo Estatal de Inversión Local*. A year later, a second wave was approved under the name of *Fondo Estatal para el Empleo y la Sostenibilidad Local*. Both waves are commonly referred to as *Plan E*.

(2009) had already been approved, and hence funding could not be devoted to local investment included in the 2009 budget.[11] Rather, to obtain the funds, municipalities had to submit project proposals to the national government. As can be seen in Panel (a) of Figure A2, almost all municipalities received the maximum possible amount 177 euros per capita—given that this was free money from a municipality's perspective, there was no incentive not to ask for it. Projects were submitted from December 10, 2008 to January 24, 2009, and approved (or, in very few cases, rejected) by the national government from January to March 2009. After the approval of projects, local governments had to follow a tender procedure to award the contracts. This procedure lasted a maximum of four months, after which construction had to start. An important feature in the design of the *Plan E* is the special emphasis placed on reducing the gap between the submission and the approval of projects (through a novel electronic system for project submissions) and between the approval and the execution of projects (special provisions allowed the tender procedures to be conducted through an expedited process). Upon approval of the projects, municipalities received up to 70% of the budget cost of the works, with the remaining funds being reimbursed upon completion. In this first wave, 8,108 out of 8,112 Spanish municipalities participated in the *Plan E*, presenting a total of 30,700 projects with an approval ratio above 99.5%.[12]

In 2010, a second wave of the stimulus (*Fondo Estatal para el Empleo y la Sostenibilidad Local*), was implemented. The procedure was very similar to the one of the first wave, but the size was smaller: municipalities could now receive up to 108 euros per capita, with the total stimulus amounting to 5 billion euros. In this wave, there was a special provision that allowed municipalities to use up to 20% of the stimulus in funding municipal government consumption (as reflected in the local budget). Because of this reason, not all municipalities allocated the resources in public investment projects, as can be seen in in Panel (b) of Figure A2.[13] In the second wave, there were 25,362 submissions regarding public investment projects, with an approval rate of 99.9%.[14]

Overall, the fiscal plan had two characteristics that make it an appealing setting to estimate local fiscal multipliers. First, the stimulus was very large quantitatively. Around 13,000 million euros were spent, amounting to 1.2% of GDP, or twice the total yearly municipal budget in public investment. PM Zapatero mentioned that the *Plan E* implied "an unprecedented mobilization of public resources". The large size of the shock will allow us to obtain precise estimates. Second, the stimulus was mostly unanticipated. The approval

[11]This can be clearly gathered from Figure A1, which plots capital transfers over time. Capital transfers are the funds received by municipalities from higher-order public administrations (including the national government) to undertake capital expenditures, such as infrastructure work. Figure A1 shows that, when the budgets were drafted (blue line), funds received to execute capital expenditures were expected to remain roughly stable. The actual, executed budget (red line) reflects that municipalities received an unexpected amount of resources both in 2009 and 2010 to fund public investment.

[12]See Ministerio de Política Territorial y Administración Pública (2010).

[13]On average, 14.8 of the stimulus resources were allocated to local government consumption, with the remaining 85.2% being allocated to public investment projects. We account for this special feature in our empirical strategy described in Section 3.2.

[14]See Ministerio de Política Territorial y Administración Pública (2011).

of the *Plan E* on 28 November 2008 came as a surprise.[15] After the approval, the process followed until the beginning of construction was extremely fast. As can be seen in Figure 1, which shows the starting dates of the projects, construction had started in 75% of the projects five months after the announcement, and in 90% after six months. The absence of long anticipation will help us validate our empirical strategy with pre-trend placebo tests, as discussed in the next section.

# 3 Data and Empirical Strategy

## 3.1 Data

We use data on the fiscal stimulus and unemployment at the municipality-month level. There are 8,112 municipalities in Spain. Given that many of these municipalities are very small and employment statistics are measured with more error, we will focus on municipalities of more than 1,000 inhabitants (3,255 municipalities).[16] Our sample period covers all months from January 2006 to December 2014.

Our data for the fiscal stimulus comes from the Ministry of Public Administration (*Ministerio de Política Territorial y Administración Pública*). For each project, we can observe the dates in which construction started and finished, its size (i.e. the amount of money spent on it), and a summary description of the project (e.g. industrial promotion *vs.* refurbishment of an existing public building). Our sample consists of a total of about 56,000 projects (30,700 in the first wave). The average size of projects was 215,290 euros (median: 75,000), ranging from 3,000 to 5,800,000 euros. In our analysis, we focus on the starting date of construction since it proxies for the moment when firms take economic decisions (particularly, hiring workers). Given that our unit of observation will be the municipality-month, we aggregate the total amount spent by municipality and month. Hence, our treatment variable, $x_{i,t}$, will be the (per capita) amount spent by municipality $i$ on projects that started during month $t$.

Our unemployment data comes from the Ministry of Employment and Social Security (*Ministerio de Empleo y Seguridad Social*). We can observe the number of registered unemployed individuals by municipality and month. Our outcome variable, $y_{i,t}$, is the logarithm of the number of unemployed individuals in municipality $i$ in month $t$. In addition to the total number of unemployed, we can observe some demographic and sectorial characteristics: the number of unemployed by gender, by age group (less than 25, 25–45, and more than 45 years old), and by sector (construction, industry, services, and agriculture). To shed light on the

[15]The first public announcement of the approval was on November 22, when PM Zapatero announced "by surprise" that he would be laying out some "fast, municipality-based" actions to stimulate the economy (Díez (2008)).

[16]More precisely, we focus on municipalities whose population was larger than 1,000 in 2007 (the year before the first wave of stimulus). For very small municipalities, registered employment statistics only report aggregate figures.

transmission mechanism, we will estimate the heterogeneous effect of the stimulus by these demographic and sectorial characteristics in Section 7. As an alternative outcome variable, we will consider the logarithm of the number of workers affiliated to Social Security (formal employment). We prefer, however, to focus on unemployment for two reasons. First, from a statistical point of view, registered unemployment is a more reliable measure of dynamics in the labor market.[17] Second, it facilitates relating our estimates of the multiplier to those of previous literature, which generally uses unemployment as the outcome variable.

Figure 2 plots the evolution of unemployment over our sample period (2006-2014), along with the amount spent in the two waves of stimulus. We can see that unemployment almost doubled in the year leading up to the *Plan E*, from 2 to 3.6 million unemployed. Unemployment continued growing, although at a slower pace, until February 2013, in which it started to decrease.

In our analysis, we exclude outliers in both the dependent and independent variables.[18] In Section 4.2, we show that the results do not change when outliers are included.

## 3.2 Empirical Strategy

To estimate the dynamic impact of the stimulus on unemployment, we follow a local projection approach (Jordà (2005)), estimating a series of $h$ regressions:

$$y_{i,t+h} = \rho y_{i,t-1} + \sum_{j=0}^{M} \beta_{h,j} x_{i,t-j} + \alpha_i + \gamma_t + \xi_{i,t+h}, \qquad (1)$$

where $y_{i,t+h}$ is the log of the unemployment in municipality $i$ at month $t+h$ ($h \in [-24, 24]$), $x_{i,t-j}$ is the size of the fiscal stimulus in municipality $i$ at month $t-j$, $\alpha_i$ is a municipality fixed effect, $\gamma_t$ is a date (month) fixed effect, and $\xi_{i,t}$ is an idiosyncratic error term. The parameter $\rho$ captures the persistence of the outcome variable, and the $\beta_{h,j}$ capture the dynamic impact of the fiscal stimulus. In the baseline specification, we take $M = 12$, but we will show the robustness of the results to different values of $M$. Note that we estimate one regression for each value of $h$: if $h = 0$, $\beta_{0,0}$ measures the contemporaneous effect; if $h > 0$, $\beta_{h,0}$ measures

[17] Contrary to unemployment data, affiliation data suffer from two drawbacks. First, it does not include workers affiliated to other welfare regimes different to the Social Security. Second, unemployed workers have strong incentives to register themselves as unemployed, since it is a pre-requisite to obtain unemployment benefits. In contrast, Social Security records may infraestimate the size of the shadow economy.

[18] Regarding outliers in the dependent variable, we drop all municipality-months in which the unemployment rate (as a fraction of total population) was above 20%. The 99th percentile in our sample is 16%. Regarding outliers in the independent variable, we exclude those municipalities that obtained (significantly) more or less than the amount stipulated by the law (179 and 108 euros per capita, respectively)—see Figure A2. More specifically, we drop those municipalities that spent more than 179 or less than 100 euros per capita in the first wave, and those that spent more than 110 or less than 106 euros per capita in the second wave. We show that our benchmark results are robust to the inclusion of these outliers.

the effect of the stimulus h periods in the future; and, if $h < 0$, $\beta_{h,0}$ captures the effects $h$ periods in the past—hence, they are placebo tests, as we discuss below.

Note that, given that we include municipality and year fixed effects, this is in fact a difference-in-difference specification, where the dynamic response is calculated through local projections.[19] Local projections are a natural way to identify the Average Treatment Effect over time.[20] An alternative method to estimate the dynamic impact of the shock would be to construct the moving average (MA) representation of equation (1). As noted by Ramey (2016), the local approach presents several advantages (at the cost of efficiency) over the traditional method based on MA. Mainly, local projections is a more robust method to the presence of misspecification, while MA estimation compounds misspecification errors at each horizon. Additionally, local projections offers a simple and transparent way to test the parallel-trend assumption with placebo tests.[21]

The identification assumption is that there are no omitted factors that affect both the timing of the projects and the unemployment rate, *conditional on the municipality and year fixed effects*. As a first approximation to the validity of this assumption, in Figure A3 we plot the starting dates of projects by municipality. We do not see any pattern suggesting that early-starters cluster in any given region of the country. Of course, this figure is far from conclusive about the validity of the approach, as there can be other relevant factors that cannot be gathered from the map. For example, municipalities that are smaller, poorer, or have a high unemployment rate may arguably be faster in requesting the funds, or in the tender procedure, and hence might start the projects earlier. Importantly, in our specification, the municipality fixed effects will absorb any time-invariant characteristic of municipalities that might correlate with the beginning of the projects, e.g. as long as the size, income, and unemployment rate of municipalities are moving in parallel around the time of the stimulus, they will be captured by the fixed effects.

Hence, what is crucial for our identification is that the starting dates of projects do not correlate with other *time-varying* characteristics of municipalities, e.g. that there is no shock to population or the unemployment rate at the same time of the stimulus that differentially affects early and late starters. Regarding this point, first note that, as argued by Montolio (2018), the narrow time window in the execution of projects implies that the timing of the approval, tender, and implementation phases of the projects is unlikely to have coincided with another simultaneous shock that might have influenced the outcome variables. Second, and more importantly, while it is not possible to directly test whether some omitted factor is changing at *precisely* the same month of starting the project, we can do a standard difference-

[19] Since we have fixed effects and a lagged dependent variable, one could be concerned about the existence of a Nickel bias (Nickell (1981)). However, given that we have a large-T panel, this bias should be negligible in our context. In particular, the size of the bias diminishes with the number of periods $T$ considered in the sample: $-(1+\rho)/(T-1)$ suggesting that for our sample size and a true parameter $\rho = 0.8$, the bias could be at most -0.015 (around 1-2% of the true value of the parameter).

[20] See Jordà and Taylor (2016) and Angrist, Jordà, and Kuersteiner (2018) for further details.

[21] Our benchmark results are similar when we construct impulse responses using the MA representation of equation 1 (not shown).

in-difference test for parallel pre-treatment trends, i.e. estimate whether unemployment of early-starters and late-starters moved in parallel in the months *before* the beginning of the treatment. We see this test as an important validitation of our empirical strategy.

We do this through an event study, in which we test whether the stimulus is correlated with past values of the outcome variable (unemployment). In terms of Equation 1, we explore the effects when $h < 0$. There are two reasons why we could find a non-zero effect: (i) anticipation, and (ii) a violation of the parallel-trend assumption. However, note that, for values $h < -12$, only (ii) is possible, so the regressions with $h < -12$ serve as "pure" placebo tests. The reason is that the maximum possible anticipation was 12 months, the time elapsed between the approval of the *Plan E* on November 28 2008 and the beginning of the very last projects in December 2009. For values $h \in (-12, 0)$, we cannot rule out that some anticipation is happening, potentially complicating our pre-trend analysis. However, note that, as explained in Section 3, 90% of the projects had started by mid May 2008. This implies that, for the vast majority of cases, we should expect an anticipation of at most 6 months. Hence, regressions for $h \in [6, 12]$ arguably serve as placebo tests as well.[22]

# 4 Results

## 4.1 Main Results

The main results are presented in Figure 3. For each $h$, it plots the point estimates and 95% confidence intervals of $\beta_{h,0}$. Reassuringly, we find no "effect" on unemployment for $h < 0$, validating the parallel-trend assumption. We do observe significant effects for $h > 0$. In particular, the starting of a public investment project has a negative effect on unemployment on impact. This effects builds up over the first year and moderates towards the end of the second year. At the peak (reached 8 months after the shock), a stimulus of 100 euros *per capita* reduces unemployment by 0.8%.

Table 1 translates these effects into economically interpretable objects: the jobs multipliers, defined as employment creation per million euros.[23] Our estimates of the jobs multipliers are significantly different from zero for all horizons considered at reasonable levels of significance.[24] The peak effect is 5.7 jobs created per million euros.

Alternatively, one can look at the cumulative multiplier, i.e. the sum of the point multipliers. We estimate a cumulative multiplier of 68.2, i.e. one million euros of stimulus reduce

[22] This is assuming the worst-case scenario in which anticipation starts from the date of the announcement. In the, perhaps more realistic, case in which effects are anticipated from the approval of projects, anticipation would be significantly shorter.

[23] To convert our estimates from Figure 3 into the jobs multipliers of Table 1, we first translate the per capita shock into an absolute shock, evaluated at the municipality with the mean population size, and then translate the percent effect on unemployment into an absolute effect, evaluated at the municipality with the mean unemployment.

[24] All multipliers are significantly different from zero at the 68% level and all but the impact are significantly different from zero at the 95% level.

unemployment by 68.2 job-months, or 100,000 euros do so by 0.57 job-years. Given that spatial effects may be present in our context, we will postpone putting these numbers into perspective within the literature until Section 6, after we have estimated those.

