Employment Effects of Stimulus Investments

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Preliminary and incomplete, please do not circulate.

We estimate the causal impact of countercyclical infrastructure investments on employment and unemployment from cross-county data. To this end, we exploit a particular feature of the sizable German infrastructure investment program enacted during the Great Recession. By law, 65 % of all stimulus funds were earmarked for investment in educational infrastructure with a special focus on the renovations of existing school buildings. The scope for stimulus investment at the local level was thus to a large part predetermined by the number of schools, opening up the possibility to use schools as an instrument for local investments. Our 2SLS estimates suggest costs for one job-year between 18'500 Euro and 35'500 Euro. The first jobs were created one year after the stimulus program was enacted. These costs per job-year are similar to estimates from evaluations of the US stimulus program.

Keywords: Stimulus Investments, Job Creation, Employment, Countercyclical Fiscal Policy
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1 Introduction

Infrastructure investment programs were a major part of many countries fiscal stimulus measures that were enacted in wake of the Great Recession to bolster economic conditions. For example the American Recovery and Reinvestment Act of February 2009 contained provisions for infrastructure investments of 105.3 billion Dollar or 0.7% of GDP. Similarly, in March 2009 the German federal government passed a stimulus package stipulating 13.3 billion Euro or 0.4% of GDP for infrastructure investments.

However, even years after the height of the crisis it remains unclear whether infrastructure investments were a timely and cost-effective way to counteract the ensuing recession. How many jobs were created? How long did it take for the stimulus to affect the economy? Answering these questions from observational data is typically difficult because stimulus investments are – by construction – endogenous to economic conditions. Furthermore, the total number of independent data points is usually low: each stimulus program is a one-time event affecting the entire economy. Finally, the presence of implementation lags and ensuing anticipation effects as well as different sources of funding – on the local, state, or federal level – for single investment projects presents a challenge for the evaluation of government investments in general (see e.g. Leeper et al., 2010; Ramey, 2011b; Leduc and Wilson, 2012a).

In this paper, we make progress on these issues by providing causal estimates of the efficacy of countercyclical infrastructure investments from local level data. We study the employment effects of the German infrastructure investment program of 2009, the particular setup of which allows us to circumvent the problems outlined above.

First, the legal restrictions of the stimulus bill were such that 65 percent of funds had to be spent on investments in the educational infrastructure, and, in particular, on energy renovations.[1] This restriction implies that the scope for investments on the local level was, in part, predetermined by the number of existing buildings belonging to the local educational infrastructure. We can thus overcome the endogeneity problem by using the historically pre-determined number of schools within a county as instrument for stimulus funds received.

Second, the bill placed particular emphasis on investment projects being implemented on the local level. As a result, stimulus funds were spread across the country, making it possible to use cross-sectional variation across the 400 German counties to identify the effect of investments on local labor markets.

Third, we collect data on all projects financed by the program such that we know exactly where projects were implemented as well as how much was spent, regardless of the financing source. Furthermore, because the program was geared towards boosting the economy as fast as possible, the time between the formal passing of the stimulus bill (or the first hearings on the bill) and the date projects had to be completed was quite short. The bill was passed in March in 2009, projects had to be underway by the end of 2010, and all funds had to be used by the end of 2011. There

[1] The remaining 35 percent were earmarked for investment in the general infrastructure on the local level, like the renovation of public buildings or construction of broadband infrastructure.

is, thus, a very well defined time-span during which we expect to see the program's main economic impact leaving little room for implementation lags and anticipation effects.[2]

As expected by the legal set-up, differences in the number of schools across counties predict differences in stimulus investments to a significant degree. Depending on the school type, one additional school in a county is associated with an increase in investments between 150'000 Euro and 1 million Euro, all other things equal. This implies that a one standard deviation in the number of schools increases investments within a county of average size by 7.8 million Euro. We also show that the total amount of public investments on the local level is directly related to the amount of stimulus investments received, which alleviates a possible concern that stimulus investments crowd out other public investments.

We find substantial effects of stimulus investments on unemployment and employment.[3] As estimated by our instrumental variables (IV) strategy, additional investments of 100'000 Euro during the entire program duration (March 2009 to December 2011) increased employment by four to five job-years while unemployment is reduced by three to four person-years. This corresponds to costs-per-job year of about 18'500 Euro to 25'000 Euro for employment and 25'000 Euro to 35'000 Euro for unemployment. The corresponding effects estimated via OLS are approximately half this size suggesting that the allocation of investments has indeed been endogenous. If we take our IV estimates at face value, the 10.0 billion Euro investment in schools alone created or saved about 405'000 to 550'000 job-years or prevented 285'000 to 405'000 unemployment-years over the three years of program duration.[4]

These employment effects built up mainly during the year 2010 and faded relatively quickly after the end of the stimulus program in 2012; the reversion was more severe for employment than for unemployment. This dynamic pattern suggests an implementation lag of at least three quarters which seems to be sensible as the stimulus bill explicitly required that investment projects must be "new and additional". Projects thus needed to be approved after the bill was signed into law. Furthermore, we do not expect to see high productivity gains given the nature of investments such that the quick fading out of the employment gains seem to be plausible as well.

The biggest threat to our identification strategy is a violation of the exclusion restriction meaning that counties with different numbers of schools per capita would have followed different employment trends absent the stimulus program. While the exclusion restriction is not testable, we show that employment dynamics are unrelated to the number of schools per capita for the years between the implementation of the major German labor market reform in 2005 and the onset of the crisis at the end of 2008. This finding is to be expected, as the number of public buildings has changed little since the 1970s, because the stock of buildings accumulated after World War II and during the population boom of the 60s and 70s is more than sufficient to meet current demand at times

[2] This does, of course, not exclude the potential presence of long-run effects of the program.

[3] We focus on employment as the outcome measure, because the available data at the local level are of sufficiently high quality.

[4] Note that because our empirical strategy exploits cross-county variation in investments we cannot take general equilibrium effects operating on the state or national level into account.

of population decline. Emphasizing this point, we show that the number of schools in 1998 is an almost perfect predictor for today's number of schools and that our results are insensitive regarding in which years schools are measured. Finally, we extensively control for different factors that may cause the cross-sectional variation in the number of schools per capita – such as population density, the size of the school-aged-population, or the education of the workforce – and show that our conclusions remain largely unchanged when doing so.

Related Literature This study is one of the first to estimate the effectiveness of countercyclical infrastructure investment on job creation using cross county variation. We thus contribute to the literature evaluating the job creation of countercyclical fiscal policy measures of various types during the Great Recession (Chodorow-Reich et al., 2012; Wilson, 2012; Conley and Dupor, 2013; Feyrer and Sacerdote, 2011). The estimated cost-per-job year in the literature vary widely between $26'000 (Chodorow-Reich et al., 2012) and $ 202'000 (Conley and Dupor, 2013). All of these studies estimate the average effect of aggregate monetary transfers from the federal to the state level on state employment using variation across 50 states.[5] We contribute to this literature by showing that one particular policy measure, infrastructure investments, can have sizable employment effects. By exploiting variation across 400 counties instead of 50 states we also gain statistical power which helps us to extensively control for potential confounding factors of fiscal policy yielding more reliable estimates of causal effects. We also provide the first evaluation of a stimulus program enacted during the Great Recession using cross-sectional data for a country other than the United States.

Most closely related to our study is the small but growing literature that aims at estimating the local multiplier of infrastructure investments. Using formulary allocation factors of federal grants to states to identify exogenous variation in highway construction, Leduc and Wilson (2012a) find a multiplier between three and eight that is more or less independent of the state of the economy and considerably larger than our estimates.[6] Recently, Acconcia et al. (2014) use unanticipated, temporary shutdowns in public works due to an Italian law fighting Mafia infiltration as instruments for contractions in public investments. They find the government spending multiplier to be between 1.5 and 1.9 in normal times. Given our estimates of costs per job-year between 18'500 and 35'500 Euro and an average GDP per worker of 30'000 Euro per worker in Germany, our estimates are within a similar range. To the extent that the effects of fiscal policy in Italy and Germany are comparable, our estimates thus suggest that the effect of public investments on local economies are of similar size during booms and recession. Note, however, that the slack in Italian labor markets may be responsible for this observation.

This study also contributes to the literature on the local employment effects of non-defense government spending by providing estimates for countries other than the United States. The estimated costs per job year of various fiscal spending measures from the US are thereby strikingly

[5] Wilson (2012)and Feyrer and Sacerdote (2011) estimates the effect of transfers from different funding agencies such as the Department of Transportation separately. As noted by Conley this transfers might not coincide with spending in any particular category because funds are fungible on the state level.

[6] See Leduc and Wilson (2012b) for a survey of the literature regarding the role of infrastructure investments in transportation.

similar to ours. Suarez Serrato and Wingender (2014) instrument government spending at the county level with measurement error in population estimates and arrive at cost per job-year of 31'000 Dollar. Shoag (2010) uses variation in the returns of state pension funds to instrument for fiscal spending and find that 35'000 Dollar generate one additional job. A lower bound on the cost per job-year – 25'000 Dollar – is provided by Clemens and Miran (2012) who use budget cuts triggered by balanced-budget regulations to instrument for the scope of fiscal policy.

