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Roads to Prosperity or Bridges to Nowhere? Theory and Evidence on the Impact of Public Infrastructure Investment

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I. Introduction

Public infrastructure investment often plays a prominent role in countercyclical fiscal policy. In the United States during the Great Depression, programs such as the Works Progress Administration (WPA) and the Tennessee Valley Authority (TVA) were key elements of the government's economic stimulus. In the Great Recession, government spending on infrastructure projects was a major component of the 2009 stimulus package. Yet, infrastructure's economic impact and how it varies with the business cycle remain subject to significant debate. Many view this form of government spending as little more than "bridges to nowhere"; that is, spending yielding few economic benefits with large cost overruns and a wasteful use of resources. Others view public infrastructure investment as an effective form of government spending that can boost economic activity not only in the long run, but over shorter horizons as well.

This paper examines the dynamic macroeconomic effects of infrastructure investment both empirically and theoretically. It first provides an empirical analysis using a rich and novel data set at the state level on highway funding, highway spending, and numerous economic outcomes. We focus on highways both because they are the largest component of public infrastructure in the United States and because the institutional design underlying the geographic distribution of US federal highway investment helps us identify shocks to state infrastructure spending. In particular, our empirical analysis exploits the formula-based mechanism by which nearly all federal highway funds are apportioned to state governments. Because the state-specific factors entering

978-0-226-05277-9/2013/2012-0201$10.00

the apportionment formulas are often largely unrelated to current state economic conditions and also lagged several years, the formula-based distribution of federal highway grants provides an exogenous source of highway funding to states, independent of states' own current economic conditions.[1]

The focus on federal grants to states has the advantage of capturing much more precisely the timing with which highway spending affects economic activity. Public highway spending in the United States is ultimately determined by state governments, which allocate a large fraction of their revenues to highway construction, maintenance, and improvement.[2] However, states report highway spending using the concept of outlays, and we show that outlays often lag considerably the movements in actual government funding obligations that give states the right to contract out and initiate projects.[3] Furthermore, there can be administrative delays between when a state's grants are initially announced and when the state starts incurring obligations. Using grants to measure the timing of highway spending shocks allows one to estimate possible economic effects stemming from agents' foresight of future government obligations and outlays, even before highway projects are initiated.

In addition, the design and distribution of federal highway spending helps us address concerns related to anticipation effects that are likely to arise in the case of large infrastructure projects. Because the US Congress typically sets the total national amount of highway grants and the formulas by which they are apportioned to states many years in advance, there is strong reason to believe that economic agents (especially state governments and private contractors) can anticipate long in advance, albeit imperfectly, the eventual level of grants a given state will receive in a given year. Such anticipation of future government spending has been shown by Ramey (2011a) to pose a serious hazard in correctly identifying spending shocks.[4]

Using the institutional details of the mechanisms by which grants are apportioned to states, and very detailed data on state-level apportionments and national budget authorizations, we construct forecasts of current and future highway grants for each state and year between 1993 and 2010. These forecasts are constructed in much the same way that the Federal Highway Administration (FHWA) constructed forecasts of future highway grants to states at the beginning of the most recent multiyear appropriations act (which covered 2005 to 2009). From these forecasts, we calculate the expected present discounted value of current and future highway grants. The difference in expectations from last year to this year forms our measure of the shock to state highway spending. This shock is driven primarily by changes in incoming data on formula factors which, as mentioned earlier, reflect information on those factors from several years earlier (because of data collection lags).

We exploit the variation of our shock measure across states and through time to examine its dynamic effect on different measures of economic activity by combining panel variation and panel econometric techniques with dynamic impulse-response estimators. Specifically, we extend the direct projections estimator in Jordà (2005) to allow for state and year fixed effects. We find that these highway spending shocks positively affect GDP at two specific horizons. First, there is a positive and significant contemporaneous impact. Second, after this initial impact fades, we find a larger second-round effect around six to eight years out. Yet there appears to be no permanent effect as GDP is back to its preshock level by ten years out. The results are robust to using alternative impulse-response estimators—in particular, a distributed-lag model as in Romer and Romer (2010) and a panel vector autoregression (VAR). We find a similar impulse response pattern when we look at other economic outcomes, though there is no evidence of an initial impact for employment, unemployment, or wages and salaries. Reassuringly, we find especially large medium-run (six to eight years out) effects in sectors most likely to directly benefit from highway infrastructure such as truck transportation output and retail sales.

From our estimated GDP impulse response coefficients, we calculate average multipliers over ten-year horizons that are slightly less than 2. However, the multipliers at specific horizons can be much larger: from roughly 3 on impact to peak multipliers of nearly 8, six to eight years out. These peak-multiplier estimates are considerably larger than those typically found in the literature, even those similarly estimating local multipliers with respect to “windfall” transfers from a central government. One plausible reason is that public infrastructure spending has a higher multiplier than the noninfrastructure spending considered in most previous studies. For instance, Baxter and King (1993) demonstrated theoretically that public infrastructure spending could have a multiplier as high as 7 in the long run, even with a relatively modest elasticity of public capital in the representative firm’s production function, though they obtained a small short-run multiplier. As we discuss in section IV, it is also possible that a shock to current and future highway grants leads to increases not just to highway projects receiving federal aid, but also to general highway spending and to state spending more broadly. Still, using state highway spending in addition to federal highway spending as a broader measure of government outlays, we estimate a lower bound for the peak multiplier of roughly 3.

Following Auerbach and Gorodnichenko (2012), we extend the analysis to investigate whether highway spending shocks occurring during recessions lead to different impulse responses than do shocks occurring in expansions. The potential empirical importance of such nonlinearities was emphasized recently in Parker's (2011) survey of the fiscal multiplier literature. The results are somewhat imprecise, but we find that the initial impact occurs only for shocks in recessions, while later effects are not statistically different between recessions and expansions.

In the second part of the paper, we use a theoretical framework to interpret our empirical findings. In line with our state-level data set and in the spirit of Nakamura and Steinsson (2011), we look at the multiplier in an open economy model with productive public capital in which "states" receive federal funds for infrastructure investment calibrated to capture the structure of a typical highway bill in the United States. Using the direct projections impulse response estimator on our simulated data, we obtain a qualitatively very similar pattern to our empirical impulse response function: GDP rises on impact, then falls for some time before rising once again. We show that this pattern is consistent with an initial effect due to nominal rigidities and a subsequent longer-term productivity effect that arises once the public capital is put in place and available for production. In accounting for our empirical results, we also demonstrate the importance of the elasticity of public capital in the private sector's production function, the time-to-build lag associated with public capital, and the persistence of shocks. Quantitatively, however, our baseline calibration generates a peak multiplier of roughly 2, smaller than the second-round effect implied by our empirical impulse response estimates.

Moreover, as our empirical estimates of the multiplier removes any possible effects from aggregate variables (monetary policy, for instance), they can differ from estimates of aggregate multipliers in the literature. To get a sense of the magnitude of this difference, we use the model to compute an aggregate multiplier and find that, under our assumed interest-rate rule and federal fiscal policy, the peak aggregate multiplier is roughly half the local one. However, this magnitude will clearly depend on the assumption regarding federal policies (see, for instance, Christiano, Eichenbaum, and Rebelo [2010] on the importance of monetary policy).

This paper is one of the first to analyze the dynamic macroeconomic effects of public infrastructure investment. The sparsity of prior work likely owes to the challenges posed by the endogeneity of public infrastructure spending to economic conditions, the partial fiscal decentralization of the spending, the long implementation lags between when spending changes are decided and when government outlays are observed, and the high degree of spending predictability leading to likely anticipation effects. These four features make public infrastructure spending unique and, in particular, different from the type of government spending often analyzed in the literature on fiscal policy, which frequently focuses on the effects of military spending (see Ramey and Shapiro 1998; Edelberg, Eichenbaum, and Fisher 1999; Fisher and Peters 2010; Ramey 2011a; Barro and Redlick 2011; and Nakamura and Steinsson 2011, among others). While defense spending is also subject to implementation lags and anticipation effects, changes in defense spending due to military conflicts are more likely to be exogenous to movements in economic activity than changes in public infrastructure spending.

Because of our focus on highway spending, our paper is more in line with the work of Blanchard and Perotti (2002), Mountford and Uhlig (2009), Fishback and Kachanovskaya (2010), or Wilson (2012), which look at the effects of nondefense spending.[5] As in the latter two studies, several recent papers have used variations in government spending across subnational regions to identify the effects of fiscal policy.[6] These studies take advantage of the fact that large portions of federal spending are often allocated to regions for reasons unrelated to regional economic performance or needs, a strategy that we also follow. Such variations can be used to identify the effects of federal spending on a local economy. How these local effects relate to the national effects of federal spending depends upon, among other factors, whether there are spillover effects to other regions and the extent to which local residents bear the tax burden of the spending (as stressed in Ramey 2011b). We are able to explore the importance of these factors with our theoretical model.

We are aware of only a few studies that explicitly investigate the overall economic effects of public highway spending.[7] Pereira (2000) examines the effects of highway spending among different types of public infrastructure investment, on output using a structural VAR and aggregate US data from 1956 to 1997. Using a timing restriction à la Blanchard and Perotti (2002), he finds an aggregate multiplier of roughly 2. This approach requires the arguably unrealistic assumption that current government spending decisions are exogenous to current economic conditions. Moreover, it cannot account for anticipation effects that are very likely to occur in the case of federal highway spending, which may lead to incorrect inference. Using US county data, Chandra and Thompson (2000) attempt to trace out the dynamics of local earnings before and after the event of a new highway completion in the county. They find that earnings are higher during the highway construction period (one to five years prior to completion) than when the highway is completed and that earnings after completion rise steadily over many years. This U-shaped pattern is broadly consistent with our estimated GDP impulse response function with respect to highway spending shocks (which would occur several years prior to a highway completion). A recent paper by Leigh and Neill (2011) estimates a static, cross-section, instrumental variable (IV) regression of local unemployment rates on local federally funded infrastructure spending in Australia. Because much of that spending in Australia is determined by discretionary earmarks rather than formulas, they use political power of localities as instruments for grants received by localities. Though one might be concerned that local political power also affects local economic conditions, which would violate the IV exclusion restriction, they find that local highway grants substantially reduce local unemployment rates.

The remainder of the paper is organized as follows. The next section provides a background discussion about the Federal-Aid Highway Program and details the process through which federal highway grants are distributed among states. We also discuss the issues of timing and forecastability of grants. In section III, we first provide evidence on the extent of implementation lags for highway grants and then describe how we construct our measure of highway grant shocks. Our empirical methodology and results are presented in section IV. In section V, we present our open economy model and the theoretical multipliers. The last section concludes.

II. Infrastructure Spending in the United States: Institutional Design

The design of the US Federal-Aid Highway Program allows us to specifically address the several issues raised in the introduction. In particular, the distribution of federal highway grants across states is subject to strict rules that reduce the concern that these distributions may be endogenous to states' current economic conditions. Moreover, the data on federal highway funding is detailed enough to distinguish between the provisions of IOUs by the federal government to states and actual government outlays, which mitigates the problem that might arise from implementation lags that obscure the timing of government spending. Highway bills are also designed to ease long-term planning and provide a natural way to tackle the concern that future spending can be anticipated. This section examines each of these features in turn after first providing some background information on highway bills.

Federal funding is provided to the states mostly through a series of grant programs collectively known as the Federal-Aid Highway Program (FAHP). Periodically, Congress enacts multiyear legislation that authorizes spending on these programs. Since 1990, Congress has adopted three such acts: the Intermodal Surface Transportation Efficiency Act (ISTEA) in 1991, which covered fiscal years (FY) 1992 through 1997; the Transportation Equity Act for the 21st Century (TEA-21) in 1998, which covered FY1998 through 2003; and the Safe, Accountable, Flexible, Efficient Transportation Equity Act: A Legacy for Users (SAFETEA-LU) in 2005, which covered FY2005 through 2009.[8] However, legislation of much shorter duration has also been adopted to fill the gap between the more comprehensive, multiyear acts. These so-called stop-gap funding bills typically simply extend funding for existing programs to keep them operational. For instance, since SAFETEA-LU expired in 2009, nine (as of the time of this writing) highway bill extensions of varying durations have been adopted to continue funding highway programs in accordance with SAFETEA-LU's provisions.

The FAHP is extensive and helps fund construction, maintenance, and other improvements on a large array of public roads that go well beyond the interstate highway system. Local roads are often considered federal-aid highways and eligible for federal construction and improvement funds, depending on their service value and importance. The cost of the work under the FAHP is mostly, but not fully, covered by the federal government. Depending on the program, the federal government will reimburse a state for 80 to 90 percent of the cost of eligible projects, up to the limit set by the state's grant apportionment. Thus, it is important to recognize that not all highway spending on federal-aid highway projects is financed by the federal government; some of it is financed by states' own funds, such as state tax revenues.

A. Formulary Mechanism for Distributing Grants to States

When a highway bill is passed, Congress authorizes the total amount of funding available for each highway program (highway construction, bridge replacement, maintenance, etc.) for each fiscal year covered by the bill.[9] For instance, SAFETEA-LU authorized $244 billion for transportation spending for 2005 to 2009; 79 percent of that was for the FAHP. Nearly all of FAHP funding takes the form of formula grants to state governments. The grants for each individual highway program (Interstate Maintenance, National Highway System, Surface Transportation Program, etc.) are distributed to the states according to statutory apportionment formulas also enacted by Congress as part of the current authorization act. The Interstate Maintenance program, for instance, apportioned funds under SAFETEA-LU according to each state's share of national interstate lane-miles, its share of vehicle-miles traveled on interstate highways, and its share of payments into the Highway Trust Fund, with equal weights on each factor.

