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Do Public Investments Increase Employment in a Recession? Evidence from Germany

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Do Public Investments Increase Employment in a Recession?

Evidence from Germany

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In 2009, Germany invested 15.4 Billion Euro in infrastructure to avert the looming recession. In this study, we evaluate whether the German stimulus program was successful in limiting the impact of the crisis on the job market. We exploit exogenous cross-sectional variation to identify the casual effect of stimulus investment on the change in unemployment on the county level. By law, 65 percent of the stimulus funds were earmarked for the renovation of existing school buildings. Thus a large part of all investment was predetermined by the number and size of schools in a county which are plausibly exogenous to local economic conditions. Thus a large part of all investment was predetermined by the number and size of schools in a county which are plausibly exogenous to local economic conditions during the crisis. This opens up the possibility to use the number of schools and students as instrumental variables for stimulus investment. Our IV-estimates indicate that the stimulus program was successful in reducing the number of unemployed: On average, one job was created for every 44,000 Euro spent. This result is in line with estimates for the effectiveness of the US stimulus program. We validate our IV strategy with extensive falsification exercises and various robustness checks.

Keywords: Fiscal policy, jobs multiplier, stimulus spending, instrumental variables.

JEL Classification: E24, E62, H50, H72

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1 Introduction

Can the government create jobs through fiscal investment programs in an economic downturn? In the wake of the Great Recession, this question is – once again – the subject of heated policy debates. Despite the widespread discussions whether fiscal stimulus works, only a few studies investigate whether the fiscal spending programs that have been enacted by all major economies after the faltering of the global economy in 2008 and 2009 have actually improved economic conditions. Specifically, at the time of writing only the United States’ stimulus program – the American Recovery and Reinvestment Act (ARRA) – has been empirically evaluated (Chodorow-Reich et al., 2012b; Conley and Dupor, 2012; Wilson, 2012; Feyrer and Sacerdote, 2011).

It may be a difficult to derive policy recommendations for other countries based on this US-centered evidence because the effectiveness of fiscal stimulus potentially depends on the state of the economy, the institutional setting and the design of the stimulus program. For example, if an inflexible labor market or a generous welfare system reduce the impact of government investments on job creation, evidence for the United States is only informative for states with similar labor market and social welfare institutions.¹ Similarly, empirical evidence suggests that it matters for the size of the fiscal multiplier, whether the stimulus is delivered through tax rebates, direct investment in physical infrastructure such as road construction, or increasing military spending. Due to this multitude of relevant factors it is necessary to evaluate the effect of stimulus spending programs under various institutional settings and in various countries in order to arrive at firm conclusions regarding the desirability of government interventions in recessions.

This paper adds to the understanding of the effectiveness of fiscal policy by studying the impact of a government investment program in Germany in 2009. Compared to the United States, Germany has a substantially larger welfare state – spending on social programs as share of GDP per capita is 35 percent larger in Germany than the United States (OECD, 2012)– and, arguably, also a less flexible labor market. Furthermore, the institutional characteristics of Germany are comparable to that of the “prototype” European economy in that the size of government and the degree of labor market regulation is at the upper end of the respective distribution among OECD countries (Alesina et al., 2001). Hence, our study sheds light on the question whether or not stimulus investments are effective under these institutional circumstances.

To identify the effect of stimulus investments we use plausibly exogenous variation of

¹Theoretical arguments also suggest that the fiscal multiplier might depend on trade openness, the indebtedness of the public and the private sector and the severness of the recession (Corsetti et al., 2012).

stimulus spending on the county (“Kreis”) level to explain unemployment changes. We find that the investment program was indeed successful: an additional investment of 100,000 Euro resulted, on average, in 1.35 fewer unemployed during the first two years of the stimulus program. This corresponds to costs of 44,000 Euro (or 55,000 Dollar) for one unemployed less per year. Despite the fundamental institutional differences, the magnitude of this effect is in a similar range as the estimated employment effects of the ARRA for the United States. Chodorow-Reich et al. (2012b) find relatively low costs per job-year of 26,000 Dollar, while Wilson (2012) arrives at 45,000 Dollar matching our results. The IV estimates of Feyrer and Sacerdote (2011) indicate costs per job-year between 30,000 Dollar and 94,000 Dollar.²³ Hence, in comparison to US estimates our results suggest that fiscal spending during the Great Recession has had similar effects on employment across different institutional settings.

By using cross-sectional variation in stimulus investment, we make use of regional focus and the specific institutional details of the stimulus bill. The bill (called “Zukunftsinvestitionsgesetz”) was a key part of the German stimulus package (“Konjunkturpaket II”) and was enacted at the height of the Great Recession in March 2009. It provided 13.3 Billion Euro – around 16.8 Billion Dollar or 0.54 percent of German GDP in 2008 – for infrastructure investments and mandated that the majority is spent on regional infrastructure projects. Specifically, 65 percent of funds were earmarked for investments in the educational infrastructure – with a focus on energy renovations of existing buildings – and the remaining 35 percent had to be spent on investments in the general infrastructure like renovations of public buildings, broadband, or hospitals. Accompanying regulation further highlights the regional focus of the bill: for the time the investment program was active, rules and regulations for public procurement were softened what enabled local authorities to award contracts to local firms without an extended bidding process.⁴ In addition, the goal of this regulation was to ensure a swift implementation of the stimulus bill, which prescribed that the majority of funds should be spent in 2009 and 2010. All

²³Wilson provides three different estimates depending on the measurement of stimulus: 45,000 Dollar using actually outlays, 98,000 Dollar using obligations, and 128,000 Dollar using announcements to measure stimulus funding. Our stimulus measure is actual spending such that our results ought to be compared to the measure of actual outlays in Wilson (2012). The most comparable results in Feyrer and Sacerdote are the IV estimates on the county (30,000 Dollar) and the state level (94,000 Dollar).