## 4.2 Robustness

**Alternative dynamic structure.** We study the robustness of the results to changing the dynamic structure. In particular, in our baseline results, we included one lag of the dependent variable and 12 lags of the shock. Here, we consider instead 0, 6, or 12 lags of the dependent variable, and 12 or 24 lags of the shock. The results (Figure 4) show that the estimates remain very similar to those of the baseline.

**Alternative sample selection.** We analyze how changes in the sample choice may affect our benchmark estimates. Figure 5 shows alternative specifications that include municipalities considered as outliers in the benchmark sample (either because of unusually high or low values of the dependent or independent variables). Results suggest that these alternative specifications do not significantly alter our baseline estimations.

**Effect on employment.** As an alternative dependent variable, we consider the number of workers affiliated to Social Security.[25] The results, which are shown in Figure A4, suggest that the parallel-trend assumption holds and that employment increases after the stimulus. The effects, however, are imprecisely estimated and non-significant.[26]

# 5 Spatial Effects

While a growing number of studies use disaggregated geographical areas to estimate the effect of fiscal policy (see, for example, Nakamura and Steinsson (2014)), most ignore that estimates at the local level omit spatial effects arising from general equilibrium aggregations.[27] Restricting the estimation of the fiscal stimulus to a particular municipality ignores two important aspects. First, it omits a direct effect, particularly relevant for the case of small and medium-sized municipalities, due to the fact that some of the works may be contracted to firms in neighboring (potentially larger) municipalities. Second, it ignores general equilibrium effects, since enhanced local economic activity could spill over to close areas.

In this section, we shed light on how large these spatial effects are. To do so, we estimate the effects that the fiscal stimulus has on nearby municipalities. In particular, we follow Moretti (2010) and Huber (2018) and enlarge our main specification in equation (1) by adding an additional regressor that contains the average shock occurring in municipalities

[25] As explained in Section 3, there are reasons to prefer unemployment as the main dependent variable.

[26] By contrast, the spatial effect on employment is significant, as we will discuss in Section 6.2.

[27] Two notable exceptions are Suárez Serrato and Wingender (2016) and Dupor and McCrory (2018). While the former do not find significant spatial effects, the latter find them to be economically relevant.

within a radius of $R$ kilometers of municipality $i$ in period $t$ ($z_{i,t}^R$). Hence, our specification becomes:

$$y_{i,t+h} = \rho^* y_{i,t-1} + \sum_{j=0}^{M} \beta_{h,j}^* x_{i,t-j} + \sum_{j=0}^{M} \sigma_{h,j} z_{i,t-j}^R + \alpha_i^* + \gamma_t^* + \xi_{i,t+h}^*, \tag{2}$$

where $\sigma_{h,j}$ captures the spatial effect.

## 5.1 Results

Figure 6 reports the estimates of coefficients $\sigma_{h,j}$ in equation (2), considering a radius of $R = 20$ kilometers.[28] The results show that the stimulus has a sizable impact on nearby municipalities. An increase in 100 euros *per capita* in municipalities within a 20 km radius of a given municipality reduces unemployment in the latter by 2%.[29] This effect is persistent, being more noticeable during the first 16 months, and becomes close to zero after that period.

When translating this percentage effect into jobs multipliers (Table 2), we observe that, at the peak (reached in the 14th month), a fiscal stimulus of one million euros in municipalities within a 20-kilometer radius of a given municipality reduces unemployment in the latter by 0.6 persons.[30] The spatial effect is statistically significant for reasonable levels of confidence throughout the response horizon.[31] This spillover effect represents about 20% of the direct effect during the first 6 months and slightly above 10% during the rest of the first year. In Figure 7, we represent graphically the jobs multipliers of the local and spillover effects.[32] We can see that both components follow a similar dynamic pattern during the first year and a half. Spillovers, although smaller in size, are a non-negligible contribution to the overall impact of the fiscal plan. The total combined effect reaches its peak at the 8th month, when an initial fiscal stimulus of one million euros is associated with a decrease of unemployment by 5.9 workers.

Our two-year *spatial* cumulative multiplier is 5.7, which is approximately 8.4% of the *local* cumulative multiplier (which, recall, was 68.2). Hence, our estimated total cumulative multiplier is 73.9, i.e. 100,000 euros of stimulus reduced unemployment by 0.62 job-years. In the next section, we will put these numbers in perspective within previous literature.

[28] Other radii are considered in the next subsection.

[29] This is approximately 2.5 times larger than the local effect estimated in Section 5. Note that this is reasonable: given that the stimulus is measured in per capita terms, and that there is in most cases more population living in municipalities in a radius of 20 kilometers than in the municipality itself, 100 euros *per capita* amounts to a larger *total* stimulus here than in the local effect from Section 5.

[30] To convert our estimates to multipliers, we proceed as we did for the local effect—see footnote 23. Here we use the average population in the 20-kilometer radius to pin down the total stimulus amount.

[31] It remains significant at 95% during the first 6 months, and remains significantly different from zero throughout the response horizon when considering confidence bands of 68%.

[32] Hence, the represented spillover effects are those from Table 2. The represented own effects are the ones obtained from estimating equation 2. As will be discussed in Section 6.2, the graph would be very similar if we considered the own effects obtained from equation 1 (shown in Table 1).

## 5.2 Robustness

**Local effect under alternative specifications.** In addition to providing an estimation of the spatial effects of the stimulus, the specification of equation 2 allows us to test the robustness of the local-effect results. If the local effects are well identified, they should not change when the shock of nearby municipalities is added to the estimating equation, i.e. the coefficients $\beta_{h,j}$ in equation (1) and $\beta^*_{h,j}$ in (2) should be similar. In Figure A5, we show that this is indeed the case.

**Alternative spatial distances.** So far, we have focused on a radius of $R = 20$ kilometers. In Figure A6, we consider alternative radii of 10 and 30 kilometers. The effect of the stimulus in a radius of 30 kilometers is similar to that in the baseline of 20 kilometers. The effects considering $R = 10$ follow a similar dynamic pattern but are considerably smaller in size, as expected.[33]

**Alternative dynamic structure.** We test the robustness of the spatial results to alternative dynamic structures. We consider the same specifications as when testing the robustness of the direct effects (see Section 4.2). The results (Figure A7) with these alternative specifications are very similar to those of the baseline.

**Alternative sample selection.** We test if the results are affected when outliers in the dependent or independent variables are added. The results (Figure A8) show that the results barely change.

**Spatial effect on employment.** We estimate the spatial effect on employment. The results (Figure A9) show that there is a significant spatial effect on employment in the first six months after the shock. In particular, our estimates imply that spending 100 euros *per capita* in municipalities within a 20 km radius of a given municipality increases employment in the latter by 1% in the peak, which is reached six months after the shock. The effect becomes non-significant after the sixth month.

# 6 Discussion of Results

Ours results from the previous sections suggest significant jobs multipliers both at the local and spatial levels. In particular, we find that the peak jobs multiplier for each million euros spend is reached in the 8th month after the stimulus and is estimated at 5.9. This translates into a cost per job of around 170,000 euros. This figure is close to Wilson (2012), which

[33] This is because the total shock increases with the radius—see footnote 29.

studies the employment effect of the ARRA program in the US, finding a jobs multiplier after a year of between 4.3 and 8.1, depending on the specification.[34]

Further to this, Chodorow-Reich (2019) summarizes recent work on local fiscal multipliers. This comparison is made in terms of the cumulative multiplier. Chodorow-Reich (2019) finds that this statistic varies within wide bounds, ranging from 3.8 job-years in Chodorow-Reich, Feiveson, Liscow, and Woolston (2012) to 0.5 in Conley and Dupor (2013). The latter report a cost of maintaining a job during a year of about 200,000 dollars. Our estimated cumulative multiplier of 0.62 job-years sits at the bottom of this wide interval.

Our estimates of the jobs multiplier reflect both the cost of hiring workers to carry out the public investment projects, but also other costs, e.g. the cost of materials. Given that the *Plan E* was exclusively a local infrastructure stimulus, the composition of the labor-to-capital mix may be different from that in the ARRA (which included, among others, health care provisions for Medicaid expenditure). This difference may explain why our estimates are in the lower range of those from the ARRA.

Our estimates of the employment effects of the *Plan E* suggest that the stimulus helped to prevent employment destruction during the recession. In particular, they imply that the stimulus impeded a destruction of around one million job-months during two years, which would be approximately equivalent to providing full-time employment to around 40,000 workers during that period.[35]

While the employment effect is a very relevant dimension in analyzing the impact of the stimulus, it does not reflect the whole economic impact of this policy. Such evaluation would require an estimation of i) the effect on output and ii) an evaluation of the welfare effects in the long-run. Both dimensions present important challenges. First, there is no availability of data on municipal output that enables a direct estimation of this effect. Second, a long run evaluation of the effects of public investment requires a different methodology that is beyond the scope of this paper. However, we now show how we can find an approximate calculation of the effect of the stimulus on output in the short run.

For this calculation, we follow Chodorow-Reich (2019), who shows how to map the employment multipliers into output multipliers. The main limitation of this approximation is that it is valid only for the short run, since it implicitly assumes that capital (either private or public) will not adjust, effectively measuring the output effect of the stimulus that is channeled through changes in employment. Given that *Plan E* was a stimulus with a special focus on the installation of public capital, our calculations are plausible for the short run, but are likely to represent a strict lower bound on longer periods, once capital becomes usable for production.[36]

[34] These numbers refer to OLS and IV specifications when the stimulus is measured as the ARRA announced funds to be received by each state (see Table 5 in Wilson (2012)). The jobs multipliers of 4.3 and 8.1 are associated to cost per job of 233,000 and 123,000 dollars, respectively.

[35] The *Plan E* provided a total stimulus of 8,000 and 5,000 million euros in the first and second waves, respectively. We use our total estimation of 73.9 jobs-months per million euros during two years.

[36] Because of general equilibrium effects, the total effect of output through employment could be even larger if we allow the marginal productivity of labor to be increasing in capital.

Our calculation of the short-run output multiplier is 0.3–0.5, depending on the calibration of elasticity parameters.[37] This magnitude is comparable to recent aggregate estimates of the *nation-wide* output multiplier of public investment. For example, Alloza, Burriel, and Pérez (2018) use exogenous time series variation to show that public investment is associated to an impact multiplier (in the first quarter) of 0.3. This figures rises to 1.1 by the end of the first year.

Our results suggest that estimates of the local and nationwide multipliers are not that different. This observation contrasts with previous work that finds local multipliers to be substantially higher than nation-wide ones (see Nakamura and Steinsson (2014) and Chodorow-Reich (2019)), while supports the view that the effect of sub-national stimuli like ARRA had an impact on output similar to nation-wide estimates (see, for example Ramey (2018)). We argue that the conceptual differences between local and nation-wide multipliers are likely to be small in countries belonging to currency unions such as Ssain.

The reaction of monetary policy is argued to be the main source of differences between local and nation-wide multipliers. At the aggregate level, an expansionary fiscal policy could trigger a tightening in the stance of monetary policy. This reaction of monetary policy is less likely to be found in local fiscal expansions such as the ARRA or the *Plan E*. Hence, in the estimations of local fiscal multipliers, dubbed as open-economy multipliers (Nakamura and Steinsson (2014)), the sub-national entities in fact act as small open economies.

This local-national differences have similarities to the Spanish context within its currency area (the euro area). To the extent that monetary policy in the euro area is not exclusively set according to Spanish conditions, aggregate fiscal policy in Spain is less likely to provoke union-wide monetary policy responses.[38] Since neither idiosyncratic changes in the nation-wide economic conditions of Spain, nor idiosyncratic differences in the relative economic performance of its municipalities are likely to have a one-to-one translation to the response of the currency-wide monetary policy, the Spanish setting may arguably be a context where the differences between nation-wide and local multipliers are attenuated.

# 7 Evidence on the Transmission of the Stimulus

To understand the channel of transmission of the stimulus, here we explore potential heterogeneity in the responses due to economic and demographic factors and by classifying municipalities according to their per capita budget and the education level of their mayors.

[37]Explanations of this calculation and further discussions on the imposed assumptions are detailed in the Appendix.

[38]Farhi and Werning (2016) discuss the implications of currency unions for the size of fiscal multipliers in a general equilibrium model.

## 7.1 Economic Sector

We estimate a version of equation (2) where the dependent variable becomes unemployment in a given economic sector: construction, industry, services, or agriculture.[39] The results are shown in Figure 8 for the local effect (left column) and the spatial effect (right column). Given that the fiscal stimulus aimed at promoting public infrastructure, we would expect the construction and, to some extent, the industry sector to be the ones more affected upon impact. However, general equilibrium effects resulting from rising disposable incomes in a given sector may spill over to the rest of the economy.

Starting with the dynamics of the construction sector, we observe a significant, contemporaneous, and large effect at the local level (larger than the benchmark results for unemployment in all sectors) that slowly converges to zero over the considered horizon. Interestingly, when considering the spatial effect, we again observe a substantially large and contemporaneous response of unemployment (more than twice as large as the benchmark results from Figure 6).[40]

The effect of unemployment on the industry sector grows over time during the first 8 months and is quantitatively large: an increase of 100 euros per capita reduces unemployment in the industrial sector by around 1% after six months. The spatial effect is also quantitatively relevant but does not response on impact and is more transitory.

The dynamics of the service sector are slightly different: at the local level, unemployment does not significantly respond upon impact but only later. At the spatial level, the effect builds slowly towards a magnitude similar to the benchmark spatial effects shown in Figure 6.

Unemployment in the agriculture sector does not respond significantly to the fiscal stimulus, probably reflecting that just few projects directly involved workers from this sector. At the local level, the only significant effect occurs upon impact. At the spatial level, the response fluctuates around zero, possible reflecting the relatively low weight of agricultural unemployment over total (around 3% in 2008).

In sum, we observe that both the construction and industrial sectors are the most responsive to the fiscal stimulus. However, we find evidence that this effect spills over later to the broader economy.