There is, of course, an extensive literature which provides estimates of the fiscal multiplier from other sources of variation. See Ramey (2011a) for a comprehensive overview.

In the next section we describe the stimulus program. In Section 3 we introduce our empirical strategy. Our data and the identifying variation within our main variables of interest are described in Section 4. Section 5 shows that our instrument, schools, is a strong predictor of the (potentially) endogenous stimulus investments. In Section 6 we present our main results and shows that instrumented investments do not explain labor market dynamics before the program was implemented. Section 7 concludes.

2 The German Stimulus Investment Program

The investment program we study (called "Zukunftsinvestitionsgesetz") was the major government spending measure within the two German stimulus packages enacted amidst the height of the Great Recession. It stipulated investments of 13.3 billion Euro into the general local infrastructure (15.2 billion or 0.6 percent of pre-crisis GDP were in fact spent). In addition to this large specific program we study, the stimulus packages contained two smaller investment programs providing 6 billion Euro for investments on the federal level, reductions in taxes and social security contributions of 66.2 billion Euro, subsidies of 9.4 billion Euro (mostly for a cash for clunkers program), as well as direct labor market interventions which are difficult to quantify as they mainly extended the duration of short-time work benefits to workers of firms in temporary financial distress.[7]

The aim of the investment program was to stimulate the economy at the local level by providing federal funds to local governments. However, the German constitution severely limits the means of the federal government to interfere with politics at the state level or below. The types of projects that can be financed via federal grants is thus heavily restricted from the outset.

For this reason, the stimulus bill entailed relatively detailed requirements that investment projects needed to fulfill in order to be financed via federal funds. Specifically, the bill mandated that 65 percent of funds were to be used for investments in the local educational infrastructure with an emphasis on (energy) renovations of existing buildings. The remaining 35 percent had to be used for investments in the general public infrastructure. The bill further specifies eleven funding lines; in terms of the share of funds spent ex post, the three most important lines were, in decreasing order, (energy) renovations of schools, (energy) renovations of universities, and hospitals. To reduce the fungibility of funds, moreover, only "new and additional" projects could be financed. In practice,

[7] See Bundesministerium für Wirtschaft und Technologie (2011) for details.

this implied that projects that were already budgeted in any public account could not receive financing from the program.

In addition to these restrictions on the types of investment projects, the bill placed a heavy emphasis on projects being implemented locally. It mandated that 70 percent of funds were to be spent on infrastructure investments at the municipal level. The federal government also loosened the rules for public procurement, mainly to speed up the implementation of projects. According to the German Court of Auditors, this had the additional effect that the share of contracts awarded to local firms increased strongly for projects financed by the program (Bundesrechnungshof, 2012).

Within the bounds of the bill's legal preconditions, the implementation of the program, in particular the decision on how funds are distributed to the local level, was at the discretion of state governments in order to meet the constitutional requirements mentioned above. Funds were allocated to states on the basis of a standard formulary allocation factor. Federal funds were matching grants financing at most 75 percent of the costs of a single project; the remaining share of 25 percent (or more) of the costs had to be provided by regional governments. The difference between the funds of 13.3 billion Dollar stipulated by the bill and the 15.2 billion Euro actually spent is due to a higher share of funds provided by governments at the state level or below.

The bill was implemented swiftly. The first parliamentary hearings of the bill took place on January 12th, 2009. The bill was passed in parliament on March 5th, and states had to report their implementation procedures to the federal government by May 30th, 2009. All projects to be financed by the program had to be under way by the end of 2010, and all funds had to be spent by the end of 2011, less than three years after passage of the program.

3 Empirical Model

We aim to identify the effect of stimulus investments on (un)employment by exploiting differentials in the total amounts invested across German counties. Specifically, we estimate the dynamic effect of total per capita investments on the (un)employment rate using a standard difference in difference (DiD) structure where the "treatment", the level of stimulus investments per capita, varies continuously:

$$\begin{aligned}\text{(un)employment rate}_{ct} = \alpha_c + \sum_{t \in \Theta} \beta_t \text{ per capita investments (2009-2011)}_c \times I(\text{date} = t) \\ + \delta_{ct} \text{ state}_c \times \text{urbanization index}_c \times I(\text{date} = t) + \mathbf{X_{ct}^{'}\Gamma} + \varepsilon_{ct}. \qquad (1)\end{aligned}$$

The index c denotes the county, while t denotes time; α_c is a county fixed effect, δ_{ct} is a time fixed effect that may vary by the interaction of a county's state and the value of a urbanization index, $\mathbf{X'_{ct}}$ is a vector of control variables, and ε_{ct} is an error term. All variables are described in Section 4 below. We estimate model (1) for the years 2006 to 2013, that is for all years after the far-reaching German labor market reforms which have been implemented in 2005.

Note that we allow the coefficient of stimulus investments, β_t, to vary within certain time periods $t \in \Theta$. First, we let β_t vary within the implementation period of the stimulus program; that is, $\Theta_1 = \{t : t > \text{Q1 2009}\}$. Since we know the total investments within a county between the second quarter of 2009 and the fourth quarter of 2011 but not the timing of investments within that time, estimating time-varying coefficients allows us to assess the dynamic effect of the "treatment" on (un)employment. This dynamic effect is recovered relative to the pre-treatment period, that is under the assumption that β_t equals zero for $t \leq \text{Q1 2009}$. This is equivalent to assuming that the main identification assumption of the DiD model – the common trend assumption – holds for the pre-treatment periods.

To test the common trend assumption for the pre-treatment period, we also estimate β_t for all treatment and pre-treatment periods relative to the first quarter of 2009, which is the last quarter before the treatment; formally, $\Theta_2 = \{t : t \neq \text{Q1 2009}\}$. Up to the first quarter of 2009, stimulus investments were non-existent such that the coefficient of these placebo-investments is predicted to equal zero.

Note that the county and time fixed effects already control for a number of confounding factors of stimulus investments. The county fixed effect accounts for the possibility that the extent of stimulus investments may depend on a county's fixed characteristics such as its medium run level of output or its industry structure. The time fixed effects eliminate the cyclical component of employment outcomes that are common to all counties in a state that fall into one of four urbanization categories ranging from "major city" to "rural".[8] To the extent that the size of cyclical shocks differs by population density and that investments may depend on the size of the (negative) shocks, this controls for another potential source of reverse causality. More importantly, however, the time fixed effects eliminate the effect of any policy that operates on the national level, such as counterbalancing monetary policy or increasing taxes to fulfill balanced budgets requirements, that may dampen the aggregate effect of stimulus investments. See Nakamura and Steinsson (2014) for an extensive discussion of this issue. Our estimates β_t are hence to be interpreted as the open economy relative employment effect of stimulus investments (as opposed to the closed economy aggregate effect).

We account for the heterogeneity in the size of counties by normalizing all variables, unless otherwise noted in the variable description, by the counties' working age population as measured in 2009.[9] Moreover, observations are weighted by the working age population in all regressions. Finally, to account for serial and spatial correlation of the variables, all inference is based on standard errors clustered at the commuting zone level.[10]

[8] These urbanization categories ("Siedlungsstruktureller Kreistyp") are defined by the German Federal Office for Building and Regional Planning on the basis of population density.

[9] Because there are time-invariant variables like stimulus investments interacted with the date as well as county fixed effects on the right hand side of (1), a time-varying normalization variable would induce the estimates of these time-invariant variables to be biased. We control for population growth by including the ratio of the of the working age population in t to the working age population in 2009 as control variable in all our regressions.

[10] There are 94 commuting zones ("Raumordnungsregionen") in our dataset. They are defined by the German Federal Office for Building and Regional Planning on the basis of commuting patterns.

3.1 Instrumental Variable Strategy

Even though we control for a wide array of confounding factors for stimulus investments through the DiD structure of the empirical model, the concern remains that the amount of investments received by a county responds to unobservable current or expected economic conditions. In this case, investments would be correlated with the error term even conditional on a county's fixed characteristics, and estimates of the effectiveness of the stimulus would be biased and inconsistent.

To circumvent this problem, we instrument the amount spent on investments by exploiting the restrictions of the federal government on the type of stimulus projects. As detailed in Section 2, these restrictions implied that a large proportion of funds had to be used for energy renovations of school buildings. A county's number of schools thus (partially) determined its scope for stimulus projects. [11]

Based on these considerations, we instrument the stimulus funds spent in a county by its number of schools. Assuming that the exclusion restriction is valid and the number of schools is related to employment outcomes only via its effect on stimulus investments, the variation in stimulus investment – the "treatment intensity" – predicted by the number of schools is exogenous. This variation can then be used to identify the causal effect of investments on (un)employment.

Note that both, investments and the number of schools, vary (predominantly) in the cross-sectional dimension of our data. To estimate the dynamics of the impact of investments, we interacted investments with dummy variables for each time period, t, within the set of time periods of interest, Θ. This procedure gives us θ endogenous variables, with $\theta = |\Theta|$ being the number of elements in Θ. We create the required number of instruments by interacting our measure(s) for the number of schools with one time dummy for each $t \in \Theta$.