³Conley and Dapor (2012) also calculate the total number of jobs created or saved by ARRA. Due to their special methodology accounting for state expenditure shortfalls, it seems not possible to calculate costs per job-year from their published results.

⁴Contracts up to 1 Million Euro were awarded through a tender among preselected group of companies instead through a public tender. This resulted in restricting competition to regional companies (Bundesrechnungshof, 2012). See “Verwaltungsvereinbarung zur Durchführung des Gesetzes zur Umsetzung von Zukunftsinvestitionen der Kommunen und Länder” for more details.

investment projects funded by the bill had to be completed by the end of 2011, less than three years after the start of the program.

While the benefit of exploiting variation in stimulus spending at the county level are a large sample size and correspondingly precise estimates, a problem of this research design is the potential endogeneity of stimulus to economic conditions at the regional level. For example, those counties that were hit harder by the recession may receive more funding; alternatively, more able politicians may be able to channel a larger share of stimulus funds into their (comparably better-off) counties. We address this potential endogeneity problem with an instrumental variable strategy. This strategy uses the fact that a large part of the investment program was targeted at energy renovations of the regional educational infrastructure. Hence, the number of existing buildings – in particular the number of schools – determined the number of projects a county can apply for, independent of economic conditions. We thus use the pre-crisis number of schools as an instrument for spending. In addition, some states distributed their funds to the regional level based on the pre-crisis number of students. Hence, we also use this measure as an instrument for spending.

Both instruments turn out to be a strong predictor of stimulus spending at the county level and all our results reported above are estimated with spending instrumented by schools and students. In order to be a valid instrument, however, the number of pre-crisis schools and students needs to be related to the economic conditions of a county only via its effects on the amount of stimulus funds a county receives. Hence, instrumented spending should be uncorrelated with the economic development (in our case changes in unemployment) before the stimulus was active. We demonstrate that this is the case for pre-crisis years in extensive falsification tests. Furthermore, our empirical results do not depend on the specific instrument – schools or students – used and are robust with respect to comprehensive robustness checks.

This paper is part of a recent literature that uses cross-sectional variation to provide evidence regarding the effectiveness of fiscal policy. Most closely related is the work by Chodorow-Reich et al. (2012b), Wilson (2012), Conley and Dupor (2012), and Feyrer and Sacerdote (2011), which evaluates the US stimulus package during the Great Recession. We have discussed the results of these papers above already. With respect to these papers, our work differs slightly in terms of the empirical approach used. While the literature on the US stimulus program focuses on the effect of transfers from the federal to the state level on state employment, our work looks at the effect of one specific investment program – which we can trace down to the funded projects – on changes in unemployment at the county level.

Additionally, there are a number of recent studies which estimate the fiscal costs to generate a job-year from cross-sectional data without a focus on stimulus programs. All of these studies focus on the United States and arrive at costs per job-year between 25,000 and 50,000 Dollar. Suarez Serrato and Wingender (2011) instrument government spending at the county level with measurement error in population estimates – they measure the measurement error at times of population updates through the decennial census – and estimate a 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.

Two further studies estimate fiscal multipliers rather than costs per job from regional variation in fiscal spending. Nakamura and Steinsson (2011) instrument for fiscal spending with military build-ups and draw-downs at the local level and find a fiscal multiplier of 1.5. Acconcia et al. (2011) estimate the fiscal multiplier for Italy and thus provide, to our knowledge, the only study using cross-sectional variation to identify the effect of fiscal policy outside of the United States. Similar to our setting, they focus on infrastructure investments. They instrument for local spending by using an institutional feature in Italy namely that all public works must be interrupted by law when there is evidence of mafia infiltration of the local authorities. Similar to our findings, their results suggest that fiscal policy is similarly effective in an European country as in the United States by estimating a fiscal multiplier between 1.2 and 1.8.⁵

The remainder of the paper is organized as follows. Section 2 describes the details of the German Stimulus Program. Section 3 describes the data and our empirical strategy. Section 4 presents our main results, a falsification test of our instrumental variable. Section 5 provides robustness checks and Section 6 concludes.

2 Details of the German Stimulus Program

In the worldwide panic following the bankruptcy of Lehman Brothers in September 2008, the German government signed two successive stimulus bills (Konjunkturpakt I & II) at an estimated cost of 100.2 Billion Euro or 3.4 percent of the GDP over two years. The total stimulus was smaller than in the US (5.9 percent of GDP), but large compared to other advanced economies such as the UK (1.5 percent of GDP), France (0.7 percent)

⁵There is, of course, an extensive literature which provides estimates of the fiscal multiplier from other sources of variation. This literature is large such that it is not feasible to give an overview here. See Ramey (2011) for a comprehensive overview.

or Canada (2.8 percent) (Prasad and Sorkin, 2009). In Germany, the majority of all spending, 78.7 Billion went to individuals or businesses in the form of tax reduction, extra credit, and transfer payments. The rest of around 21.5 Billion Euro was earmarked for investment. With the exception of 2.7 Billion Euro, all provisions for investment were part of the second stimulus bill, which is the focus of our study.