## 7.2 Demographics

Given that the gender and age distributions may vary across economic sectors, we now investigate how the fiscal stimulus affected different demographic groups. We estimate a new version of equation (2) where the dependent variable reflects unemployment classified by gender (male and female) and age (younger than 25, 25–45, and older than 45 years).

[39] The classification of unemployment in different sectors is based on the activity from the last previous employment. Those without previous experience are not assigned to any sector.

[40] As explained above, a municipality receiving a stimulus aimed at developing local public infrastructure may hire a construction company in a different municipality.

Figure 9 displays the differential response of unemployment by gender, both at the local (left column) and spatial levels (right column). At the local level, male unemployment responds significantly upon impact. The effect becomes close to zero by the end of the first year. Looking at the effect arising from stimulus in surrounding municipalities, we also observe that male unemployment responds contemporaneously and in a more persistent manner (the unemployment contraction associated to the stimulus is still significant after 16 months). Female unemployment, by contrast, does not respond contemporaneously to the shock. This is consistent with the results from Figure 8, since the sectors that responded more clearly upon impact (construction and industry) have relatively low female presence. The effect at the local level is quantitatively large towards the end of the fist year, with female unemployment lowering by more than 1% after a fiscal stimulus of 100 euros per capita. At the spatial level, the effect of the stimulus on female unemployment is not significant.

We next analyze the dynamic response of unemployment by age (Figure A10). When considering young unemployed persons ($<$ 25 years), we do not observe a generally significant response to the fiscal stimulus (neither at the local nor at the spatial level). The effect is however quantitatively relevant when considering adults aged 25-45 and older adults (above 45 years). In those cases, we observe that the stimulus reduced unemployment both at the local and the spatial levels.

We conclude that it is important to notice that the fiscal stimulus has a heterogeneous impact both in quantitative and dynamic terms for different types of workers (male versus female, young versus old). These differences are likely tied to the performance of the economic sectors more directly affected by the fiscal stimulus. We find that these sources of heterogeneity are particularly relevant for the design of policies that aim at fostering employment in a gender-balanced way.

## 7.3 Size of Budget

The *Plan E* stimulus transferred the same nominal amounts (in per capita terms) to each municipality. The stimulus, however, may represent different *relative* shocks to municipalities, depending on their level of spending. Figure 10 displays the distribution of per capita spending in municipal budgets in 2009.[41] For the median municipality, the first wave of the stimulus, which transferred around 178 euros per capita to each municipality, implies a fiscal shock of around 30% of their budget. For the 5% municipalities with the highest budget, the stimulus amounted to 10% of their annual spending. For the bottom 5%, this figure rises above 80%—for some of them, the stimulus was higher than their annual budgeted spending.

We investigate how different the response to the stimulus was depending on the size of the relative shocks. Hence, we estimate equation (2) for two different samples: one with per

[41] The median spending per capita in 2009 municipal budgets is around 560 euros, with an average of 170 euros and a standard deviation of 1,275. The fact that not all municipalities run the same budget may reflect political differences, income levels, or geographical factors (e.g. the cost of providing public infrastructure is higher in towns with difficult access).

capita spending below the 2009 median (which we label *low spending* municipalities) and another for those with a per capita spending above the median (*high spending*).

Results from these estimations for the local and spatial effects (left and right panels, respectively) are shown in Figure 11. Starting with the local effect, we observe a quantitatively different response of unemployment for municipalities with low spending and those with large spending. For the former, the stimulus triggers a significant contraction upon impact that builds up until the end of the first year, with a 100-euro per capita stimulus estimated to reduce unemployment by around 1.2%. For the latter, the effect is not significant.

When considering the spatial dimension, the effects are relatively similar for both types of municipalities in terms of size, although the dynamics are quite different. Municipalities with lower spending benefit from stimulus in surrounding municipalities during the first year. In the case of municipalities with high level of spending, this beneficial effect occurs later on, with a peak effect 14 months after the stimulus.

These results suggest that the *relative* size of the shock is an important factor driving the results of the fiscal stimulus. When the stimulus represents a higher share of the budget, it is more effective in reducing unemployment. However, it is important to note that these heterogeneous effects may reflect other factors that cause the differential levels of spending (e.g., political preferences, income levels, geographical factors).

## 7.4 Education of Mayors

There is evidence that more educated politicians implement better policies.[42] In our context, it was municipal governments who selected the projects. Hence, we expect that more educated municipal governments come up with projects that are more effective in reducing unemployment.

To test this hypothesis, we use data on the education of mayors (heads of the local governments) and split the sample based on whether the mayor had (at least) a university undergraduate degree or not.[43][44] We then estimate equations (1) and (2) separately for the two subsamples. The results (Figure 12) reveal no heterogeneous response by the educational level of the mayor: the effect of the stimulus is remarkably similar in the two subsamples.

We can think of three possible reasons why we do not observe any differential effects by the educational level of the government. First, while the education of political leaders may be beneficial in other domains, it may not have an impact in our context of a fiscal stimulus.

[42] For example, Besley, Montalvo, and Reynal-Querol (2011) find that having leaders who are more highly educated increases economic growth.

[43] 28% of the municipalities in our sample belong to the first group, i.e. had a college-educated mayor in 2009.

[44] In Spain, power at the municipal level is shared by the mayor and a city council, but the mayor plays a more influential role (for a detailed description of their duties, see Fujiwara and Sanz (2018)). Hence, we focus on the education of the mayor. In any case, the results are very similar if we consider instead the education of the city councilors.

Second, it may be that the education of mayors does have an impact in our context but only in the long run (e.g. more educated mayors choose more productivity-enhancing projects), and hence we cannot observe the differential effects. Third, it may be that only postgraduate education (masters or PhD) matters. In our sample, we only observe 2.5% of mayors with postgraduate level and we do not have enough statistical power to estimate the effects in such a small subsample.

# 8 Conclusion

During the last recession, governments across the world embarked in fiscal expansionary programs to prevent job destruction. In Spain, the *Plan E* channeled almost 13 billion euros (around 1.2% of GDP) to local authorities to execute public investment plans. The institutional design of this program, along with the availability of detailed municipality and monthly unemployment data, allows us to implement a difference-in-difference strategy to identify the causal effect of the fiscal stimulus on unemployment.

Our estimate of the multiplier is comparable to those at the bottom range of similar work. This suggests that local fiscal multipliers are not necessarily larger than nation-wide (aggregate) multipliers. We have argued that the difference between both statistics is likely to be attenuated in countries that belong to currency unions, as is the case in Spain.

Our work opens up avenues for future research. While we have focused on the effect of the fiscal stimulus on unemployment, there are other relevant dimensions that should be taken into consideration when evaluating the overall impact of the program. For example, it would be interesting to see how the stimulus affected welfare in the medium and long run. We leave this question for future research.

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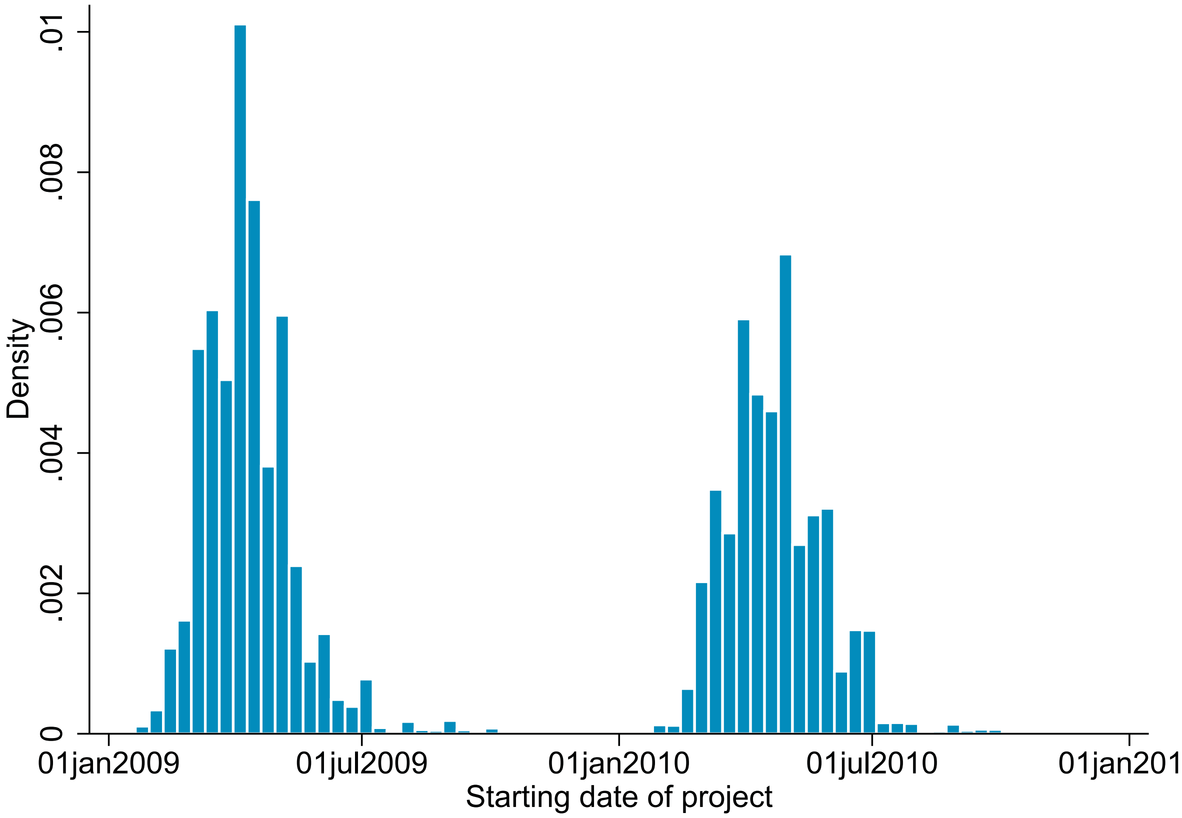
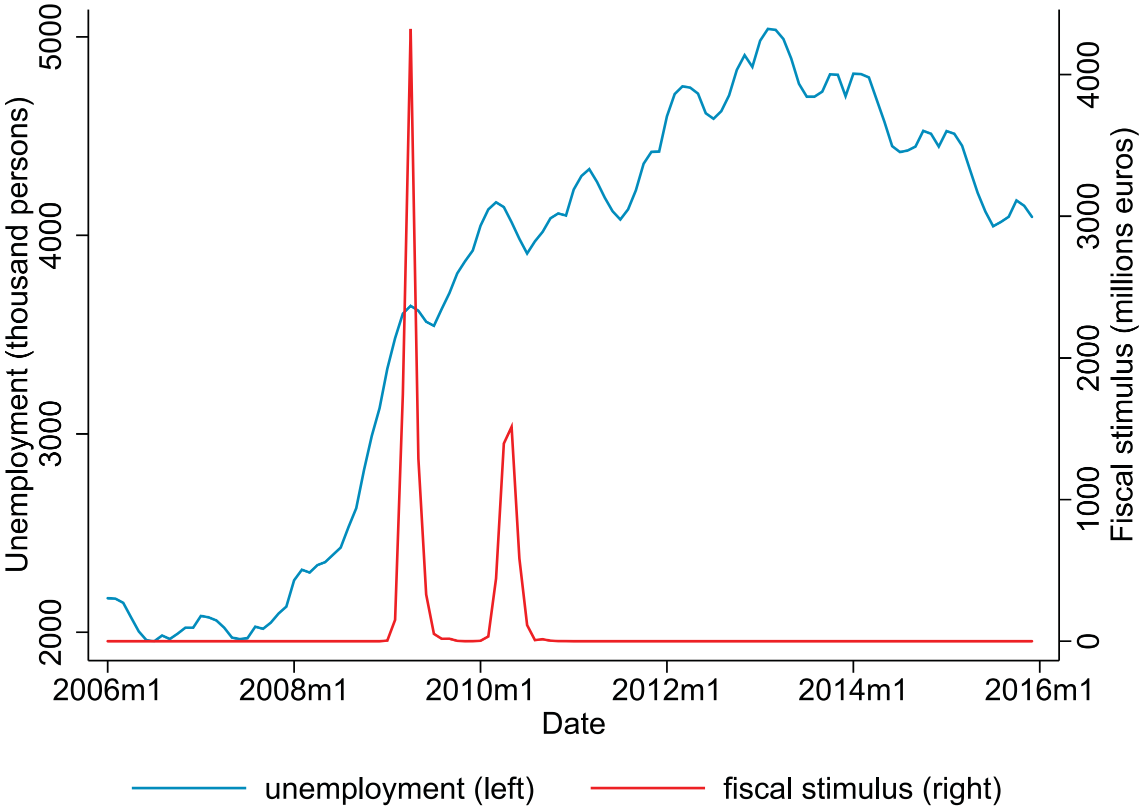
# Figures

Figure 1: Time distribution of starting dates



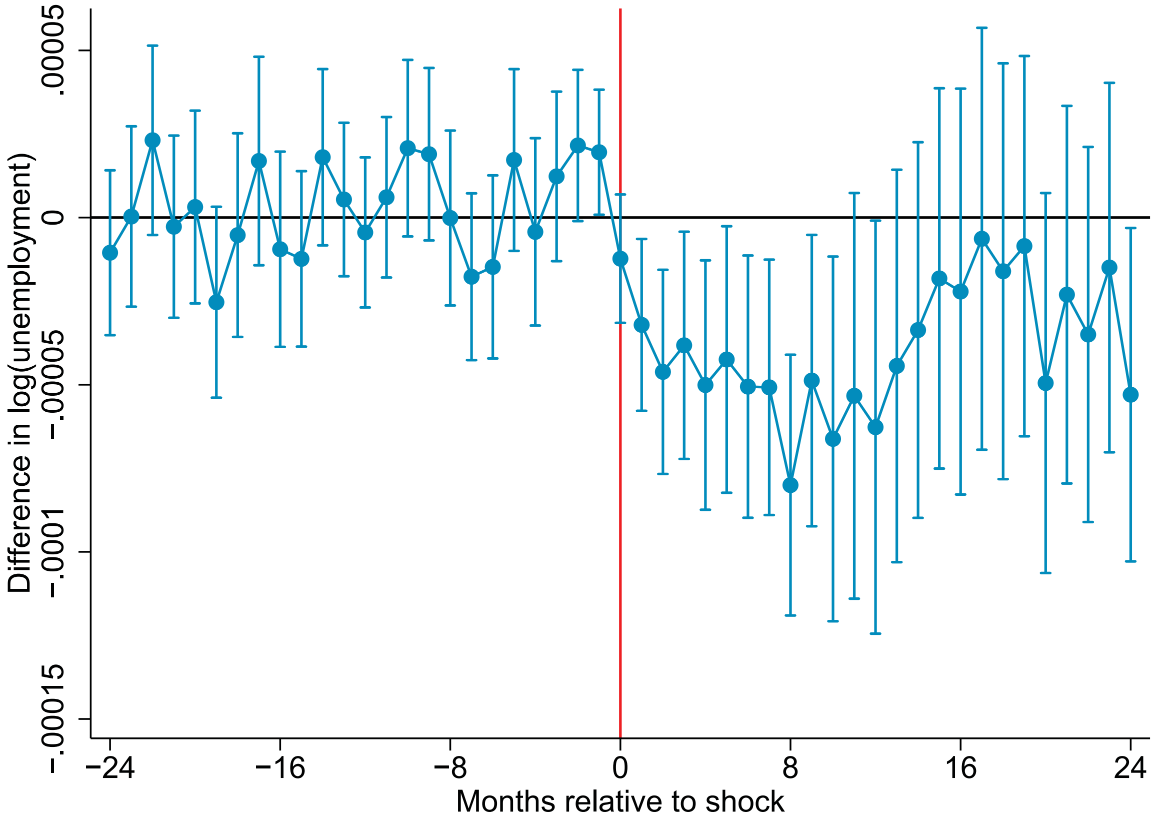

Notes: Distribution of starting dates of projects. Bins are 10-day wide.