Formally, the first stage corresponds to a system of θ equations of the following form:

$$\begin{aligned}\text{investments p.c. (2009-2011)}_c \times I(\text{date} = \tilde{t}) = \lambda_{1c} + \sum_t \lambda_{2t} \,\text{schools p.c.} \times I(\text{date} = t) \\ + \lambda_{3ct}\, \text{state}_c \times \text{urbanization index}_c \times I(\text{date} = t) + \mathbf{X_{ct}'}\mathbf{\Lambda} + \nu_{ct}. \quad (2)\end{aligned}$$

Similar to the notation in (1), λ_{1c} denotes the county fixed effect, λ_{2t} is the effect of schools per capita interacted with a time dummy for t on investments interacted with $\tilde{t}$, λ_{3ct} is a time fixed effect conditional on the interaction of the countie's state and urbanization index, and ν_{ct} is an error term.

Clearly, the validity of this instrumental variable strategy depends on whether the exclusion restriction holds and whether schools are a sufficiently strong predictor of stimulus investments. We will argue in the next section that the number of school buildings are historically determined and should hence be independent of current economic conditions; we show that the latter is the case for the pre-treatment periods in Section **??**. In the description of the data on stimulus projects in

[11] Note that this holds even conditional on the number of school students. Because the volume of a building increases faster than its outer surface, which is what matters for energy renovations, the scope for renovations is larger if a given number of students is distributed over a larger number of (smaller) school buildings.

Table 1: Classification of Spending in our Data and Official Aggregate Accounts

	Our database	Official accounts
Total investments in Million Euro	14300.6	15218.59
Total number of projects	40720	42254
Investments in Million Euro by Program Line		
Program 1: Investment in educational infrastructure	10655.81	9875.15
School infrastructure (in particular energy renovation)	6687.50	5955.98
Universities (in particular energy renovation)	1048.73	1832.63
Other lines of program 1	2919.58	2086.53
Program 2: Investments in general infrastructure	3015.93	5811.75
Hospitals	757.06	1167.0
Other lines of program 2	2258.87	4644.75
Projects not classified	628.86	

the next section, we will see that investment in schools were a major fraction of total investments. We will show that the number of schools and investments are strongly correlated in Section 5.

4 Data and Identifying Variation

4.1 Variable of Interest: Investments

We collect data on 40,720 investment projects financed by stimulus funds with a total of 14.3 Billion Euro spent. Our primary source of funding data are the project databases of state governments. States were legally required to report all projects financed by the bill to the federal government in order to document that their usage of funds is in accordance with the restrictions imposed by the bill. We obtained the final versions of these databases directly from the state governments.[12]

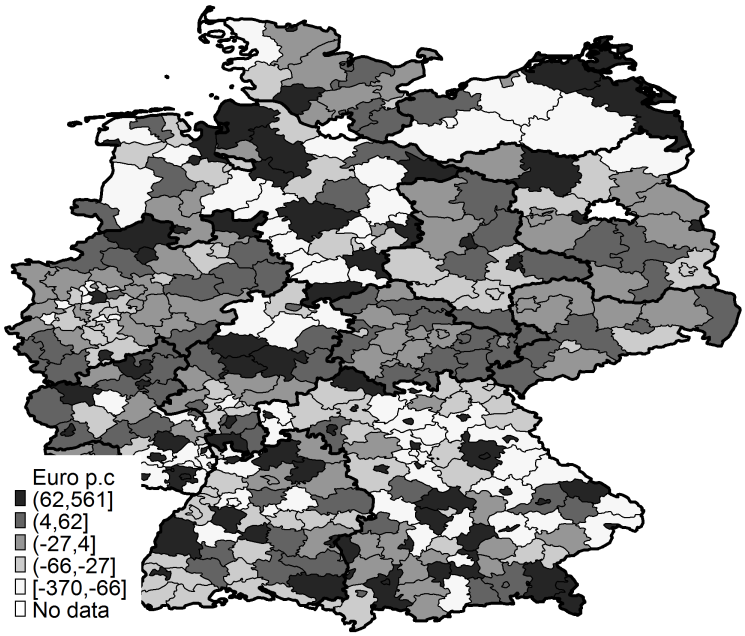
The databases of all states contain, for each project, the total funds spent, the project's location (usually the street address or the municipality), as well as a short project description. Unfortunately, there is no information on the timing of projects or expenses within the years 2009 to 2011, the time span during which all projects needed to be completed and all funds had to be spent.

For one state, Bavaria, the database also contains the program line from which the projects were funded. On the basis of this information, we generate a propensity score algorithm that matches projects to funding lines using a textual analysis of the project description for all remaining states. While we believe that this procedure works reasonably well in general, the matching is obviously not free of errors. We hence use the clean data on total funding for our main analyses and the data disaggregated by funding category for plausibility and robustness checks.

[12]The only exception is North Rhine Westphalia, which did not respond to our request. However, the (high-quality) project database of this state were published on the state government's website.

Figure 1: Geographic Distribution of Stimulus Investments

Note: This map shows the geographic distribution of stimulus investment across counties in Germany. The investment is shown net of an fixed effect for each of the four urbanization categories in each of the 16 states. The colors correspond to the quintiles in investment and darker color indicate higher investment.



We aggregate all investments on the county level based on the location of the projects. As summarized in Table 1, aggregated values in our database correspond reasonably well to the respective aggregated values at the federal level from official accounts.[13] Our database contains 94 percent of total spending (14'300.6 out of 15'218.6 Billion Euro) and 96 percent of all projects (40720 out of 42254 projects).[14] Moreover, our matching generates similar total investment values for investments in the educational infrastructure, where the vast majority of funds went to investments in the school infrastructure. Our matching is less precise for investments in the general infrastructure. This is not surprising, given that our textual matching procedure is based on the data of one state. More projects within one category lead to a closer association of particular textual components to this category, and hence to a more precise matching.

The effect of stimulus investments on employment will be in part identified by the variation in stimulus investments across counties within states. In Figure 1 we plot the geographic distribution of investments per capita across Germany. Counties are shaded according to their quintile of investments per capita purged for the interaction of fixed effects for state and urbanization index. It becomes apparent that the inter-quintile-range of purged investments is almost 130 Euro which is high compared to mean investments of 285 Euro per capita. For the mean county with a working age population of about 127'000, this inter-quintile-range corresponds to sizable differences in total

[13] This data is part of a presentation by Dr. Marc Schattenmann, Secretariat of the Budget Committee of the German Parliamt available here: `http://www.oecd.org/gov/budgeting/47741404.pdf` [accessed on February 11, 2015].

[14] The difference between the roughly 15.2 Billion Euro spent and the 13.3 Billion Euro provided by the bill is due to additional funds provided by the states.

county spending of 16.5 Million Euro even after controlling for state-urbanization fixed effecs. Moreover, there is no noticeable geographical clustering of levels of per capita investment.

4.2 Instrument: Number of Schools

We instrument stimulus investments by the pre-crisis number of schools per capita as measured in the year 2008. Data on the number of schools at the county level, available separately for five major and six minor school types, are provided by the German Federal Statistical Office (Destatis).

Because the size of schools, which we measure by the average number of students per school and which is what matters for the scope for energy renovations, varies significantly across school types, we count the number of schools within two school categories. The first category encompasses all school types that, in principle, offer a degree ("Abitur") which is the pre-condition for pursuing a college education. We call this category "academic high schools"; a school in this category has, on average, 730 students. The second category encompasses all remaining school types, the most important ones of which are elementary schools, with on average 207 students per school, and secondary schools, which offer degrees that are the prerequisite for receiving vocational training and have on average 282 students per school.

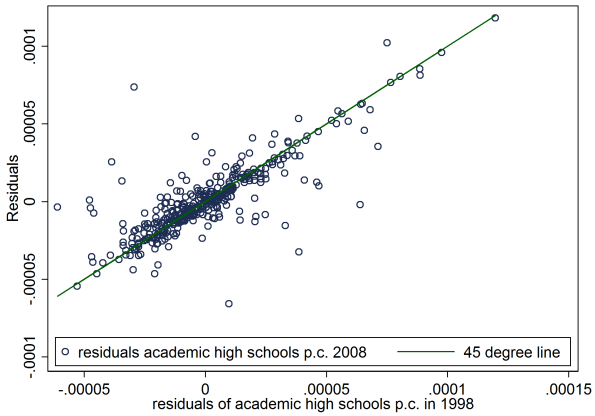
Academic high schools are, on average, thus about three times as large as secondary schools. These substantial differences in size are our main reason for introducing different school categories, as otherwise the requirement of instrument monotonicity would not necessarily be given. This requirement implies that a (counterfactual) county with a larger value of the instrument should receive a more intense treatment, in our case receive a higher amount of stimulus funds. If we were to count the total number of schools only, this requirement would not necessarily be met as a county with a higher share of academic high schools may well receive more funding than the (counterfactual) county with a higher number of schools but also a higher share of elementary and secondary schools. That being said, however, our results are quantitatively robust to using the total number of schools as the only instrument.