The formulas for most highway programs have changed little over time (i.e., over different authorization acts). However, highway legislation since 1982 also has included a guaranteed minimum return on a state's estimated contributions to the Highway Trust Fund (HTF), which is nominally the financing source for highway authorizations. A state's HTF contributions are the revenues from the HTF's fuel, tire, and truck-related taxes that can be attributed to the state and are estimated by the FHWA based on the same factors used in apportionment formulas. In 1991, the adoption of ISTEA set this minimum guaranteed return to 90 percent, which was then raised to 90.5 percent under TEA-21 in 1998 and 92 percent under SAFETEA-LU. (See online appendix A, http://www.nber.org/data-appendix/c12750/appendices.pdf, for more detail.)

A benefit of the minimum return requirement, along with the statutory formula apportionment of individual programs, is that it mitigates the potential role of political influence on the distribution of federal funding from year to year. That said, highway bills contain funds earmarked for certain projects that are clearly subject to political influence. For instance, prior to SAFETEA-LU's final legislation, an earlier proposal included an earmark of over $200 million for the so-called "Bridge to Nowhere" that was to link Ketchikan, Alaska—with a population of 8,900—to the Island of Gravina—with a population of 50. Though this and many other proposed earmarks were ultimately dropped from the final legislation, $14.8 billion out of SAFETEA-LU's $199 billion of highway authorizations was set aside for earmarks.[10] However, since earmarks are not distributed according to formulas, we do not use them in our empirical work.

A key feature of the formulary apportionment process that is critical for our empirical strategy is that the factors used in the formulas are lagged three years, since timely information is not readily available to the FHWA. Although the apportionment of federal grants is partly based on factors exogenous to economic activity (lane-miles, for instance), others, such as payments into the HTF, may be correlated with movements in current GDP. The use of three-year-old data for the factors in the apportionment formulas mitigates the concern that highway spending is reacting contemporaneously to movements in activity.

B. Implementation Lags: Apportionments, Obligations, and Outlays

Another important aspect of the FAHP is that it can entail substantial implementation lags between funding authorization and actual spending. The bureaucratic process underlying these lags is well detailed in FHWA (2007). The process begins each fiscal year when federal grant distributions are announced. Each state may then write contracts with vendors, obligating funds up to a maximum determined by current grants and unobligated past grants. Contractors submit bills to the state over the course of projects and/or at the completion of projects. The state passes those bills on to the FHWA, which approves them and instructs the US Treasury to transfer funds to the state which, in turn, sends funds to the contractor. Note that it is these final transfers of funds by the federal and state governments that show up as "outlays" in official government statistics and ultimately enter the calculation of a state's GDP as part of (state) government spending.

There are at least two steps in this process that can introduce substantial delays between grants and outlays. First, states legally have up to four years to obligate funds from a given year of grants. Second, and more importantly, once a contract has been written, the work itself may take several years. This time-to-build lag is, of course, a distinguishing characteristic of infrastructure spending. We use this distinction between apportionment announcements, obligations, and outlays to provide evidence on the importance of timing in studying the effects of highway spending on states' economic activity.[11]

C. The Forecastability of Grants

The use of formulas in allocating road funds among states has a long history, going as far back as 1912 with the adoption of the Post Office Appropriation Act, which provided federal aid for the construction of rural postal roads. Such formulas were introduced to make annual grant distribution more predictable and less subject to political influence. They serve the same purpose today, as most highway programs require long-term planning, and advance knowledge of future funding commitments helps smooth operations from year to year. Indeed, before a new highway bill is introduced, the FHWA often estimates what each state is likely to receive each year, using the apportionment formulas. As a result, the transportation department in each state has a good sense of how much the state should expect for each program and can plan accordingly. In the following section, we use these formulas to generate forecasts, as of each year from 1992 to 2010, of apportionments for each program and for all future years. We show that our forecasts closely match those produced by the FHWA for those years in which FHWA projections are available.

To summarize, there are three key institutional features of US federal highway spending that we will account for and exploit in our empirical strategy: (1) federal grants are apportioned to states via formulas that use three-year-old factors; (2) there can be long implementation lags between highway funding announcements and actual roadwork; and (3) by design, the amount of federal grants states receive each year is partially forecastable.

III. Measuring Shocks to Highway Spending

In this section, we detail the construction of our shocks to highway spending, which use revisions in forecasts of federal grant apportionments. Before turning to that topic, however, we first discuss the importance of implementation lags and timing in highway infrastructure projects, which supports our use of grants, as opposed to outlays, to construct our shocks.

A. Implementation Lags and Correctly Measuring the Timing of Highway Spending

Leeper, Walker, and Yang (2009) and others have convincingly argued that implementation lags between government spending authorization and government outlays can greatly distort inferences regarding the economic impacts of government spending. As described earlier, this is especially true for highway and other infrastructure spending. Using state panel data that we collected from the FHWA Highway Statistics series (see the data glossary in the online appendix B, http://www.nber.org/data-appendix/c12750/appendices.pdf, for details), we can estimate precisely what these implementation lags look like. First, we estimate the dynamic lag structure from federal highway grants ("apportionments") received by a state to its obligations of funds for federal-aid highway projects. Specifically, we estimate the following distributed lag model with state and year fixed effects:

$$OBLIG_{it} = \alpha_i + \alpha_t + \sum_{s=0}^{3} \beta_s A_{i,t-s} + \varepsilon_{it}, \tag{1}$$

where *OBLIG* is obligations and *A* is apportionments, both per capita.

The results are shown in table 1. The bottom line is that 70 percent of grant money is obligated in the same year the grants are announced and the remaining (roughly speaking) 30 percent is obligated the following year. All funds are obligated well within the four-year statutory time frame within which states must obligate federal funds. Thus, the step from grants to obligations introduces only modest implementation lags.

The step from obligations to outlays, however, can lead to substantial lags. This can be seen by estimating a distributed lag panel model as above but with outlays of federal aid as the dependent variable and obligations on the right-hand side.[12] Both variables are again per capita. We include current-year and up to seven years of lagged obligations to fully describe the implementation lag process. Further lags are found to be economically and statistically insignificant. The results are shown in the second column of table 1. We find that a dollar of obligations of federal-aid funds by a state takes up to six years to result in actual outlays (reimbursements to the state) by the federal government. The results in columns (1) and (2) suggest that the implementation lag—often referred to as the "spend-out rate"—between grants and outlays is quite long, and this is indeed confirmed when we regress FHWA outlays on current-year and seven lags of grants. As shown in the third column, \$1 in grants does eventually lead to \$1 in outlays (our point estimate is \$0.98 and the 95 percent confidence interval is \$0.88 to \$1.09), but the process can take up to seven years. In sum, states obligate federal grant funds in the current and following year and those obligations are outlaid over six years, so that the whole process from grants

Table 1
The Implementation Lags of Highway Spending

	FHWA Obligations β/SE	FHWA Outlays β/SE	FHWA Outlays β/SE
FHWA Grants	**0.700**	—	**0.122**
	(0.106)		**(0.064)**
FHWA Grants, Lagged 1 year	**0.345**	—	**0.526**
	(0.133)		**(0.081)**
FHWA Grants, Lagged 2 years	–0.037	—	**0.108**
	(0.101)		**(0.062)**
FHWA Grants, Lagged 3 years	–0.020	—	**0.044**
	(0.038)		**(0.023)**
FHWA Grants, Lagged 4 years	–0.016	—	**0.058**
	(0.036)		**(0.022)**
FHWA Grants, Lagged 5 years	—	—	**0.053**
			(0.016)
FHWA Grants, Lagged 6 years	—	—	**0.063**
			(0.015)
FHWA Grants, Lagged 7 years	—	—	0.021
			(0.015)
FHWA Obligations	—	**0.231**	—
		(0.019)	
FHWA Obligations, Lagged 1 year	—	**0.208**	—
		(0.032)	
FHWA Obligations, Lagged 2 years	—	**0.112**	—
		(0.021)	
FHWA Obligations, Lagged 3 years	—	**0.119**	—
		(0.031)	
FHWA Obligations, Lagged 4 years	—	**0.143**	—
		(0.030)	
FHWA Obligations, Lagged 5 years	—	**0.070**	—
		(0.030)	
FHWA Obligations, Lagged 6 years	—	–0.007	—
		(0.030)	
FHWA Obligations, Lagged 7 years	—	0.030	—
		(0.028)	
Year fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
Cumulative Effect	**0.973**	**0.906**	**0.996**
	(0.064)	**(0.033)**	**(0.042)**
N	784	735	735
R^2	0.386	0.764	0.693

Notes: Bold indicates significance at 10 percent level. All variables are per capita. Sample covers years 1993 to 2008 and all fifty states except Alaska.

to outlays can take up to seven years. That said, it should also be noted that the process is still highly skewed toward the first two or three years that federal grants are announced, with about 75 percent of grant funds showing up as outlays in the first three years.

These results provide strong evidence that there are substantial implementation lags between when highway spending amounts are authorized, and hence known with certainty to all agents in the economy, and when final outlays occur. That is, agents have near-perfect foresight of outlays several years in advance. Thus, one would not want to use outlays in deriving a measure of highway spending shocks in order to estimate the dynamic effects of highway spending. For this reason, we rely instead on information from apportionments (i.e., announced grants) in our analysis. Unanticipated shocks to such announcements may have economic effects both in the short run, as agents respond now to known future increases in government spending, and in the medium run as they lead to obligations, then actual roadwork, and finally real infrastructure capital being put in place that can potentially enhance productivity in the economy.

B. Distinguishing Unanticipated from Anticipated Changes in Highway Grants

In this subsection, we construct a measure of highway spending shocks using data from the FHWA on apportionments, statutory formulas, and formula factors from 1993 to 2010. In doing so, we make use of the fact that highway spending is likely to be partially forecastable owing to the multiyear nature of the federal highway appropriations acts which, as discussed in section II, typically cover a five- to six-year period. In a given year, agents know the full path of aggregate (national) grants for each highway program for the remaining years of the current appropriations bill and they also know the formulas by which each program's grants are apportioned to states. However, they do *not* know the future values of the factors that go into those formulas and that will determine the distribution of grants among states.[13]

The partial forecastability of future highway apportionments means that observed movements in apportionments may not represent true shocks to expected current and future highway spending. Therefore, we use the information provided in each highway appropriations bill to forecast current and future highway spending and then measure the shock to expectations as the difference between the current forecast and last year's forecast. This is similar in spirit to the approach of Ramey (2011a) and especially Auerbach and Gorodnichenko (2011). The latter paper measures shocks to government spending in Organization for Economic Cooperation and Development (OECD) countries as the year-over-year change in one-year-ahead forecasts of government spending made by the OECD. One difference is that our shock is based on a forecast of the present discounted value of all future government (highway) spending rather than just next year's spending.

To construct real-time forecasts of future highway grants, we follow and extend the methodology used by the FHWA Office of Legislation and Strategic Planning (FHWA 2005) in its report providing forecasts, as of 2005, of apportionments by state for the years 2005 to 2009 SAFETEA-LU highway bill. Basically, the methodology involves assuming that each state's current formula factors (relative to national totals), and hence each state's current share of federal grants for each of the seventeen FHWA apportionment programs, are constant over the forecast horizon.[14] That is, the best guess of what the relative values of formula factors will be going forward is their current-year relative values. Given apportionment shares for each program, one can then distribute to states the known nationwide totals for each program for the remaining years of the current legislation. One can then aggregate across programs to get a state's total apportionments in each of these future years.

We extend this methodology such that, if one is forecasting for years beyond the current legislation, one assumes a continuation of the use of current formulas (i.e., one's best guess of the formulas to be used in future legislation is the formula currently in use) and one assumes that nationwide apportionments by program grow at the expected inflation rate, which we get from the Survey of Professional Forecasters, from the last authorized amount in the current legislation. Assuming formulas for future bills will remain constant is reasonable since, as discussed in section II, there's been relatively little change in the formulas used to apportion federal grants over the past twenty years. The details of how we construct these forecasts are provided in online appendix C (see http://www.nber.org/data-appendix/c12750/appendices.pdf).

As a check on whether our forecast methodology is reasonable and similar to best practice for entities interested in forecasting highway apportionments, we compare our forecasts to forecasts we were able to obtain from the FHWA as of 2005. The scatterplot shown in figure 1 compares our four-year-ahead forecasts, as of 2005 (the first year of the

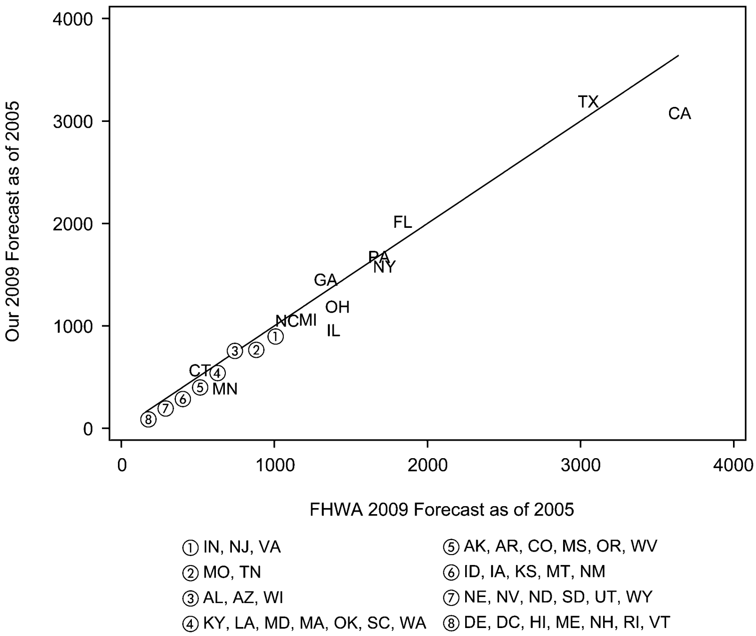

Fig. 1. Forecasts as of 2005 of Federal Highway Grants to States in 2009

2005 to 2009 SAFETEU-LU appropriations bill), and of 2009 highway apportionments to that done by the FHWA. The solid line is a 45-degree line. Not surprisingly, given that we use a similar forecasting methodology, our forecasts are very close to the FHWA's.