Figure 2: Unemployment and fiscal stimuli



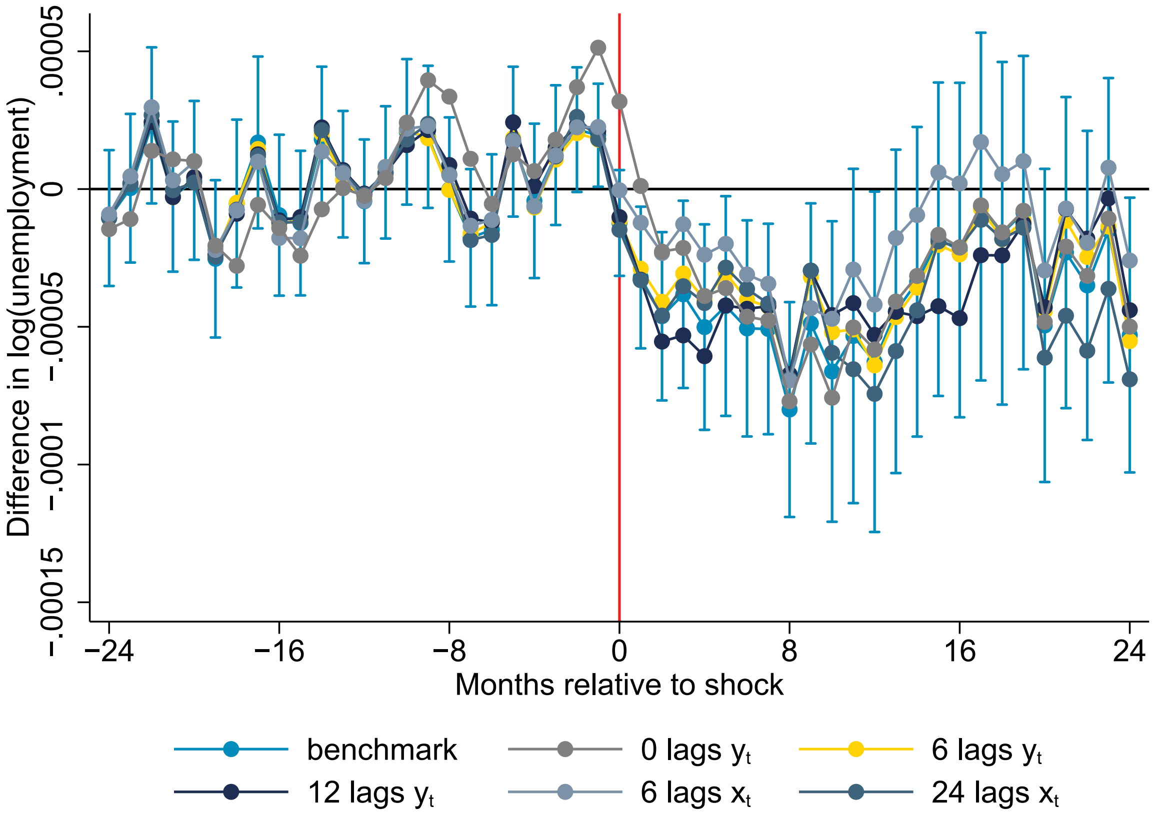

Notes: The blue line represent monthly unemployment between 2006m1 and 2015m12. The red line represents the size of the fiscal stimulus.

Figure 3: Effect of the stimulus on unemployment



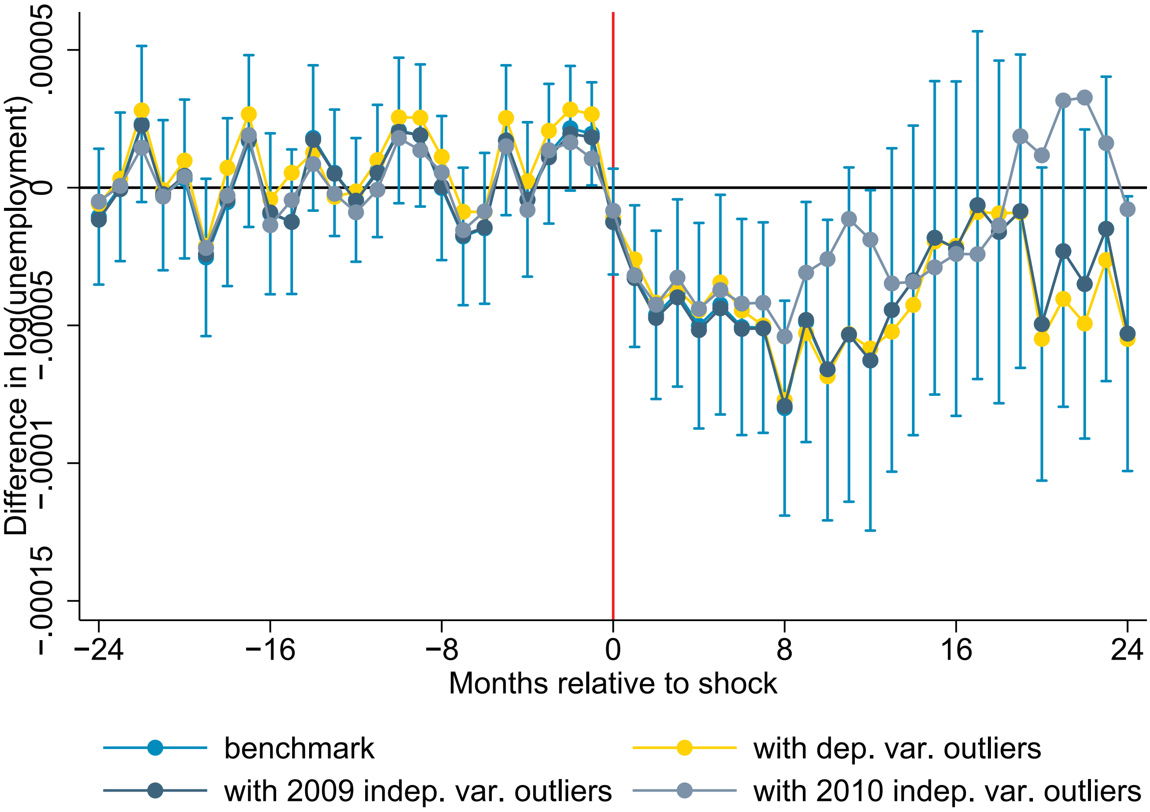

Notes: Blue points represent the dynamic response of unemployment to a fiscal stimulus of one euro per capita. Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

Figure 4: Robustness: Alternative dynamic structure



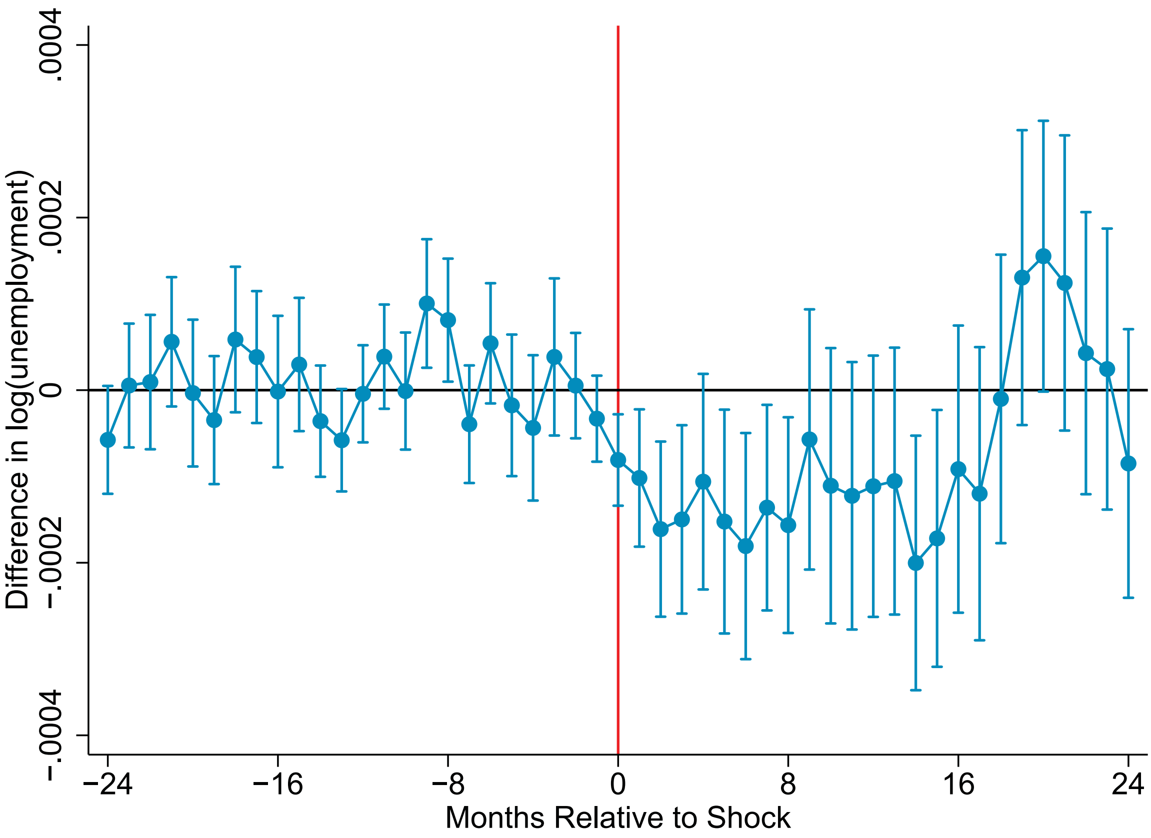

Notes: Blue points represent the dynamic response of unemployment to a fiscal stimulus of one euro per capita, under different dynamic specifications. Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

Figure 5: Robustness: Alternative sample selection



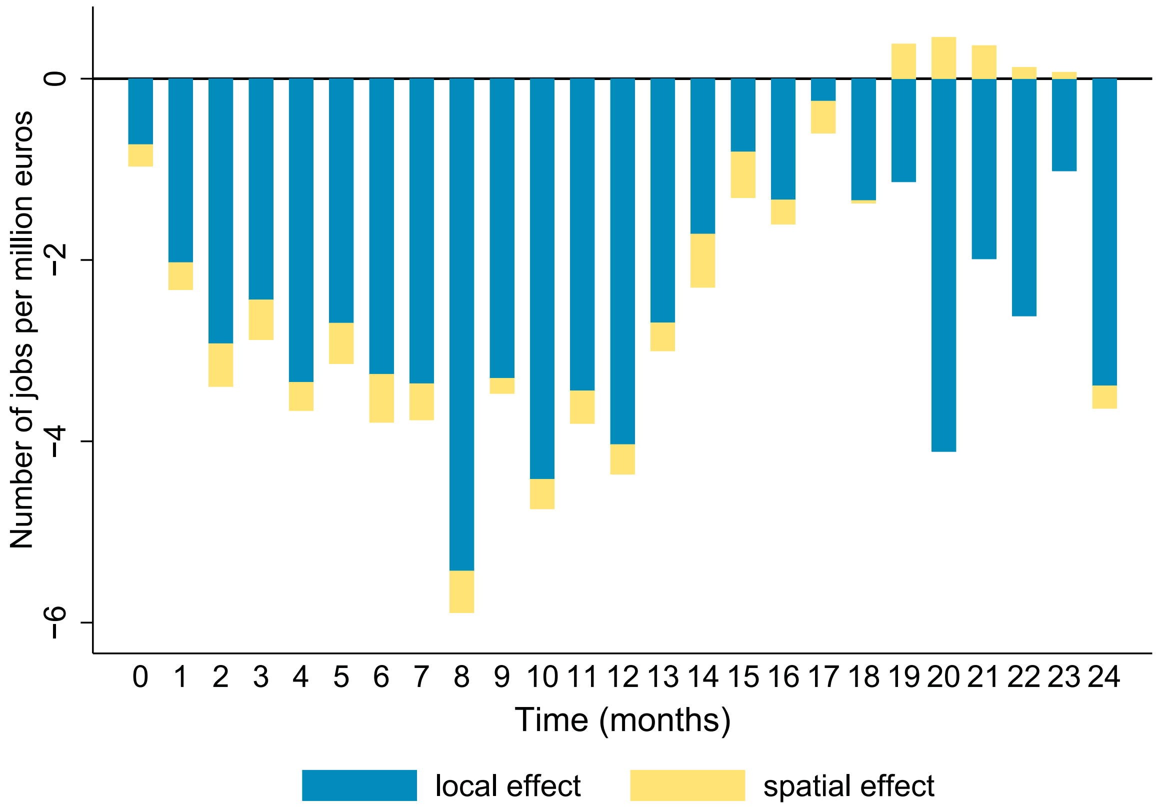

Notes: Blue points represent the dynamic response of unemployment to a fiscal stimulus of one euro per capita, for different samples. Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