For schools to be a valid instrument for stimulus investments, it needs to hold that the number of schools is unrelated to medium-run labor market dynamics in other ways than its effect on investments. This assumption is plausible, because the vast majority of schools has most likely been constructed decades ago and is thus historically predetermined. While there is no data for the age distribution of schools available, it is natural to assume that the age distribution of schools is roughly the same as the age distribution of all public buildings which is available from the German Census of 2011. According to the Census, 84 percent of public buildings were constructed before 1978, and 93 percent before 1993. The reason for these high shares of relatively old public buildings is that most of them were built either after the war or during the high demand times of the baby boomer generations and that there is thus ample supply of buildings at the current times of demographic change.

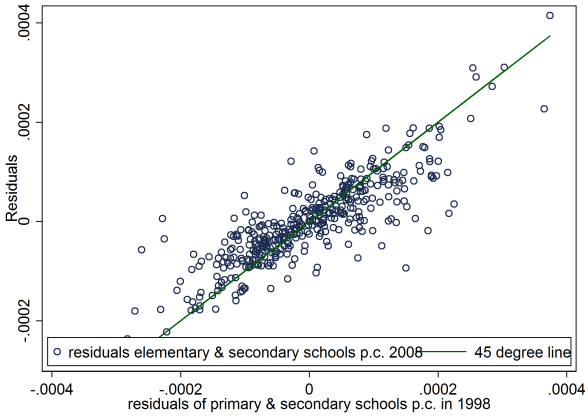
Observations from our data for the decade prior to the stimulus program also show that contemporaneously very few schools are being buit. Here, 91 percent of counties have weakly less schools

Figure 2: Correlation of Schools between 1998 and 2008

Note: The figure shows the correlation between the number of schools in 1998 and the number of schools in 2008. The number of schools shown is net of an fixed effect for each of the four urbanization categories within each of the 14 states.



(a) Instrument 1: Academic High Schools



(b) Instrument 2: Elementary and Secondary Schools

in 2008 than in 1998. Moreover, the number of schools is very stable over time. In Figure 2, we plot, for each county, the number of schools in 2008 against the number of schools in 1998 for both our school categories. The displayed number of schools 1998 and 2008 is thereby relative to the mean number of schools conditional on the countie's state and urbanization index value (and their interaction). It can be seen that the observations in both figures are very close to the 45 degree line, meaning that the number of schools in 1998 and 2008 are approximately the same.

Finally, we like to point out that there is substantial identifying variation in the number of schools per capita across counties. Figure 3 displays the number of schools per 1000 individuals of working age within the German states. Darker markers represent more densely populated counties as defined by their index value of the urbanization index. Again, we demean the number of schools using the interaction of fixed effects for the state and the index values of the urbanization index. Despite controlling for population density, the most obvious determinant of the number of schools per capita, academic high schooltreatments vary by values between 0.5 and 1 school per 1000 individuals, while secondary schools varies by 2 to 4 schools per 1000 individuals.

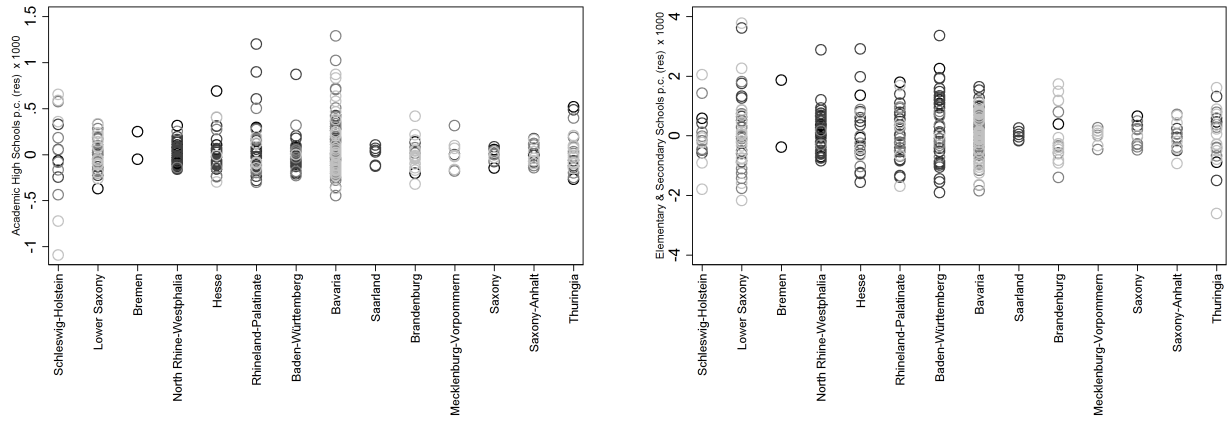
4.3 Dependent Variables: Employment and Unemployment

We get aggregated quarterly county-level employment and unemployment series from the Federal Employment Agency (Bundesagentur für Arbeit). This administrative data is collected to determine unemployment benefits and social security contributions. It is of single-digit precision and contains minimal sampling error, because all German workers with the exception of civil servants and self-employed individuals, who are not included in the dataset, contribute to social security and therefore are included in the employment statistics.

The raw data contains the exact number of unemployed and employed individuals on the last

Figure 3: Distribution of Schools Per Capita within States

Note: The figure shows variation in the number of schools in 2008 across states separately for elementary and secondary schools and academic high schools. Each circle corresponds to a county. The different colors of the circles correspond to the four urbanization categories. Darker colors imply more densly populated counties. The number of schools is shown net of an fixed effect for each of the four urbanization categories in each of the 14 states.



(a) Instrument 1: Academic High Schools

(b) Instrument 2: Elementary and Secondary Schools

day of the quarter. We normalize these numbers by the working age population in 2008 to arrive at employment rates, and de-seasonalize this data using quarter fixed effects for each county.[15] Note that in our empirical model (1), we interact the variable of interest, stimulus investments, with date fixed effects which potentially creates a very large number of coefficients of interest. To preserve the readability of our results, we use yearly data to arrive at all results reported in tables, but provide the estimates quarter by quarter in graphical analyses of our results. Because stimulus projects had to be undertaken between the second quarter in 2009 and the last quarter of 2011, we use data from the first quarter in all estimations involving yearly data.

4.4 Control Variables

Except for the data on employment and unemployment, which we obtain from the Federal Employment Agency, all control variables are obtained from the Federal Statistical Office (Destatis). Furthermore, unless otherwise noted, all variables are normalized by the working age population (all individuals between 15 and 65 years of age) measured in 2008. Because we fix the normalization year, we control for population growth in all but the most parsimonious of our empirical specification by the ratio of the current working age population to the working age population in 2008.

Our first set of control variables are ones that may influence the demand for schools in the cross section as well as labor market dynamics. These variables are the size of the school-age population (population between 6 and 18 years of age), which represents demand for schools and

[15] As pointed out in Footnote 9, normalizing by time-varying working age population would induce the coefficients of the variable of interest to be biased.

may be related to labor market inflows and outflows. The second of these variables is the level of educational attainment of the workforce, which we measure by the share of university graduates among employees and the share of employees with educational training. As the children of more educated parents typically stay longer in school, this may influence demand for schools. The education structure of the workforce is, at the same time, a natural factor determining the regional employment dynamics. Because both of these variables vary mainly across counties and less so across time and because our first stage operates at the cross-sectional dimension, we measure these variables in the year 2008 and interact them with date fixed effects. This controls for (potential) differentials in employment trends due to these variables in a very detailed way.

We proceed similarly for a second set of variables that aims to control for differences in stimulus investments that arise through program lines that do not involve school renovations. The biggest of the remaining program lines are renovations of universities and investments in hospitals. We thus control for the share of university students in the working age population as well as for the number of hospitals per capita. As above, we interact these variables with date fixed effects.

5 First Stage

In this section, we demonstrate that the number of schools and stimulus investments are strongly related. Usually, this would be accomplished by presenting estimates for the first stage of the two stage least squares estimator. In our case, the first stage (2) is a system of equations – one for every date-investment-interaction – making the presentation of the results difficult. However, this system of equations is, in fact, very similar to a model that is estimated via repeated cross-sections, one for each date-investment interaction. To see why this is the case, note that the instruments as well as most control variables are interacted with the date as well such that all parameters of the panel equations are identified from cross-sectional variation. In fact, the only variable without a time-varying coefficient on the right hand side of (2) is population growth.

In this section, we therefore concentrate predominantly on analyzing the cross-sectional variant of (2) and estimate this model for a given year only.[16] To mimic the presence of the county fixed effect in the panel dimension, we moreover demean all variables by their county average.

Table 2 presents the results from estimating the cross-sectional variant of the first stage. It can be seen that the number of schools is a strong predictor of total investments. For the interpretation of the coefficients, note that they estimate the average increase of investments (in 100'000 Euro) due to one additional school; this is because both, schools and investments are normalized by the working age population. One additional academic high school is hence associated with an increase in investments between 1 and 1.8 Million Euro, while one additional elementary or secondary school leads to a gain in stimulus funds between 100'000 and 300'000 Euro. Furthermore, weak instruments should not be a concern, as our F-statistic is above the critical values for two instruments and one

[16] Technically, this means that in (2) all coefficients with $t \neq \tilde{t}$ are set to zero. We estimate this variant of the first stage for $\tilde{t} = 2008$; this choice is completely arbitrary and does not influence the results reported in this section.