How forecastable are highway grant apportionments? The answer depends on the forecast year and the forecast horizon and, in particular, on whether one is forecasting grants within the current highway bill or forecasting beyond the current bill. As one would expect, the forecasts tend to be more accurate for forecasts of grants in out-years that are covered by the same highway bill as the current year. Yet, even "out-of-bill" forecasts are fairly accurate and the forecast errors are primarily driven by aggregate, rather than state, factors. For instance, forecasts of 2009 grants miss substantially on the downside because they could not have anticipated the large aggregate increase in highway grants affected by the 2009 American Recovery and Reinvestment Act. Overall, our forecasts explain 83 percent of the total variation in grants over states and years, and 84 percent of the variation net of state and year fixed effects.

Using our one-year-ahead to five-year-ahead forecasts, we calculate the present discounted value (PDV) of current and expected future highway grants for a given state *i*:

$$E_t[PV_i] = \sum_{s=0}^{5} \frac{E_t[A_{i,t+s}]}{(1 + r_t)^s} + \frac{E_t[A_{i,t+5}]}{(1 + r_t)^5} \frac{1}{(1 - \beta_t)} \quad (2)$$

where $E_t [A_{i,t+s}]$ is the forecast as of t of apportionments (in nominal dollars) in year $t + s$ and $\beta_t = (1 + \pi_t^e)/(1 + r_t)$. The second term on the right-hand side reflects that, because highway appropriation bills cover at most six years (t to $t + 5$), forecasts beyond $t + 5$ simply assume perpetual continuation of $A_{i,t+5}$ (discounted by $(1 + r_t)^5$) growing with expected future inflation of π_t^e. We measure the nominal discount rate, r_t, using a ten-year trailing average of the ten-year Treasury bond rate as of the beginning of the fiscal year t (e.g., Oct. 1, 2008, is the beginning of fiscal year $t = 2009$). The trailing average is meant to provide an estimate of the long-run expected nominal interest rate. We measure expected future inflation, π_t^e, using the median five- or ten-year-ahead inflation forecast for the first quarter of the fiscal year (fourth quarter of prior calendar year) from the Survey of Professional Forecasters (SPF).[15]

The difference between this year's expectation of grants from t onward, $E_t [PV_{i,t}]$, and last year's expectation of grants from t onward, $E_{t-1}[PV_{i,t}]$, is then a measure of the unanticipated shock to current and future highway grants. When both t and $t - 1$ are covered by the same appropriations bill, as is the case for most of the sample period, this difference primarily will reflect shocks to incoming data on formula factors. When t and $t - 1$ span different appropriations bills, this difference also will reflect news in year t about the new path of aggregate apportionments for the next five to six years and about any changes to apportionment formulas. Notice that this difference can be decomposed into errors in the forecast of current grants and revisions to forecasts of future grants:

$$E_t[PV_{i,t}] - E_{t-1}[PV_{i,t}] = \underbrace{(A_{i,t} - E_{t-1}[A_{i,t}])}_{\text{Error in Forecast of Current Spending}} + \underbrace{\sum_{s=1}^{\infty} \frac{E_t[A_{i,t+s}]}{(1+R_t)^s} - \sum_{s=1}^{\infty} \frac{E_{t-1}[A_{i,t+s}]}{(1+R_{t-1})^s}}_{\text{Revisions to Forecast of Future Spending}}.$$

This decomposition highlights an important difference between our shock measure and the government spending shock measures used in some other studies, such as Auerbach and Gorodnichenko (2011) or Clemens and Miran (2012), which are constructed from one-period-ahead forecast errors. Forecast errors potentially miss important additional news received by agents at date t about spending more than one period ahead. For certain types of spending with long forecast horizons,

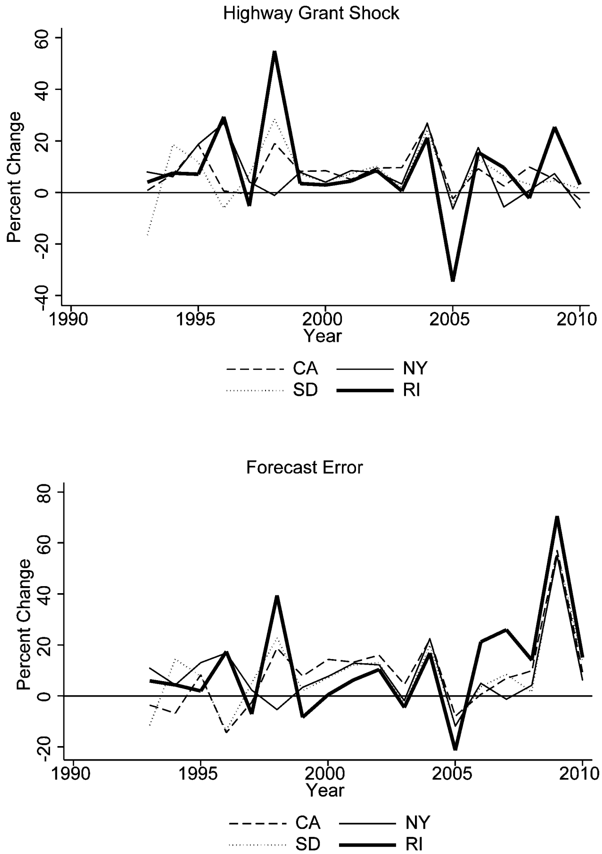

Fig. 2. Unanticipated change in expected present value of highway grants in selected states

such as highway spending, revisions to forecasts of future spending are likely to be important.

We convert these dollar-value shocks into percentage terms (to be comparable across states) using the symmetric percentage formula such that positive and negative shocks of equal dollar amounts are treated symmetrically:

$$shock_t = \frac{E_t[PV_{i,t}] - E_{t-1}[PV_{i,t}]}{(0.5 \times E_t[PV_{i,t}] + 0.5 \times E_{t-1}[PV_{i,t}])}. \tag{3}$$

To get a sense for what these shocks look like over time and states, in figure 2 we plot $shock_t$ for a selection of states over the time period covered by our data. We include in our data a couple of states with large populations (California, CA; New York, NY), a state with large area but small population (South Dakota, SD), and a state with small area and small population (Rhode Island, RI). There is considerable variation over both time and space. As expected, there are large shocks in the first years of appropriations bills—1998 and 2005. But there also are some large shocks in other years, such as 1996 and 2004. There are no obvious differences in volatility relating to state size or population. For instance, Rhode Island tends to experience large shocks but Delaware (not shown) does not.

IV. Results: The Dynamic Effects of Highway Spending Shocks on GDP

A. Estimation Technique

Our objective in this section is to use our measure of highway spending shocks to estimate the dynamic effects of highway spending on GDP. Our empirical methodology uses the Jordà (2005) direct projections approach to estimate impulse response functions (IRFs) extended to a panel context. This approach was also used recently by Auerbach and Gorodnichenko (2011) in their study of the dynamic effects of government spending, using panel data on OECD countries. The basic specification is:

$$y_{i,t+h} = \alpha_i^h + \alpha_t^h + \sum_{s=1}^{p} \beta_s^h y_{i,t-s} + \sum_{s=1}^{q} \gamma_s^h g_{i,t-s} + \delta^h \cdot shock_{it} + \varepsilon_{i,t+h}, \qquad (4)$$

where $y_{i,t}$ and $g_{i,t}$ are the logarithms of GDP and government highway spending, respectively, for state i in year t, and $shock_{it}$ is the government highway spending shock defined earlier. The parameter δ^h identifies the IRF at horizon h. Equation 4) is estimated separately for each horizon h. Lags of output and highway spending are included to control for any additional forecastability or anticipation of highway apportionment changes missed by our forecasting approach that generates $shock_t$. We use (log) state federal-aid highway obligations to measure $g_{i,t-s}$ (though using other measures of state highway spending yield similar results). We set $p = q = 3$, but find the results to be robust to alternative lag lengths, including $p = q = 0$, as we show in the following robustness checks.

The inclusion of state and time fixed effects are important for identification and warrant further discussion. The previous literature estimating the dynamic effects of government spending generally has omitted aggregate time fixed effects. This omission likely is due to the difficulty in a dynamic time series model, such as a direct projection or a vector autoregression, of separately identifying a time trend or time fixed effects from the parameters describing the dynamics of the model. The advantage of estimating a dynamic model with panel data is that it allows one to control for aggregate time effects. This is potentially important when estimating the impact of government spending as it allows one to control for other national macroeconomic factors, particularly monetary policy and federal tax policy, that are likely to be correlated over time (but not over states) with government spending.

Notice, however, that by sweeping out any potential effect of federal tax policy, we effectively are removing any negative wealth (Ricardian) effects on output associated with agents expecting increases in government spending to be financed by current and future increases in federal taxes. In other words, to the extent that increases in state government spending are paid for with federal transfers, this spending is "windfall-financed" rather than "deficit-financed" (see Clemons and Miran 2012). In reality, state government highway spending, even on federal-aid highways, is part windfall-financed—because it is partially reimbursed by federal transfers—and partially deficit-financed—both because of the matching requirements for states to receive the transfers and because even reimbursable outlays on federal-aid highways necessitates additional nonreimbursable expenditures such as police services, traffic control, snow and debris removal, future maintenance, and so forth. Our estimated IRFs will reflect any wealth effects from state deficit financing of matching requirements and nonreimbursable spending, but not wealth effects from the federal government's fiscal policy.

The state fixed effects in equation (4) control for state-specific time trends. Level differences between states in the dependent variable are already removed by the inclusion of a lagged dependent variable on the right-hand side. This can be seen by subtracting the lagged dependent variable from both sides,

$$y_{i,t+h} - y_{i,t-1} = \alpha_i^h + \alpha_t^h + (\beta_1^h - 1)y_{i,t-1} + \sum_{s=2}^{p} \beta_s^h y_{i,t-s} + \sum_{s=1}^{q} \gamma_s^h g_{i,t-s} + \delta^h \cdot shock_{it} + \varepsilon_{i,t+h}.$$

From this equation, it is clear that α_i^h represents the average $(h+1)$-year growth in y_i for state i over the sample. Controlling for such state-specific time trends is potentially important, as states that are growing faster than other states could continually receive higher-than-forecasted grant shares and hence persistently positive shocks. Thus, state-specific shocks could be positively correlated with state-specific trends, and omitting such trends could lead to a positive bias on the impulse response coefficients.

This equation also shows that, if one were willing to assume a constant linear annual growth rate for each state, a more efficient estimator could be achieved by imposing the constraint that $\alpha_i^h = \alpha_i(h + 1)$. For instance, one could estimate the state-specific time trend, α_i, from the $h = 0$ regression, which uses the maximum number of observations, and then subtract this estimated parameter from the dependent variable for the other horizon regressions. We found that imposing this constraint led to only a very small narrowing of the confidence interval around the impulse response estimates (and virtually no effect on the IRF itself). Hence, the regressions presented following do not impose this constraint. Because $shock_{it}$ is constructed to be exogenous and unanticipated, the equation can be estimated via ordinary least squares (OLS). However, because the equation contains lags of the dependent variable, the error term is expected to be serially correlated. For this reason, we allow for arbitrary serial correlation by allowing the covariance matrix to be clustered within state.

How does our methodology for estimating IRFs differ from that derived from a VAR? Mechanically, the differences are that (1) the direct projections methodology does not require the simultaneous estimation of the full system (e.g., a three-variable VAR consisting of GDP, highway spending, and the grants shock) to obtain consistent estimates of the IRF of interest (e.g., GDP); and (2) the direct projections methodology estimates the underlying forecasting model separately for each horizon. This methodology offers a number of advantages, particularly in our context, over the recursive-iteration methodology for obtaining impulse responses from an estimated VAR (see Jordà [2005] for discussion). First, direct projections are more robust to misspecification, such as too few lags in the model or omitted endogenous variables from the system. The IRF from a VAR is obtained by recursively iterating on the estimated one-period ahead forecasting model. Thus, as Jordà puts it, this IRF is a function of forecasts at increasingly distant horizons, and therefore misspecification errors are compounded with the forecast horizon. This is a particular concern in our context given that public infrastructure spending, by its nature, may have real effects many years into the future. By directly estimating the impulse response at each forecast horizon separately, the direct projections approach avoids this compounding problem.

Second, the confidence intervals from the direct projections IRF are based on standard variance-covariance (VC) estimators and hence can easily accommodate clustering, heteroskedasticity, and other deviations from the OLS VC estimator, whereas standard errors for VAR-based IRFs must be computed using delta-method approximations or bootstrapping, which can be problematic in small samples. Third, the direct projections approach can easily be expanded to allow for nonlinear impulse responses (for instance, allowing shocks in recessions to have different effects than shocks in expansions, as we explore later). To assess the sensitivity of our results to using the direct projections approach, we also have estimated the GDP impulse response from two alternative estimators: a three-variable (GDP, highway spending, and our shock) panel VAR and a distributed-lag model. We discuss the results in the following.

B. *Baseline Results*

We estimate equation (4) using state panel data from 1990 to 2010. The shock variable is only available for years 1993–2010, but the regressions use three lags of spending (obligations) and GDP (or alternative dependent variables). We start by looking at the effects of our shock measure on GDP, before turning to other macroeconomic variables.

The baseline results are shown in table 2. Panel A of figure 3 displays the IRF—that is, the estimates of δ^h—for horizons $h = 0$ to ten years. The shaded band in the figure gives the 90 percent confidence interval. This IRF indicates that state highway spending shocks lead to a positive and statistically significant increase in state output on impact and one year out. The effect on output falls and becomes negative (though not statistically significantly) over the next few years but then increases sharply around six to eight years out, before fading back to zero by nine to ten years out.