Figure 6: Spatial effects (20km)



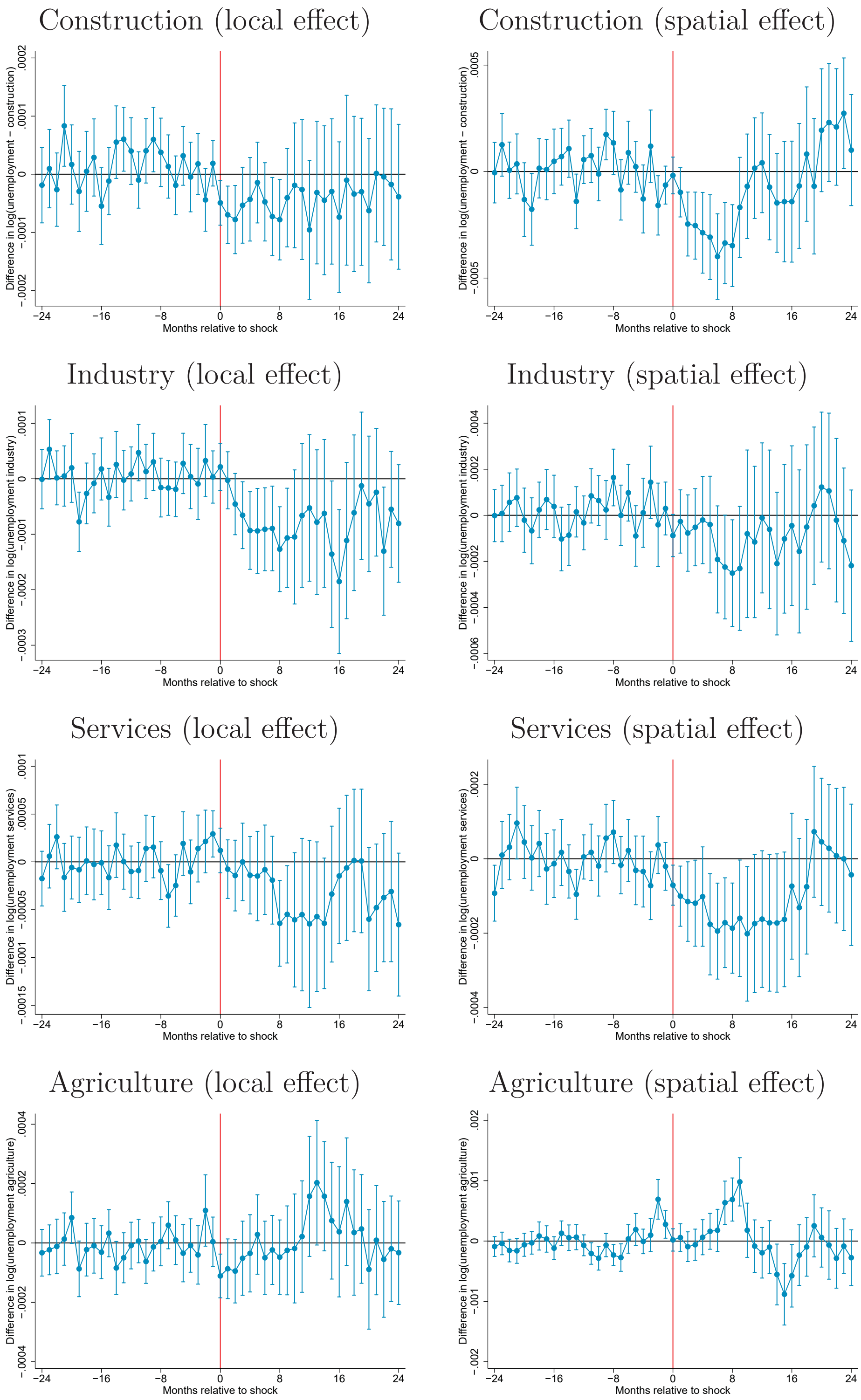

Notes: Blue points represent the dynamic response of unemployment in a municipality to an increase in 1 euro per capita from the fiscal stimulus in surrounding municipalities (within 20km). Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

Figure 7: Total jobs multiplier effect



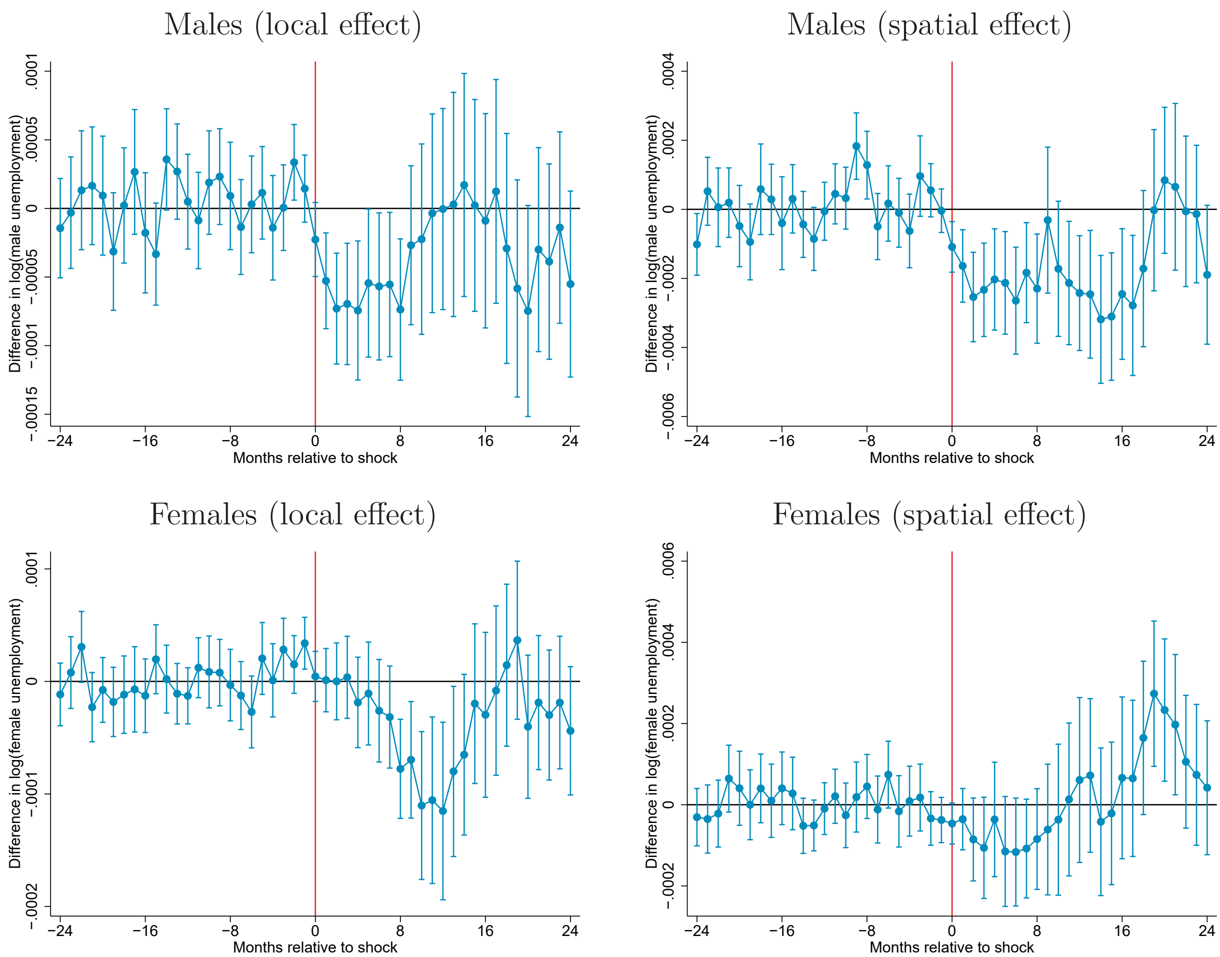

Notes: Blue bars represent the total number of jobs created per million euros spent in the municipality. Yellow bars represent the total number of jobs created per million euros spent in surrounding municipalities (within 20km).

Figure 8: Response of unemployment by sectors



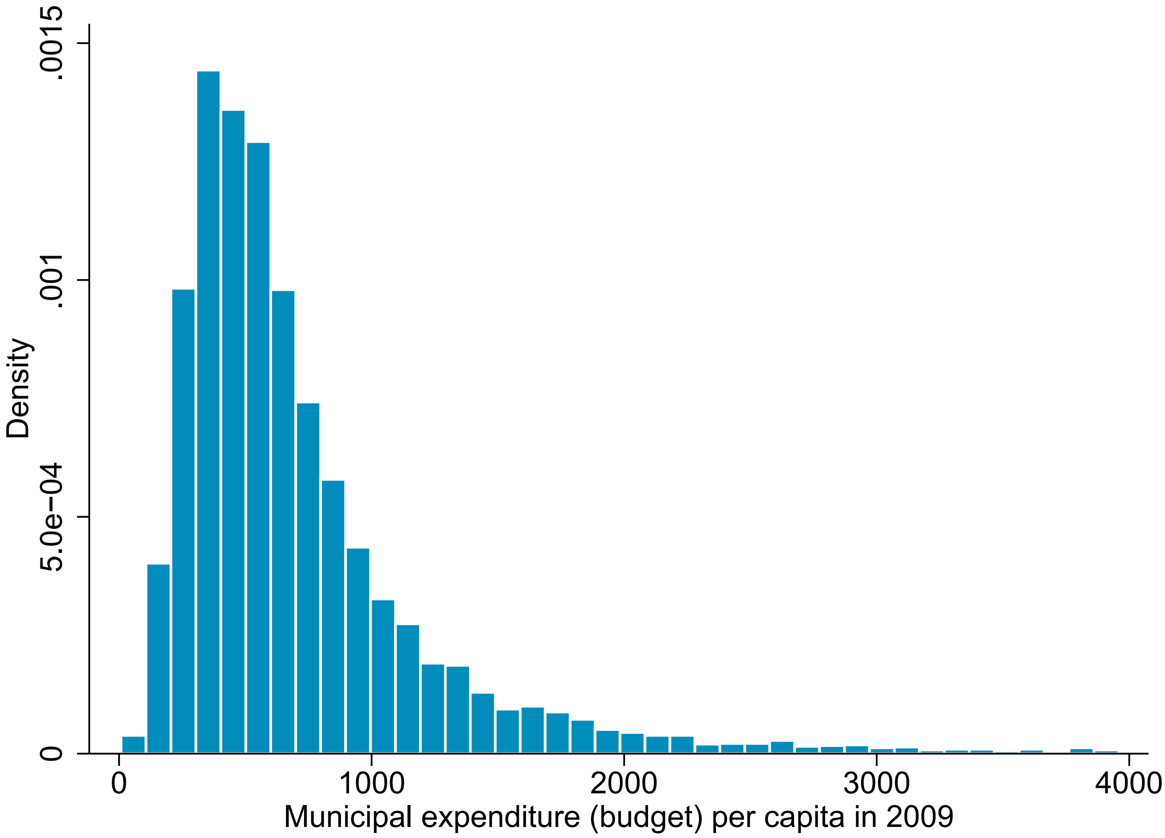

Notes: Blue points represent the dynamic response of unemployment in a municipality to an increase in 1 euro per capita from the fiscal stimulus in the same municipality (local effect, in left column) and in surrounding municipalities (spatial effect, in right column). Each row shows the response of registered unemployment from different economic sectors. Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

Figure 9: Response of unemployment by gender



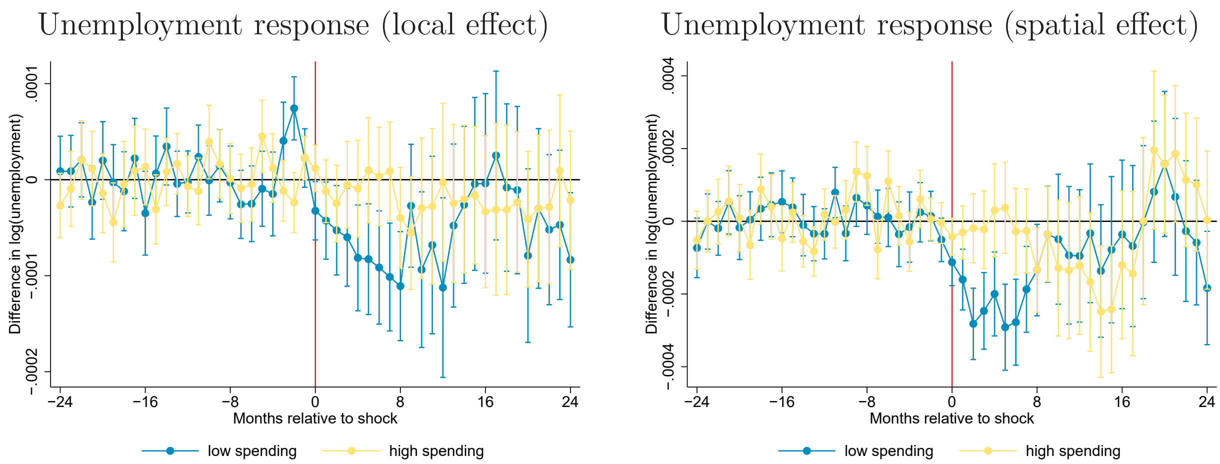

Notes: Blue points represent the dynamic response of unemployment in a municipality to an increase in 1 euro per capita from the fiscal stimulus in the same municipality (local effect, in left column) and in surrounding municipalities (spatial effect, in right column). Upper and bottom rows show the response of male and female registered unemployment, respectively. Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

Figure 10: Distribution of per capita spending in municipal budgets



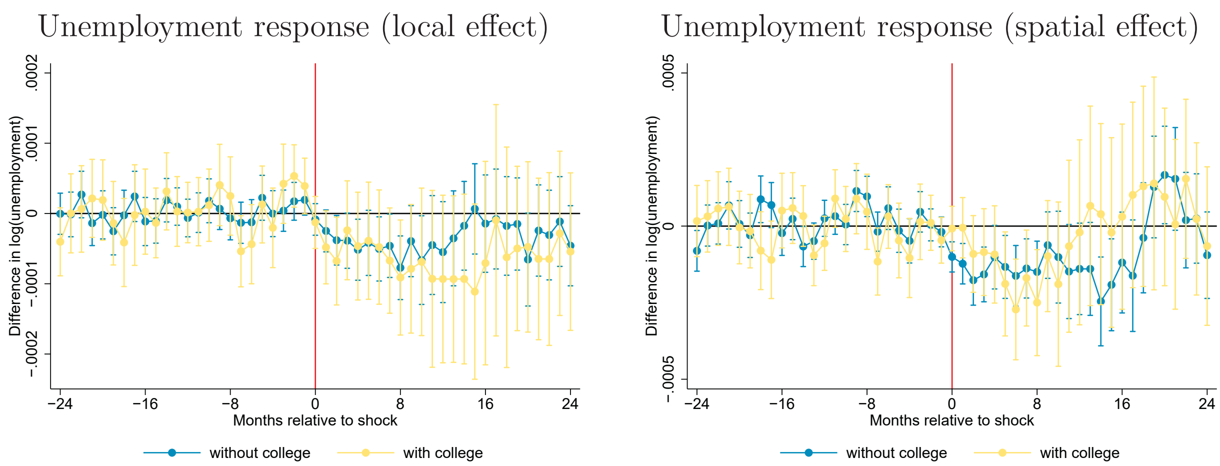

Notes: Distribution of the ratio of spending to population from municipal budgets.