Table 2: First Stage

	Stimulus investments p.c. in 100'000 EUR		
	(1)	(2)	(3)
Academic High Schools p.c.	18.46***	14.50***	10.24***
	(3.83)	(3.43)	(3.06)
Primary & Second. Schools p.c.	0.96	2.95***	1.62**
	(0.64)	(0.72)	(0.69)
Population Growth		-0.01	-0.01
		(0.01)	(0.01)
Share School-Age Pop /100		-1.16**	-0.46
		(0.51)	(0.56)
Share Empl. w College /100		1.24***	0.67
		(0.39)	(0.42)
Share Empl. w Vocational Tr. /100		0.50*	0.33
		(0.27)	(0.25)
Share Univ. Students /100			0.78***
			(0.28)
Hospitals p.c.			6.36***
			(2.17)
State × Urbanization Index FE	yes	yes	yes
F-statistic instruments	12.15	17.01	7.48
Adjusted R2	0.33	0.40	0.48
Observations	400	400	400

Notes: The dependent variable "Stimulus invesments p.c. in 100'000 EUR" is the sum of stimulus investments per capita in a particular county over the years 2009 to 2011. "Academic High Schools p.c." are the number of high schools in a county which award the "Abitur", the entry requirement for universities. "Primary and Secondary Schools p.c" is the total number of elementary schools and secondary schools which offer degrees that allow for the pursuit of vocational training. The other control variables are described in the text. All variables are normalized by the working age population at the end of December of 2008 and all the regression are weighted by the working age population. We control for a fixed effects for each combination of state and the value of the urbanization index. Standard errors are clustered on the level of 94 commuting zones. ***, **, * denotes statistical significance on the 1%, 5%, and 10% level.

endogenous variable even though we cluster standard errors on only 94 commuting regions.

The determinants of the fullfillment of project requirements for other funding lines are good predictors of total investments as well. Column (3) shows that 100 additional university students lead to an increase in investments by on average 78'000 Euro. One additional hospital yields additional investments of 640'000 Euro.

We further confirm that the preconditions for receiving stimulus funds from a specific project line explain predominantly the variation of investments within the respective investment program. To this end, we regress investments within the single program lines on the instruments and the most extensive set of controls. The results are reported in Columns two to five of Table 3. The result is as expected. Variation in schools per capita is strongly correlated with the project costs classified as school investments. University funding is correlated with the number of university students and hospital funding is correlated with the number of hospitals. This supports our assumption that stimulus funds were indeed allocated according to the legal rules of the stimulus bill and that our instrument is indeed related to funds allocated to schools. There are some coefficients which are not as expected, however. For example, primary schools are correlated with university spending, and other infrastructure investments (Column 5) are correlated with academic high schools. This might be due to the necessarily imperfect classification procedure based on textual analysis.

In the last two Columns of Table 3, we use the count of school projects and the count of non-school projects as dependent variable. The results are again as expected. The number of school projects is significantly correlated with schools while non-school projects are only correlated with primary and secondary schools

Table 3 furthermore reveals the source of the discrepancy between the impacts academic high schools on the one hand and elementary or secondary schools on the other hand on total investments that is apparent in Table 2. For school investments only, the increase of investments due to one academic high school is less than six times the increase due to an elementary or secondary school. A factor of this magnitude should be expected, as the former schools are about three times as large as the latter type and receive twice the number of projects. The relatively large increase in total investments for academic high schools is thus due to their somewhat stronger correlation with investments that are not classified as school investments by our textual matching procedure.

A final concern regarding the relevance of the instruments could be that investments via the stimulus program crowd out other investments at the local level. To investigate this issue, we return to the panel dimension of our data and use stimulus investments interacted with the date to explain yearly variation in counties' total investment expenditures as reported in their balance sheets.[17] Specifically, we estimate the following model

$$\begin{aligned}\text{total investment expenditures}_{ct} = \zeta_1 + \sum_{t \neq 2008} \zeta_{2t}\, \text{stimulus investments (2009-2011)}_c \times I(\text{date} = t) \\ + \zeta_{3ct}\, \text{state}_c \times \text{urbanization index}_c \times I(\text{date} = t) + \xi_{ct}\end{aligned}$$

[17] Yearly aggregated balance sheet data is provided by the Federal Statistical Office.

Table 3: First Stage: Other Measures for Investments

	Stimulus Investments p.c. in 100'000 EUR					School	Other
	Total	Schools	Univ	Hosp	Other	Projects	Projects
Academic High Schools p.c.	10.24***	5.72***	2.01	1.21*	2.56***	0.95**	0.23
	(3.06)	(1.82)	(1.54)	(0.71)	(0.97)	(0.45)	(0.24)
Primary & Second. Schools p.c.	1.62**	1.04***	0.77**	-0.01	-0.16	0.47***	0.23**
	(0.69)	(0.39)	(0.34)	(0.30)	(0.28)	(0.14)	(0.09)
Population Growth	-0.01	-0.00	-0.00	-0.00	-0.00	-0.00	0.00
	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Share School-Age Pop /100	-0.46	0.50**	-0.54	0.02	-0.38**	-0.04	-0.01
	(0.56)	(0.23)	(0.42)	(0.22)	(0.18)	(0.11)	(0.04)
Share Empl. w College /100	0.67	0.15	0.74***	0.01	-0.20**	0.09	0.01
	(0.42)	(0.14)	(0.27)	(0.16)	(0.09)	(0.06)	(0.02)
Share Empl. w Vocational Tr. /100	0.33	0.03	0.32**	-0.05	0.12*	0.19***	0.06***
	(0.25)	(0.09)	(0.16)	(0.05)	(0.07)	(0.06)	(0.02)
Share Univ. Students /100	0.78***	0.13*	0.64***	-0.04	0.01	0.01	0.01
	(0.28)	(0.07)	(0.22)	(0.05)	(0.04)	(0.02)	(0.01)
Hospitals p.c.	6.36***	2.18	1.64	1.92***	1.14	0.71	-0.27
	(2.17)	(1.35)	(1.15)	(0.71)	(1.33)	(0.45)	(0.28)
Constant	-0.00	-0.00	-0.00	0.00	0.00	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	400	384	384	384	384	400	400
State × Urbanization Index FE	yes	yes	yes	yes	yes	yes	yes
F-statistic instruments	7.48	6.86	4.29	1.50	4.10	7.32	3.62
Adjusted R2	0.48	0.39	0.63	0.15	0.24	0.78	0.76

Notes: The dependent variable "Stimulus invesments p.c. in 100'000 EUR - Total" in the first column is the sum of stimulus investments per capita in Euro in a particular county over the years 2009 to 2011. The dependent variable in column 2 to 5 is the sum of stimulus investments per capita in a particular county classified as school, university, hospital other infrastructure investment. Other infrastructure investment are all investment in the funding line related to infrastructure of the stimulus package which are not hospital investments. The dependent variable in column 6 is the total number of projects classified as school projects. The dependent variable in colum 7 is the total number of projects which are not school projects. "Academic High Schools p.c." are the number of high schools in a county which award the "Abitur", the entry requirement for universities. "Primary and Secondary Schools p.c" is the total number of elementary schools and secondary schools which offer degrees that allow for the pursuit of vocational training. The other control variables are described in the text. Except for the share of employees with a college degree or vocational training, all variables are normalized by the working age population at the end of December of 2008 and all the regression are weighted by the working age population. We control for a fixed effect for each combination of state and the value of the urbanization index. Standard errors are clustered on the level of 94 commuting zones. ***, **, * denotes statistical significance on the 1%, 5%, and 10% level.

Table 4: The Correlation of Stimulus Investments with Total County Investment Expenditures

	Total County Investment Expenditures (Balance Sheet)	
Independent Variable:	All Stimulus Investments	School Investments
Investment × 2006	0.05	0.02
	(0.06)	(0.15)
Investment × 2007	-0.04	-0.05
	(0.04)	(0.08)
Investment × 2009	-0.02	-0.04
	(0.04)	(0.07)
Investment × 2010	0.14**	0.28***
	(0.06)	(0.10)
Investment × 2011	-0.06	-0.03
	(0.07)	(0.13)
Investment × 2012	-0.12*	-0.08
	(0.07)	(0.15)
State × UrbanizationI × YearFE	yes	yes
Observations	2435	2337

Notes: The dependent variable "Total County Investment Expenditures (Balance Sheet)" is the total sum of investment in physical goods recorded in the balance sheet of the county or either of the municipalities within the county for the years 2006 to 2012. In the first column, "Investment × 2006" is the total sum of stimulus investment in a county between 2009 and 2011 interacted with an indicator that equal to 1 for observations from 2006. The other interactions are defined accordingly. In the second column only stimulus investments targeted to schools are used for the interactions. The baseline year is 2008. All variables are normalized by the working age population at the end of December of 2008 and all the regression are weighted by the working age population. We control for time fixed effects at the level of state urbanization-index interactions. Standard errors are clustered on the level of 94 commuting zones. ***, **, * denotes statistical significance on the 1%, 5%, and 10% level.

for one specification with total stimulus investments and one specification with school investments as the independent variable of interest.