In the second stimulus package 10.0 Billion Euro of federal money was transferred to the 16 state governments according to a formula based on the states' population size and tax revenue of 2007.⁶ The stimulus bill mandated, that the states had to transfer at least 70 percent to counties and municipalities for infrastructure investments, while they could use the remainder for investment in state-wide projects such as renovation of university buildings. Consequently, the selection of projects and the execution was decentralized to the lowest level of the individual communities.

Each state had large discretion how to allocate funds to municipalities: Some states such as Brandenburg and Lower-Saxony decided to use a formula allocation based on the number of students, population or area size to distribute funds to communities. Other states, such as Bavaria, held open competitions among municipalities to determine which projects should receive funding.

There were several strings attached for the municipalities: First, federal funds were matching grants; 25 percent of investment costs had to be provided at the state, county or municipality level.⁷ This increased the total invested amount to 13.4 Billion Euro. Second, to reduce the fungibility of funds, the federal government restricted the composition of investment projects: 65 percent of all funds was earmarked for investment in educational infrastructure with a particular emphasis on the energy renovation of existing building. The remainder could be spent on the general infrastructure, such as hospitals, urban renewal, and broadband internet. These quotas were enforced by the state government, pending review from the federal government.⁸ Another safeguard against the diversion of funds was that only new and additional projects could get funding. In practice this implied that projects for which already funds have been reserved in any budget or in any public investment scheme could not receive funding.

The stimulus was designed to have a swift impact on the economy. By law at least half of the funds had to be spent by the end of 2009, and the remainder had to be spent by

⁶The employed distribution formula, the so-called "Königsteiner Schlüssel", is regularly used within Germany to distribute funds between states.

⁷A special provision in the law ensured, that also communities in dire financial circumstances also had access to stimulus funding.

⁸The official accounting shows that 64.8 percent of realized investment across states was in educational infrastructure.

December 2011. The bill also changed the regulation of public procurement to speed up the decision process: Normally, projects above 100'000 Euro have to be awarded to the lowest bidder in an open EU-wide competition, while projects with a lower investment sum can be awarded in a auction among selected bidders. With enactment of the stimulus package this threshold was multiplied by 10, because open auctions are slower to implement than limited participation auctions. As unintended consequence this change in regulation ensured that the stimulus was regionally spend: For projects awarded in a limited participation auction the average distance between company and construction site is only half the distance compared to an open competition (Bundesrechnungshof, 2012).

The first stimulus package was signed into law on the December 5th, 2008 and the second stimulus package on March 5th, 2009. The German Cabinet first discussed an emergency stimulus package on October 5th, 2008, just two days after the enactment of the TARP program. The parliament passed the measures on December 4th, 2008. Already in October 2009 a second stimulus package was considered, on which the Cabinet agreed on January, 12th 2009 and which was passed in parliament on March 5. The law took effect retroactively on January 1st 2009. Consequently our analysis should begin in March 2009 and end in December 2011, the last month stimulus could be spent.

3 Empirical Strategy & Data

3.1 Empirical Specification

We begin with a simple equation which relates government investment to the change in unemployment on the county-level. Our functional specification is the same as in the evaluations of the ARRA stimulus program in the U.S. and therefore our results are directly comparable (Chodorow-Reich, Feiveson, Liscow, and Gui, 2012a; Wilson, 2012; Feyrer and Sacerdote, 2011). The change in the unemployment rate in a county is a function of a common trend α , direct government investment per capita, *Investment*, controls and a municipality specific shock ε :

$$\frac{U_{Q1/2011}^c - U_{Q1/2009}^c}{N_{2008}^c} = \alpha + \beta_I \cdot \frac{Investment}{N_{2008}^c} + \gamma \cdot Controls_{2008} + \varepsilon, \quad (1)$$

where U_t^c is the level of unemployment in county c at time t and N is the working age population in county c .

If *Investment* were exogenous, we could estimate Equation (1) consistently with OLS. However, given that politicians and government officials had some discretion where to

allocate funds, public investment is quite certainly endogenous to unobserved factors which are correlated with the change in unemployment. For example, if on the one hand, regions which were harder hit by the recession received more stimulus funding, then the estimate for β_I is biased towards zero (given $\beta_I < 0$). On the other hand, more investment was potentially given to economically stronger regions because they are politically connected, then our estimates are biased away from zero.

In order to mitigate this endogeneity bias we focus on investment related to schools by using the number of pre-recession schools and pre-recession students as instrumental variables. The stimulus bill mandated that 65 percent of the stimulus had to be invested in educational infrastructure with a particular emphasis on energy renovation. In practice, municipalities had a strong incentive to start such projects as renovation projects were more likely to be approved. But a county can renovate only as many schools as exist. Therefore spending should be higher in counties with more school buildings compared to counties with fewer schools.

The number of students should be an important determinant for regional investment, because states used formulary allocation based on this number to distribute investment budgets to municipalities. Within these budgets, municipalities could then start projects which conformed with the general rules of the stimulus law. So they were still bound to use the funds for educational or general infrastructure and each project had to be additional investment. The main idea was, that such a procedure would speed up the administrative process. For example North-Rhine Westphalia, Germany's most populous state, allocated 1.384 out of 1.848 Billion Euro reserved for investment in schools according to the number school students.