Figure 11: Response of unemployment by spending per capita




Notes: Points represent the dynamic response of unemployment in a municipality to an increase in 1 euro per capita from the fiscal stimulus in the same municipality (local effect, in left column) and in surrounding municipalities (spatial effect, in right column). Blue and yellow lines refer to spending per capita below and above median, respectively. Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

Figure 12: Response of unemployment by level of education of Mayors




Notes: Points represent the dynamic response of unemployment in a municipality to an increase in 1 euro per capita from the fiscal stimulus in the same municipality (local effect, in left column) and in surrounding municipalities (spatial effect, in right column). Blue and yellow lines refer to level of education per capita with and without college education, respectively. Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

# Tables

Table 1: Jobs multipliers (employment creation per million euros), local effect

| | impact | 3m | 6m | 12m | 24m | peak |
|---|---|---|---|---|---|---|
| point estim. | -0.88 | -2.72 | -3.6 | -4.46 | -3.77 | -5.69 |
| CI 68% | [-1.6,-0.2] | [-4.0,-1.5] | [-5.1,-2.2] | [-6.8,-2.2] | [-5.6,-2.0] | [-7.2,-4.3] |
| CI 95% | [-2.3,0.5] | [-5.2,-0.3] | [-6.5,-0.8] | [-9.0,-0.1] | [-7.4,-0.2] | [-8.6,-3.0] |

Notes: The first row show the estimated jobs multiplier in a municipality (number of jobs created per million euros spent in the same municipality). Second and third rows display 68 and 95% confidence intervals, respectively.

Table 2: Jobs multipliers (employment creation per million euros), spatial effect

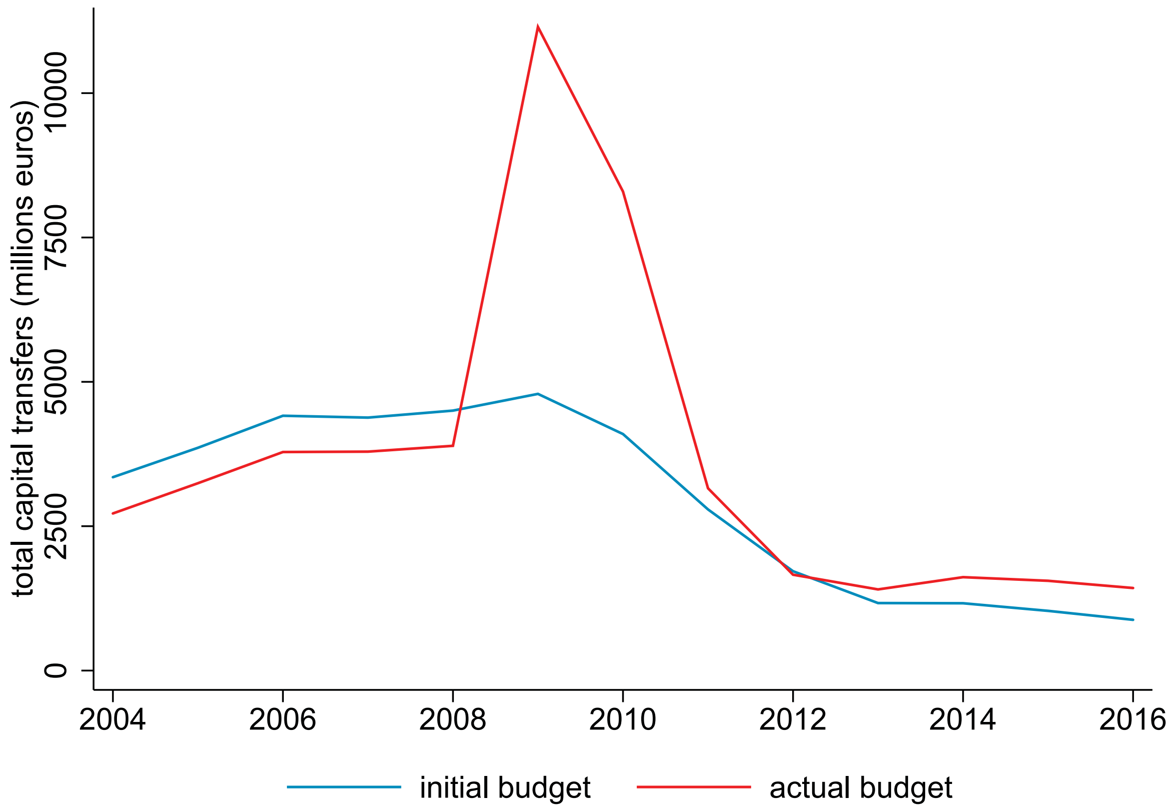
| | impact | 3m | 6m | 12m | 24m | peak |
|---|---|---|---|---|---|---|
| point estim | -0.24 | -0.44 | -0.53 | -0.33 | -0.25 | -0.59 |
| CI 68% | [-0.32,-0.16] | [-0.60,-0.28] | [-0.73,-0.34] | [-0.56,-0.10] | [-0.48,-0.02] | [-0.81,-0.37] |
| CI 95% | [-0.39,-0.08] | [-0.76,-0.12] | [-0.92,-0.15] | [-0.77,0.12] | [-0.71,0.21] | [-1.02,-0.16] |

Notes: The first row show the estimated jobs multiplier in a municipality (number of jobs created per million euros spent in the surrounding municipalities within 20km). Second and third rows display 68 and 95% confidence intervals, respectively.

# Online Appendices

## Appendix Figures

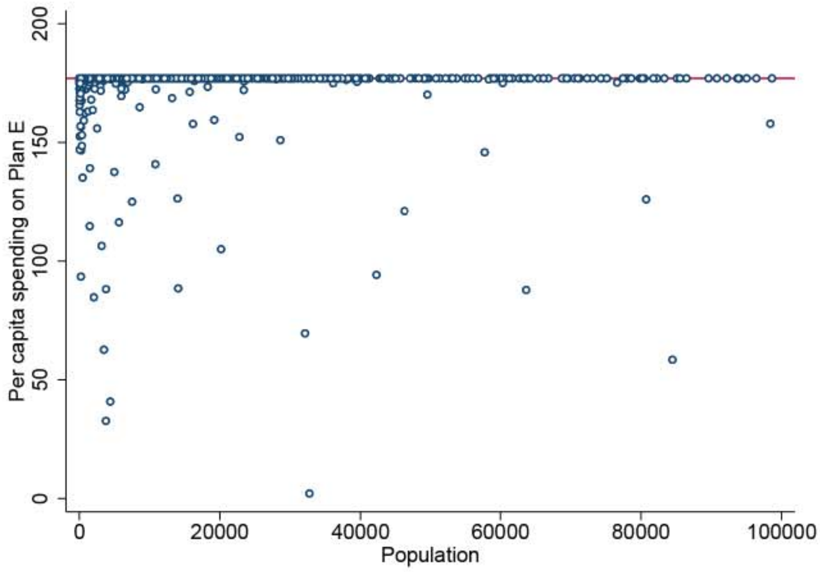
Figure A1: Initial and actual capital transfers



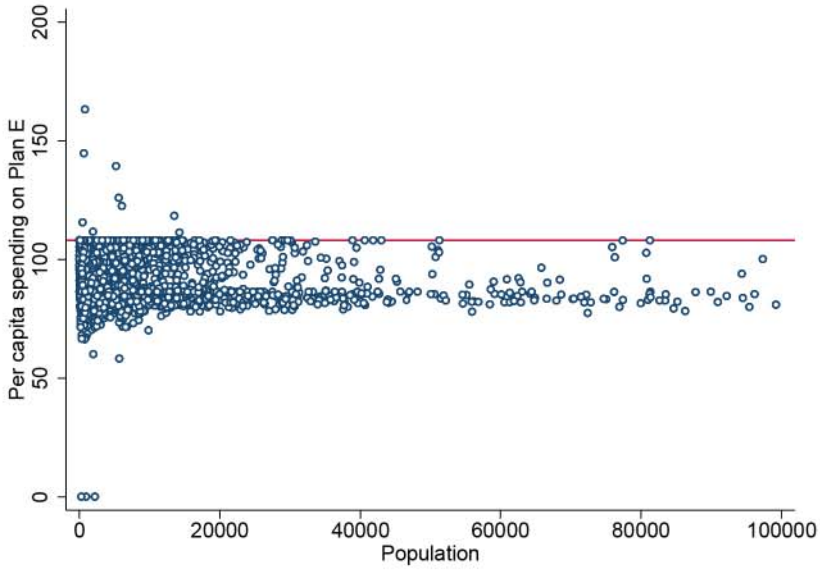

Notes: The blue line represents the sum of capital transfers received by all municipalities in Spain, as reflected in the initial budget. The red line represents the same variable as reflected in the actual, executed budget.