The results in Table 4 show that stimulus investments signficantly increase a county's total investment expenditures during the implementation of the stimulus program in 2010 but not in other years. More precisely, one additional Euro spent on any stimulus project increases total investment expenditures by 0.14 Euro, while one Euro spent on a school related project increases expenditures by 0.28 Euro. This results suggest that the bulk of stimulus funds were spent in 2010.

While it is reassuring that stimulus investments show up in counties' balance sheets, we can conclude very little about the degree of crowding out from this data. The reason for this is that only those expenses that have been on the books of the county are counted in the balance sheets. Yet, 75 percent of the costs of stimulus projects were financed by the federal government and only the remaining 25 percent were financed by state and local governments. If none of the federal funds enter the books of the local level governments, the estimates in Table 3 suggest no crowding out of investments. If, on the other hand, federal funds were first granted to local governments (via state governments), these grants should be contained in the investments expenditures of the balance sheets; this could imply substantial crowding out. However, the substantial effects of total stimulus investments on local employment reported in the next section suggest that the former interpretation

is the more plausible one. Furthermore this discussion nicely illustrates how project level data can help to overcome typical problems with measuring the level of government investments on the local level.

6 The Effect of Stimulus Investments on Employment and Unemployment

We investigate the effects of stimulus investments on employment and unemployment by estimating equation (1) for the years 2006 to 2013 both via OLS and, together with the first stage specified in (2), via two stage least squares (2SLS). Because we will estimate the effect of stimulus investments for each date, this would give many coefficients of interest with quarterly data and thus impair the readability of tables. For this reason, we use yearly (un)employment rates measured at the end of the first quarter as the dependent variable in most of our regressions. We chose the first quarter, because it is the last quarter before the begin and the first quarter after the end of the stimulus program. For the graphical representation of the results in Figures 4 and 5 we then use deseasonalized quarterly data.

The effect of stimulus investments on employment Our main analysis is concerned with identifying the efficacy of investments for job creation for each year from 2009 onwards, that is, for all years after the passage of the stimulus bill. To do so, we interact total investments per capita in 100'000 Euro (all stimulus investments during the duration of the program between Q2 2009 and Q4 2011) with year fixed effect for all years from 2009 onwards. The coefficient of investments for a given year is hence to be interpreted as the predicted difference between the (un)employment rate in that year and its pre-stimulus mean for an 100'000 Euro increase in total investments per capita. Since both, (un)employment and investments are normalized by the working age population, an equivalent interpretation of the coefficients is the gain in the number of (un)employed individuals per investments of 100'000 Euro at a particular date relative to average (un)employment before 2009.

Table 5 summarizes the effects of investments on employment. Columns 1 and 4 report the results as estimated by OLS and 2SLS, respectively, for the most parsimonious specification, in which we include only county fixed effects as well as time fixed effects by state and value of the urbanization index as controls. For both empirical models, the largest gain in employment is estimated to occur at the beginning of 2012. No coefficient for the 2SLS model is significant, while for OLS the predicted employment increase per 100'000 Euro spent is statistically significantly different from zero even before the passage of the stimulus bill at the end of the first quarter of 2009. This implausible initial difference in employment levels between the OLS and 2SLS specification, which persists throughout the years, suggests that this simple model is misspecified.

In Columns 2 and 5 we add population growth as well as possible sources of demand for schools (and, hence, investments) to the vector of controls. These variables are the ratio of school age to

Table 5: The Treatment Effect of Investments on Employment

	Employment Rate					
	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Investment × 2009	0.52*	0.05	0.18	-0.72	-0.38	-0.19
	(0.28)	(0.22)	(0.26)	(0.63)	(0.59)	(0.90)
Investment × 2010	1.26***	0.45	0.51	-0.14	0.49	0.55
	(0.42)	(0.33)	(0.36)	(0.81)	(0.83)	(1.19)
Investment × 2011	1.66***	0.89**	0.84*	1.11	2.08**	2.75*
	(0.56)	(0.44)	(0.47)	(1.00)	(0.99)	(1.50)
Investment × 2012	1.85***	1.32**	1.61**	1.29	1.53	2.18
	(0.70)	(0.63)	(0.65)	(1.24)	(1.27)	(1.77)
Investment × 2013	1.67**	0.80	1.21**	0.22	0.51	0.89
	(0.82)	(0.60)	(0.60)	(1.49)	(1.32)	(1.92)
Job Years	4.78	2.66	2.97	2.26	4.10	5.48
Costs per Job Year	20922	37607	33636	44272	24368	18240
State × UrbanizationI × YearFE	yes	yes	yes	yes	yes	yes
Population Growth	no	yes	yes	no	yes	yes
School Age Pop	no	yes	yes	no	yes	yes
Workforce Education	no	yes	yes	no	yes	yes
University Students	no	no	yes	no	no	yes
Hospitals	no	no	yes	no	no	yes
Observations	3200	3200	3200	3200	3200	3200

Notes: The dependent variable is the number of employed in a county for years 2006 to 2012. "Investment × 2009" is the total sum of stimulus investment in a county between 2009 and 2011 interacted with an indicator that equal to 1 for observations from 2009. The other interactions are defined accordingly. The excluded interactions is investment with the first quarter of the years 2006, 2007 and 2008. Job-Years is the total number of jobs created for 100'000 Euro of stimulus investment. Costs per Job-Year is amount of investment required to create one job. In all regressions, we control for county fixed effects as well as time fixed effects for the state urbanization-index interaction. The control set "Population Growth" contains the population in the respective year, "School age population" is the number of people in between 8 and 18 years. The control set "Workforce education" contains the number of employees with university degree and the number of employees which completed vocational training. "University students" are the number of students in public and private universities ("Universitäten") and universities of applied sciences ("Fachhochschulen") in a county. "Hospitals" is the total number of hospitals in a county. The control variables are measured in 2008. All variables are normalized by the working age population at the end of December of 2008 and each regression is weighted by the working age population. Standard errors are clustered on the level of 94 commuting zones. ***, **, * denotes statistical significance on the 1%, 5%, and 10% level.

working age population as well as the educational structure of the workforce as measured by the share of workers with college degree and vocational training, respectively.[18] As the educational attainment of a county's inhabitants is plausibly correlated with employment dynamics, it is not surprising that the coefficient estimates change relative to Column 1 and 4. The 2SLS estimates of this specification are now considerably larger than the respective OLS estimates. For example, at its maximum in 2011, the 2SLS estimates predicts that 2.08 jobs were created relative to the pre-treatment period, while the maximum employment gain is 1.32 for OLS in 2012. Both of these values are statistically different from zero at the 5 percent level, but not significantly different from each other. Moreover, neither of the models predicts an significant impact of investments on employment for the year 2009, as should be expected.

Adding further determinants of investments – the share of university students and the number of hospitals – to the controls in Columns 3 and 6 does not alter the general picture any further.[19] The only notable difference is that adding these determinants of spending to the controls leads to a weaker first stage (as can be seen in Column 3 of Table 2) and, hence, to higher standard errors for the 2SLS estimates.

To put the quantitative impact of the stimulus in perspective and to make our estimates comparable to the literature, we calculate the predicted number of job-years created by investments of 100'000 Euro. This is done by summing the predicted employment differences relative to pre-stimulus employment for the years 2010 to 2012.[20] For the more extensive and more reliable specifications in Columns 2 and 3 (OLS) and 5 and 6 (2SLS) this yields between 2.6 and 3.0 job-years created by 100'000 Euro invested, and almost twice as large estimates between 4.1 and 5.5 job-years for 2SLS. The corresponding costs per job-year are about 35'000 Euro as estimated via OLS and about 20'000 Euro as estimated via 2SLS.

Our estimated cost per job-year are at the lower range of the estimates found in the literature. For the main stimulus package of the United States, the American Recovery and Reinvestment Act, Chodorow-Reich et al. (2012) find costs per job-year of around 26'300 Dollar. Wilson (2012) finds the costs per job-year between 45'000 Dollar and 125'000 Dollar, and Conley and Dupor (2013) arrive at rather high costs of 202'000 Dollar. For normal times, Suarez Serrato and Wingender (2014) find costs per job-years of 30'000 Dollars for various sorts of state transfers to the local level.

To look at the dynamic pattern of the impact of investments on employment we estimate the most extensive 2SLS specification from Column 3 of Table 5 for quarterly data. In Figure 4a) we plot the resulting coefficients as well as their 95 percent confidence intervals for each date between Q1 2009 and Q4 2013; the vertical lines represent the begin and the end date of the stimulus program.