Our two instruments are exogenous, if the number of schools and students is uncorrelated with the change in unemployment across counties conditional on all control variables. In particular, counties with more schools are not allowed to be more or less cyclical than counties with fewer schools. In our results section we show that this is the case for all years prior to the start of the stimulus.

3.2 Investment Data and Instruments

The states had legal reporting requirements to the federal government about stimulus-financed projects. Most states published the resulting lists in the internet. We also contacted the state or the federal government for further information, if this data was unavailable. Each project record contains the total invested amount and a description indicating the project type and either the municipality code, the ZIP code or the name of the city where the investment took place. We match each city to a county using the

official community directory of Destatis (the German statistical office).⁹

Compared to the official accounting data which publishes only aggregate summary statistics of stimulus spending our project database is reasonably complete. We match 96.3 percent of the total number of projects and 95.2 percent of total spending. Our database thus contains 41.348 of 43.038 projects and 14.74 out of 15.7 Billion Euro of total investment.¹⁰ With a textual analysis of the projects description we can classify project according to investment object. For example we match 97.3 percent of all spending on schools (7.0 of 7.2 Billion Euro).

We use the number of schools and students as instrumental variable. Data on schools and students on the county-level are available from the statistical offices of the states and the federal government in the database “Regionaldatenbank Deutschland”.¹¹ This administrative data is collected to allocate teachers and funds from the states to the regions.

3.3 Dependent Variable and Controls

County-level unemployment series are from the Bundesagentur für Arbeit, the Federal Employment Office of Germany.¹² This data is collected to determine the unemployment benefits and therefore contains minimal sampling error. For each county in Germany we obtain monthly data from January 2000 to December 2012. The total number of unemployed is normalized with the pre-stimulus working age population of 2008 to arrive at unemployment rates. By using the first difference in unemployment rates as dependent variable we already control for all time-invariant characteristics of a county which may influence the level of the unemployment rate.

We control for the composition of the workforce and the industry structure, because they might be both be good predictors of the change in unemployment and correlated with the instruments for the stimulus. For example, more schools in a region might lead to a better educated workforce which is less likely unemployed during a crisis or a particular industry structure which is more or less pro-cyclical. For that reason we control for the share of worker which have a university degree or vocational training, the share of employment and GDP in services, manufacturing and construction as well as county-level GDP. In all specification, we use a complete set of state dummies to adjust

⁹The community directory is available on

<https://www.destatis.de/DE/ZahlenFakten/LaenderRegionen/Regionales/Gemeindeverzeichnis/Administrativ/Adminis>

¹⁰The difference in spending between the intended amount of 13.4 Billion Euro and the actual amount of 15.7 Billion Euro is due to over-investment of the states.

¹¹The “Regionaldatenbank Deutschland” is available on www.regionalstatistik.de.

¹²The data is available on statistik.arbeitsagentur.de.

Table 1: Summary Statistics

	Mean	SD	Min	Max	N
Q1/11-Q1/09 Δ Unemployment in %	-0.01	0.01	-0.03	0.01	400
Stimulus p.c.	0.28	0.13	0.08	1.08	400
Schools p.c. \times 1000	0.68	0.16	0.36	1.33	400
School Students in %	0.17	0.04	0.09	0.32	400
Q1/09-Q1/08 Δ Unemp.	-0.00	0.01	-0.02	0.02	400
2008 Unemployment in %	0.06	0.03	0.01	0.15	400
% Empl. w. Univ.	0.09	0.04	0.04	0.30	400
% Empl. w. Vocational T.	0.61	0.06	0.39	0.74	400
Regional GDP p.c.	0.04	0.02	0.02	0.13	400
% Empl. Manufacturing	0.21	0.09	0.03	0.51	400
% GDP Manufacturing	0.21	0.09	0.02	0.61	400
% Empl. Construction	0.06	0.02	0.02	0.14	400
% GDP Construction	0.04	0.02	0.01	0.11	400
% Empl. Services	0.69	0.10	0.39	0.92	400
% GDP Services	0.60	0.09	0.26	0.81	400
County Type Dummy	0.26	0.44	0.00	1.00	400
2008 Working Age Pop(in Thd.)	127.05	97.29	22.31	918.71	400

Notes: All variables denoted by “p.c.” (per capita) and all unemployment variables are normalized by working age population measured in 2008. Subgroups of employees are normalized by the total number of employees in 2008.

for the state-wise allocation of stimulus spending.