In figure A1, we demonstrate the robustness of this baseline impulse response to a number of potential concerns one might have. Specifically, we find that the results are robust to (a) dropping lags of highway spending; (b) dropping all autoregressive terms; (c) controlling for an index of state leading indicators (from the Federal Reserve Bank of Philadelphia) in case the grant shock is affected by state expected future

Table 2
Response of GDP to Highway Grant Shock

Dependent Variable	Shock Variable β/SE	GDP_{t-1} β/*SE*	GDP_{t-2} β/*SE*	GDP_{t-3} β/*SE*	$Obligations_{t-1}$ β/*SE*	$Obligations_{t-2}$ β/*SE*	$Obligations_{t-3}$ β/*SE*	N
GDP_t	**0.012**	**1.044**	0.001	**–0.152**	–0.003	–0.003	–0.002	882
	(0.005)	**(0.043)**	(0.079)	**(0.056)**	(0.008)	(0.004)	(0.006)	
GDP_{t+1}	**0.014**	**1.092**	**–0.199**	–0.112	–0.006	–0.008	0.001	833
	(0.008)	**(0.077)**	**(0.076)**	(0.087)	(0.011)	(0.007)	(0.007)	
GDP_{t+2}	–0.008	**0.861**	–0.145	–0.055	–0.007	–0.006	–0.000	784
	(0.008)	**(0.115)**	(0.092)	(0.093)	(0.008)	(0.007)	(0.013)	
GDP_{t+3}	–0.015	**0.661**	–0.125	0.018	–0.005	–0.012	0.005	735
	(0.010)	**(0.112)**	(0.076)	(0.111)	(0.009)	(0.011)	(0.016)	
GDP_{t+4}	–0.007	**0.451**	0.037	–0.032	–0.007	–0.003	0.007	686
	(0.009)	**(0.124)**	(0.078)	(0.101)	(0.012)	(0.012)	(0.017)	
GDP_{t+5}	0.008	**0.396**	–0.009	–0.009	0.006	0.000	–0.006	637
	(0.008)	**(0.121)**	(0.104)	(0.095)	(0.013)	(0.014)	(0.016)	
GDP_{t+6}	**0.026**	**0.297**	0.092	–0.089	0.016	–0.010	–0.004	588
	(0.009)	**(0.112)**	(0.086)	(0.104)	(0.016)	(0.013)	(0.016)	
GDP_{t+7}	**0.024**	**0.345**	**–0.152**	0.063	0.007	–0.007	–0.003	539
	(0.008)	**(0.130)**	**(0.072)**	(0.093)	(0.016)	(0.014)	(0.017)	
GDP_{t+8}	**0.011**	**0.223**	–0.097	0.100	–0.002	–0.008	0.004	490
	(0.005)	**(0.127)**	(0.103)	(0.088)	(0.016)	(0.016)	(0.017)	
GDP_{t+9}	0.001	0.150	–0.074	0.106	–0.009	0.002	0.002	441
	(0.006)	(0.115)	(0.076)	(0.088)	(0.018)	(0.014)	(0.015)	
GDP_{t+10}	–0.005	0.105	–0.100	0.130	0.001	0.001	0.004	392
	(0.006)	(0.141)	(0.153)	(0.098)	(0.018)	(0.015)	(0.015)	

Notes: Bold indicates significance at 10 percent level. All regressions include state and year fixed effects. Sample covers years 1993 to 2010 and all fifty states except Alaska. Variables are in logs.

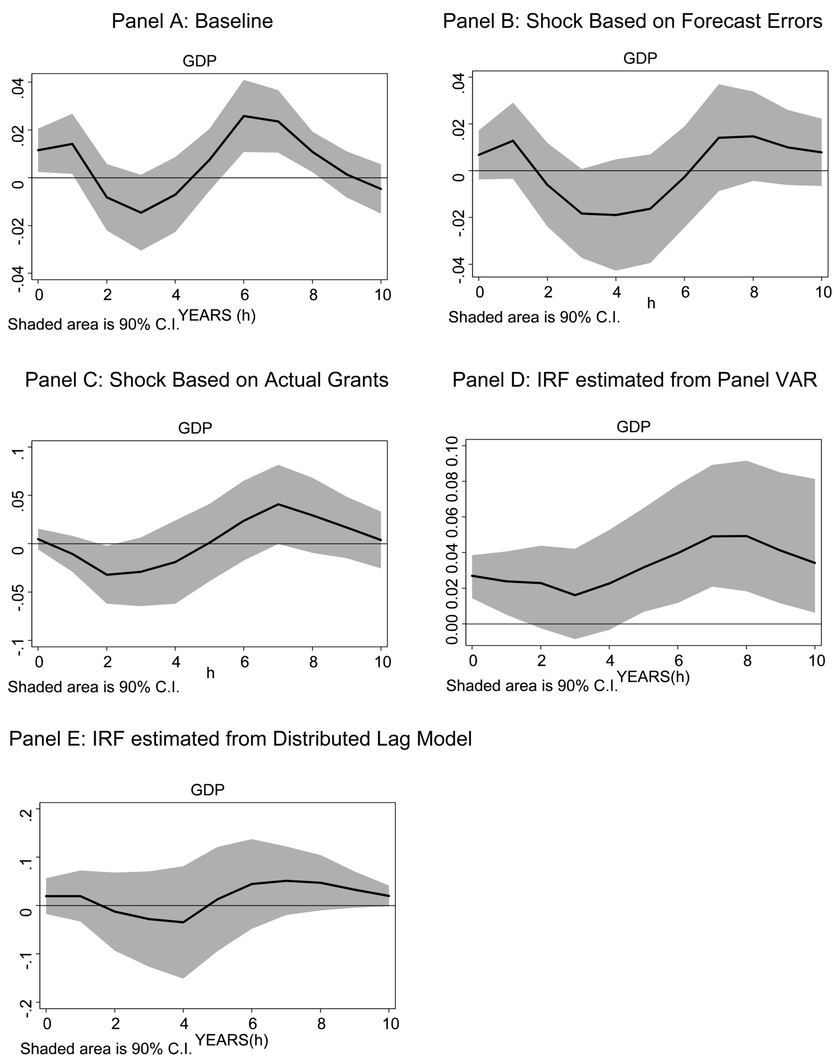

Fig. 3. Alternative estimates of GDP response to highway grant shocks

Notes: Panel A IRF based on direct projections estimator and our highway grant shock. Panel B replaces our shock with one-year ahead forecast error. Panel C replaces our shock with actual grants change. Panel D based on panel VAR IRF estimator and our shock. Panel E based on distributed lag model IRF estimator and our shock. GDP measured in logs. Regressions control for state and year fixed effects.

output; (d) excluding the years 1998 and 2005 in case shocks in the year a highway bill is adopted are endogenous to states' political influence, as states with more political and economic clout could influence the design of apportionment formulas to favor their states[16]; (e) considering only the early part of our sample (1993 to 2004); and (f) considering only the later part of our sample (1999 to 2010).

In figure 3, panels B and C show the estimated GDP impulse response functions based on two alternative identification strategies. Panel B shows the results if we measure the $shock_{it}$ variable using only one-year-ahead forecast errors of current grants.[17] As mentioned in the previous section, this shock measure should accurately capture the timing of actual news about government spending but may not fully capture the quantity of that news. In particular, some forecast errors may reflect transitory shocks to government spending, while other forecast errors may reflect more persistent shocks that would prompt agents to revise their forecasts of future spending. The current-year spending forecast errors will not differentiate between these two types of shocks. Panel B shows that the IRF obtained from using forecast errors has a similar shape to the baseline IRF (panel A), except that the peak response is smaller and occurs one year later and the GDP response is still positive by the end of the eleven-year window. This suggests that accounting for revisions in forecasts of future spending may not be crucial for estimating short-run effects but can be quite important for estimating longer-run effects. In addition, the IRF based on forecast errors is estimated much less precisely.

Panel C shows the results from following the traditional structural VAR type of identification strategy à la Blanchard and Perotti (2002) or Pereira (2000). Specifically, we replace $shock_{it}$ with current grants in equation (4). Identification here rests on the assumption that the unforecastable component of grants—obtained by controlling for lags of GDP and highway spending (obligations)—can contemporaneously affect GDP but not vice versa. In other words, this is just the direct projections counterpart to the standard SVAR approach to estimating fiscal policy IRFs. This approach may potentially miss the fact that grants—even conditional on past GDP and spending—may be anticipated to some extent years in advance and hence will not accurately reflect the timing of news. Panel C shows that the resulting IRF has similar longer-run responses to our baseline IRF but essentially no short-run impact. This may be because agents previously anticipated the shock and hence responded in earlier periods.[18]

We now turn to assessing the sensitivity of our results to the methodology for estimating the IRF, essentially holding fixed the identification of the shock. Specifically, we estimate impulse responses using two alternative methodologies to the direct projections approach: a three-variable (GDP, highway spending, and our shock measure) panel VAR with six lags and a distributed lag model similar to that used in Romer and Romer (2010). For the panel VAR, the IRF is estimated by recursive iteration on the estimated VAR and standard errors are obtained by bootstrapping. The distributed lag model simply regresses log GDP on zero to ten lags of the shock variable. The implied IRF is simply the coefficients on these lags.

The results are shown in panels D and E of figure 3. Compared with the direct projections baseline, the panel VAR implies more positive responses throughout the forecast horizon while the distributed lag model implies a larger confidence interval. Both, however, yield the same up-down-up-down IRF as that obtained by direct projections, indicating that this pattern is not an artifact of the direct projection methodology. It is worth noting, though, that the IRF obtained from the panel VAR is quite sensitive to the number of lags included in the VAR. When we estimate the IRF from a panel VAR with, for example, three lags (mirroring the three lags of GDP and obligations in our baseline direct projections model), GDP shows an initial positive boost before falling and staying negative (though not statistically significantly so) through the end of the eleven-year horizon. This sensitivity of VAR-based IRFs to misspecification from omitting relevant lags parallels Jordà's (2005) Monte Carlo results showing that VAR-based IRFs can be very sensitive to lag length misspecification, unlike those based on direction projections.

We now turn to estimating the impulse responses of other macroeconomic variables to the highway grants shock. Figure 4 shows the estimate IRFs for GDP per worker, employment (number of workers by state of employment), personal income, wages and salaries, the unemployment rate, and population.[19] The impulse responses for the first five variables have more or less the same shape as the GDP response. The initial impact, however, is small and insignificant for employment, unemployment, and wages and salaries.[20] All five variables exhibit a positive and significant response around six to eight years followed by a return to preshock levels. Interestingly, population is the only variable that appears to be permanently affected by the highway shock. A natural interpretation of this result is that highway/road improvements enable population growth as, for example, new housing developments are built around new or improved roads and as new commuting options

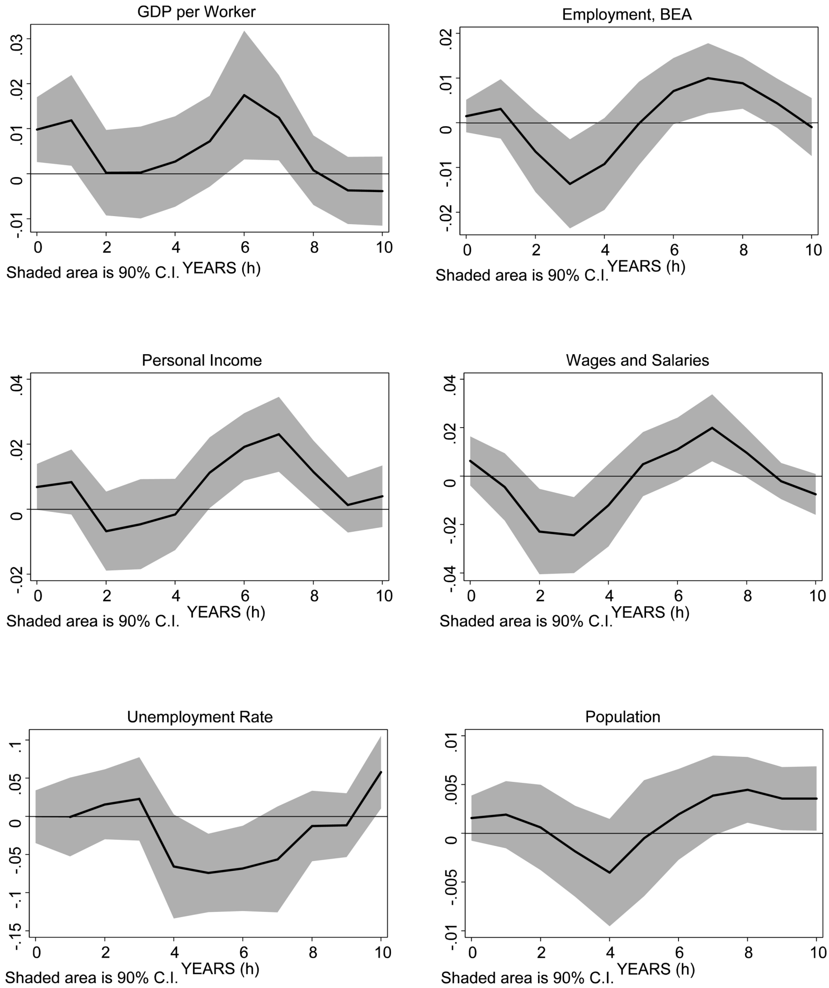

Fig. 4. Additional macroeconomic variables

are made possible. Such a response is consistent with the Duranton and Turner (2011) finding in that increases in a state's road lane-miles cause proportionate increases in vehicle miles traveled.