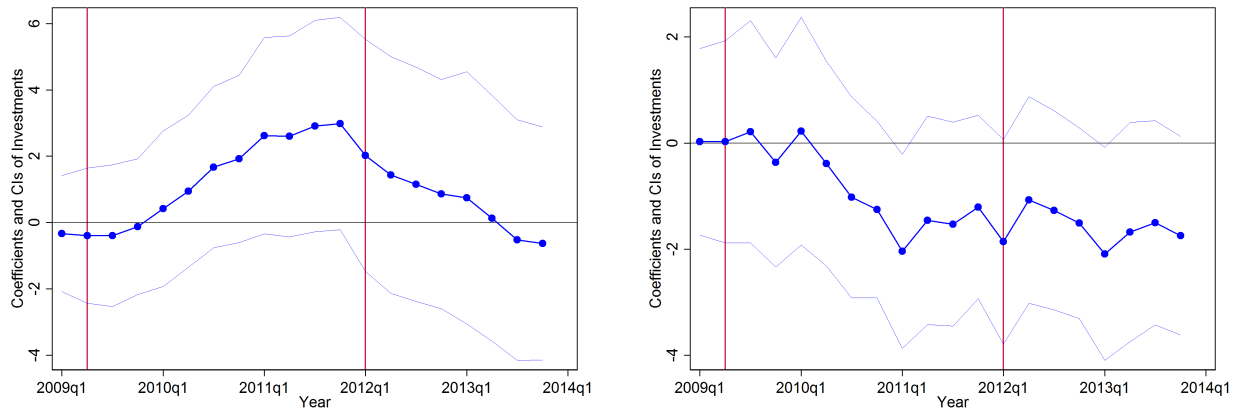
[18] Recall from Section 4.4 that we measure these variables in 2008 and interact each of them with time fixed effects to properly control for their cross-sectional variation, which is the identifying variation for the first stage. Also, this controls very flexibly for the employment dynamics that may be correlated with these variables.

[19] As the variables controlling for demand for schools, the share of university students and the number of hospitals are measured in 2008 and interacted with time-fixed effects.

[20] This is a good approximation if employment gains and losses are close to linear. We disregard 2009 because the stimulus program has not been implemented yet at that time, and 2013 because the evidence for long-run impacts of investments is, at best, weak.

Figure 4: Treatment Effects of Stimulus Investments on (Un)Employment

Note: The figure plots the regression coefficients and 95% confidence intervals obtained estimating equation(1) via 2SLS for the specifications reported in column (6) of Table 5 for employment and Table 6 for unemployment for quarterly data of (un)employment. The point estimates show, for every date, the impact of an additional 100'000 Euro investment on the number of employed or unemployed at that date relative to mean (un)employment between 2006 and 2008. The vertical lines indicate the begin and the end of the stimulus program.



(a) Employment

(b) Unemployment

Already in Table 5, but even more so in Figure 4a) it can be seen that there is no detectable impact on investments on either employment or unemployment before the enactment of the stimulus bill as well as three quarters into the program. In 2010, however, higher total investments between 2009 and 2011 cause employment to rise relative to the mean of the pre-stimulus period. The employment differences peak in 2011 only to drop beginning in 2012, with a particularly sharp drop directly at the end of the program. This pattern is consistent with the evidence presented in Table 4 which suggests that the bulk of funds was spent in 2010.

The effect of stimulus investments on unemployment We now turn to the effects of stimulus investments on unemployment, which are reported in Table 6 and Figure 4b). Given our results from employment, we expect to see negative coefficients, which correspond to reductions in unemployment, for the investment-year-interactions past 2009.

While this is generally the case, the most striking difference relative to the employment effects is that neither of the OLS estimates is statistically or economically different from zero. In contrast, for the more extensive specifications in Columns 5 and 6, the 2SLS estimates suggest a sizable impact of investments on unemployment, with peak differences of 1.5 and 2 unemployed individuals less for each 100'000 Euro spent. This result suggests that counties were selected to receive stimulus funds on the basis of expectations regarding the unemployment dynamics, i.e. the distribution of funds was indeed endogenous. This makes sense, because the level of unemployment is a first order concern to German politicians, and because bolstering the rise in unemployment was the purpose of the stimulus program in the first place. Moreover, comparing the respective 2SLS estimates,

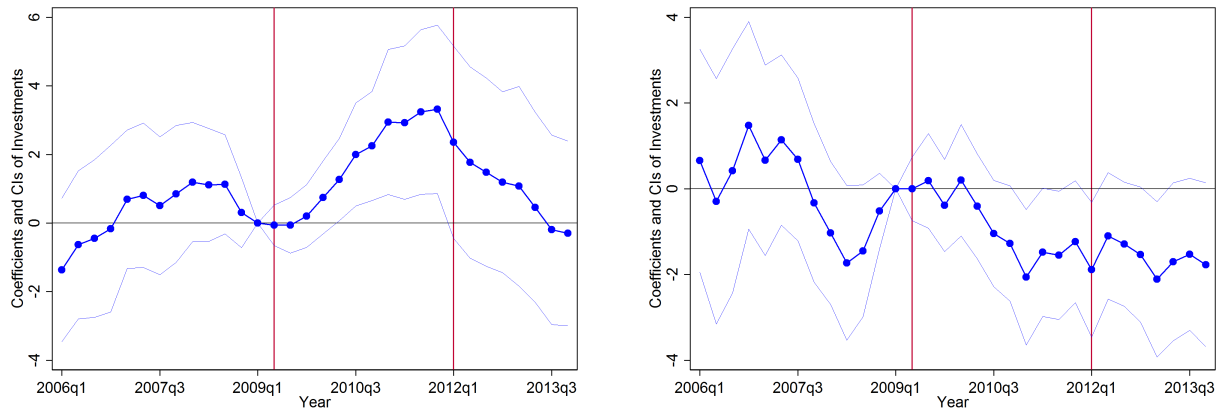
Table 6: The Treatment Effect of Investments on Unemployment

	Unemployment Rate					
	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Investment × 2009	0.06	0.12	0.07	-0.00	0.17	-0.10
	(0.24)	(0.23)	(0.25)	(0.64)	(0.56)	(0.94)
Investment × 2010	-0.27	-0.16	-0.20	0.18	0.09	0.10
	(0.27)	(0.27)	(0.31)	(0.83)	(0.71)	(1.11)
Investment × 2011	-0.05	-0.28	-0.16	-0.70	-1.48**	-2.16**
	(0.27)	(0.27)	(0.28)	(0.76)	(0.62)	(0.96)
Investment × 2012	-0.09	-0.26	0.01	-0.92	-1.50**	-1.98**
	(0.32)	(0.33)	(0.34)	(0.83)	(0.64)	(1.01)
Investment × 2013	0.07	-0.11	-0.01	-0.64	-1.38**	-2.21**
	(0.34)	(0.36)	(0.38)	(0.86)	(0.65)	(1.03)
Job Years	0.40	0.71	0.34	1.44	2.90	4.03
Costs per Job Year	247079	141402	293658	69409	34497	24801
State × UrbanizationI × YearFE	yes	yes	yes	yes	yes	yes
Population Growth	no	yes	yes	no	yes	yes
School Age Pop	no	yes	yes	no	yes	yes
Workforce Education	no	yes	yes	no	yes	yes
University Students	no	no	yes	no	no	yes
Hospitals	no	no	yes	no	no	yes
Observations	3200	3200	3200	3200	3200	3200

Notes: The dependent variable is the number of unemployed in a county for the years 2006 to 2012. "Investment × 2009" is the total sum of stimulus investment in a county between 2009 and 2011 interacted with an indicator that equal to 1 for observations from 2009. The other interactions are defined accordingly. The excluded interactions is investment with the first quarter of the years 2006, 2007 and 2008. Job-Years is the total number of jobs created for 100'000 Euro of stimulus investment. Costs per Job-Year is amount of investment required to create one job. In all regressions, we control for county fixed effects as well as time fixed effects for the state urbanization-index interaction. The control set "Population Growth" contains the population in the respective year, "School age population" is the number of people in between 8 and 18 years. The control set "Workforce education" contains the number of employees with university degree and the number of employees which completed vocational training. "University students" are the number of students in public and private universities ("Universitäten") and universities of applied sciences ("Fachhochschulen") in a county. "Hospitals" is the total number of hospitals in a county. The control variables are measured in 2008. All variables are normalized by the working age population at the end of December of 2008 and each regression is weighted by the working age population. Standard errors are clustered on the level of 94 commuting zones. ***, **, * denotes statistical significance on the 1%, 5%, and 10% level.

Figure 5: Placebo Effects of Stimulus Investments on (Un)Employment

Note: The figure plots the regression coefficients and 95% confidence intervals obtained estimating equation(1) via 2SLS for the specifications reported in column (6) of Table 7 for employment and Table 8 for unemployment for quarterly data of (un)employment. The point estimates show, for every date, the impact of an additional 100'000 Euro investment on the number of employed or unemployed at that date relative to the first quarter of 2009. The vertical lines indicate the begin and the end of the stimulus program.



(a) Employment

(b) Unemployment

the costs per job-year are somewhat higher for unemployment than the corresponding figures for employment and lie within the range of 25'000 to 35'000 Euro for the model specifications in Columns 5 and 6 of Table 6.

The dynamic pattern of the impact of investments on unemployment displayed in Figure 4b) is similar to the one for employment in the sense that there is no discernible effect of the stimulus program before the third quarter of 2010. However, in contrast to the results for employment, the reductions in unemployment that are achieved through the program persist well into 2013.