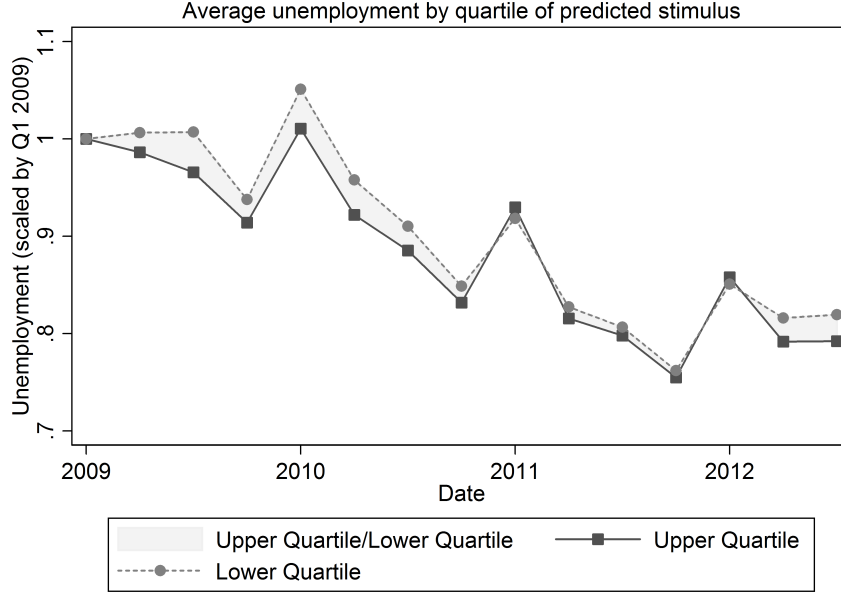
As mentioned above, it might be possible that counties in a worse economic shape received more stimulus funding. Accordingly, we controls for the lagged unemployment growth from 2007 to 2008 and the initial level of unemployment at the end of 2008 following the employment growth model of Blanchard et al. (1992). We document summary statistics for all variables used in our study in Table 1.

4 Baseline Results

4.1 Descriptive Evidence

Before turning to the results of our main specification, we check whether there is evidence in the raw data that stimulus spending indeed influences unemployment. We do this by first predicting with our instruments the stimulus spending for every county. Then we divide the counties into groups of 100, i.e. into quartiles of predicted stimulus spending and compare the median unemployment rate over the duration of the stimulus for the upper and the lower quartile. Both data series are normalized with unemployment in the

Figure 1: Employment in High versus Low Predicted Stimulus States



first quarter of 2009. The stimulus was enacted in this quarter and ended in the Q4/2011. According to Figure 1, counties with a high predicted stimulus had less unemployment compared to the quarter in which the stimulus was enacted. The effect appears to peter out over time. This figure provides a first indication for the effectiveness of the fiscal stimulus. However, there might be other confounding factors, for which we cannot control in this simple descriptive exercise.

4.2 Results of the Baseline Model

Table 2 provide first stage estimates. We estimate a weighted OLS with population sizes as weights and robust standard errors, which are reported in parentheses. The outcome variable is the total investment as of December 2011 normalized by the working age population. In the first column we regress both instruments, schools per capita and students per capita, on the actual investment in a county and find that both instruments have a positive and statistically significant mean estimate. In the second and third column we use each instrument individually as explanatory variable and find that both are positively related with spending per capita. Therefore, more schools and students led to more spending on the county-level.

Table 3 reports results from the two-stage IV regressions along with a OLS result for

Table 2: First Stage

	<i>Stimulus p.c. (in 100 Thd.)</i>		
Schools p.c. \times 1000	0.32*** (0.065)	0.41*** (0.066)	
School Students in %	1.12*** (0.33)		1.69*** (0.32)
Q1/09-Q1/08 Δ Unemp.	0.40 (1.63)	0.34 (1.64)	0.52 (1.71)
2008 Unemployment in %	0.06 (0.74)	-0.42 (0.73)	0.11 (0.75)
Regional GDP p.c.	-0.17 (0.56)	-0.07 (0.62)	0.14 (0.58)
Education Controls	Yes	Yes	Yes
Industry Structure	Yes	Yes	Yes
State FE's	Yes	Yes	Yes
Observations	400	400	400
Adjusted R2	0.32	0.30	0.28

Notes: This table shows in the first three columns weighted OLS regressions with population-sizes as weights on the county level. The dependent variable is the stimulus investment per capita as of December 2011. Robust standard-error are in parentheses. ***, **, * indicate statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively.

the baseline model of Equation (1). In all regressions the outcome variable is the change in unemployment in a county from the Q1/2009 to the Q1/2011, i.e. two years into the stimulus. The choice of the end period is basically arbitrary and we report the results for other end-months below. The end of 2010 was a milestone for implementation of the stimulus package, because the stimulus law stated that the communities had to start all stimulus-financed projects by this date. Thus, we expect much impact by this date. In all columns we use three set of control variables: the lagged unemployment level, the lagged unemployment change, the educational attainment in the workforce and the industry structure in the considered county. In the OLS regressions, the mean estimate of stimulus spending is negative and borderline significant on the 10 percent level. If we use both, the number of school students and the number of schools, as instrumental variables, the resulting coefficient is negative and significant on the one percent level. In the last two column, we report results for the two instruments individually and these are quantitatively and qualitatively similar to the specification using both.

The OLS estimate for the multiplier on unemployment is -0.34 (s.e.=0.18). This means

Table 3: Second-Stage Results

Instruments	Δ Unemployment rate from Q1 2009 - Q1 2011			
	OLS	IV Both	IV Schools	IV Students
Stimulus p.c.	-0.34* (0.18)	-1.35*** (0.44)	-1.41*** (0.53)	-1.25** (0.54)
Q1/09-Q1/08 Δ Unemp.	-0.35*** (0.073)	-0.34*** (0.072)	-0.34*** (0.073)	-0.34*** (0.072)
2008 Unemployment in %	-0.19*** (0.025)	-0.19*** (0.026)	-0.19*** (0.026)	-0.19*** (0.026)
Regional GDP p.c.	-0.06*** (0.021)	-0.05** (0.023)	-0.05** (0.023)	-0.05** (0.022)
Education Controls	Yes	Yes	Yes	Yes
Industry Structure	Yes	Yes	Yes	Yes
State FE's	Yes	Yes	Yes	Yes
Observations	400	400	400	400
Adjusted R2	0.70	0.68	0.67	0.68
F-statistic 1st stage		30.86	46.38	34.34
OverId test (p-value)		0.79	1.00	1.00