C. Transmission Mechanism

What explains these macroeconomic responses? In this subsection, we first look at the responses of variables that could be *directly* affected by a highway grant shock, as opposed to indirectly affected through general

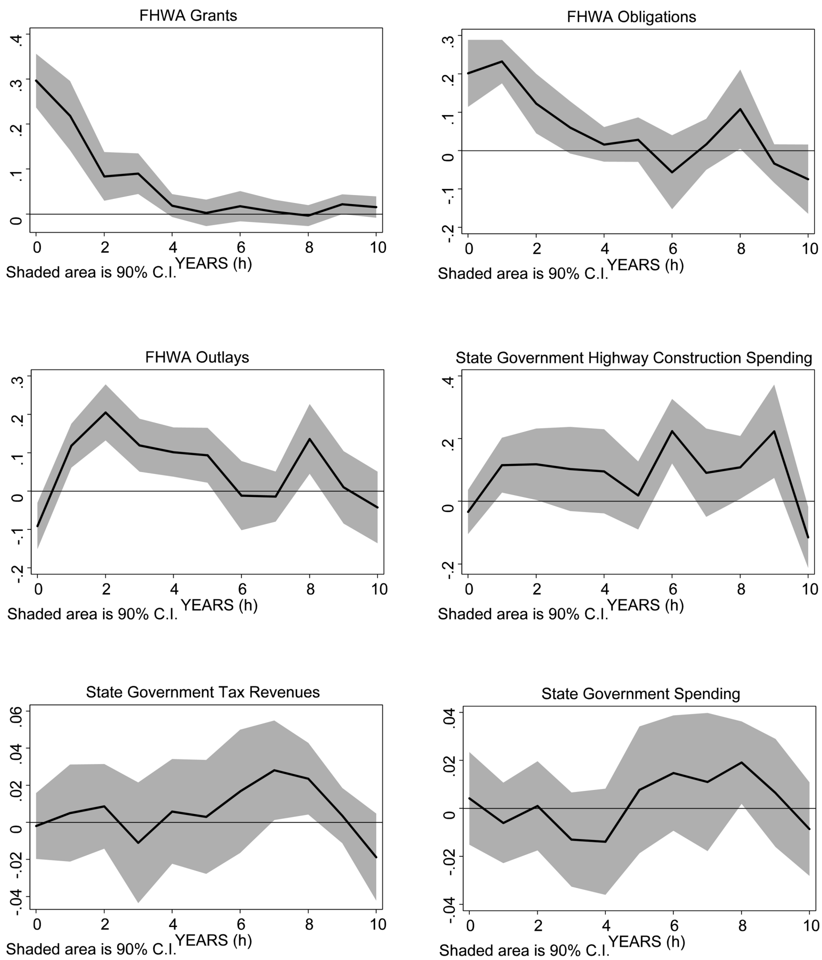

Fig. 5. State fiscal variables

equilibrium channels, to begin to formulate a general explanation of the macroeconomic effects of highway grants. We thus look at the response of actual grants, obligations, and outlays on federal-aid highways. We analyzed the relationships of these three variables in section III, and the results are shown in figure 5. Not surprisingly, an unanticipated shock to expectations of current and future grants is in fact followed by actual increases in grants immediately and up to four years out. This is also consistent with the fact that grants become increasingly difficult to forecast as the forecast horizon goes beyond six or more years, which is the typical length of a highway bill. Obligations also increase for the first three to four years after the shock and also appear to rise again eight years out. Outlays actually fall on impact but then are higher for years $t + 1$ to $t + 5$ and again at $t + 8$.

These patterns are consistent with the notion that a shock to expected future grants leads to initiation of actual highway projects—and hence obligations—over the next three to four years, which with some lag leads to project completions and hence outlays. This interpretation is supported by the response of state government total highway construction spending (total, not just on federal-aid roads), which is also shown in figure 5. State highway construction spending increases from years $t + 1$ to $t + 4$ (though it is only statistically significant for $t + 1$) and then rises again around $t + 6$ to $t + 9$. This latter increase in state highway spending could reflect improved state finances due to higher overall economic activity. Indeed, as shown in the bottom two panels of figure 5, state government tax revenues and overall state government spending are found to be higher around seven to eight years after an initial highway grant shock.

Combining these results with the macroeconomic responses in figure 4, particularly the increase in GDP per worker six to eight years after the shock, the results point to a possible productivity effect of improved highway infrastructure. Under this interpretation of our results, an initial shock to federal grants leads to highway construction activity over the following three to five years and results in new (or improved) highway capital put in place around six to eight years out. In turn, the new highway capital triggers higher productivity in transportation-intensive sectors, reducing goods prices and boosting demand. Ultimately, the increase in economic activity raises state tax revenues and increases state government spending as a result.

To dig deeper into this interpretation of our results, we examine whether transportation-intensive sectors do in fact experience a boost in activity around the time new highway capital would be coming online by estimating the response of GDP in the truck transportation sector to our shock measure. The results are shown in figure 6. Consistent with the response of overall GDP, we find a small initial response, which is followed by a very large second-round effect five to six years out, in line with the view that completed highway projects would directly benefit the local truck transportation sector. Similarly, the response of retail sales shown in figure 6 also rises when highway projects are likely completed, six to seven years after a shock to federal grants.[21] The increase in retail sales likely also reflects higher overall consumption occurring

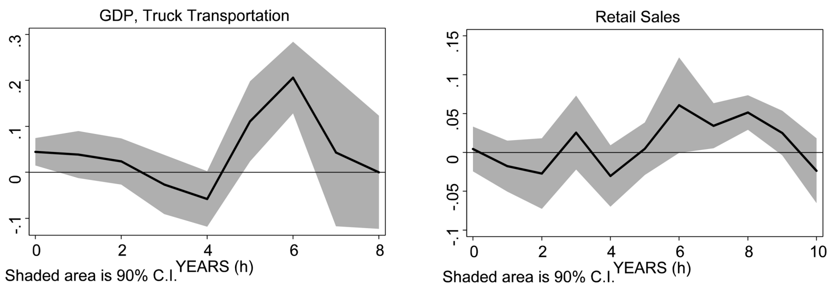

Fig. 6. Additional outcomes

in tandem with the increase in GDP, personal income, wages and salaries, and other macroeconomic variables.

D. The GDP Multiplier

How large are our baseline GDP effects? The impulse response estimates, δ^h, represent the percentage change in GDP with respect to a one-unit change in $shock_{it}$. The common practice in the literature for converting such percentage responses into dollar multipliers is to first normalize the GDP responses such that a one-unit change in the shock represents a 1 percent change in government spending. One can then multiply the resulting elasticity by the average ratio of GDP to highway spending in the sample to obtain a multiplier. However, it is not always clear in such an exercise which measure of spending to use, especially in a context like ours where there are multiple concepts of highway spending that one might consider. Here, we report multipliers based on a range of alternatives. For each alternative, we report the multiplier on impact, the peak multiplier, and the mean multiplier. If one measures highway spending using only FHWA grants (or obligations), the multiplier on impact is about 3.4, the peak multiplier (at six years out) is 7.8, and the mean multiplier is 1.7.[22] These multipliers may well be unrealistically large in that a shock to current and future grants may fail to reflect broader changes to government highway spending. For instance, highway grants for federal-aid highways may lead to subsequent expenditures by state and local governments on local roads, traffic control, highway police services, and so forth. The extent to which federal transfers to local governments earmarked for a specific purpose actually increase spending by regional governments on that purpose is known as the flypaper effect.[23]

If one uses a broader measure of highway spending, such as state government outlays on highway construction, the implied multipliers are smaller but still large. The impact multiplier would be 2.7, the peak multiplier 6.2, and the mean multiplier 1.3.[24] One might also consider using an even broader measure, like state government spending for all road-related activities. However, while such spending represents a larger fraction of GDP than the other measures, we obtain a much smaller (and imprecisely estimated) response of total road spending to the grant's shock.[25] Nonetheless, if one allows for the possibility that a shock-induced rise in grants lead to a proportional rise in total state government road spending, our estimated responses multiplied by the average ratio of GDP to road spending provide a lower bound on the impact multiplier of 1.4,the peak multiplier of 3.0, and the mean multiplier of 0.6. The bottom line is that, based on the most sensible measures of government highway infrastructure investment, the GDP multiplier implied by our estimated impulse responses appear to be considerably larger than those based on defense or overall government spending, as estimated in previous studies.

E. Extensions

Impact of Highway Spending Shocks in Expansions versus Recessions

In this subsection, we report the results of a number of interesting extensions of the baseline results. First, we explore whether the effects of government highway spending are different depending on whether the shock occurs in an expansion or a recession. We follow the approach of Auerbach and Gorodnichenko (2011), which involves calculating the probability of being in an expansion (vs. recession), based on a regime-switching model, and interacting that probability with the right-hand side variables in the direct projection regressions (equation [4]). Expansions and recessions here are local (state-specific). As in Auerbach and Gorodnichenko, we first calculate for each state and year the deviation of real GDP growth from the state's long-run trend (estimated from a Hodrick–Prescott [HP] filter with a high smoothing parameter of 10,000). We then take a logistic transformation of that variable to map it onto the [0,1] range. The IRF of output with respect to highway spending shocks during an expansion is given by the coefficient, for each horizon h, on the interaction between the shock and the expansion probability.[26] Conversely, the IRF during a recession is given by the

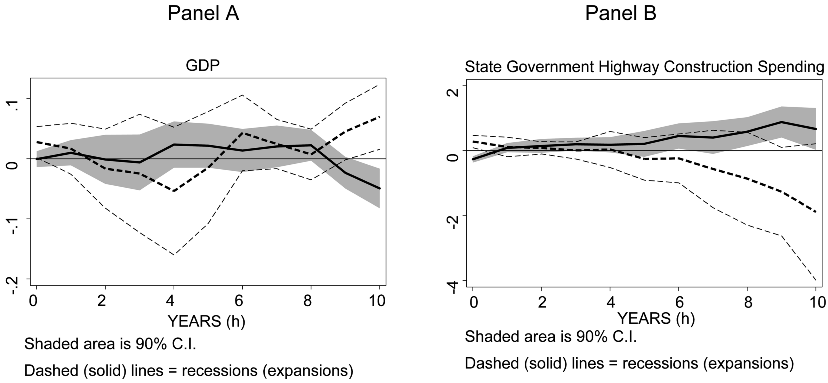

Fig. 7. GDP in recessions vs. expansions

coefficient, for each horizon, on the interaction between the shock and one minus the expansion probability. Note that because the regression controls for aggregate time fixed effects, the identifying variation for our IRFs is states' expansion probabilities relative to the national business cycle. Also note that the use of the direct projections approach, as opposed to a nonlinear VAR as in Auerbach and Gorodnichenko (2012), does not require an assumption that the local economy remains in the same regime throughout the interval t to $t + h$.[27] The direct projections approach simply estimates the conditional mean of GDP h years after a shock that occurs in a recession (or expansion). The fact that GDP typically exits recession within a year or two will not affect this conditional mean because we control for the recession probability term separately from the interaction of that probability with the shock. Moreover, if the shock itself helps push a local economy out of recession, this will be reflected in the impulse response function.

The results are shown in figure 7. The left panel shows the results for (log) real GDP, while the right panel shows the results for state government highway construction spending. The dashed lines in each panel show the impulse response function (and 90 percent confidence interval) with respect to shocks occurring during recessions; the solid lines show the IRF with respect to shocks occurring during expansions. Interestingly, the initial impact of highway spending shocks are much larger for both GDP and highway spending when they occur in state-years experiencing a recession. The impact GDP elasticity in recessions is 0.028 (standard error = 0.015), which is statistically significant at the 10 percent level and about twice as large as the average impact response (as found in our baseline regressions in table 2). The impact GDP elasticity in expansions, on the other hand, is slightly below zero and statistically insignificant. After the initial shock, the output response from shocks hitting during recessions falls and becomes statistically insignificant. For shocks hitting during expansions, the output response grows slightly over time but remains statistically insignificant. There is a significant increase in GDP at $t + 10$ for recessions and a significant decrease at $t + 10$ for expansions. Overall, these results suggest that the initial positive impact of highway spending shocks found in the baseline results is driven by the large effect on such spending in recessions, while the second-round positive effects coming six to eight years later may be independent of the business cycle conditions at the time of the shock.[28]

Fast-Growing versus Slow-Growing States

The previous results suggest that the initial impact of news about current and future highway spending depends upon the overall level of slack in the economy. Relatedly, the effects of such shocks may also depend on the slack, or capacity utilization, of the existing transportation infrastructure. In particular, do highway spending shocks have more beneficial effects in states that are growing fast, and hence are more likely to face transportation capacity/congestion constraints, than in slower-growing states where current road capacity may already be underutilized? To answer this question, we split states according to whether their 1977 to 1992 (i.e., pre-regression sample) real GDP trend growth rate was above or below the median. We then interact the above-median-growth indicator with the highway shock variable in the direct projection regressions. The estimated IRFs we obtain for fast- and slow-growing states are shown in figure 8. The dashed line corresponds to fast-growing states; the solid line corresponds to slow-growing states. The estimates broadly support the notion that transportation infrastructure improvements have more beneficial effects in regions that are already growing rapidly. In particular, we find that while the initial impact of the highway grant shock is the same for fast- and slow-growing states (positive but not quite significant), the GDP response in slow-growing states is negative and significant two to three years after the shock before becoming positive and significant six to seven years out (and then fading away), as in our baseline case. In con-

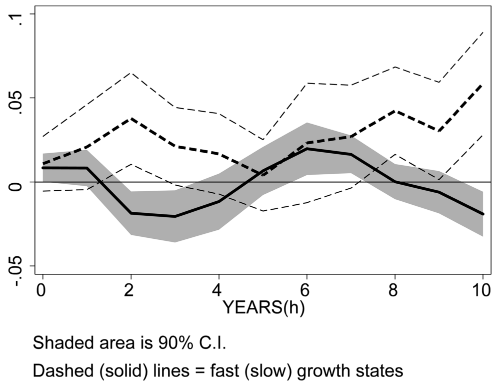

Fig. 8. GDP in fast- vs. slow-growing states

trast, the response in fast-growing states is positive at all horizons and generally larger and more statistically significant than in slow-growing states. These results imply that, in general, highway spending may be more effective, at least in the short-run, as a facilitator of strong economic growth rather than a boost to weak growth.