Figure A2: *Plan E* Spending

Panel A: First wave

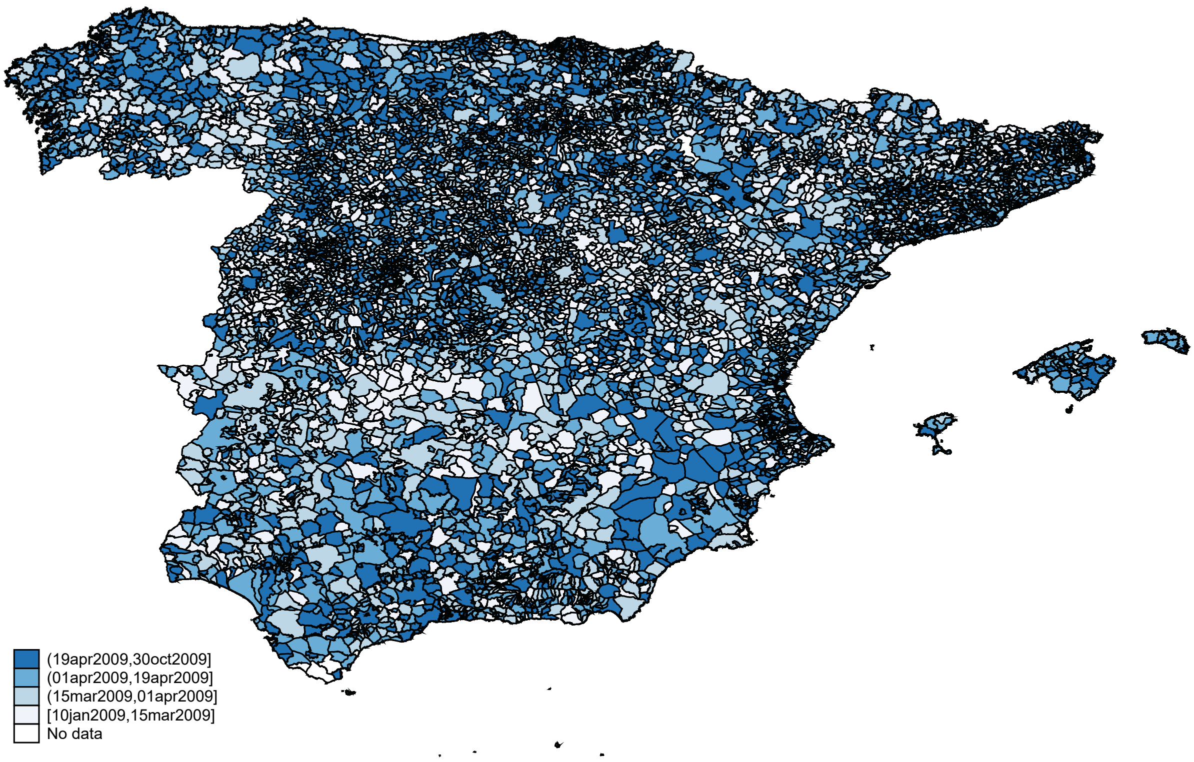



Panel B: Second wave



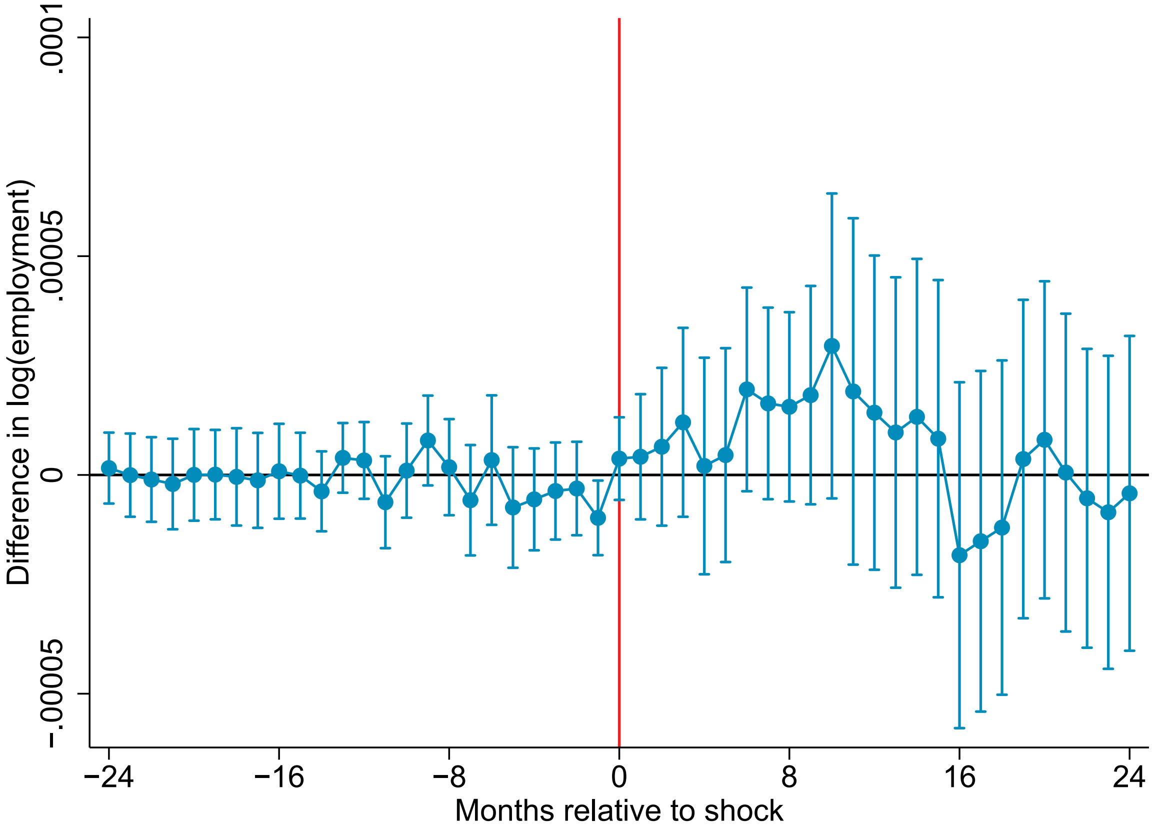

Notes: Each point represents a Spanish municipality. Panel A shows per capita spending in euros as a function of population size of the municipality in the first wave of *Plan E* (2009). Panel B shows per capita spending during the second wave (2010). Municipalities with more than 100,000 inhabitants are not shown in the graphs.

Figure A3: Geographical distribution of starting dates



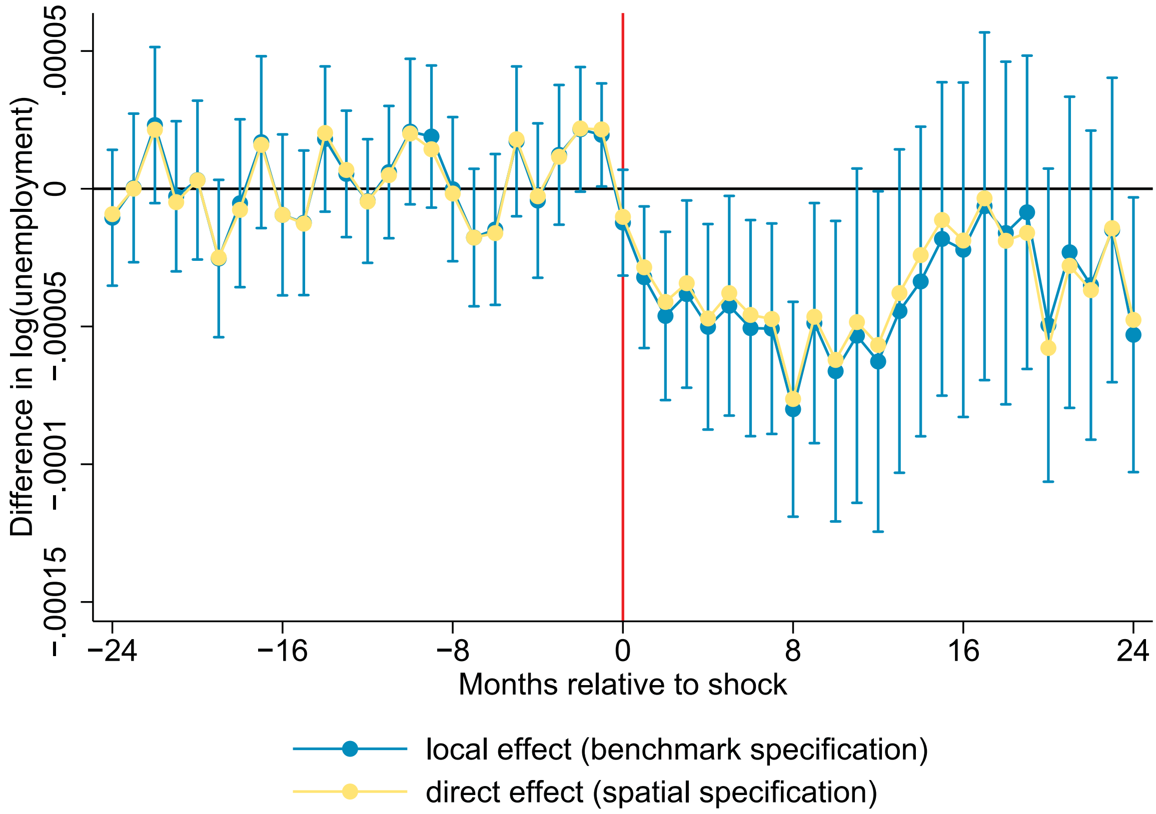

Notes: Distribution of the average starting date of projects.

Figure A4: Robustness: Effect on employment (local effect)



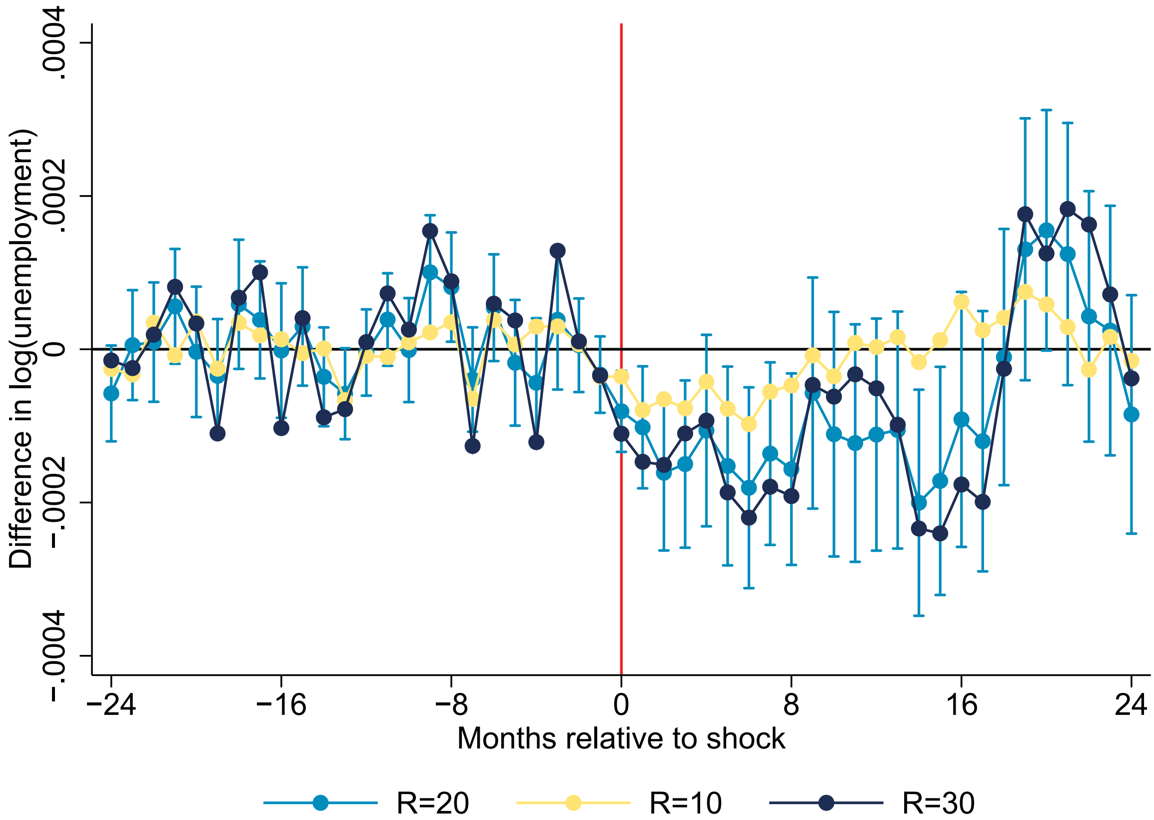

Notes: Blue points represent the dynamic response of employment (Social Security affiliations) to a fiscal stimulus of one euro per capita. Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

Figure A5: Robustness: Local effect with and without spatial control



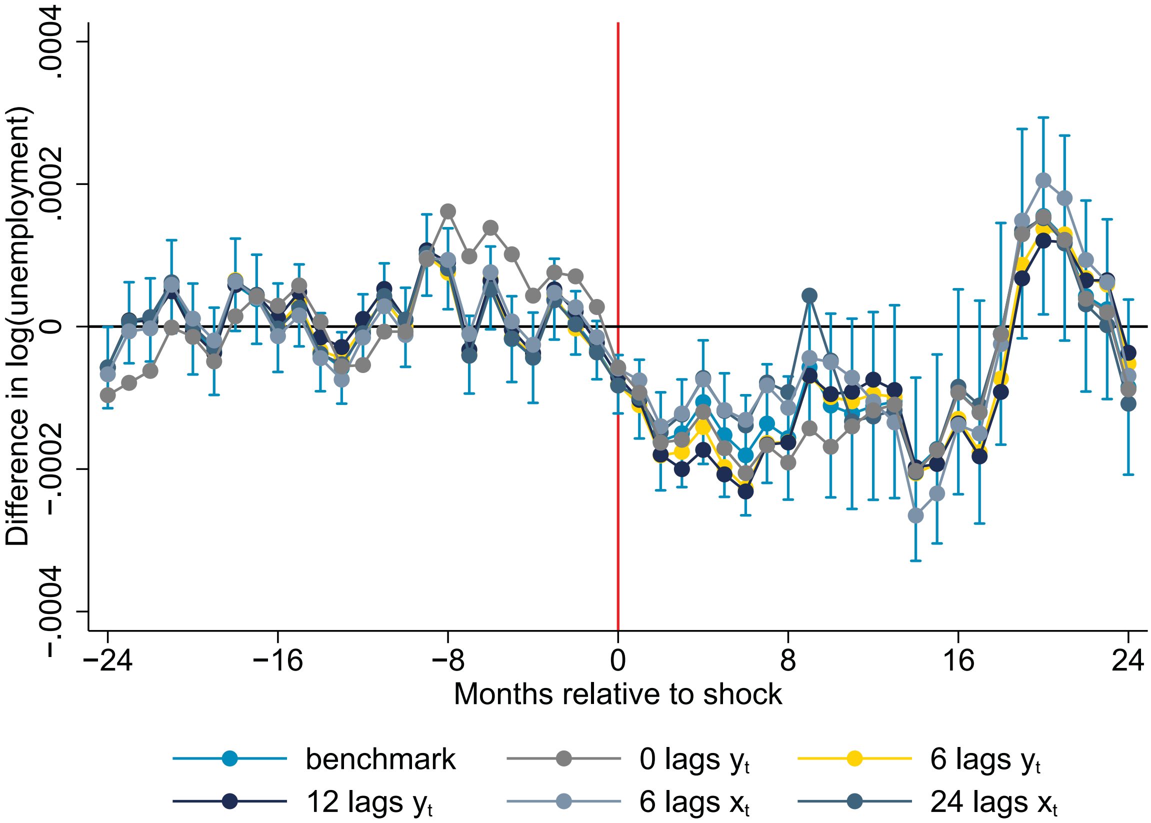

Notes: Blue and yellow points represent the dynamic response of unemployment in a municipality to an increase in one euro per capita from the fiscal stimulus in the same municipality estimated from equations 1 and 2, respectively. Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

Figure A6: Robustness: Alternative radius (spatial effects)