Notes: This table shows weighted OLS and IV regressions with population-sizes as weights on the county level. The dependent variable is the change in the unemployment rate from the first quarter 2009 to the first Quarter of 2011. Robust standard-error are in parentheses. ***, **, * indicate statistical significance at the 1 percent, 5 percent, and 10 percent level, respectively.

that 0.34 jobs have been created in a county for each 100'000 Euro in stimulus investment between the first quarter of 2009 and the first quarter 2011. The IV estimate using both instruments is -1.35 with a standard error of 0.44. Therefore the number of unemployed was on average 1.35 persons lower for every 100,000 Euro outlays from Q1/2009 to Q1/2011. The fact that the OLS estimate is smaller than the IV estimate suggests that stimulus spending may have been endogenously directed disproportionately toward counties within a state which were in a worse economic shape.

We cannot deduce the costs per job-year from these results because we do not know when the jobs were created and how long they lasted. It is not sensible to fill in this lack of information with different set of assumption, because the resulting bounds are too large to be informative. To see this note that if all jobs were created in the first quarter of 2009 and lasted for the duration of the stimulus then the costs per job-year are equal to $\frac{100,000}{3 \cdot 1.35} \approx 24,600\text{€}$. On the other hand, if the all jobs were created in the first quarter

of 2011 then the implied costs per year are $\frac{100,000}{1.35} \approx 74,000\text{€}$. To better approximate the costs per job-year we have to estimate when the stimulus did create jobs.

It is of independent interest how fast the stimulus improved the economy and thus if the investment program was able to counteract in a timely manner the beginning recession of 2009. In order to estimate the time-path of the stimulus effect we estimate our main regression using different end-quarters for the unemployment change.

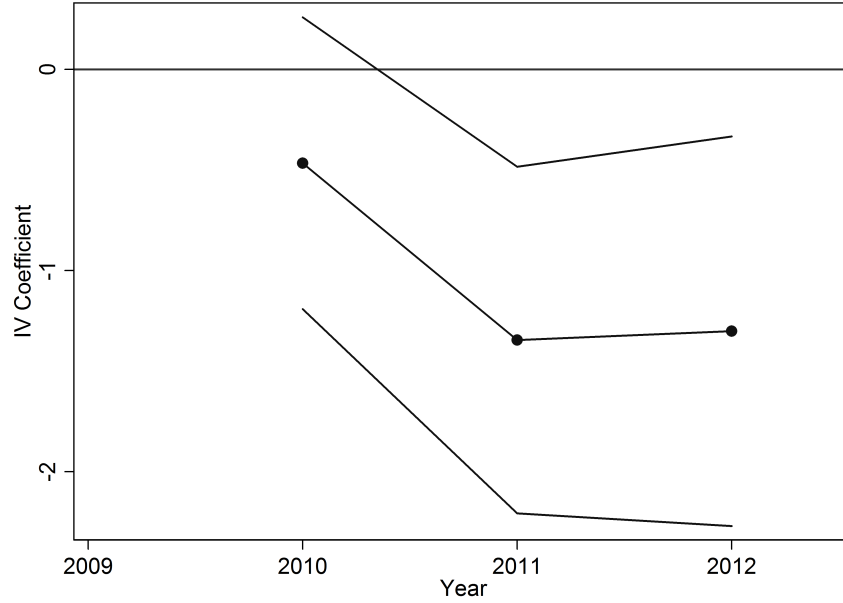
Specifically we estimate

$$\frac{U_{Q1/t}^c - U_{Q1/2009}^c}{N_{2008}^c} = \alpha + \beta_{I,t} \cdot \frac{Investment}{N_{2008}^c} + \gamma \cdot Controls_{2008} + \varepsilon, \quad (2)$$

for $t \in \{Q1/2010, Q1/2011, Q1/2012\}$. Hence, $\beta_{I,t}$ gives the cumulative effect of stimulus between its enactment and the first quarter in t . We plot the resulting estimates for our preferred IV specification in Figure 2 along with their 95 percent confidence intervals. Apparently the stimulus affected unemployment mainly in 2009 and 2010, as the size of the coefficient does not increase and even slightly decreases in 2011.

A back of the envelope calculation of costs per job-year based on these estimates entails first to calculate the impact of the stimulus per year and then multiply this number with the duration of the jobs. As we do not see any significant reversal in our data, we assume that the jobs lasted till the end of stimulus in the fourth quarter of 2011. The estimated coefficient are $\beta_{I,2010} = -0.47$, $\beta_{I,2011} = -1.35$ and $\beta_{I,2012} = -1.30$ and therefore the respective impact for 2009 was 0.47, 0.88 for 2010 and -0.05 for 2011. If all jobs were created at the end of the year, then the stimulus created $0.47 \cdot 2 + 0.88 \cdot 1 = 1.82$ job-years for 100,000 Euro investment. On the other hand if all jobs were created at the beginning of each year $0.47 \cdot 3 + 0.88 \cdot 2 + (-0.05) \cdot 1 = 3.12$ job years were created. These two numbers imply that the costs of a job-year are in the interval of [32,000 Euro, 55,000 Euro]. The mid-point, corresponding to continuous job creation over the year is then 2.47 jobs per 100 Thsd Euro or 43'500 Euro per job-year.