The 2009 American Recovery and Reinvestment Act and the Great Recession

The 2008 to 2009 severe recession (and subsequent weak recovery) and the large onetime increase in federal highway grants from the 2009 American Recovery and Reinvestment Act (ARRA) suggest that the response of local economic activity to government highway spending may have been different over this time period than the usual response. First, we ask whether the effect of highway grants on local GDP was unusually large during the Great Recession. We investigate this by extending our baseline direct projections regressions (equation [4]) by interacting the shock with year dummies. As we only have data through 2010, we focus here on the contemporaneous and one-year-ahead responses. The estimated impulse response coefficients by year are shown in panel A of table 3. We find that both the contemporaneous and year-ahead effects on GDP were significantly higher from highway shocks in 2009 than the average effect over the 1993 to 2010 sample (0.012 from table 2). We also find other years that have significantly different effects than the average: highway shocks in 2000 also had large positive

Table 3
GDP Impulse Response, by Year

Year	Contemporaneous β/SE	One-Year Ahead β/SE
	A. Total Highway Grant Shock	
1993	.014	.002
	(.019)	(.027)
1994	.000	.055
	(.053)	(.075)
1995	.009	.005
	(.019)	(.027)
1996	.011	.022
	(.013)	(.019)
1997	–.050	–.048
	(.035)	(.049)
1998	.012	.023
	(.012)	(.017)
1999	–.055	.003
	(.012)	(.076)
2000	**.146**	**.233**
	(.073)	**(.102)**
2001	**–.221**	–.213
	(.107)	(.151)
2002	–.057	–.125
	(.086)	(.121)
2003	–.009	–.041
	(.034)	(.048)
2004	.041	.129
	(.096)	(.135)
2005	.011	–.001
	(.019)	(.027)
2006	**–.077**	**–.104**
	(.039)	**(.056)**
2007	.035	.045
	(.040)	(.057)
2008	–.040	–.162
	(.072)	(.101)
2009	**.110**	**.122**
	(.028)	**(.040)**
2010	–.007	—
	(.063)	—
	B. ARRA Grant Shock vs. non-ARRA Grant Shock	
2009 ARRA	**.033**	**.032**
	(.006)	**(.009)**
2009 Non-ARRA	**.067**	**.083**
	(.029)	**(.041)**
2010 ARRA	–.004	—
	(.004)	—
2010 Non-ARRA	–.016	—
	(.063)	—

Notes: Bold indicates significance at 10 percent level. All variables are per capita.

effects, while shocks in 2001 and 2006 had negative effects. Notice that these effects cannot simply be explained by national cyclical conditions because national conditions are swept out by the aggregate time fixed effects. Rather, these results indicate that local GDP relative to national GDP was affected more by highway grant shocks in 2000, 2001, 2006, and 2009 than in other years. This could, for instance, be due to differences in the nature or composition of highway grants in different years.

Of course, 2009 was an atypical year not just because of the severe recession, but also because of the extraordinary fiscal and monetary policy actions taking place. In particular, the American Recovery and Reinvestment Act enacted in February 2009 authorized a very large onetime increase of $27.5 billion in highway grants. Because the act was designed to provide short-term economic stimulus, ARRA stipulated that these grants had to be entirely obligated by March 2010. Therefore, the ARRA grants typically were used by state governments for projects involving shorter planning and construction horizons than were non-ARRA grants. It is quite possible that such shorter-horizon projects have different effects on GDP than longer-horizon projects.

To assess this further, we separated out the ARRA grants from the non-ARRA grants in our construction of the expected present value of current and future grants (see equation [2]) to obtain an ARRA grants shock and a non-ARRA grants shock. The bulk of ARRA grants were apportioned in fiscal year 2009, but some were also apportioned in fiscal year 2010 (October 2009 through September 2010). We then extended the regression underlying panel A by replacing the overall shock (interacted with year dummies) with these two separate shocks (interacted with year dummies). Of course, in years prior to 2009, the non-ARRA shock is just the overall shock and the ARRA shock is zero. The results are shown in panel B of table 3. We find that a state with 10 percent higher 2009 ARRA grants than the national average saw 0.33 percent higher GDP in 2009 and 0.32 percent higher GDP in 2010. A state with 10 percent higher non-ARRA grants in 2009 saw 0.67 percent higher GDP in 2009 and 0.83 percent higher GDP in 2010. Both types of grants appear to have had no contemporaneous impact in 2010. Given that the ratio of non-ARRA grants to ARRA grants in 2009 was about 2.8, the estimated multiplier on a dollar of ARRA grants is just slightly higher than that of non-ARRA grants. Thus, we find that the ARRA grants did have a significantly positive effect on state economies and that the effect of a dollar of ARRA grants was not materially different from the effect of a dollar of ordinary federal highway grants.

V. Theory: Multipliers in a Model with Productive Public Capital

In this section we turn to assessing the impact of public infrastructure investment in a theoretical framework with productive public capital. Our model is relatively standard and contains many features that have proven useful in addressing the macroeconomic impact of fiscal policy (see Baxter and King [1993], or the more recent analysis of Leeper, Walker, and Yang [2010] and Uhlig [2010], using closed economy models, and Corsetti, Kuester, and Müller [2011], in the context of a small open economy). In line with our empirical framework and in the spirit of Nakamura and Steinsson (2011), we conduct our analysis in a monetary union using an open economy model, which allows us to remove the effects of aggregate shocks and monetary policy, as well as federal fiscal policy on the local fiscal multiplier.

We consider a cashless national economy consisting of two regions, H and F, of possibly different sizes, n and $1 - n$. The national government invests in public infrastructure projects in the two regions and finances these investments by levying taxes. Each region specializes in one type of tradable good, produced in a number of varieties or brands, defined over a continuum of unit mass. Firms are monopolistic suppliers that combine private and public capital with domestic labor to produce one brand of goods. Throughout the section, we assume complete financial markets.

We first provide a description of the households and the behavior of the monetary and fiscal authorities, before presenting the firms' problem.

A. Households

The Home region is populated by a continuum of infinitely lived households who choose a consumption basket, C_t, and hours worked, L_t, to maximize the expected value of their lifetime utility given by

$$E_0 \sum_{t=0}^{\infty} \beta^t U(C_t, L_t), \tag{5}$$

where β denotes the agent's subjective discount factor.[29] Home households consume all the different types of traded goods produced in the two regions, with $C_t(h)$ representing the consumption of the Home region's brand h at time t, while $C_t(f)$ is the consumption of the Foreign region's brand f. For each type of good, we assume that one brand is an imperfect substitute for all other brands produced in the same region, with constant elasticity of substitution (CES) θ. Consumption of Home and Foreign goods by the Home agent is defined as:

$$C_{H,t} \equiv \left[\int_0^1 C_t(h)^{(\theta-1)/\theta} dh\right]^{\theta/(\theta-1)}, \qquad C_{F,t} \equiv \left[\int_0^1 C_t(f)^{(\theta-1)/\theta} df\right]^{\theta/(\theta-1)}. \qquad (6)$$

In turn, Home households' full consumption basket is composed of the bundles of Home and Foreign produced goods defined by the following CES aggregator

$$C_t \equiv [a_H^{1/\eta} C_{H,t}^{(\eta-1)/\eta} + (1 - a_H)^{1/\eta} C_{F,t}^{(\eta-1)/\eta}]^{\eta/(\eta-1)}, \qquad \eta > 0, \qquad (7)$$

where a_H dictates the degree of home bias in consumption ($a_H = 0.5$ equates to no home bias) and where the elasticity of substitution between the consumption of Home goods and the consumption of imports is given by η. The price index associated with the consumption aggregator is given by

$$P_t \equiv [a_H P_{H,t}^{1-\eta} + (1 - a_H) P_{F,t}(j)^{1-\eta}]^{1/(1-\eta)}, \qquad (8)$$

where $P_{H,t}$ is the price subindex for Home-produced goods and $P_{F,t}$ is the price subindex for Foreign-produced goods, both expressed in the common national currency:

$$P_{H,t} \equiv \left[\int_0^1 P_t(h)^{1-\theta} dh\right]^{1/(1-\theta)}, \qquad P_{F,t} \equiv \left[\int_0^1 P_t(f)^{1-\theta} df\right]^{1/(1-\theta)}. \qquad (9)$$

The Home households derive income from working, $W_t L_t$, from renting capital to firms, $R_t K_t$, and from the state-contingent payoffs $B_t(s)$ in state of nature s. We assume that the profits of Home firms are rebated to Home households in the form of dividends, $\Pi(h)$.

In line with the spirit of highway infrastructure financing in the United States, our baseline model assumes that public infrastructure spending is financed with a consumption tax, τ^c.[30] That said, since 2005 every state received as much or more funding for highway programs than they contributed in highway taxes (see Government Accountability Office 2010). This reflects the fact that more funding has been authorized and apportioned to the states than funds in the HTF allowed, with the discrepancy paid for with general revenues. For simplicity, our baseline model abstracts from this possibility. Note, however, that our approach to calculating our theoretical multipliers follows our empirical approach and thus removes the effects of federal fiscal policy through the introduction of time fixed effects.

Households use their disposable income to consume, invest in domestic capital, and buy state-contingent assets $B_{t+1}(s)$, which pays one unit of Home consumption goods if a particular state of nature s occurs in period $t + 1$, at price $p_{bt,t+1}$. We assume that, as with aggregate consumption, aggregate private investment is a CES composite of Home and Foreign tradable goods with identical weight and elasticity. Private capital accumulates according to the following law of motion

$$K_{t+1} = (1 - \delta)K_t + I_t, \tag{10}$$

where δ denotes the depreciation rate. The individual flow budget constraint for the representative agent in the Home country is therefore:

$$\begin{aligned}(1 + \tau_t^c)(P_{\mathrm{H},t}C_{\mathrm{H},t} + P_{\mathrm{F},t}C_{\mathrm{F},t}) + P_t I_t + \textstyle\int_s p_{bt,t+1}B_{t+1}(s) \\ \leq W_t L_t + R_t K_t + B_t(s) + \textstyle\int_0^1 \Pi_t(h)dh - T_t.\end{aligned} \tag{11}$$

B. *Fiscal and Monetary Policies*

As discussed in section II, there can be long implementation lags between the time when government transportation spending is authorized and when actual outlays occur. Following Leeper, Walker, and Yang (2010), we capture this feature of government investment by assuming that only a fraction of authorized funds shows up as spending in a given year.

Let $A_{\mathrm{H},t}$ denote the federal grants per capita apportioned to region H at time t, which we assume follows an AR(1) process

$$A_{\mathrm{H},t} = (1 - \rho_A)\bar{A}_{\mathrm{H}} + \rho_A A_{\mathrm{H},t-1} + \varepsilon_{A,t}, \tag{12}$$

where $\bar{A}_{\mathrm{H}}$ is the steady-state level of region H's apportionments and $\varepsilon_{A,t}$ denotes an unanticipated shock. In turn, we denote per capita government infrastructure spending (by all levels of government, net of intergovernmental transfers) in the Home region by $I_{\mathrm{H},t}^G$ and assume that it evolves according to the following process

$$I_{\mathrm{H},t}^G = \sum_{n=0}^{N-1} \omega_n A_{\mathrm{H},t-n}, \tag{13}$$

where $\sum_{n=0}^{N-1} \omega_n = 1$. The spend-out rates—that is, the rate at which authorized funds will show up as government spending—is determined by ω_i, $i = 0, \ldots, N - 1$.

Because it may take time for public infrastructure projects to be completed, we introduce a time-to-build component by letting government funds apportioned at time t only impact the public capital stock J periods later:

$$K^G_{H,t+1} = (1-\delta^G)K^G_{H,t} + I^G_{H,t-J}. \tag{14}$$

We assume that public capital in a region is a composite good, given as a CES index of the differentiated goods in that region, and for simplicity we assume that the public investment index has the same form as the consumption index in (6)

$$I^G_{H,t} = \left[\int_0^1 I^G_t(h)^{(\theta-1)/\theta} dh\right]^{\theta/(\theta-1)}, \tag{15}$$

so that the government's demand for each type of differentiated good is given by

$$I^G_t(h) = \left(\frac{P_t(h)}{P_{H,t}}\right)^{-\theta} I^G_{H,t}, \qquad I^G_t(f) = \left(\frac{P_t(f)}{P_{F,t}}\right)^{-\theta} I^G_{F,t}. \tag{16}$$

Using consumption taxes to finance government purchases, the national government's budget constraint is

$$\tau^c_t(nP_tC_t + (1-n)P^*_tC^*_t) = nP_{H,t}I^G_{H,t} + (1-n)P^*_{F,t}I^G_{F,t}, \tag{17}$$

where asterisks denote foreign variables.

Similar to Nakamura and Steinsson (2011), monetary policy is set at the national level according to an interest rate rule that is a function of aggregate consumer price inflation, $\hat{\pi}^{ag}_t$, and aggregate output, $\hat{y}^{ag}_t$, given by

$$\hat{r}_t = \rho_R\hat{r}_{t-1} + \beta_\pi(1-\rho_R)\hat{\pi}^{ag}_t + \beta_y(1-\rho_R)\hat{y}^{ag}_t, \tag{18}$$

where hatted variables denote deviations from steady state and where aggregate inflation and aggregate output are weighted sums of respective variables in the Home and Foreign regions:

$$\hat{\pi}^{ag}_t = n\hat{\pi}_t + (1-n)\hat{\pi}^*_t \text{ and } \hat{y}^{ag}_t = n\hat{y}_t + (1-n)\hat{y}^*_t.$$

C. Firms' Problem

Firms producing Home tradables are monopolistic in producing their brand; they employ a technology that combines domestic labor with private and public capital inputs, according to the following Cobb–Douglas function:

$$Y_t(h) = L_t(h)^{\phi} K_t(h)^{1-\phi} K_t^G(h)^{\phi_g}, \tag{19}$$

where $K_{t-1}^G(h)$ is public capital used in the production of good h. A positive value of ϕ_g, the elasticity of output with respect to public capital, implies that the production function has increasing returns to scale, as in the analysis of Baxter and King (1993) and Leeper, Walker, and Yang (2010).[31]

We assume that there is no impediment to goods trade across regions, so that the law of one price holds. Moreover, in setting their prices, firms take into account the fact that, in any given period, there is a probability α that they will have to leave prices unchanged as in Calvo (1983). When they can reset their prices (which occurs with probability $1 - \alpha$), firms act to maximize the expected discounted sum of profits

$$\Pi_t(h) = E_t \left\{ \sum_{k=0}^{\infty} p_{bt,t+k} \alpha^k (P_t(h) Y_{t+k}(h) - MC_{t+k}(h) Y_{t+k}(h) \right\},$$

where MC_t is the firm's nominal marginal cost and where the firm's demand at time t is given by

$$Y_t(h) = \left(\frac{P_t(h)}{P_{H,t}} \right)^{-\theta} (nC_{H,t} + (1-n)C_{H,t}^* + nI_{H,t} + (1-n)I_{H,t}^* + nI_{H,t}^G).$$

D. Calibration

In our baseline calibration, we parameterize the size of the Home country, n, to 1/50 to correspond to a US state in our empirical data set. We use the following preferences

$$U(C_t, L_t) = \frac{C_t^{1-\sigma}}{1-\sigma} - \psi \frac{L_t^{1+\xi}}{1+\xi},$$

and set the coefficient of relative risk aversion, σ, to 1 and the value of ξ to imply a Frisch labor elasticity of 0.75. As an alternative, we also consider the preferences in Greenwood, Hercowitz, and Huffman (1988), which have been used to study the effects of fiscal policy (see, among others, Monacelli and Perotti 2008 and Nakamura and Steinsson 2011). We calibrate the model to an annual frequency and set the discount factor, β, to 0.96. To determine the value of the elasticity of substitution across goods' varieties we use a markup of 20 percent in steady state, implying that $\theta = 6$.