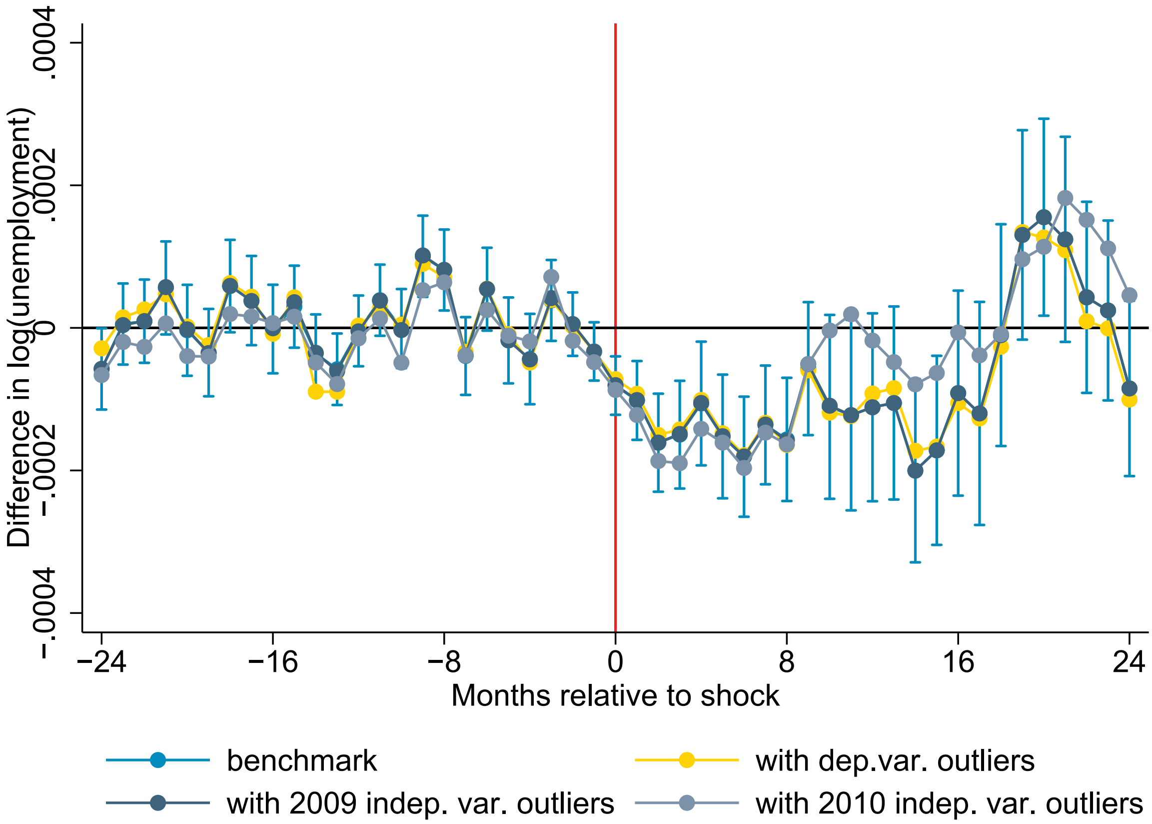

Notes: Points represent the dynamic response of unemployment in a municipality to an increase in 1 euro per capita from the fiscal stimulus in the surrounding municipalities for different radii (10, 20, and 30 km). Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

Figure A7: Robustness: Alternative dynamic structure (spatial effects)



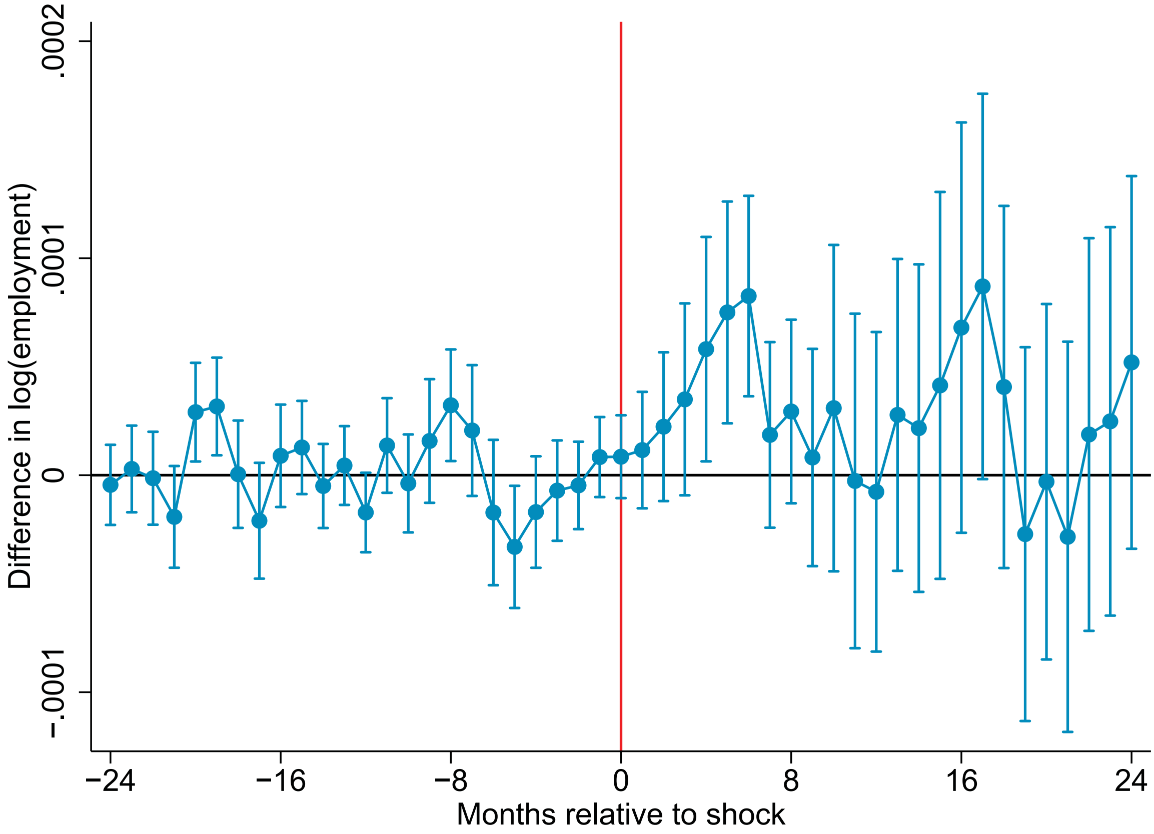

Notes: Blue points represent the dynamic response of employment in a municipality to an increase in 1 euro per capita from the fiscal stimulus in the surrounding municipalities (within 20km), under different dynamic specifications. Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

Figure A8: Robustness: Alternative sample selection (spatial effects)



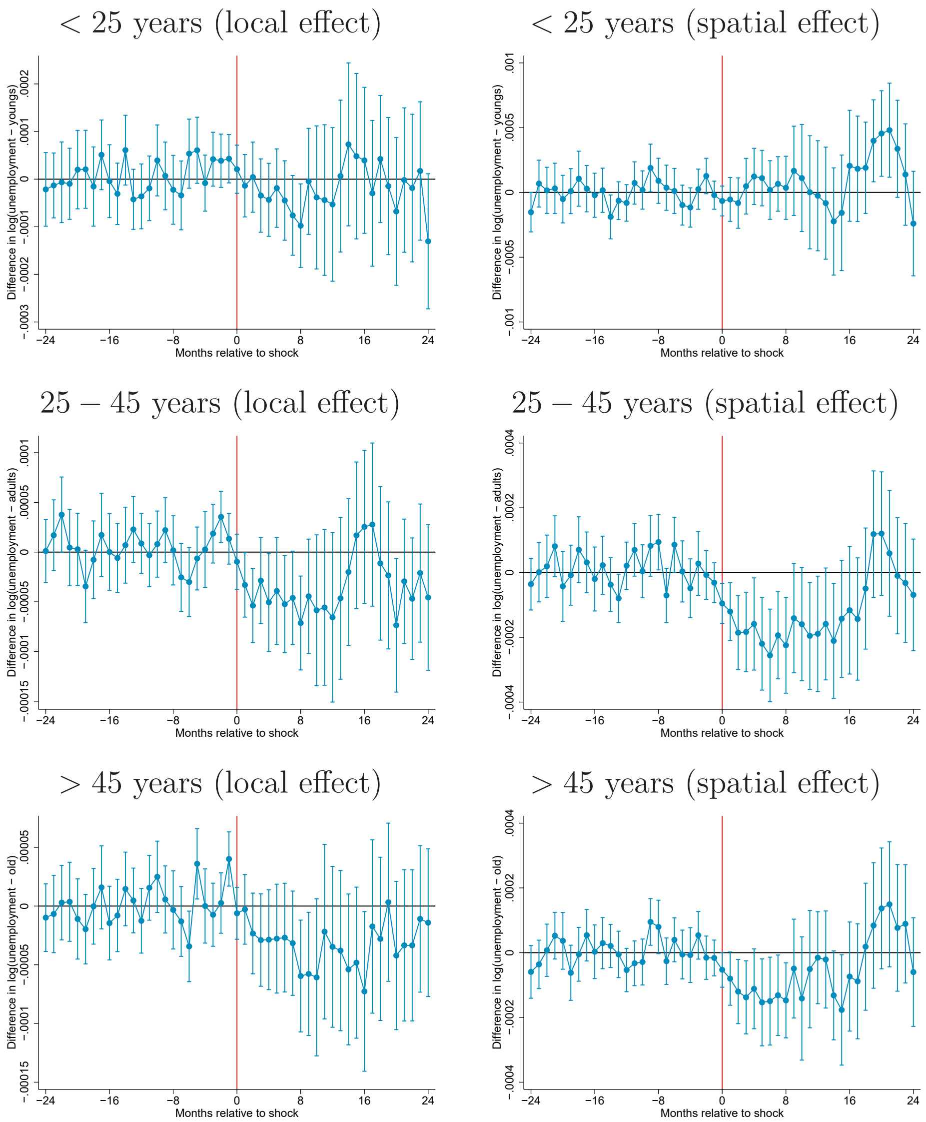

Notes: Blue points represent the dynamic response of unemployment in a municipality to an increase in 1 euro per capita from the fiscal stimulus in the surrounding municipalities (within 20km), for different samples. Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

Figure A9: Robustness: Effect on employment (spatial effect)




Notes: Blue points represent the dynamic response of employment (Social Security affiliations) in a municipality to an increase in 1 euro per capita from the fiscal stimulus in the surrounding municipalities (within 20km). Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

Figure A10: Response of unemployment by age




Notes: Blue points represent the dynamic response of unemployment in a municipality to an increase in 1 euro per capita from the fiscal stimulus in the same municipality (local effect, in left column) and in surrounding municipalities (spatial effect, in right column). Upper, middle, and bottom rows show the response of registered unemployment amongst young workers (up to 25 years), adults (25-45 years) and older workers (above 45), respectively. Vertical lines are 95% confidence bands computed using standard errors clustered by municipality.

# Appendix: Mapping Jobs Multipliers to Output Multipliers

Aggregate nation-wide government spending multipliers are often expressed in term of changes in dollars of output per dollar of public spending.[45] In contrast, local fiscal multipliers are often expressed in terms of jobs or employment multipliers due to the lack of output at a geographically disaggregated level. In the case of Spain, although there is a limited availability of annual regional measures of output, these do not exist at the municipal level.

In this section, we follow Chodorow-Reich (2019) to establish an approximate mapping between the employment and output multipliers.

First, consider the following relationships (geographical subindices are dropped for clarity purposes):

$$\begin{aligned} \Delta Y_{t+h} &= \beta_h^Y \Delta G_t \\ \Delta E_{t+h} &= \beta_h^E \Delta G_t, \end{aligned}$$

where $Y_{t+h}$ is output at period $t+h$ and $E_{t+h}$ denotes employment (number of workers in the economy). $\beta_h^Y$ measures the output multiplier to a fiscal shock (change in dollars of output per change of dollars in government spending), which is the object that we want to recover. $\beta_h^E$ is the jobs multipliers, as estimated in sections 4 and 5.

For convenience, we define the percentage deviations of output and employment due to changes in government spending as $y_{t+h} = \frac{\Delta Y_{t+h}}{Y_t}$ and $e_{t+h} = \frac{\Delta E_{t+h}}{E_t}$, respectively. Let $g_t = \frac{\Delta G_t}{Y_t}$ the change in spending as a share of total output. We can then express the percentage deviation of employment as:

$$e_{t+h} = \beta_h^E \frac{Y_t}{E_t} g_t.$$

Next, consider a production function as:

$$Y_t = A \left(H_t E_t\right)^{\gamma} \left(K_t^P\right)^{\alpha} \left(K_t^G\right)^{\zeta},$$

where $L_t = H_t E_t$ is the total number of labor (measured in hours) supplied in an economy in time $t$. $K_t^P$ and $K_t^G$ represent private and public capital, respectively. The most important simplification of this analysis is to assume that $\alpha = \zeta = 0$, that is, neither private or public capital become available for the production in time $t$. While this is a plausible assumption often encountered in macroeconomic models, it makes our calculation only informative for

[45] See for example Blanchard and Perotti (2002) for a seminal estimation of the aggregate output multipliers in the US and Ramey (2018) for a recent review.

the short run. For medium and long-run horizons it is sensible to assume that both public and private capital will become usable. Under the common assumption that the marginal product of labor is increasing in both types of capital, our calculation will recover a strict lower bound of public investment.[46]

Let $h_t = \frac{\Delta H_{t+h}}{H_t}$ denote the percentage variation in hours per worker and $\chi = \frac{h_t}{e_t}$ the elasticity of the hours per worker to total employment. We can then express the output multiplier as a function of the employment multiplier:

$$\beta_h^Y = \frac{y_{t+h}}{g_t} = \frac{y_{t+h}}{e_{t+h}} \frac{e_{t+h}}{g_t} \approx (1-\gamma)(1+\chi)\frac{Y_y}{E_t}\beta_h^E.$$

In the case of the US, Chodorow-Reich (2019) argues that $(1-\gamma)(1+\chi) \approx 1$, concluding that the output multiplier can be approximated by just considering the employment multiplier (in terms of cost per job) by output per worker $(Y_t/E_t)$. For the case of Spain, we find that for reasonable parameters of $\gamma$ and $\chi$, $(1-\gamma)(1+\chi)$ could be between 0.75 and 1.3.[47] The ratio $\frac{Y_t}{E_t}$ was 19,639 and 18,342 in 2010 and 2012, respectively. Following ours previous results, we consider a jobs multiplier of 5.89 (which represents a cost of about 170,000 euros per job). Under this calibration, we find the output multiplier to be 0.3–0.5 in the short run.

[46] The final effect of public investment on output depends crucially on the parameter $\zeta$, which measures the efficiency of public capital. Baxter and King (1993) find that public investment can crowd in private investment in a calibrated general equilibrium model even for low parameters of $\zeta$, delivering an aggregate multiplier multiplier of above 4 in the long run. Alloza, Burriel, and Pérez (2018) use aggregate time series variation to find that the output multiplier from public investment in Spain is 0.3 upon impact raising up to 1.1. by the end of the first year, and around 2.3 by the second year.

[47] Labor share $\gamma$ is around 0.63 using standard calculations and Spanish national accounts data. Data for total employment, nominal output, and hours per worker are obtained from the Spanish National Statistics Institute (INE) for years 2008–2012.

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