Figure 2: Effects over time: $\beta_{I,t}$ between 2010 and 2012



Notes: This figure shows the coefficient of β_t and its 95 percent confidence interval for the change in the unemployment rate from Q1 2009 to Q1 2010, Q1 2011 and Q1 2012, respectively.

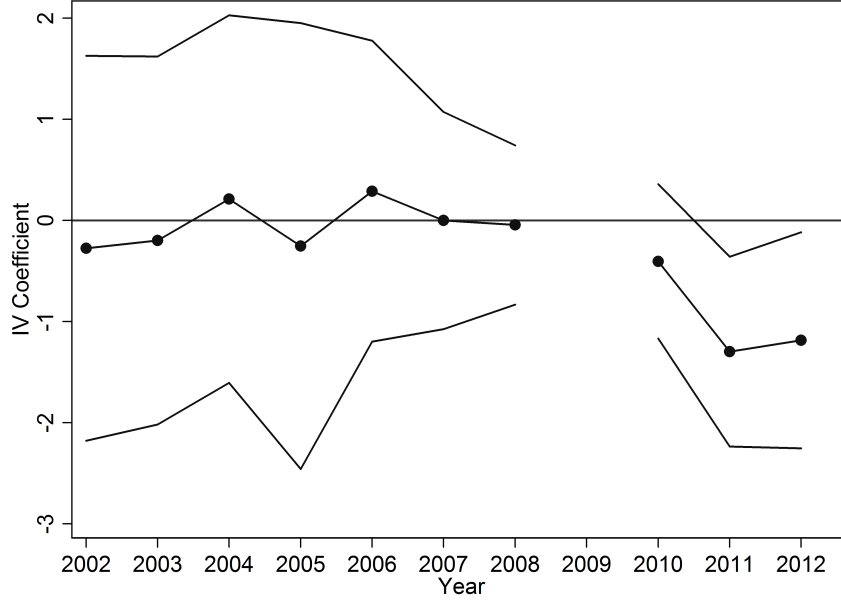
4.3 A Falsification Test of the Instruments

The empirical strategy outlined above hinges critically on the validity of our instruments. In particular our instruments are not allowed to causally influence the change in the unemployment rate in any other way than through stimulus. We can check the plausibility of this assumption by estimating the aggregate effects of the stimulus for periods before Q1 2009. The idea is, that instrumented stimulus spending should have no effect on unemployment before stimulus was enacted unless the instruments are correlated with the change in unemployment and therefore invalid.

To perform this falsification check we calculate the change unemployment rates from the quarter in which the stimulus was enacted (Q1/2009) to the first quarter of earlier years. Then we re-estimate Equation (2) with these pre-stimulus unemployment changes as dependent variables. Consequently we use for our first regression the change in unemployment from the first quarter of 2008 to the first quarter of 2009. Next, we calculate the impact of the stimulus on the change in unemployment from Q1/2007 to Q1/2009. We repeat this procedure for every year down to 2002.

Figure 3 adds the resulting IV-coefficients along with the 95 percent confidence bands to Figure 2, which showed the effect of instrumented stimulus during the time the stimulus

Figure 3: Falsification Test



Notes: This figure shows the coefficient of β_t and its 95 percent confidence interval for the change in the unemployment rate from Q1 2009 to the first quarter of year t with $t \in \{2002, 2003, \dots, 2012\}$.

was active. According to these estimates, there is no discernible effect of the stimulus spending before the stimulus was enacted. All coefficients before 2009 are centered around zero and are not statistically different from zero on conventional levels. This suggests that our instruments are plausibly exogenous.

5 Robustness

In Table 4, we evaluate the robustness of our baseline results. We check robustness along four dimensions: First, we consider the size of the effect in different regional subsamples, second we use other plausible instruments, then we vary the clustering scheme and lastly we change the set of controls.

Regional Characteristics

We first split the sample into East and West Germany and report the results in line two and three. The main effect in the baseline specification is only driven by the West Germany, where the estimated coefficient is about the same size as in our baseline results while the mean estimate in East Germany is virtually zero. The reason is, that our

Table 4: Robustness

	β_I	SE	F stat.	Overid.	N
Baseline	-1.35***	0.44	30.86	0.79	400
Regional Differences					
West Germany	-1.39***	0.41	32.72	0.28	324
East Germany	-0.07	1.85	1.86	0.29	76
Independent Cities	-1.11	0.73	12.35	0.53	105
w/ Independent Cities	-2.13**	1.04	6.52	0.78	295
Clustering and Instrument					
Region (ROR) Cluster	-1.28***	0.48	31.25	0.81	392
Region (ROR) Dummies and Cluster	-1.61***	0.61	14.92	0.56	392
Instrument Schools 1995	-1.12**	0.44	29.33	.	400
Controls					
With migration	-1.31***	0.44	29.93	0.75	400
With population density	-1.55***	0.55	21.64	0.79	400
No Industry & Education	-2.41***	0.75	16.17	0.05	400
No Industry Structure	-1.43***	0.46	35.42	0.17	400
No Education	-1.08**	0.46	25.02	0.70	400
No Unemployment Difference	-1.36***	0.47	30.94	0.74	400
No lagged level	-0.46	0.46	31.88	0.04	400
Unemployment level 2006	-1.26***	0.43	31.52	0.53	400
Mean unem. level 2000-2008	-1.18***	0.42	30.85	0.60	400

Notes: This table shows IV regressions of (1) weighted by population size on the county level; instruments are schools and students, except in row “Instrument schools 1995”, in which the instrument is only schools in 1995. The dependent variable is the change in the unemployment rate from the first quarter 2009 to the first Quarter of 2011. Robust standard-error are in parantheses if not indicated otherwise. Level of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

instruments are weak (F-statistics 1.86) in the East and therefore the IV estimate is quite likely biased. Accordingly, the 95 percent confidence interval for east Germany is with $[-3.71, 3.31]$ too large to be informative. We conjecture that there were no schools left for renovation after the investment program caused by the German Unification.