The extent to which regions are relatively open to trade can have an important effect on the size of the fiscal multiplier through a leakage effect associated with movements in goods between regions. Our baseline calibration follows Nakamura and Steinsson (2011), as we set a_H to 0.69 in light of their evidence on goods shipments across US states. Moreover, we assume that households view goods from different US regions as being fairly substitutable, and set the elasticity of substitution to 4. Since there is a lot of uncertainty surrounding this parameter value empirically, we look at the robustness of our results to variation around this baseline calibration.

For the goods production function, we use a labor share of 70 percent. However, the range of empirical estimates of the output elasticity of public capital, ϕ_g, is very wide. In a review of the early estimates of this elasticity for the United States, Munnell (1992) reports the findings of nine studies, with estimates ranging between 0.05 and 0.4. While we set $\phi_g = 0.1$ in our baseline model to facilitate comparison with other studies (e.g., Baxter and King 1993 and Leeper, Walker, and Yang 2010), we also experiment with different values given this uncertainty. In particular, we examine the change in the fiscal multiplier when public capital is unproductive; that is, $\phi_g = 0$.

We calibrate the steady-state share of government purchases in output to 0.3 percent in line with the 1993 to 2010 average value across states in our data set. We think of infrastructure spending as being authorized for five years, the same duration as the SAFETEA-LU bill covering 2005 through 2009 (inclusive), but less than the previous two bills that both lasted six years. Because implementation lags make the concept of obligations more meaningful for economic activity than that of outlays, we use the implementation lags between grants and obligations estimated in table 1 to calibrate the spend-out rate ω in equation (13). Thus, 70 percent of grant apportionments are obligated in the current year and 30 percent in the following one.

The construction of new highways takes a very long time. The Government Accountability Office (GAO) reports that typical new highway construction projects take between nine to nineteen years from planning to completion (see GAO 2002). However, new highway construction projects constitute only about 3 percent of federally funded projects. Although most of the spending in highway bills is directed toward road improvement and maintenance instead of the construction of new roads, the GAO nonetheless reports that most such projects necessitate between four to six years before being completed. Based on this assessment, we assume that the time-to-build process in equation 14) takes four years ($J = 4$). We also set the depreciation rate of the public and private capital stocks to 10 percent per year. This parameterization of the depreciation rate of the public capital stock is broadly in line with the range of FHWA estimates of road pavement, which has an average life duration of fifteen to thirty years depending on the type of road, quality of pavement, and traffic.[32]

The probability that firms update their prices is chosen such that prices are on average fixed for four quarters. The coefficients in the interest-rate rule are set to the following values—$\rho_R = 0.8$, $\beta_p = 1.5$, and $\beta_y = 0.5$, though monetary policy will not affect our estimates of the local multiplier as it will be differenced out.

Finally, we set the persistence of the shocks to apportionments to 0.27, a value consistent with regressing states' highway grants on one lag, as well as state and time fixed effects for the period covered by our data set. Thus a shock essentially dies out after four years, which is also consistent with the response of highway grants to our shock measure in figure 5. Throughout our exercises, we look at the effect of 1 percent shocks to government spending.

E. Findings

In this section, we examine the theoretical analog to our empirical multiplier. As in section IV, we apply Jordà's (2005) direct projection method on our simulated data. Specifically, we calculate the multiplier as a regression of the logarithm of regional output on its first three lags and on the logarithm of shocks to regional public investment with state and time fixed effects.[33] Figure 9 reports our theoretical estimate of the dynamic output multiplier in our baseline model. The figure shows that the path of the multiplier follows a pattern similar to the empirical one in figure 3. The multiplier rises on impact before falling back for two years, at which point it increases again and peaks around eight to nine years, then starts to decline over time. We find the peak multiplier to be slightly below 2, but the impact multiplier to be much smaller and closer to 0.3, which contrasts with the data where both the impact and the peak multipliers are considerably larger.

The top two charts in figure 10 indicate that this dynamic pattern of

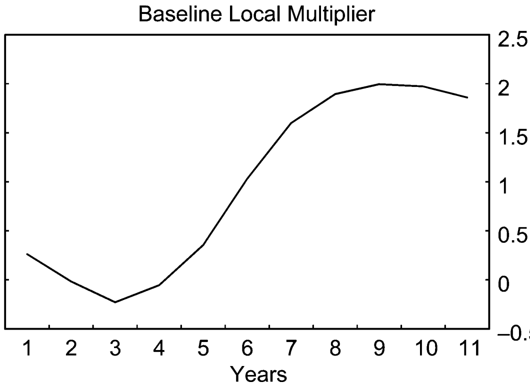

Fig. 9. Responses to a home increase in public spending

the output multiplier is due to a combination of the persistence of the shock, the presence of a time-to-build process of four years for public capital, and price rigidities. For instance, the multiplier rises monotonically for ten years when we increase the persistence of the shocks from 0.27 to 0.8. Similarly, absent time-to-build, the path of the multiplier is hump-shaped, peaking sooner as the public capital stock is available for production earlier. Moreover, the impact multiplier is roughly zero in the model with flexible prices (not shown), since time fixed effects remove the negative wealth effect of current or future increases in federal consumption taxes that would otherwise boost labor supply and output.[34]

Intuitively, in our baseline calibration, the initial increase in economic activity triggered by the rise in government spending fades away as government spending quickly declines. At that point, new public infrastructures have yet to be completed. When the new infrastructure is in place around year $t + 4$ and becomes available for production, the economy's productivity increases, boosting real wages, hours worked, and investment. As a result, output rises once again.

The remaining four charts in figure 10 assess the robustness of our baseline results to the different features of our model. The middle left panel considers different values of the output elasticity of public capital, clearly a crucial parameter in our analysis. While the movements in the multiplier are similar with a lower value for that elasticity ($\phi_g = 0.05$), the peak multiplier is roughly halved. Interestingly, our methodology correctly predicts the absence of a second increase in output when government spending is unproductive ($\phi_g = 0$). Overall, we find

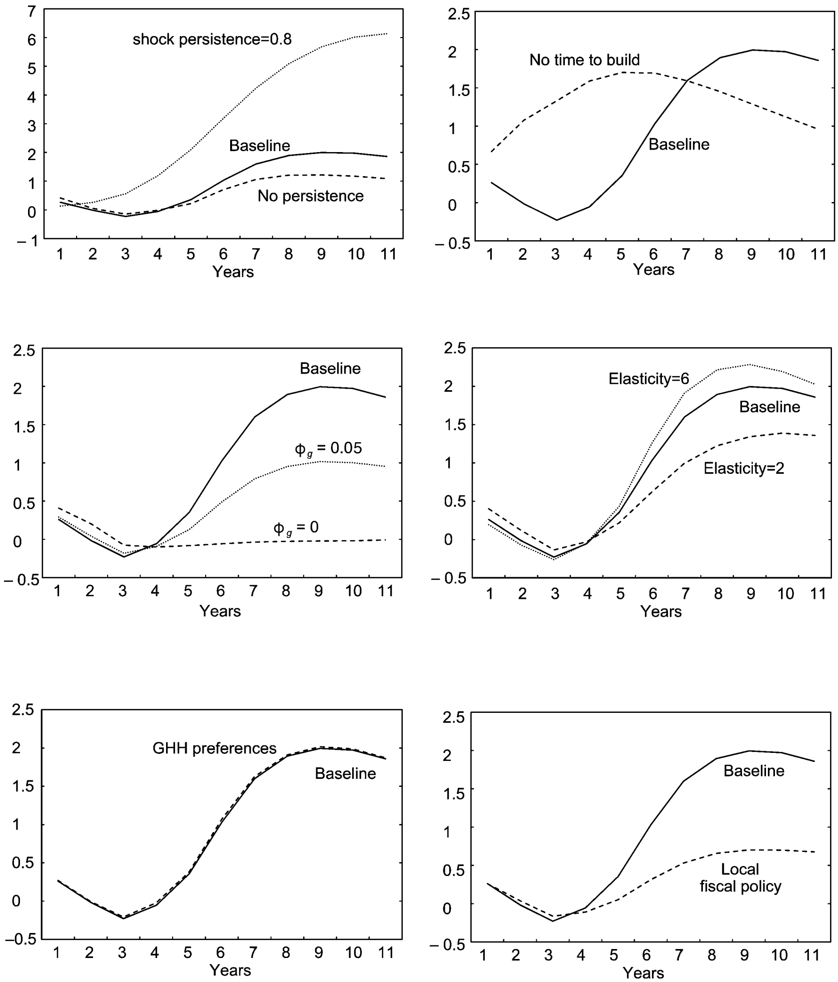

Fig. 10. Theoretical multipliers

it reassuring that the direct projection method is able to clearly distinguish between frameworks.

The degree to which goods in the two regions are substitutable also affects the size of the output multiplier, as indicated in the middle right panel of figure 10. In the longer run, greater goods substitutability leads to a higher multiplier, as cheaper goods resulting from the increased productive capacity of the economy can more easily be exported. The reverse is true initially, since government spending has yet to boost the productive capacity of the economy, and the innovation to government spending operates like a standard demand shock in that case. As a result, lower goods substitutability across regions boosts the multiplier, as there is less leakage to the other region. The bottom left panel of figure 10 also shows that introducing complementarities between consumption and hours worked in household preferences push the path of the multiplier up, but that the effect is relatively muted in our model.

As discussed in section II, an important aspect of the federal-aid highway program is that states are required to finance about 20 percent of the federal-aid highway projects. This introduces important fiscal aspects, as nearly all states have balanced budget requirements and must therefore either increase tax revenues or cut spending to pay for the funds necessary to have access to federal grants. This is an important issue, since changes in local fiscal policy will not be differenced out using our approach, contrary to changes in federal fiscal policy. In the following exercise, we assume that regional governments levy local consumption taxes to pay for financing 20 percent of the cost of federal-aid infrastructure projects, as well as their own infrastructure spending. We also assume that the local consumption tax rate is fixed to 5 percent.

We report the results of this exercise in the bottom right panel of figure 10. The chart shows that introducing local fiscal policy has an important effect on the size of the multiplier, reducing it significantly over longer horizons. This reflects the fact that, to finance 20 percent of federal infrastructure projects, local governments must decrease their own infrastructure spending to the extent that any increase in economic activity coming from the increased federal spending is insufficient to boost government revenues enough to cover this cost. Therefore, the contraction of local infrastructure spending partly offsets the effect of federal spending, which accounts for the lower multiplier in the longer run. Similar issues have been emphasized by Cogan and Taylor (2010) in their critique of the fiscal stimulus package of 2009.

In closing, we note that aggregate multipliers can be quite different from the local multipliers that our methodology is meant to measure, since they will also include effects related to national fiscal and monetary policies. Applying the direct projections method to a population weighted average of the two regions' output and spending shocks, we find the aggregate multiplier to be −0.14 on impact and 1.1 at its peak, significantly lower than our baseline results. However, these results will necessarily depend on the particular forms that fiscal and monetary policies are assumed to take.

VI. Concluding Remarks

This paper analyzed the dynamic economic effects of public infrastructure investment. The prior literature on dynamic fiscal multipliers generally has shied away from studying this type of government spending because of several unique and challenging features of public infrastructure investment related to identification, implementation lags, and forecastability.

Given these unique features, our paper utilized the institutional details of public highway spending in the United States. Many aspects of the institutional mechanism behind how federal highway funds are distributed to US states allow us both to avoid the potential pitfalls posed by the features above and to turn them to our advantage in providing strong identification of exogenous shocks to infrastructure spending. In particular, federal funds are distributed to states based on strict formulas, which are set many years in advance and make use of formula-factor data that are several years old, making these distributions exogenous with respect to current local economic conditions. Furthermore, we construct forecasts of these distributions based on information available to agents in the years prior to the distributions, and measure spending shocks as revisions in those forecasts.

Using these shocks to estimate dynamic panel regressions following the direct projections approach of Jordà (2005), we find that highway spending shocks positively affect GDP at two specific horizons. There is a significant impact in the first couple of years and then a larger second-round effect after six to eight years. The multipliers that we calculate from these impulse responses are large, between 1 and 3 on impact and between 3 and 7 at six to eight years out. Other estimates of local fiscal multipliers tend to be between 1 and 2.