Placebo-tests of the common trend assumption for the pre-stimulus period For the 2SLS estimates to represent causal effects of stimulus investments, it needs to hold that counties' counterfactual employment trends are independent of the instrument. We cannot test this assumption for the stimulus-period, but proceed to test it for the years prior to the passage of the stimulus bill. To this end, we estimate the effect of (placebo) investments on (un)employment past and prior to the first quarter of 2009. All coefficients are to be interpreted as the differences of (un)employment relative to mean (un)employment at that date.

Table 7 reports the results for employment and Table 8 for unemployment. In Figure 5, we plot the corresponding coefficients and standard errors of investments interacted by date for the 2SLS specification in Columns 6 of Table 7 (employment) and 8 (unemployment) estimated for quarterly data.

For employment, the pre-stimulus "impact" of instrumented investments is reasonably flat throughout, showing that the variation in schools that explains variations in investments is unrelated to employment dynamics prior to the stimulus program. For unemployment, the coefficients of

Table 7: Placebo Estimates of the Effect of Investments on Employment

	Employment Rate					
	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Investment × 2006	-0.66	-0.40	-0.42	0.51	-0.81	-1.34
	(0.40)	(0.31)	(0.35)	(0.76)	(0.75)	(1.07)
Investment × 2007	-0.47	0.12	-0.03	0.80	0.82	0.71
	(0.30)	(0.23)	(0.27)	(0.70)	(0.64)	(1.03)
Investment × 2008	-0.42**	0.14	-0.08	0.85	1.14**	1.21
	(0.19)	(0.19)	(0.21)	(0.56)	(0.57)	(0.89)
Investment × 2010	0.74***	0.40**	0.34*	0.58*	0.87**	0.73
	(0.20)	(0.18)	(0.20)	(0.34)	(0.38)	(0.54)
Investment × 2011	1.15***	0.84***	0.67**	1.83***	2.45***	2.93***
	(0.36)	(0.31)	(0.34)	(0.62)	(0.68)	(1.08)
Investment × 2012	1.34***	1.27**	1.43***	2.01**	1.91*	2.37*
	(0.51)	(0.52)	(0.55)	(0.85)	(0.99)	(1.43)
Investment × 2013	1.15*	0.75	1.03**	0.94	0.88	1.08
	(0.61)	(0.47)	(0.46)	(1.02)	(0.97)	(1.48)
Job Years	3.23	2.52	2.44	4.41	5.23	6.03
Costs per Job Year	30976	39730	41055	22653	19106	16574
State × UrbanizationI × YearFE	yes	yes	yes	yes	yes	yes
Population Growth	no	yes	yes	no	yes	yes
School Age Pop	no	yes	yes	no	yes	yes
Workforce Education	no	yes	yes	no	yes	yes
University Students	no	no	yes	no	no	yes
Hospitals	no	no	yes	no	no	yes
Observations	3200	3200	3200	3200	3200	3200

Notes: The dependent variable is the number of employed in a county for years 2006 to 2012. "Investment × 2006" is the total sum of stimulus investment in a county between 2006 and 2011 interacted with an indicator that equal to 1 for observations from 2006. The other interactions are defined accordingly. The excluded interactions is investment with the first quarter of 2009. Job-Years is the total number of jobs created for 100'000 Euro of stimulus investment. Costs per Job-Year is amount of investment required to create one job. In all regressions, we control for county fixed effects as well as time fixed effects for the state urbanization-index interaction. The control set "Population Growth" contains the population in the respective year, "School age population" is the number of people in between 8 and 18 years. The control set "Workforce education" contains the number of employees with university degree and the number of employees which completed vocational training. "University students" are the number of students in public and private universities ("Universitäten") and universities of applied sciences ("Fachhochschulen") in a county. "Hospitals" is the total number of hospitals in a county. The control variables are measured in 2008. All variables are normalized by the working age population at the end of December of 2008 and each regression is weighted by the working age population. Standard errors are clustered on the level of 94 commuting zones. ***, **, * denotes statistical significance on the 1%, 5%, and 10% level.

Table 8: Placebo Estimates of the Effect of Investments on Unemployment

	Unemployment Rate					
	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Investment × 2006	-0.29	0.05	0.01	0.06	0.51	0.65
	(0.35)	(0.31)	(0.33)	(0.95)	(0.78)	(1.33)
Investment × 2007	-0.02	-0.18	-0.16	0.44	0.02	0.66
	(0.26)	(0.27)	(0.31)	(0.71)	(0.67)	(1.13)
Investment × 2008	0.13	-0.24	-0.05	-0.49	-1.05**	-1.03
	(0.20)	(0.21)	(0.23)	(0.55)	(0.53)	(0.85)
Investment × 2010	-0.32**	-0.28*	-0.26	0.19	-0.08	0.21
	(0.15)	(0.15)	(0.22)	(0.43)	(0.44)	(0.67)
Investment × 2011	-0.11	-0.41**	-0.22	-0.70	-1.65***	-2.05**
	(0.20)	(0.19)	(0.23)	(0.55)	(0.51)	(0.81)
Investment × 2012	-0.15	-0.39*	-0.05	-0.92	-1.67***	-1.88**
	(0.20)	(0.22)	(0.25)	(0.57)	(0.52)	(0.80)
Investment × 2013	0.01	-0.24	-0.08	-0.64	-1.55***	-2.11**
	(0.22)	(0.27)	(0.29)	(0.63)	(0.55)	(0.92)
Job Years	0.57	1.08	0.54	1.43	3.40	3.73
Costs per Job Year	174389	92895	186173	69713	29378	26802
State × UrbanizationI × YearFE	yes	yes	yes	yes	yes	yes
Population Growth	no	yes	yes	no	yes	yes
School Age Pop	no	yes	yes	no	yes	yes
Workforce Education	no	yes	yes	no	yes	yes
University Students	no	no	yes	no	no	yes
Hospitals	no	no	yes	no	no	yes
Observations	3200	3200	3200	3200	3200	3200

Notes: The dependent variable is the number of unemployed in a county for the years 2006 to 2012. "Investment × 2006" is the total sum of stimulus investment in a county between 2006 and 2011 interacted with an indicator that equal to 1 for observations from 2006. The other interactions are defined accordingly. The excluded interactions is investment with the first quarter of 2009. Job-Years is the total number of jobs created for 100'000 Euro of stimulus investment. Costs per Job-Year is amount of investment required to create one job. In all regressions, we control for county fixed effects as well as time fixed effects for the state urbanization-index interaction. The control set "Population Growth" contains the population in the respective year, "School age population" is the number of people in between 8 and 18 years. The control set "Workforce education" contains the number of employees with university degree and the number of employees which completed vocational training. "University students" are the number of students in public and private universities ("Universitäten") and universities of applied sciences ("Fachhochschulen") in a county. "Hospitals" is the total number of hospitals in a county. The control variables are measured in 2008. All variables are normalized by the working age population at the end of December of 2008 and each regression is weighted by the working age population. Standard errors are clustered on the level of 94 commuting zones. ***, **, * denotes statistical significance on the 1%, 5%, and 10% level.

placebo-investments fluctuate more strongly. In particular, counties with more schools seemed to exhibit a slight delay of two quarters in the increase of unemployment at the height of the crisis in 2008. They return to similar unemployment levels for Q4 2008 to Q1 2010, however, before the effect of stimulus investments on unemployment becomes visible. Moreover, it is reassuring that the estimated effects of investment on employment and unemployment reported in Tables 7 and 8 change only slightly in response to the change in the baseline period.

Overall, these results suggest that pre-treatment employment trends are not correlated with investments, both for the more extensive OLS and 2SLS specifications. This means that selection into "treatment" occurred, if at all, on the basis of expected employment trends. As we control for counties' medium-run employment levels via county fixed effects, this is not surprising. However, given that we find striking differences between the OLS and 2SLS estimates for the effect of investments on unemployment, it seems difficult to argue that the fixed effects take care of all endogeneity problems.

7 Conclusion

This study analyzes the effect of the infrastructure program contained in the German stimulus program of 2009 on employment and unemployment. As stimulus spending is potentially endogenous to local economic conditions we use the number of schools as instrumental variables. Schools are a relevant instrument, because 65 percent of total spending was earmarked by law for investment of local educational infrastructure with a particular emphasis on energy renovation. We find a significant impact of the stimulus on investment: 100'000 Euro in stimulus investment resulted in between 4.1 and 5.5 jobs over the three years the program was active.

Our study evaluates the effect of a particularly designed investment program in Germany in 2009. The stimulus program was focused on the improvement of infrastructure and implemented in an unprecedented economic downturn. This particularity is both a weakness and a strength: On the one hand, it is unlikely that our results are applicable to differently designed stimulus programs in normal economic circumstances.

On the other hand, we are the first to provide quantitative evidence that an investment program in physical infrastructure can help to cushion the impact of a recession in a European-type economy. Therefore our results might be informative for policy makers in continental Europe how to implement fiscal stimulus if the need should again arise.

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