In line four and five we split the sample along county characteristics. In principle, German counties can be of two types: Independent cities which are medium to large cities (“kreisfreie Städte”) or counties with rather rural characteristics (“Landkreise”).¹³

¹³This distinction is not sharp: For example Zweibrücken, a town of 35'000 inhabitants is an independent city while the “Landkreise” Mettmann in the state of North Rhine-Westphalia has a 1215 inhabitants per square kilometer - comparable to the neighbouring independent city of Münster (963

The estimated standard errors for both subsamples are larger than in our baseline specification. The mean estimate for the independent cities is with -1.11 similar to our main results, but not significantly different from zero on conventional levels (p-value = 0.13). In contrast, the estimated coefficient for the “Landkreise” is with 2.13 much larger but also only significant on the five percent level. These results indicate that the stimulus potentially benefited rural areas more than independent cities.

Clustering & Instruments

One concern for our results is that idiosyncratic shocks to counties may be regionally correlated. In this case we overstate the precision of our estimates by reporting robust standard error. We cannot cluster the standard errors at the state level, because this would leave us with only 14 independent observation after excluding the city-states of Hamburg and Berlin. Therefore, we cluster at the regional level of “Raumordnungsregionen” (ROR), which are entities for regional planning. RORs are designed to capture areas of economic integration - for example commuting routes are taken into account when defining a RORs.

In total, there are 97 RORs which consist of four counties on average. We drop Berlin and Hamburg as well as the state of Mecklenburg-Vorpommern, because the latter reorganized its counties in 2011 and RORs have not been adapted since. This leaves us with 90 RORs. In row seven we report the estimates with clustered standard errors which are reassuringly about the same size as the robust standard errors. In row six, we substitute the state dummies of our main specification with ROR dummies. Thus we identify the effectiveness of the stimulus only from variation of the instrumented variation in spending within the RORs. This demands a lot from our data, but the estimated coefficients are of similar size and statistically significant.

Our empirical strategy is based on the idea that the number of schools and students is predetermined and thus independent of short-term variation in economic conditions. Based on this argument, the number of schools in the more distant past should be also viable instrument for stimulus investment – after all, the actual number of schools in 2008 may be in part determined by recent migration and thus not be fully independent of current economic conditions. To accommodate this potential concern, we re-estimate our model using the number of schools in 1995 – about 15 years before stimulus enactment – as instrument. This exercise reduces the size of the estimates a bit, but they remain qualitatively similar.

inhabitants per km²).

Control Variables

Next, we check whether the results are sensitive to the presence or absence of the control variables included in the baseline specification. As mentioned above, schools and students may be correlated with migration which could be correlated with economic conditions. Controlling for this potential confound leaves the estimated coefficient unchanged. The same holds true for adding population density as additional control in line ten. Population density may be correlated with the number of schools and with economic conditions in a way that we do not account for with the industry structure and education controls. This does not seem to be the case. In the following lines we selectively drop the controls of our main specification. The resulting coefficients are negative, significant and about the same size as in our baseline specification. The only exception is, that we have to include some kind of lagged unemployment level to find an effect.

6 Conclusion

This study analyzes the effect of the German stimulus program of 2009 on the labor market. In particular we evaluate the impact of the 15.4 Billion Euro infrastructure investment program on the change in unemployment rates at the county level. As stimulus spending is potentially endogenous to local economic conditions we use the pre-crisis number of schools and students as instrumental variables to address this problem. These instruments are relevant, because 65 percent of total spending was earmarked by law for the renovation of local educational infrastructure. Thus the number of school buildings determined the total number of potential investment projects. In addition municipalities received in some states formula allocations of funds based on their number of school students.

With this research design we find that the stimulus had a negative and statistically significant impact on the change in unemployment on the county level. The stimulus created around 2.47 job-years for every 100'000 Euro investment. This implies a cost per job-year of 43'500 Euro. Further analysis reveals, that the stimulus affected unemployment already in 2009 and 2010, rather quickly after the law was passed in March 2009. We conduct extensive falsification and robustness checks to show that our instruments are plausibly exogenous and our quantitative estimates are stable.

Our study evaluates the effect of a particularly designed investment program in Germany in 2009. The stimulus program was strongly 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. Along these lines, Feyrer and Sacerdote (2011) show for the U.S. stimulus program that investing in infrastructure was much more effective in creating jobs than investment in education. Furthermore, Auerbach and Gorodnichenko (2012) suggest that the fiscal multiplier might be larger in recession than booms.

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. In addition, our results are remarkably similar to estimates for the ARRA stimulus program in the U.S. (which was also heavily focused on infrastructure investment) suggesting a broader applicability of our findings.

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