We looked at three extensions that relate to the important current policy debate over the efficacy of countercyclical fiscal policy. Infrastructure spending, because it is perceived as being more productive (in the sense of increasing private sector productivity) than other types of spending, is often pointed to as an attractive form of Keynesian spending. However, critics argue that the long lags between increases in infrastructure funding and actual spending make it unlikely that such spending can provide short-run stimulus. The results in this paper can help inform this debate. We found that, on average over our 1993 to 2010 sample period, unanticipated funding increases in a given state boost GDP in the short run but do not boost employment. While the short-run GDP boost appears to be driven by funding shocks that occur during recessions, employment does not appear to rise even in this case. We also found that the short-run (and long-run) GDP effects of highway funding shocks are smaller for states whose GDP is growing slower than the median state. Overall, these results suggest that highway spending—at least the kind of highway spending typically done over the past twenty years—may not be well-suited to be an effective type of stimulus spending. On the other hand, we found that the highway funding shocks occurring during 2009, the year of the ARRA stimulus package as well as the trough of the Great Recession, had unusually large short-run impacts on GDP. A possible implication is that, on average, highway spending may not be especially effective at providing short-run stimulus, but that it can be more effective during times of very high economic slack and/or when monetary policy is at the zero lower bound.

In the final part of the paper, we used a theoretical framework to interpret our empirical findings. We looked at the multiplier in an open economy model with productive public capital in which states receive federal funds for infrastructure investment calibrated to capture the institutional framework of highway funding in the United States. Applying the direct projections method to our simulated data, we found that our empirical responses are qualitatively consistent with an initial effect due to nominal rigidities and a subsequent medium-term productivity effect that arises once the public capital is put in place and available for production. However, the magnitude of the multipliers coming out of our simulated data are smaller than those implied by our empirical impulse responses. One possible reason, suggested by our empirical finding that the impact multiplier only occurs for shocks during a recession, is that our model abstracts from important nonlinearities that cause cycle-dependent heterogeneity in the multiplier. Developing nonlinear general equilibrium models capable of yielding such cycle-dependent multipliers is a critical area for future research.

Appendix

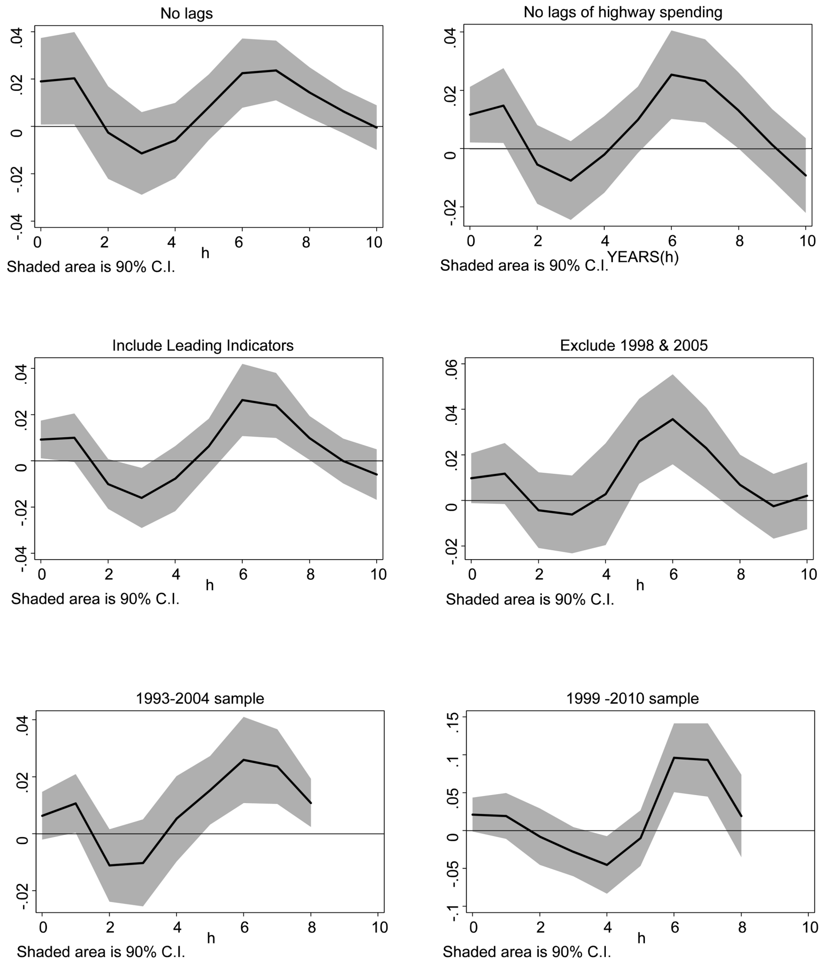

Fig. A1. Robustness checks

Endnotes

We thank Brian Lucking and Elliot Marks for superb and tireless research assistance. We are grateful to John Fernald, Bart Hobijn, Òscar Jordà, John Williams, and seminar attendees at the Federal Reserve Bank of San Francisco, the University of Nevada, and the SEEK/CEPR Workshop on "News, Sentiment, and Confidence in Fluctuations" for helpful comments. We thank the many transportation officials who improved our understanding of the institutional complexities of highway financing and spending, especially Ken Simonson (Associated General Contractors of America), Nancy Richardson (formerly of Iowa DOT), Jack Wells (US DOT), and Alison Black and William Buechner (both of

American Road and Transportation Builders Assn). Finally, we are grateful to the editors of the 2012 NBER Macroeconomic Annual for excellent guidance. The views expressed in this paper are solely the responsibility of the authors and should not be interpreted as reflecting the views of the Federal Reserve Bank of San Francisco, or of any other person associated with the Federal Reserve System. For acknowledgments, sources of research support, and disclosure of the authors' material financial relationships, if any, please see http://www.nber.org/chapters/c12750.ack.

1. Kraay (forthcoming) uses a related approach when looking at the effects of government spending in developing countries, appealing to the fact that spending on World Bank-financed projects is determined by project approval decisions made in previous years.

2. Local governments also spend a considerable amount on roads, though the vast majority of that spending is on minor residential roads (according to statistics from the Federal Highway Administration) that generally are not considered part of the nation's highway infrastructure.

3. The theoretical implications of these bureaucratic implementation lags have been analyzed by Leeper, Walker, and Young (2009) and others.

4. Ramey (2011a) notes that the difficulties may be especially severe with regard to highway spending: "One should be clear that timing is not an issue only with defense spending. Consider the interstate highway program. In early 1956, Business Week was predicting that the 'fight over highway building will be drawn out.' By May 5, 1956, Business Week thought that the highway construction bill was a sure bet. In fact it passed in June 1956. However, the multi-billion dollar program was intended to stretch out over 13 years. It is difficult to see how a VAR could accurately reflect this program" (20–21).

5. Ilzetzki, Mendoza, and Végh (2010) also apply the methodology of Blanchard and Perotti (2002) to look at the effects of fiscal shocks in countries other than the United States.

6. In addition to those discussed later, some notable examples using US regional or county-level data include Shoag (2010), Chodorow-Reich et al. (forthcoming), Feyrer and Sacerdote (2011), Conley and Dupor (2012), and Suarez Serrato and Wingender (2011). Likewise, Acconcia, Corsetti, and Simonelli (2011) use variations in public works across Italian provinces. Giavazzi and McMahon (2012) employ a similar approach by looking at the effects of government spending on households' behavior, using disaggregated household information from the Panel Study of Income Dynamics.

7. Our paper is also related to the long empirical literature on the contribution of public infrastructure capital to the productivity of the private economy (see, for instance, Aschauer 1989, Holtz-Eakin 1994, Fernald 1999, or Morrison and Schwartz 1996).

8. The US federal fiscal year begins October 1 of the prior calendar year. For instance, FY2012 runs from October 1, 2011, through September 30, 2012.

9. Transportation authorization acts since the Federal-Aid Highway Act of 1956 have been nominally financed by the Highway Trust Fund (HTF), which receives revenue from fuel, tire, and truck-related excise taxes. However, it is debatable whether the HTF actually plays much of a role in ultimately determining transportation funding levels. That is because there are instances (as in 2008) in which Congress has replenished the HTF from the general fund when the HTF was low, and there are instances in which Congress has taken funds from the HTF to add to the general fund (see FHWA 2007). That would suggest the HTF balance at a point in time is largely irrelevant to how much Congress authorizes for subsequent transportation spending.

10. See appendix B of FHWA (2007). Earmarks are funded by the High-Priority Projects Program.

11. We are unaware of prior research exploiting data on funding announcements and obligations to better measure the timing of government spending shocks, with the exception of Wilson (2012). Using as instruments formula factors used to distributed funds from the American Recovery and Reinvestment Act (ARRA) of 2009, Wilson estimated the employment effect of ARRA funds alternately based on announcements, obligations, and outlays. He found the results for announcements and obligations were similar, but that the estimated effect of ARRA funding based on outlays was much larger, likely because

a low level of outlays at a given point in time actually represents a much larger level of announcements or obligations, which are the true shocks to government spending.

12. The data on outlays by the FHWA to states are from the FHWA Highway Statistics for various years. See table FA-3, "Expenditure of Federal Funds Administered by the Federal Highway Administration During Fiscal Year."

13. Moreover, they do not know whether they or other states will be subject to the various minimum guarantees and equity bonuses discussed in section 2 and online appendix A, (http://www.nber.org/data-appendix/c12750/appendices.pdf), which will affect the distribution of grants among states.

14. Actually, our assumption is slightly weaker than that. We assume states that qualify for the minimum apportionment share (usually 0.5 percemt) for a given program continue to qualify, which allows for those states to experience changes in relative formula factors as long as the changes are not big enough to push the state above the minimum apportionment share.

15. Five-year-ahead forecasts are available in the SPF only from 2006 onward. Prior to 2006, we use the ten-year-ahead forecast. The two forecasts are very similar in the data.

16. We also tested this idea that political factors could affect our shocks if political influence sways the apportionment mechanisms adoption in new highway bills by regressing on shocks in 1998 and 2005 on the same political factors considered in Knight's (2002) study of the flypaper effect of highway grants. Our shocks are found to be uncorrelated with these political factors.

17. Specifically, the shock here is the symmetric percentage difference between year t grants and the forecast of those grants as of last year: $(A_{i,t} - E_{t-1}[A_{i,t}])/(A_{i,t} + E_{t-1}[A_{i,t}])$.

18. In addition to these two, we explored some other alternative identification strategies as well (results not shown, but available upon request). First, we estimated equation (4), but replaced our highway grant shock with current federal-aid obligations and instrumented for obligations with current and four lags of actual grants. Similar to the SVAR-type identification, discussed above, identification here relies on the assumption that a state's grants (relative to the nation's)—being driven by formula factors that are determined three years earlier and only loosely related to GDP—are exogenous with respect to current and future GDP. Again, the drawback of this approach is that it ignores anticipation effects. We find that the IRF from this IV estimation gives very similar results to that based on simply using current grants as in Panel C.

19. Data on the first four of these variables comes from the Bureau of Economic Analysis (BEA). We also estimated an IRF based on employment count data from the Bureau of Labor Statistics (BLS) and obtained virtually identical results. Data on unemployment was obtained from the BLS, while data on population comes from the Census Bureau.

20. The lack of a positive employment response on impact might be surprising given the estimated increase in output, but road construction is a very capital intensive activity with labor accounting for at most 8 percent of the total production costs (see FHWA Highway Statistics 2008: http://www.fhwa.dot.gov/policyinformation/statistics/2008/pt2.cfm).

21. We thank Chris Carroll and Xia Zhou for providing their state-by-year data on retail sales (see Zhou and Carroll 2012). Unfortunately, state-level data on overall consumption (beyond extrapolations from retail sales) are not available.

22. The impact and peak impulse response coefficients are 0.0115 and 0.0259, as seen in table 3. The mean response from the impulse response coefficients in table 3 is 0.0055. The cumulative percent response of grants to a one unit change in our shock is roughly 1, and the average ratio of state GDP to grants is about 300. So the implied impact multiplier is the estimated GDP IRF coefficient, 0.0115, times 300, which equals 3.4.

23. The recent literature on the flypaper effect of federal grants has found mixed results. Studies by Baicker (2001), Evans and Owens (2005), Singhal (2008), and Feiveson (2011) find evidence of strong flypaper effects across a variety of spending categories. However, Knight (2002) and Gordon (2004) find the opposite.

24. The cumulative percent response of this variable to our shock also is close to one, and the average ratio of GDP to highway construction spending is 238.

25. The difficulty in estimating the response of total state government road spending to

a shock in current and future grants likely stems from the fact that, while data on outlays exist, data on obligations do not. As we pointed out in section II, outlays represent a poor measure of actual roadwork and related activities. If obligations data existed, this would allow an instrumental variables strategy for calculating the multiplier. Specifically, one could replace the shock in equation (4) with obligations and instrument for obligations using the shock. One could then multiply the resulting IV coefficient by the ratio of GDP to obligations to obtain the multiplier on an exogenous shift in state road obligations.

26. To avoid potential simultaneity bias from the fact that the expansion probability will be contemporaneously correlated with the dependent variable (log output), we follow Auerbach and Gorodnichenko (2011) in lagging the expansion probability by one year.

27. See Ramey (2011b) for a critique of that assumption.

28. We also looked at whether the employment IRF is different for expansions versus recessions. The impact effect was small and insignificant for both, while the peak effect was slightly larger for expansions.

29. For convenience, we do not index variables by households.

30. In practice, the revenues of the HTF are derived from excise taxes collected on motor fuel and truck-related taxes. For simplicity, we proxy those taxes with a general consumption tax.

31. Studying optimal taxation in a model with productive capital, Lansing (1998) assumes a production function with constant returns to scale. Moreover, we abstract from issues related to congestion of public goods. On this question, see the work of Glomm and Ravikumar (1994).

32. See table 5.6 of FHWA's "Highway Economic Requirement System—State Version: Technical Report," that can be found at http://www.fhwa.dot.gov/asset/hersst/pubs/tech/tech05.cfm.

33. We abstracted from lags of government spending since the spending shock in our simulated data is, by construction, exogenous with respect to lagged output or spending. As we documented earlier, our empirical results are robust to removing lags of the dependent and independent variables in the regression.

34. Note, however, that the positive (regional) wealth effect of future increases in output is not taken out by the introduction of time fixed effects. Ceteris paribus, this will tend to lower labor supply and output in the region experiencing an increase in public infrastructure spending.

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